British Association for the Advancement of Science

ADDRESSES

1912

J. H. Cameron

Dundee, 5th Sept: 1912.

307, Sherbourne St.
Toronto

British Association for the Advancement of Science.

DUNDEE MEETING, 1912

THE PRESIDENT'S ADDRESS

AND THE

SECTIONAL ADDRESSES

LONDON:
OFFICES OF THE ASSOCIATION,
BURLINGTON HOUSE, W.

Price Two Shillings.

British Association for the Advancement of Science.

DUNDEE, 1912.

ADDRESS

BY

PROFESSOR E. A. SCHÄFER, LL.D., D.Sc., M.D., F.R.S.,

PRESIDENT.

Introductory.

IT is exactly forty-five years ago—to the day and hour—that the British Association last met in this city and in this hall to listen to a Presidential Address. The President was the Duke of Buccleugh; the General Secretaries, Francis Galton and T. Archer Hirst; the General Treasurer, William Spottiswoode; and the Assistant General Secretary, George Griffith, who was for many years a mainstay of the Association. The Evening Discourses were delivered by John Tyndall 'On Matter and Force,' by Archibald Geikie 'On the Geological Origin of the Scenery of Scotland,' and by Alexander Herschel 'On the Present State of Knowledge regarding Meteors and Meteorites.' The Presidents of Sections, which were then only seven in number, were for Mathematics and Physics, Sir William Thomson—later to be known as Lord Kelvin; for Chemistry, Thomas Anderson; for Geology, Archibald Geikie, who now as President of the Royal Society worthily fills the foremost place in science within the realm; for Biology, William Sharpey, my own revered master, to whose teaching and influence British physiology largely owes the honourable position which it at present occupies; for Geography, Sir Samuel Baker, the African explorer, who with his intrepid wife was the first to follow the Nile to its exit from the Albert Nyanza; for Economic Science, Mr. Grant Duff; and for Mechanical Science, Professor Rankine.

Other eminent men present were Sir David Brewster, J. Clerk Maxwell, Charles Wheatstone, Balfour Stewart, William Crookes, J. B. Lawes and J. H. Gilbert (names inseparable in the history of agricultural science), Crum Brown, G. D. Liveing, W. H. Russell, Alexander Williamson, Henry Alleyne Nicholson, William Allmann, John Hutton Balfour, Spencer Cobbold, Anton Dohrn, Sir John Lubbock (now Lord Avebury), William McIntosh, E. Ray Lankester,

C. W. Peach, William Pengelly, Hughes Bennett, John Cleland, John Davy, Alexander Christison, Alfred Russel Wallace, Allen Thomson, William Turner, George Busk, Michael Foster (not yet founder of the Cambridge School of Physiology), Henry Howorth, Sir Roderick Murchison, Clements R. Markham, Sir William (afterwards Lord) Armstrong, and Douglas Galton. Many of those enumerated have in the course of nature passed away from us, but not a few remain, and we are glad to know that most of these retain their ancient vigour in spite of the five-and-forty years which separate us from the last meeting in this place.

Selection of Subject of Address.

For the Address with which it is usual for the President to open the proceedings of the annual assembly, the field covered by the aims of the British Association provides the widest possible range of material from which to select. One condition alone is prescribed by custom, viz., that the subject chosen shall lie within the bounds of those branches of knowledge which are dealt with in the Sections. There can be no ground of complaint regarding this limitation on the score of variety, for within the forty years that I have myself been present (not, I regret to say, without a break) at these gatherings, problems relating to the highest mathematics on the one hand, and to the most utilitarian applications of science on the other, with every possible gradation between these extremes, have been discussed before us by successive Presidents; and the addition from time to time of new Sections (one of which, that of Agriculture, we welcome at this Meeting) enables the whilom occupant of this chair to traverse paths which have not been previously trodden by his predecessors. On the last two occasions, under the genial guidance of Professors Bonney and Sir William Ramsay, we have successively been taken in imagination to the glaciers which flow between the highest peaks of the Alps and into the bowels of the earth; where we were invited to contemplate the prospective disappearance of the material upon which all our industrial prosperity depends. Needless to say that the lessons to be drawn from our visits to those unaccustomed levels were placed before us with all the eloquence with which these eminent representatives of Geology and Chemistry are gifted. It is fortunately not expected that I should be able to soar to such heights or to plunge to such depths, for the branch of science with which I am personally associated is merely concerned with the investigation of the problems of living beings, and I am able to invite you to remain for an hour or so at the level of ordinary mortality to consider certain questions which at any rate cannot fail to have an immediate interest for every one present, seeing that they deal with the nature, origin, and maintenance of life.

Everybody knows, or thinks he knows, what life is; at least, we are

Definition.

all acquainted with its ordinary, obvious manifestations. It would, therefore, seem that it should not be difficult to find an exact definition. The quest has nevertheless baffled the most acute thinkers. Herbert Spencer devoted two chapters of his 'Principles of Biology' to the discussion of the attempts at definition which had up to that date been proposed, and himself suggested another. But at the end of it all he is constrained to admit that no expression had been found which would embrace all the known manifestations of animate, and at the same time exclude those of admittedly inanimate, objects.

The ordinary dictionary definition of life is 'the state of living.' Dastre, following Claude Bernard, defines it as 'the sum total of the phenomena common to all living beings.'[1] Both of these definitions are, however, of the same character as Sydney Smith's definition of an archdeacon as 'a person who performs archidiaconal functions.' I am not myself proposing to take up your time by attempting to grapple with a task which has proved too great for the intellectual giants of philosophy, and I have the less disposition to do so because recent advances in knowledge have suggested the probability that the dividing line between animate and inanimate matter is less sharp than it has hitherto been regarded, so that the difficulty of finding an inclusive definition is correspondingly increased.

Life not identical with soul.

As a mere word 'life' is interesting in the fact that it is one of those abstract terms which has no direct antithesis; although probably most persons would regard 'death' in that light. A little consideration will show that this is not the case. 'Death' implies the pre-existence of 'life'; there are physiological grounds for regarding death as a phenomenon of life—it is the completion, the last act of life. We cannot speak of a non-living object as *possessing* death in the sense that we speak of a living object as *possessing* life. The adjective 'dead' is, it is true, applied in a popular sense antithetically to objects which have never possessed life; as in the proverbial expression 'as dead as a door-nail.' But in the strict sense such application is not justifiable, since the use of the terms dead and living implies either in the past or in the present the possession of the recognised properties of living matter. On the other hand, the expressions *living* and *lifeless*, *animate* and *inanimate*, furnish terms which are undoubtedly antithetical. Strictly and literally, the words animate and inanimate express the presence or absence of 'soul'; and not infrequently we find the terms 'life' and 'soul' erroneously employed as if identical. But it is hardly necessary for me to state that the remarks I have to make regarding 'life' must not be taken to apply to the conception to which the word 'soul' is attached.

the breath of life

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[1] *La vie et la mort,* English translation by W. J. Greenstreet, 1911, p. 54.

Problems of life are problems of matter.

The fact that the formation of such a conception is only possible in connection with life, and that the growth and elaboration of the conception has only been possible as the result of the most complex processes of life in the most complex of living organisms, has doubtless led to a belief in the identity of life with soul. But unless the use of the expression ‘soul’ is extended to a degree which would deprive it of all special significance, the distinction between these terms must be strictly maintained. For the problems of life are essentially problems of matter; we cannot conceive of life in the scientific sense as existing apart from matter. The phenomena of life are investigated, and can only be investigated, by the same methods as all other phenomena of matter, and the general results of such investigations tend to show that living beings are governed by laws identical with those which govern inanimate matter. The more we study the manifestations of life the more we become convinced of the truth of this statement and the less we are disposed to call in the aid of a special and unknown form of energy to explain those manifestations.

Phenomena indicative of life: Movement.

The most obvious manifestation of life is ‘spontaneous’ movement. We see a man, a dog, a bird move, and we know that they are alive. We place a drop of pond water under the microscope, and see numberless particles rapidly moving within it; we affirm that it swarms with ‘life.’ We notice a small mass of clear slime changing its shape, throwing out projections of its structureless substance, creeping from one part of the field of the microscope to another. We recognise that the slime is living; we give it a name—*Amœba limax*—the slug amœba. We observe similar movements in individual cells of our own body; in the white corpuscles of our blood, in connective tissue cells, in growing nerve cells, in young cells everywhere. We denote the similarity between these movements and those of the amœba by employing the descriptive term ‘amœboid’ for both. We regard such movements as indicative of the possession of ‘life’; nothing seems more justifiable than such an inference.

Similarity of movements in living and non-living matter.

But physicists[2] show us movements of a precisely similar character in substances which no one by any stretch of imagination can regard as living; movements of oil drops, of organic and inorganic mixtures, even of mercury globules, which are indistinguishable in their character from those of the living organisms we have been studying: movements which can only be described by the same term amœboid, yet obviously produced as the result of purely physical and chemical reactions causing changes in surface tension of the fluids under exami-

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[2] G. Quincke, *Annal. d. Physik u. Chem.* 1870 and 1888.

nation.[3] It is therefore certain that such movements are not specifically 'vital,' that their presence does not necessarily denote 'life.' And when we investigate closely even such active movements as those of a vibratile cilium or a phenomenon so closely identified with life as the contraction of a muscle, we find that these present so many analogies with amœboid movements as to render it certain that they are fundamentally of the same character and produced in much the same manner.[4] Nor can we for a moment doubt that the complex actions which are characteristic of the more highly differentiated organisms have been developed in the course of evolution from the simple movements characterising the activity of undifferentiated protoplasm; movements which can themselves, as we have seen, be perfectly imitated by non-living material. The chain of evidence regarding this particular manifestation of life—movement—is complete. Whether exhibited as the amœboid movement of the proteus animalcule or of the white corpuscle of our blood; as the ciliary motion of the infusorian or of the ciliated cell; as the contraction of a muscle under the governance of the will, or as the throbbing of the human heart responsive to every emotion of the mind, we cannot but conclude that it is alike subject to and produced in conformity with the general laws of matter, by agencies resembling those which cause movements in lifeless material.[5]

It will perhaps be contended that the resemblances between the movements of living and non-living matter may be only superficial, and that the conclusion regarding their identity to which we are led will be dissipated when we endeavour to penetrate more deeply into the working of living substance. For can we not recognise along with the possession of movement the presence of other phenomena which are equally characteristic of life and with which non-living material is not endowed? Prominent among the characteristic phenomena of life are the processes of assimilation and disassimilation, the taking in of food and its elabora-

Assimilation and disassimilation.

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[3] The causation not only of movements but of various other manifestations of life by alterations in surface tension of living substance is ably dealt with by A. B. Macallum in a recent article in Asher and Spiro's *Ergebnisse der Physiologie*, 1911. Macallum has described an accumulation of potassium salts at the more active surfaces of the protoplasm of many cells, and correlates this with the production of cell-activity by the effect of such accumulation upon the surface tension. The literature of the subject will be found in this article.

[4] G. F. Fitzgerald (*Brit. Assoc. Reports*, 1898, and *Scient. Trans. Roy. Dublin Society*, 1898) arrived at this conclusion with regard to muscle from purely physical considerations.

[5] 'Vital spontaneity, so readily accepted by persons ignorant of biology, is disproved by the whole history of science. Every vital manifestation is a response to a stimulus, a provoked phenomenon. It is unnecessary to say this is also the case with brute bodies, since that is precisely the foundation of the great principle of the inertia of matter. It is plain that it is also as applicable to living as to inanimate matter.'—Dastre, *op. cit.*, p. 280.

tion.[6] These, surely, it may be thought, are not shared by matter which is not endowed with life. Unfortunately for this argument, similar processes occur characteristically in situations which no one would think of associating with the presence of life. A striking example of this is afforded by the osmotic phenomena presented by solutions separated from one another by semipermeable membranes or films, a condition which is precisely that which is constantly found in living matter.[7]

Chemical phenomena accompanying life.

It is not so long ago that the chemistry of organic matter was thought to be entirely different from that of inorganic substances. But the line between inorganic and organic chemistry, which up to the middle of the last century appeared sharp, subsequently became misty and has now disappeared. Similarly the chemistry of living organisms, which is now a recognised branch of organic chemistry, but used to be considered as so much outside the domain of the chemist that it could only be dealt with by those whose special business it was to study 'vital' processes, is passing every day more out of the hands of the biologist and into those of the pure chemist.

The colloid constitution of living matter. Identity of physical and chemical processes in living and non-living matter.

Somewhat more than half a century ago Thomas Graham published his epoch-making observations relating to the properties of matter in the colloidal state: observations which are proving all-important in assisting our comprehension of the properties of living substance. For it is becoming every day more apparent that the chemistry and physics of the living organism are essentially the chemistry and physics of nitrogenous colloids. Living substance or protoplasm always, in fact, takes the form of a colloidal solution. In this solution the colloids are associated with crystalloids (electrolytes), which are either free in the solution or attached to the molecules of the colloids. Surrounding and enclosing the living substance thus constituted of both colloid and crystalloid material is a film, probably also formed of colloid, but which may have a lipoid substratum associated with it (Overton). This film serves the purpose of an osmotic membrane, permitting of exchanges by diffusion between the colloidal solution constituting the protoplasm

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[6] The terms 'assimilation' and 'disassimilation' express the physical and chemical changes which occur within protoplasm as the result of the intake of nutrient material from the circumambient medium and its ultimate transformation into waste products which are passed out again into that medium; the whole cycle of these changes being embraced under the term 'metabolism.'

[7] Leduc (*The Mechanism of Life*, English translation by W. Deane Butcher, 1911) has given many illustrations of this statement. In the Report of the meeting of 1867 in Dundee is a paper by Dr. J. D. Heaton (On Simulations of Vegetable Growths by Mineral Substances) dealing with the same class of phenomena. The conditions of osmosis in cells have been especially studied by Hamburger (*Osmotischer Druck und Ionenlehre*, Wiesbaden, 1902-4).

and the circumambient medium in which it lives. Other similar films or membranes occur in the interior of protoplasm. These films have in many cases specific characters, both physical and chemical, thus favouring the diffusion of special kinds of material into and out of the protoplasm and from one part of the protoplasm to another. It is the changes produced under these physical conditions, associated with those caused by active chemical agents formed within protoplasm and known as *enzymes*, that effect assimilation and disassimilation. Quite similar changes can be produced outside the body (*in vitro*) by the employment of methods of a purely physical and chemical nature. It is true that we are not yet familiar with all the intermediate stages of transformation of the materials which are taken in by a living body into the materials which are given out from it. But since the initial processes and the final results are the same as they would be on the assumption that the changes are brought about in conformity with the known laws of chemistry and physics, we may fairly conclude that all changes in living substance are brought about by ordinary chemical and physical forces.

Similarity of the processes of growth and reproduction in living and non-living matter.

Should it be contended that growth and reproduction are properties possessed only by living bodies and constitute a test by which we may differentiate between life and non-life, between the animate and inanimate creation, it must be replied that no contention can be more fallacious. Inorganic crystals grow and multiply and reproduce their like, given a supply of the requisite pabulum. In most cases for each kind of crystal there is, as with living organisms, a limit of growth which is not exceeded, and further increase of the crystalline matter results not in further increase in size but in multiplication of similar crystals. Leduc has shown that the growth and division of artificial colloids of an inorganic nature, when placed in an appropriate medium, present singular resemblances to the phenomena of the growth and division of living organisms. Even so complex a process as the division of a cell-nucleus by karyokinesis as a preliminary to the multiplication of the cell by division—a phenomenon which would *primâ facie* have seemed and has been commonly regarded as a distinctive manifestation of the life of the cell—can be imitated with solutions of a simple inorganic salt, such as chloride of sodium, containing a suspension of carbon particles; which arrange and rearrange themselves under the influence of the movements of the electrolytes in a manner indistinguishable from that adopted by the particles of chromatin in a dividing nucleus. And in the process of sexual reproduction, the researches of J. Loeb and others upon the ova of the sea-urchin have proved that we can no longer consider such an apparently vital phenomenon as the fertilisation of the egg as being the result of living

The question of vitalism and vital force.

material brought to it by the spermatozoon, since it is possible to start the process of division of the ovum and the resulting formation of cells, and ultimately of all the tissues and organs—in short, to bring about the development of the whole body—if a simple chemical reagent is substituted for the male element in the process of fertilisation. Indeed, even a mechanical or electrical stimulus may suffice to start development. *Kurz und gut*, as the Germans say, vitalism as a working hypothesis has not only had its foundations undermined, but most of the superstructure has toppled over, and if any difficulties of explanation still persist, we are justified in assuming that the cause is to be found in our imperfect knowledge of the constitution and working of living material. At the best vitalism explains nothing, and the term 'vital force' is an expression of ignorance which can bring us no further along the path of knowledge. Nor is the problem in any way advanced by substituting for the term 'vitalism' 'neo-vitalism,' and for 'vital force' 'biotic energy.'[8] 'New presbyter is but old priest writ large.'

The possibility of the synthesis of living matter.

Further, in its chemical composition we are no longer compelled to consider living substance as possessing infinite complexity, as was thought to be the case when chemists first began to break up the proteins of the body into their simpler constituents. The researches of Miescher, which have been continued and elaborated by Kossel and his pupils, have acquainted us with the fact that a body so important for the nutritive and reproductive functions of the cell as the nucleus—which may be said indeed to represent the quintessence of cell-life—possesses a chemical constitution of no very great complexity; so that we may even hope some day to see the material which composes it prepared synthetically. And when we consider that the nucleus is not only itself formed of living substance, but is capable of causing other living substance to be built up; is, in fact, the directing agent in all the principal chemical changes which take place within the living cell, it must be admitted that we are a long step forward in our knowledge of the chemical basis of life. That it is the *form* of nuclear matter rather than its chemical and molecular structure which is the important factor in nuclear activity cannot be supposed. The form of nuclei, as every microscopist knows, varies infinitely, and there are numerous living organisms in which the nuclear matter is without form, appearing simply as granules distributed in the protoplasm. Not that the form assumed and the transformations undergone by the nucleus are without import-

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[8] B. Moore, in *Recent Advances in Physiology*, 1906; Moore and Roaf, *ibid.*; and *Further Advances in Physiology*, 1909. Moore lays especial stress on the transformations of energy which occur in protoplasm. See on the question of vitalism Gley (*Revue Scientifique*, 1911) and D'Arcy Thompson (Address to Section D at Portsmouth, 1911).

ance; but it is none the less true that even in an amorphous condition the material which in the ordinary cell takes the form of a 'nucleus' may, in simpler organisms which have not in the process of evolution become complete cells, fulfil functions in many respects similar to those fulfilled by the nucleus of the more differentiated organism.

A similar anticipation regarding the probability of eventual synthetic production may be made for the proteins of the cell-substance. Considerable progress in this direction has indeed already been made by Emil Fischer, who has for many years been engaged in the task of building up the nitrogenous combinations which enter into the formation of the complex molecule of protein. It is satisfactory to know that the significance of the work both of Fischer and of Kossel in this field of biological chemistry has been recognised by the award to each of these distinguished chemists of a Nobel prize.

The chemical constitution of living substance.

The elements composing living substance are few in number. Those which are constantly present are carbon, hydrogen, oxygen, and nitrogen. With these, both in nuclear matter and also, but to a less degree, in the more diffuse living material which we know as protoplasm, phosphorus is always associated. 'Ohne Phosphor kein Gedank' is an accepted aphorism; 'Ohne Phosphor kein Leben' is equally true. Moreover, a large proportion, rarely less than 70 per cent., of water appears essential for any manifestation of life, although not in all cases necessary for its continuance, since organisms are known which will bear the loss of the greater part if not the whole of the water they contain without permanent impairment of their vitality. The presence of certain inorganic salts is no less essential, chief amongst them being chloride of sodium and salts of calcium, magnesium, potassium, and iron. The combination of these elements into a colloidal compound represents the chemical basis of life; and when the chemist succeeds in building up this compound it will without doubt be found to exhibit the phenomena which we are in the habit of associating with the term 'life.'[9]

Source of life. The possibility of spontaneous generation.

The above considerations seem to point to the conclusion that the possibility of the production of life—*i.e.*, of living material—is not so remote as has been generally assumed. Since the experiments of Pasteur, few have ventured to affirm a belief in the spontaneous generation of bacteria and monads and other micro-organisms, although before his time this was by many believed to be of universal occurrence. My esteemed friend Dr. Charlton Bastian is, so far as I am aware, the only scientific man of eminence who still adheres to the old creed, and Dr. Bastian, in spite of numerous experiments and the publication of many

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[9] The most recent account of the chemistry of protoplasm is that by Botazzi (Das Cytoplasma u. die Körpersäfte) in Winterstein's *Handb. d. vergl. Physiologie*, Bd. I., 1912. The literature is given in this article.

books and papers, has not hitherto succeeded in winning over any converts to his opinion. I am myself so entirely convinced of the accuracy of the results which Pasteur obtained—are they not within the daily and hourly experience of everyone who deals with the sterilisation of organic solutions?—that I do not hesitate to believe, if living torulae or mycelia are exhibited to me in flasks which had been subjected to prolonged boiling after being hermetically sealed, that there has been some fallacy either in the premisses or in the carrying out of the operation. The appearance of organisms in such flasks would not furnish to my mind proof that they were the result of spontaneous generation. Assuming no fault in manipulation or fallacy in observation, I should find it simpler to believe that the germs of such organisms have resisted the effects of prolonged heat than that they became generated spontaneously. If spontaneous generation is possible, we cannot expect it to take the form of living beings which show so marked a degree of differentiation, both structural and functional, as the organisms which are described as making their appearance in these experimental flasks.[10] Nor should we expect the spontaneous generation of living substance of any kind to occur in a fluid the organic constituents of which have been so altered by heat that they can retain no sort of chemical resemblance to the organic constituents of living matter. If the formation of life—of living substance—is possible at the present day—and for my own part I see no reason to doubt it—a boiled infusion of organic matter—and still less of inorganic matter—is the last place in which to look for it. Our mistrust of such evidence as has yet been brought forward need not, however, preclude us from admitting the possibility of the formation of living from non-living substance.[11]

Setting aside, as devoid of scientific foundation, the idea of immediate

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[10] It is fair to point out that Dr. Bastian suggests that the formation of ultramicroscopic living particles may precede the appearance of the microscopic organisms which he describes. *The Origin of Life*, 1911, p. 65.

[11] The present position of the subject is succinctly stated by Dr. Chalmers Mitchell in his article on 'Abiogenesis' in the *Encyclopædia Britannica*. Dr. Mitchell adds: 'It may be that in the progress of science it may yet be possible to construct living protoplasm from non-living material. The refutation of abiogenesis has no further bearing on this possibility than to make it probable that if protoplasm ultimately be formed in the laboratory, it will be by a series of steps, the earlier steps being the formation of some substance, or substances, now unknown, which are not protoplasm. Such intermediate stages may have existed in the past.' And Huxley in his Presidential Address at Liverpool in 1870 says: 'But though I cannot express this conviction' (*i.e.*, of the impossibility of the occurrence of abiogenesis, as exemplified by the appearance of organisms in hermetically sealed and sterilised flasks) 'too strongly, I must carefully guard myself against the supposition that I intend to suggest that no such thing as abiogenesis ever has taken place in the past or ever will take place in the future. With organic chemistry, molecular physics and physiology yet in their infancy and every day making prodigious strides, I think it would be the height of presumption for any man to say that the conditions under which matter assumes the properties we call "vital" may not, some day, be artificially brought together.'

Life a product of evolution.

supernatural intervention in the first production of life, we are not only justified in believing, but compelled to believe, that living matter must have owed its origin to causes similar in character to those which have been instrumental in producing all other forms of matter in the universe; in other words, to a process of gradual evolution.[12] But it has been customary of late amongst biologists to shelve the investigation of the mode of origin of life by evolution from non-living matter by relegating its solution to some former condition of the earth's history, when, it is assumed, opportunities were accidentally favourable for the passage of inanimate matter into animate; such opportunities, it is also assumed, having never since recurred and being never likely to recur.[13]

Various eminent scientific men have even supposed that life has not actually originated upon our globe, but has been brought to it from another planet or from another stellar system. Some of my audience may still remember the controversy that was excited when the theory of the origin of terrestrial life by the intermediation of a meteorite was propounded by Sir William Thomson in his Presidential Address at the meeting of this Association in Edinburgh in 1871. To this 'meteorite' theory[14] the apparently fatal objection was raised that it would take some sixty million years for a meteorite to travel from the nearest stellar system to our earth, and it is inconceivable that any kind of life could be maintained during such a period. Even from the nearest planet 150 years would be necessary, and the heating of the meteorite in passing through our atmosphere and at its impact with the earth would, in all probability, destroy any life which might have existed within it. A cognate theory, that of *cosmic panspermia*, assumes that life may exist and may have existed indefinitely in cosmic dust in the interstellar spaces (Richter, 1865; Cohn, 1872), and may with this dust fall slowly to the earth without undergoing the heating which is experienced by a meteorite. Arrhenius,[15] who adopts this theory, states that if living germs were carried through the ether by luminous and other radiations the time necessary for their transportation from our globe to the nearest stellar system would be only nine thousand years, and to Mars only twenty days!

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[12] The arguments in favour of this proposition have been arrayed by Meldola in his Herbert Spencer Lecture, 1910, pp. 16-24. Meldola leaves the question open whether such evolution has occurred only in past years or is also taking place now. He concludes that whereas certain carbon compounds have survived by reason of possessing extreme stability, others—the precursors of living matter—survived owing to the possession of extreme lability and adaptability to variable conditions of environment. A similar suggestion was previously made by Lockyer, *Inorganic Evolution*, 1900, pp. 169, 170.

[13] T. H. Huxley, Presidential Address, 1870; A. B. Macallum, 'On the Origin of Life on the Globe,' in *Trans. Canadian Institute*, VIII.

[14] First suggested, according to Dastre, by de Salles-Guyon (Dastre, *op. cit.*, p. 252). The theory received the support of Helmholtz.

[15] *Worlds in the Making*, transl. by H. Borns, chap. viii., p. 221, 1908.

But the acceptance of such theories of the arrival of life on the earth does not bring us any nearer to a conception of its actual mode of origin; on the contrary it merely serves to banish the investigation of the question to some conveniently inaccessible corner of the universe and leaves us in the unsatisfactory position of affirming not only that we have no knowledge as to the mode of origin of life—which is unfortunately true—but that we never can acquire such knowledge—which it is to be hoped is not true.[16] Knowing what we know, and believing what we believe, as to the part played by evolution in the development of terrestrial matter, we are, I think (without denying the possibility of the existence of life in other parts of the universe[17]) justified in regarding these cosmic theories as inherently improbable—at least in comparison with the solution of the problem which the evolutionary hypothesis offers.[18]

The evolutionary hypothesis as applied to the origin of life.

I assume that the majority of my audience have at least a general idea of the scope of this hypothesis, the general acceptance of which has within the last sixty years altered the whole aspect not only of biology, but of every other branch of natural science, including astronomy, geology, physics, and chemistry.[19] To those who have not this familiarity I would recommend the perusal of a little book by Professor Judd entitled 'The Coming of Evolution,' which has recently appeared as one of the Cambridge manuals. I know of no similar book in which the subject is as clearly and succinctly treated. Although the author nowhere expresses the opinion that the actual origin of life on the earth has arisen by evolution from non-living matter, it is impossible to read either this or any similar exposition in which the essential unity of the evolutionary process is insisted upon

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[16] 'The history of science shows how dangerous it is to brush aside mysteries —*i.e.*, unsolved problems—and to interpose the barrier placarded "eternal—no thoroughfare."'—R. Meldola, Herbert Spencer Lecture, 1910.

[17] Some authorities, such as Errera, contend, with much probability, that the conditions in interstellar space are such that life, as we understand it, could not possibly exist there.

[18] As Verworn points out, such theories would equally apply to the origin of any other chemical combination, whether inorganic or organic, which is met with on our globe, so that they lead directly to absurd conclusions.—*Allgemeine Physiologie*, 1911.

[19] As Meldola insists, this general acceptance was in the first instance largely due to the writings of Herbert Spencer: 'We are now prepared for evolution in every domain. . . . As in the case of most great generalisations, thought had been moving in this direction for many years. . . . Lamarck and Buffon had suggested a definite mechanism of organic development, Kant and Laplace a principle of celestial evolution, while Lyell had placed geology upon an evolutionary basis. The principle of continuity was beginning to be recognised in physical science. . . . It was Spencer who brought these independent lines of thought to a focus, and who was the first to make any systematic attempt to show that the law of development expressed in its widest and most abstract form was universally followed throughout cosmical processes, inorganic, organic, and super-organic.'—*Op. cit.*, p. 14.

without concluding that the origin of life must have been due to the same process, this process being, without exception, continuous, and admitting of no gap at any part of its course. Looking therefore at the evolution of living matter by the light which is shed upon it from the study of the evolution of matter in general, we are led to regard it as having been produced, not by a sudden alteration, whether exerted by natural or supernatural agency, but by a gradual process of change from material which was lifeless, through material on the borderland between inanimate and animate, to material which has all the characteristics to which we attach the term 'life.' So far from expecting a sudden leap from an inorganic, or at least an unorganised, into an organic and organised condition, from an entirely inanimate substance to a completely animate state of being, should we not rather expect a gradual procession of changes from inorganic to organic matter, through stages of gradually increasing complexity until material which can be termed living is attained? And in place of looking for the production of fully formed living organisms in hermetically sealed flasks, should we not rather search Nature herself, under natural conditions, for evidence of the existence, either in the past or in the present, of transitional forms between living and non-living matter?

The difficulty, nay the impossibility, of obtaining evidence of such evolution from the past history of the globe is obvious. Both the hypothetical transitional material and the living material which was originally evolved from it may, as Macallum has suggested, have taken the form of diffused ultra-microscopic particles of living substance[20]; and even if they were not diffused but aggregated into masses, these masses could have been physically nothing more than colloidal watery slime which would leave no impress upon any geological formation. Myriads of years may have elapsed before some sort of skeleton in the shape of calcareous or siliceous spicules began to evolve itself, and thus enabled 'life,' which must already have possessed a prolonged existence, to make any sort of geological record. It follows that in attempting to pursue the evolution of living matter to its beginning in terrestrial history we can only expect to be confronted with a blank wall of nescience.

The problem would appear to be hopeless of ultimate solution, if we are rigidly confined to the supposition that the evolution of life has only occurred once in the past history of the globe. But are we justified in assuming that at one period only, and as it were by a fortunate and fortuitous concomitation of substance and circumstance, living matter became evolved out of non-living matter—life became

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[20] There still exist in fact forms of life which the microscope cannot show us (E. A. Minchin, Presidential Address to Quekett Club, 1911), and germs which are capable of passing through the pores of a Chamberland filter.

established? Is there any valid reason to conclude that at some previous period of its history our earth was more favourably circumstanced for the production of life than it is now?[21] I have vainly sought for such reason, and if none be forthcoming the conclusion forces itself upon us that the evolution of non-living into living substance has happened more than once—and we can be by no means sure that it may not be happening still.

It is true that up to the present there is no evidence of such happening: no process of transition has hitherto been observed. But on the other hand, is it not equally true that the kind of evidence which would be of any real value in determining this question has not hitherto been looked for? We may be certain that if life is being produced from non-living substance it will be life of a far simpler character than any that has yet been observed—in material which we shall be uncertain whether to call animate or inanimate, even if we are able to detect it at all, and which we may not be able to visualise physically even after we have become convinced of its existence.[22] But we can look with the mind's eye and follow in imagination the transformation which non-living matter may have undergone and may still be undergoing to produce living substance. No principle of evolution is better founded than that insisted upon by Sir Charles Lyell, justly termed by Huxley 'the greatest geologist of his time,' that we must interpret the past history of our globe by the present; that we must seek for an explanation of what has happened by the study of what is happening; that, given similar circumstances, what has occurred at one time will probably occur at another. The process of evolution is universal. The inorganic materials of the globe are continually undergoing transition. New chemical combinations are constantly being formed and old ones broken up; new elements are making their appearance and old elements disappearing.[23] Well may we ask ourselves why the production of living matter alone should be subject to other laws than those which have produced, and are producing, the various forms of non-living matter; why what has happened may not happen? If living matter has been evolved from lifeless in the past, we are justified in accepting the

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[21] Chalmers Mitchell (Article 'Life,' *Encycl. Brit.*, eleventh edition) writes as follows: 'It has been suggested from time to time that conditions very unlike those now existing were necessary for the first appearance of life, and must be repeated if living matter is to be reconstituted artificially. No support for such a view can be derived from observations of the existing conditions of life.'

[22] 'Spontaneous generation of life could only be perceptually demonstrated by filling in the long terms of a series between the complex forms of inorganic and the simplest forms of organic substance. Were this done, it is quite possible that we should be unable to say (especially considering the vagueness of our definitions of life) where life began or ended.'—K. Pearson, *Grammar of Science*, second edition, 1900, p. 350.

[23] See on the production of elements, W. Crookes, Address to Section B, Brit. Assoc., 1886; T. Preston, *Nature*, vol. lx., p. 180; J. J. Thomson, *Phil. Mag.*, 1897, p. 311; Norman Lockyer, *op. cit.*, 1900; G. Darwin, Pres. Addr. Brit. Assoc., 1905.

conclusion that its evolution is possible in the present and in the future. Indeed, we are not only justified in accepting this conclusion, we are forced to accept it. When or where such change from non-living to living matter may first have occurred, when or where it may have continued, when or where it may still be occurring, are problems as difficult as they are interesting, but we have no right to assume that they are insoluble.

Since living matter always contains water as its most abundant constituent, and since the first living organisms recognisable as such in the geological series were aquatic, it has generally been assumed that life must first have made its appearance in the depths of the ocean.[24] Is it, however, certain that the assumption that life originated in the sea is correct? Is not the land-surface of our globe quite as likely to have been the nidus for the evolutionary transformation of non-living into living material as the waters which surround it? Within this soil almost any chemical transformation may occur; it is subjected much more than matters dissolved in sea-water to those fluctuations of moisture, temperature, electricity, and luminosity which are potent in producing chemical changes. But whether life, in the form of a simple slimy colloid, originated in the depths of the sea or on the surface of the land, it would be equally impossible for the geologist to trace its beginnings, and were it still becoming evolved in the same situations, it would be almost as impossible for the microscopist to follow its evolution. We are therefore not likely to obtain direct evidence regarding such a transformation of non-living into living matter in Nature, even if it is occurring under our eyes.

An obvious objection to the idea that the production of living matter from non-living has happened more than once is that, had this been the case, the geological record should reveal more than one palæontological series. This objection assumes that evolution would in every case take an exactly similar course and proceed to the same goal—an assumption which is, to say the least, improbable. If, as might well be the case, in any other palæontological series than the one with which we are acquainted the process of evolution of living beings did not proceed beyond Protista, there would be no obvious geological evidence regarding it; such evidence would only be discoverable by a carefully directed search made with that particular object in view.[25] I would not by any means minimise the difficulties which attend the suggestion that the

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[24] For arguments in favour of the first appearance of life having been in the sea, see A. B. Macallum, 'The Palæochemistry of the Ocean,' *Trans. Canad. Instit.*, 1903-4.

[25] Lankester (Art. 'Protozoa,' Encycl. Brit., tenth edition) conceives that the first protoplasm fed on the antecedent steps in its own evolution. F. J. Allen (*Brit. Assoc. Reports*, 1896) comes to the conclusion that living substance is probably constantly being produced, but that this fails to make itself evident

evolution of life may have occurred more than once or may still be happening, but on the other hand, it must not be ignored that those which attend the assumption that the production of life has occurred once only are equally serious. Indeed, had the idea of the possibility of a multiple evolution of living substance been first in the field, I doubt if the prevalent belief regarding a single fortuitous production of life upon the globe would have become established among biologists—so much are we liable to be influenced by the impressions we receive in scientific childhood!

Further course of evolution of life.

Assuming the evolution of living matter to have occurred—whether once only or more frequently matters not for the moment—and in the form suggested, viz., as a mass of colloidal slime possessing the property of assimilation and therefore of growth, reproduction would follow as a matter of course. For all material of this physical nature—fluid or semi-fluid in character—has a tendency to undergo subdivision when its bulk exceeds a certain size. The subdivision may be into equal or nearly equal parts, or it may take the form of buds. In either case every separated part would resemble the parent in chemical and physical properties, and would equally possess the property of taking in and assimilating suitable material from its liquid environment, growing in bulk and reproducing its like by subdivision. *Omne vivum e vivo.* In this way from any beginning of living material a primitive form of life would spread, and would gradually people the globe. The establishment of life being once effected, all forms of organisation follow under the inevitable laws of evolution. *Ce n'est que le premier pas qui coûte.*

We can trace in imagination the segregation of a more highly phosphorised portion of the primitive living matter, which we may now consider to have become more akin to the protoplasm of organisms with which we are familiar. This more phosphorised portion might not for myriads of generations take the form of a definite nucleus, but it would be composed of material having a composition and qualities similar to those of the nucleus of a cell. Prominent among these qualities is that of catalysis—the function of effecting profound chemical changes in other material in contact with it without itself undergoing permanent change. This catalytic function may have been exercised directly by the living substance or may have been carried

owing to the substance being seized and assimilated by existing organisms. He believes that 'in accounting for the first origin of life on this earth it is not necessary that, as Pflüger assumed, the planet should have been at a former period a glowing fire-ball.' He 'prefers to believe that the circumstances which support life would also favour its origin.' And elsewhere: 'Life is not an extraordinary phenomenon, not even an importation from some other sphere, but rather the actual outcome of circumstances on this earth.'

on through the agency of the enzymes already mentioned, which are also of a colloid nature but of simpler constitution than itself, and which differ from the catalytic agents employed by the chemist in the fact that they produce their effects at a relatively low temperature. In the course of evolution special enzymes would become developed for adaptation to special conditions of life, and with the appearance of these and other modifications, a process of differentiation of primitive living matter into individuals with definite specific characters gradually became established. We can conceive of the production in this way from originally undifferentiated living substance of simple differentiated organisms comparable to the lowest forms of Protista. But how long it may have taken to arrive at this stage we have no means of ascertaining. To judge from the evidence afforded by the evolution of higher organisms it would seem that a vast period of time would be necessary for even this amount of organisation to establish itself.

Formation of the nucleated cell.

The next important phase in the process of evolution would be the segregation and moulding of the diffused or irregularly aggregated nuclear matter into a definite nucleus around which all the chemical activity of the organism will in future be centred. Whether this change were due to a slow and gradual process of segregation or of the nature of a jump, such as Nature does occasionally make, the result would be the advancement of the living organism to the condition of a complete nucleated cell: a material advance not only in organisation but—still more important—in potentiality for future development. Life is now embodied in the cell, and every living being evolved from this will itself be either a cell or a cell-aggregate. *Omnis cellula e cellulâ.*

Establishment of sexual differences.

After the appearance of a nucleus—but how long after it is impossible to conjecture—another phenomenon appeared upon the scene in the occasional exchange of nuclear substance between cells. In this manner became established the process of sexual reproduction. Such exchange in the unicellular Protista might and may occur between any two cells forming the species, but in the multicellular Metazoa it became—like other functions—specialised in particular cells. The result of the exchange is rejuvenescence; associated with an increased tendency to subdivide and to produce new individuals. This is due to the introduction of a stimulating or catalytic chemical agent into the cell which is to be rejuvenated, as is proved by the experiments of Loeb already alluded to. It is true that the chemical material introduced into the germ-cell in the ordinary process of its fertilisation by the sperm-cell is usually accompanied by the introduction of definite morphological elements which blend with others already contained within the germ-cell, and it is believed that the transmission of such morphological elements of the parental nuclei is related to the transmission of parental qualities. But we must not be blind to the possibility that these transmitted qualities may be connected with specific chemical characters of the transmitted elements; in other words, that heredity also is one of the questions the eventual solution of which we must look to the chemist to provide.

Aggregate life.

So far we have been chiefly considering life as it is found in the simplest forms of living substance, organisms for the most part entirely microscopic and neither distinctively animal nor vegetable, which were grouped together by Haeckel as a separate kingdom of animated nature—that of Protista. But persons unfamiliar with the microscope are not in the habit of associating the term 'life' with microscopic organisms, whether these take the form of cells or of minute portions of living substance which have not yet attained to that dignity. We most of us speak and think of life as it occurs in ourselves and other animals with which we are familiar; and as we find it in the plants around us. We recognise it in these by the possession of certain properties—movement, nutrition, growth, and reproduction. We are not aware by intuition, nor can we ascertain without the employment of the microscope, that we and all the higher living beings, whether animal or vegetable, are entirely formed of aggregates of nucleated cells, each microscopic and each possessing its own life. Nor could we suspect by intuition that what we term our life is not a single indivisible property, capable of being blown out with a puff like the flame of a candle; but is the aggregate of the lives of many millions of living cells of which the body is composed. It is but a short while ago that this cell-constitution was discovered: it occurred within the lifetime, even within the memory, of some who are still with us. What a marvellous distance we have travelled since then in the path of knowledge of living organisms! The strides which were made in the advance of the mechanical sciences during the nineteenth century, which is generally considered to mark that century as an age of unexampled progress, are as nothing in comparison with those made in the domain of biology, and their interest is entirely dwarfed by that which is aroused by the facts relating to the phenomena of life which have accumulated within the same period. And not the least remarkable of these facts is the discovery of the cell-structure of plants and animals!

Evolution of the cell-aggregate.

Let us consider how cell-aggregates came to be evolved from organisms consisting of single cells. Two methods are possible—viz. (1) the adhesion of a number of originally separate individuals; (2) the subdivision of a single individual without the products of its subdivision breaking loose from one another. No doubt this last is the manner whereby the cell-aggregate was originally formed, since it is that by which it is still produced, and we know that the life-history of the individual is an epitome of that of the species. Such aggregates were in the beginning solid; the cells in contact with one another and even in continuity: subsequently a space or cavity became formed in the interior of the mass, which was thus converted into a hollow sphere. All the cells of the aggregate were at first perfectly similar in structure and in function; there was no subdivision of labour. All would take part in effecting locomotion; all would receive stimuli from outside; all would take in and digest nutrient matter, which would then be passed into the cavity of the sphere to serve as a common store of nourishment. Such organisms are still found, and constitute the lowest types of Metazoa. Later one part of the hollow sphere became dimpled to form a cup; the cavity of the sphere became correspondingly altered in shape. With this change in structure differentiation of function between the cells covering the outside and those lining the inside of the cup made its appearance. Those on the outside subserved locomotor functions and received and transmitted from cell to cell stimuli, physical or chemical, received by the organism; while those on the inside, being freed from such functions, tended to specialise in the direction of the inception and digestion of nutrient material; which, passing from them into the cavity of the invaginated sphere, served for the nourishment of all the cells composing the organism. The further course of evolution produced many changes of form and ever-increasing complexity of the cavity thus produced by simple invagination. Some of the cell-aggregates settled down to a sedentary life, becoming plant-like in appearance and to some extent in habit. Such organisms, complex in form but simple in structure, are the Sponges. Their several parts are not, as in the higher Metazoa, closely interdependent: the destruction of any one part, however extensive, does not either immediately or ultimately involve death of the rest: all parts function separately, although doubtless mutually benefiting by their conjunction, if only by slow diffusion of nutrient fluid throughout the mass. There is already some differentiation in these organisms, but the absence of a nervous system prevents any general co-ordination, and the individual cells are largely independent of one another.

Our own life, like that of all the higher animals, is an *aggregate life*; the life of the whole is the life of the individual cells. The life of some of these cells can be put an end to, the rest may continue to live. This is, in fact, happening every moment of our lives. The cells which cover the surface of our body, which form the scarf-skin and the hairs and nails, are constantly dying and the dead cells are rubbed off or cut away, their place being taken by others supplied from living layers beneath. But the death of these cells does not affect the vitality of the body as a whole. They serve merely as a protection, or an ornamental covering, but are otherwise not material to our existence. On the other hand, if a few cells, such as those nerve-cells under the influence of which respiration is carried on, are destroyed or injured, within a minute or two the whole living machine comes to a standstill, so that to the bystander the patient is dead; even the doctor will pronounce life to be extinct. But this pronouncement is correct only in a special sense. What has happened is that, owing to the cessation of respiration, the supply of oxygen to the tissues is cut off. And since the manifestations of life cease without this supply, the animal or patient appears to be dead. If, however, within a short period we supply the needed oxygen to the tissues requiring it, all the manifestations of life reappear.

It is only some cells which lose their vitality at the moment of so-called 'general death.' Many cells of the body retain their individual life under suitable circumstances long after the rest of the body is dead. Notable among these are muscle-cells. McWilliam showed that the muscle-cells of the blood-vessels give indications of of life several days after an animal has been killed. The muscle-cells of the heart in mammals have been revived and caused to beat regularly and strongly many hours after apparent death. In man this result has been obtained by Kuliabko as many as eighteen hours after life had been pronounced extinct: in animals after days had elapsed. Waller has shown that indications of life can be elicited from various tissues many hours and even days after general death. Sherrington observed the white corpuscles of the blood to be active when kept in a suitable nutrient fluid weeks after removal from the blood-vessels. A French histologist, Jolly, has found that the white corpuscles of the frog, if kept in a cool place and under suitable conditions, show at the end of a year all the ordinary manifestations of life. Carrell and Burrows have observed activity and growth to continue for long periods in the isolated cells of a number of tissues and organs kept under observation in a suitable medium. Carrell has succeeded in substituting entire organs obtained after death from one animal for those of another of the same species, and has thereby opened up a field of surgical treatment the limit of which cannot yet be descried. It is a well-established fact that any part or organ of the body can be maintained alive for hours isolated from the rest if the blood-vessels are perfused with an oxygenated solution of salts in certain proportions (Ringer). Such revival and prolongation of the life of separated organs is an ordinary procedure in laboratories of physiology. Like all the other instances enumerated, it is based on the fact that the individual cells of an organ have a life of their own which is largely independent, so that they will continue in suitable circumstances to live, although the rest of the body to which they belonged may be dead.

But some cells, and the organs which are formed of them, are more necessary to maintain the life of the aggregate than others, on account of the nature of the functions which have become specialised in them. This is the case with the nerve-cells of the respiratory centre, since they preside over the movements which are necessary to effect oxygenation of the blood. It is also true for the cells which compose the heart, since this serves to pump oxygenated blood to all other cells of the body: without such blood most cells soon cease to live. Hence we examine respiration and heart to determine if life is present: when one or both of these are at a standstill we know that life cannot be maintained. These are not the only organs necessary for the maintenance of life, but the loss of others can be borne longer, since the functions which they subserve, although useful or even essential to the organism, can be dispensed with for a time. The life of some cells is therefore more, of others less, necessary for maintaining the life of the rest. On the other hand, the cells composing certain organs have in the course of evolution ceased to be necessary, and their continued existence may even be harmful. Wiedersheim has enumerated more than a hundred of these organs in the human body. Doubtless Nature is doing her best to get rid of them for us, and our descendants will some day have ceased to possess a vermiform appendix or a pharyngeal tonsil: until that epoch arrives we must rely for their removal on the more rapid methods of surgery!

The maintenance of the life of the cell-aggregate in the higher animals. Co-ordinating mechanisms.

We have seen that in the simplest multicellular organisms, where one cell of the aggregate differs but little from another, the conditions for the maintenance of the life of the whole are nearly as simple as those for individual cells. But the life of a cell-aggregate such as composes the bodies of the higher animals is maintained not only by the conditions for the maintenance of the life of the individual cell being kept favourable, but also by the co-ordination of the varied activities of the cells which form the aggregate. Whereas in the lowest Metazoa all cells of the aggregate are alike in structure and function and perform and share everything in common, in higher animals (and for that matter in the higher plants also) the cells have become specialised, and each is only adapted for the performance of a particular function. Thus the cells of the gastric glands are only adapted for the secretion of gastric juice, the cells of the villi for the absorption of digested matters from the intestine, the cells of the kidney for the removal of waste products and superfluous water from the blood, those of the heart for pumping blood through the vessels. Each of these cells has its individual life and performs its individual functions. But unless there were some sort of co-operation and subordination to the needs of the body generally, there would be sometimes too little, sometimes too much gastric juice secreted; sometimes too tardy, sometimes too rapid an absorption from the intestine; sometimes too little, sometimes too much blood pumped into the arteries, and so on. As the result of such lack of co-operation the life of the whole would cease to be normal and would eventually cease to be maintained.

We have already seen what are the conditions which are favourable for the maintenance of life of the individual cell, no matter where situated. The principal condition is that it must be bathed by a nutrient fluid of suitable and constant composition. In higher animals this fluid is the lymph, which bathes the tissue elements and is itself constantly supplied with fresh nutriment and oxygen by the blood. Some tissue-cells are directly bathed by blood; and in invertebrates, in which there is no special system of lymph-vessels, all the tissues are thus nourished. All cells both take from and give to the blood, but not the same materials or to an equal extent. Some, such as the absorbing cells of the villi, almost exclusively give; others, such as the cells of the renal tubules, almost exclusively take. Nevertheless, the resultant of all the give and take throughout the body serves to maintain the composition of the blood constant under all circumstances. In this way the first condition of the maintenance of the life of the aggregate is fulfilled by insuring that the life of the individual cells composing it is kept normal.

The second essential condition for the maintenance of life of the cell-aggregate is the co-ordination of its parts and the due regulation of their activity, so that they may work together for the benefit of the whole. In the animal body this is effected in two ways: first, through the nervous system; and second, by the action of specific chemical substances which are formed in certain organs and carried by the blood to other parts of the body, the cells of which they excite to activity. These substances have received the general designation of 'hormones' (ὁρμάω, to stir up), a term introduced by Professor Starling. Their action, and indeed their very existence, has only been recognised of late years, although the part which they play in the physiology of animals appears to be only second in importance to that of the nervous system itself; indeed, maintenance of life may become impossible in the absence of certain of these hormones.

Part played by the nervous system in the maintenance of aggregate life.

Evolution of a nervous system.

Before we consider the manner in which the nervous system serves to co-ordinate the life of the cell-aggregate, let us see how it has become evolved.

The first step in the process was taken when certain of the cells of the external layer became specially sensitive to stimuli from outside, whether caused by mechanical impressions (tactile and auditory stimuli) or impressions of light and darkness (visual stimuli) or chemical impressions. The effects of such impressions were probably at first simply communicated to adjacent cells and spread from cell to cell throughout the mass. An advance was made when the more impressionable cells threw out branching feelers amongst the other cells of the organism. Such feelers would convey the effects of stimuli with greater rapidity and directness to distant parts. They may at first have been retractile, in this respect resembling the long pseudopodia of certain Rhizopoda. When they became fixed they would be potential nerve-fibres and would represent the beginning of a nervous system. Even yet (as Ross Harrison has shown), in the course of development of nerve-fibres, each fibre makes its appearance as an amœboid cell-process which is at first retractile, but gradually grows into the position it is eventually to occupy and in which it will become fixed.

In the further course of evolution a certain number of these specialised cells of the external layer sank below the general surface, partly perhaps for protection, partly for better nutrition: they became nerve-cells. They remained connected with the surface by a prolongation which became an afferent or sensory nerve-fibre, and through its termination between the cells of the general surface continued to receive the effects of external impressions; on the other hand, they continued to transmit these impressions to other, more distant cells by their efferent prolongations. In the further course of evolution the nervous system thus laid down became differentiated into distinct *afferent*, *efferent*, and *intermediary* portions. Once established, such a nervous system, however simple, must dominate the organism, since it would furnish a mechanism whereby the individual cells would work together more effectually for the mutual benefit of the whole.

It is the development of the nervous system, although not proceeding in all classes along exactly the same lines, which is the most prominent feature of the evolution of the Metazoa. By and through it all impressions reaching the organism from the outside are translated into contraction or some other form of cell-activity. Its formation has been the means of causing the complete divergence of the world of animals from the world of plants, none of which possess any trace of a nervous system. Plants react, it is true, to external impressions, and these impressions produce profound changes and even comparatively rapid and energetic movements in parts distant from the point of application of the stimulus—as in the well-known instance of the sensitive plant. But the impressions are in all cases propagated directly from cell to cell—not through the agency of nerve-fibres; and in the absence of anything corresponding to a nervous system it is not possible to suppose that any plant can ever acquire the least glimmer of intelligence. In animals, on the other hand, from a slight original modification of certain cells has directly proceeded in the course of evolution the elaborate structure of the nervous system with all its varied and complex functions, which reach their culmination in the workings of the human intellect. 'What a piece of work is a man! How noble in reason! How infinite in faculty! In form and moving how express and admirable! In action how like an angel! In apprehension how like a god!' But lest he be elated with his psychical achievements, let him remember that they are but the result of the acquisition by a few cells in a remote ancestor of a slightly greater tendency to react to an external stimulus, so that these cells were brought into closer touch with the outer world; while on the other hand, by extending beyond the circumscribed area to which their neighbours remained restricted, they gradually acquired a dominating influence over the rest. These dominating cells became nerve-cells; and now not only furnish the means for transmission of impressions from one part of the organism to another, but in the progress of time have become the seat of perception and conscious sensation, of the formation and association of ideas, of memory, volition, and all the manifestations of the mind!

Regulation of movements by the nervous system. Voluntary movements.

The most conspicuous part played by the nervous system in the phenomena of life is that which produces and regulates the general movements of the body—movements brought about by the so-called voluntary muscles. These movements are actually the result of impressions imparted to sensory or afferent nerves at the periphery—*e.g.*, in the skin or in the several organs of special sense; the effect of these impressions may not be immediate, but can be stored for an indefinite time in certain cells of the nervous system. The regulation of movements—whether they occur instantly after reception of the peripheral impression or result after a certain lapse of time; whether they are accompanied by conscious sensation or are of a purely reflex and unconscious character—is an intricate process, and the conditions of their co-ordination are of a complex nature involving not merely the causation of contraction of certain muscles, but also the prevention of contraction of others. For our present knowledge of these conditions we are largely indebted to the researches of Professor Sherrington.

Involuntary movements.

A less conspicuous but no less important part played by the nervous system is that by which the contractions of involuntary muscles are regulated. Under normal circumstances these are always independent of consciousness, but their regulation is brought about in much the same way as is that of the contractions of voluntary muscles—viz., as the result of impressions received at the periphery. These are transmitted by afferent fibres to the central nervous system, and from the latter other impulses are sent down, mostly along the nerves of the sympathetic or autonomic system of nerves, which either stimulate or prevent contraction of the involuntary muscles. Many involuntary muscles have a natural tendency to continuous or rhythmic contraction which is quite independent of the central nervous system; in this case the effect of impulses received from the latter is merely to increase or diminish the amount of such contraction. An example of this double effect is observed in connection with the heart, which—although it can contract regularly and rhythmically when cut off from the nervous system and even if removed from the body—is normally stimulated to increased activity by impulses coming from the central nervous system through the sympathetic, or to diminished activity by others coming through the vagus. It is due to the readiness by which the action of the heart is influenced in these opposite ways by the spread of impulses generated during the nerve-storms which we term 'emotions' that in the language of poetry, and even of every day, the word 'heart' has become synonymous with the emotions themselves.

Effects of emotions.

The involuntary muscle of the arteries has its action similarly balanced. When its contraction is increased, the size of the vessels is lessened and they deliver less blood; the parts they supply accordingly become pale in colour. On the other hand, when the contraction is diminished the vessels enlarge and deliver more blood; the parts which they supply become correspondingly ruddy. These changes in the arteries, like the effects upon the heart, may also be produced under the influence of emotions. Thus 'blushing' is a purely physiological phenomenon due to diminished action of the muscular tissue of the arteries, whilst the pallor produced by fright is caused by an increased contraction of that tissue. Apart, however, from these conspicuous effects, there is constantly proceeding a less apparent but not less important balancing action between the two sets of nerve-fibres distributed to heart and blood-vessels; which are influenced in one direction or another by every sensation which we experience and even by impressions of which we may be wholly unconscious, such as those which occur during sleep or anæsthesia, or which affect our otherwise insensitive internal organs.

A further instance of nerve-regulation is seen in secreting glands. Not all glands are thus regulated, at least not directly; but in those which are, the effects are striking. Their regulation is of the same general nature as that exercised upon involuntary muscle, but it influences the chemical activities of the gland-cells and the outpouring of secretion from them. By means of this regulation a secretion can be produced or arrested, increased or diminished. As with muscle, a suitable balance is in this way maintained, and the activity of the glands is adapted to the requirements of the organism. Most of the digestive glands are

Regulation of secretion by the nervous system.

thus influenced, as are the skin-glands which secrete sweat. And by the action of the nervous system upon the skin-glands, together with its effect in increasing or diminishing the blood-supply to the cutaneous blood-vessels, the temperature of our blood is regulated and is kept at the point best suited for maintenance of the life and activity of the tissues.

Regulation of body temperature.

The action of the nervous system upon the secretion of glands is strikingly exemplified, as in the case of its action upon the heart and blood-vessels by the effects of the emotions. Thus an emotion of one kind—such as the anticipation of food—will cause saliva to flow—'the mouth to water'; whereas an emotion of another kind—such as fear or anxiety—will stop the secretion, causing the 'tongue to cleave unto the roof of the mouth,' and rendering speech difficult or impossible. Such arrest of the salivary secretion also makes the swallowing of dry food difficult: advantage of this fact is taken in the 'ordeal by rice' which used to be employed in the East for the detection of criminals.

Effects of emotions on secretion.

The activities of the cells constituting our bodies are controlled, as already mentioned, in another way than through the nervous system, viz., by chemical agents (hormones) circulating in the blood. Many of these are produced by special glandular organs, known as internally secreting glands. The ordinary secreting glands pour their secretions on the exterior of the body or on a surface communicating with the exterior; the internally secreting glands pass the materials which they produce directly into the blood. In this fluid the hormones are carried to distant organs. Their influence upon an organ may be essential to the proper performance of its functions or may be merely ancillary to it. In the former case removal of the internally secreting gland which produces the hormone, or its destruction by disease, may prove fatal to the organism. This is the case with the suprarenal capsules: small glands which are adjacent to the kidneys, although having no physiological connection with these organs. A Guy's physician, Dr. Addison, in the middle of the last century showed that a certain affection, almost always fatal, since known by his name, is associated with disease of the suprarenal capsules. A short time after this observation a French physiologist, Brown-Séquard, found that animals from which the suprarenal capsules are removed rarely survive the operation for more than a few days. In the concluding decade of the last century interest in these bodies was revived by the discovery that they are constantly yielding to the blood a chemical agent (or hormone) which stimulates the contractions of the heart and arteries and assists in the promotion of every action which is brought about through the sympathetic nervous

Regulation by chemical agents: hormones. Internal secretions.

Suprarenals.

system (Langley). In this manner the importance of their integrity has been explained, although we have still much to learn regarding their functions.

Another instance of an internally secreting gland which is essential to life, or at least to its maintenance in a normal condition, is the

Thyroid.

thyroid. The association of imperfect development or disease of the thyroid with disorders of nutrition and inactivity of the nervous system is well ascertained. The form of idiocy known as cretinism and the affection termed myxœdema are both associated with deficiency of its secretion: somewhat similar conditions to these are produced by the surgical removal of the gland. The symptoms are alleviated or cured by the administration of its juice. On the other hand, enlargement of the thyroid, accompanied by increase of its secretion, produces symptoms of nervous excitation, and similar symptoms are caused by excessive administration of the glandular substance by the mouth. From these observations it is inferred that the juice contains hormones which help to regulate the nutrition of the body and serve to stimulate the nervous system, for the higher functions of which they appear to be essential. To quote M. Gley, to whose researches we owe much of our knowledge regarding the functions of this organ: 'La genèse et l'exercice des plus hautes facultés de l'homme sont conditionnés par l'action purement chimique d'un produit de sécrétion. Que les psychologues méditent ces faits!'

The case of the parathyroid glandules is still more remarkable. These organs were discovered by Sandström in 1880. They are four

Parathyroids.

minute bodies, each no larger than a pin's head, imbedded in the thyroid. Small as they are, their internal secretion possesses hormones which exert a powerful influence upon the nervous system. If they are completely removed, a complex of symptoms, technically known as 'tetany,' is liable to occur, which is always serious and may be fatal. Like the hormones of the thyroid itself, therefore, those of the parathyroids produce effects upon the nervous system, to which they are carried by the blood; although the effects are of a different kind.

Another internally secreting gland which has evoked considerable interest during the last few years is the pituitary body. This is a small

Pituitary.

structure no larger than a cob-nut attached to the base of the brain. It is mainly composed of glandular cells. Its removal has been found (by most observers) to be fatal—often within two or three days. Its hypertrophy, when occurring during the general growth of the body, is attended by an undue development of the skeleton, so that the stature tends to assume gigantic proportions. When the hypertrophy occurs after growth is completed, the extremities—viz., the hands and feet, and the bones of the face—are mainly affected; hence the condition has been termed 'acromegaly' (enlargement of extremities). The association of this condition with affections of the pituitary was pointed out in 1885 by a distinguished French physician, Dr. Pierre Marie. Both 'giants' and 'acromegalists' are almost invariably found to have an enlarged pituitary. The enlargement is generally confined to one part—the anterior lobe—and we conclude that this produces hormones which stimulate the growth of the body generally and of the skeleton in particular. The remainder of the pituitary is different in structure from the anterior lobe and has a different function. From it hormones can be extracted which, like those of the suprarenal capsule, although not exactly in the same manner, influence the contraction of the heart and arteries. Its extracts are also instrumental in promoting the secretion of certain glands. When injected into the blood they cause a free secretion of water from the kidneys and of milk from the mammary glands, neither of which organs are directly influenced (as most other glands are) through the nervous system. Doubtless under natural conditions these organs are stimulated to activity by hormones which are produced in the pituitary and which pass from this into the blood.

The internally secreting glands which have been mentioned (thyroid, parathyroid, suprarenal, pituitary) have, so far as is known, no other function than that of producing chemical substances of this character for the influencing of other organs, to which they are conveyed by the blood. It is interesting to observe that these glands are all of very small size, none being larger than a walnut, and some—the parathyroids—almost microscopic. In spite of this, they are essential to the proper maintenance of the life of the body, and the total removal of any of them by disease or operation is in most cases speedily fatal.

Pancreas.

There are, however, organs in the body yielding internal secretions to the blood in the shape of hormones, but exercising at the same time other functions. A striking instance is furnished by the pancreas, the secretion of which is the most important of the digestive juices. This—the pancreatic juice—forms the external secretion of the gland, and is poured into the intestine, where its action upon the food as it passes out from the stomach has long been recognised. It was, however, discovered in 1889 by von Mering and Minkowski that the pancreas also furnishes an internal secretion, containing a hormone which is passed from the pancreas into the blood, by which it is carried first to the liver and afterwards to the body generally. This hormone is essential to the proper utilisation of carbohydrates in the organism. It is well known that the carbohydrates of the food are converted into grape sugar and circulate in this form in the blood, which always contains a certain amount; the blood conveys it to all the cells of the body, and they utilise it as fuel. If, owing to disease of the pancreas or as the result of its removal by surgical procedure, its internal secretion is not available, sugar is no longer properly utilised by the cells of the body and tends to accumulate in the blood; from the blood the excess passes off by the kidneys, producing diabetes.

Duodenum.

Another instance of an internal secretion furnished by an organ which is devoted largely to other functions is the 'pro-secretin' found in the cells lining the duodenum. When the acid gastric juice comes into contact with these cells it converts their pro-secretin into 'secretin.' This is a hormone which is passed into the blood and circulates with that fluid. It has a specific effect on the externally secreting cells of the pancreas, and causes the rapid outpouring of pancreatic juice into the intestine. This effect is similar to that of the hormones of the pituitary body upon the cells of the kidney and mammary gland. It was discovered by Bayliss and Starling.

Internal secretions of the reproductive organs.

The reproductive glands furnish in many respects the most interesting example of organs which—besides their ordinary products, the germ- and sperm-cells (ova and spermatozoa)—form hormones which circulate in the blood and effect changes in cells of distant parts of the body. It is through these hormones that the secondary sexual characters, such as the comb and tail of the cock, the mane of the lion, the horns of the stag, the beard and enlarged larynx of a man, are produced, as well as the many differences in form and structure of the body which are characteristic of the sexes. The dependence of these so-called secondary sexual characters upon the state of development of the reproductive organs has been recognised from time immemorial, but has usually been ascribed to influences produced through the nervous system, and it is only in recent years that the changes have been shown to be brought about by the agency of internal secretions and hormones, passed from the reproductive glands into the circulating blood.[26]

Chemical nature of hormones.

It has been possible in only one or two instances to prepare and isolate the hormones of the internal secretions in a sufficient condition of purity to subject them to analysis, but enough is known about them to indicate that they are organic bodies of a not very complex nature, far simpler than proteins and even than enzymes. Those which have been studied are all dialysable, are readily soluble in water but insoluble in alcohol, and are not destroyed by boiling. One at least—that of the medulla of the suprarenal capsule—has been prepared synthetically, and when their

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[26] The evidence is to be found in F. H. A. Marshall, *The Physiology of Reproduction*, 1911.

exact chemical nature has been somewhat better elucidated it will probably not be difficult to obtain others in the same way.

From the above it is clear that not only is a co-ordination through the nervous system necessary in order that life shall be maintained in a normal condition, but a chemical co-ordination is no less essential. These may be independent of one another; but on the other hand they may react upon one another. For it can be shown that the production of some at least of the hormones is under the influence of the nervous system (Biedl, Asher, Elliott); whilst, as we have seen, some of the functions of the nervous system are dependent upon hormones.

Protective chemical mechanisms. Toxins and antitoxins.

Time will not permit me to refer in any but the briefest manner to the protective mechanisms which the cell aggregate has evolved for its defence against disease, especially disease produced by parasitic micro-organisms. These, which belong with few exceptions to the Protista, are without doubt the most formidable enemies which the multicellular Metazoa, to which all the higher animal organisms belong, have to contend against. To such micro-organisms are due *inter alia* all diseases which are liable to become epidemic, such as anthrax and rinderpest in cattle, distemper in dogs and cats, small-pox, scarlet fever, measles, and sleeping sickness in man. The advances of modern medicine have shown that the symptoms of these diseases—the disturbances of nutrition, the temperature, the lassitude or excitement, and other nervous disturbances—are the effects of chemical poisons (*toxins*) produced by the micro-organisms and acting deleteriously upon the tissues of the body. The tissues, on the other hand, endeavour to counteract these effects by producing other chemical substances destructive to the micro-organisms or antagonistic to their action: these are known as *anti-bodies*. Sometimes the protection takes the form of a subtle alteration in the living substance of the cells which renders them for a long time, or even permanently, insusceptible (immune) to the action of the poison. Sometimes certain cells of the body, such as the white corpuscles of the blood, eat the invading micro-organisms and destroy them bodily by the action of chemical agents within their protoplasm. The result of an illness thus depends upon the result of the struggle between these opposing forces—the micro-organisms on the one hand and the cells of the body on the other—both of which fight with chemical weapons. If the cells of the body do not succeed in destroying the invading organisms it is certain that the invaders will in the long run destroy them, for in this combat no quarter is given. Fortunately we have been able, by the aid of animal experimentation, to acquire some knowledge of the manner in which we are attacked by micro-organisms and of the methods which the cells of our body adopt to repel the attack, and the knowledge is now extensively utilised to assist our defence. For this purpose protective serums or antitoxins, which have been formed in the blood of other animals, are employed to supplement the action of those which our own cells produce. It is not too much to assert that the knowledge of the parasitic origin of so many diseases and of the chemical agents which on the one hand cause, and on the other combat, their symptoms, has transformed medicine from a mere art practised empirically, into a real science based upon experiment. The transformation has opened out an illimitable vista of possibilities in the direction not only of cure, but, more important still, of prevention. It has taken place within the memory of most of us who are here present. And only last February the world was mourning the death of one of the greatest of its benefactors—a former President of this Association[27]—who, by applying this knowledge to the practice of surgery, was instrumental, even in his own lifetime, in saving more lives than were destroyed in all the bloody wars of the nineteenth century!

Parasitic nature of diseases.

The question has been debated whether, if all accidental modes of destruction of the life of the cell could be eliminated, there would remain a possibility of individual cell-life, and even of aggregate cell-life, continuing indefinitely; in other words, Are the phenomena of senescence and death a natural and necessary sequence to the existence of life? To most of my audience it will appear that the subject is not open to debate. But some physiologists (*e.g.*, Metchnikoff) hold that the condition of senescence is itself abnormal; that old age is a form of disease or is due to disease, and, theoretically at least, is capable of being eliminated. We have already seen that individual cell-life, such as that of the white blood-corpuscles and of the cells of many tissues, can under suitable conditions be prolonged for days or weeks or months after general death. Unicellular organisms kept under suitable conditions of nutrition have been observed to carry on their functions normally for prolonged periods and to show no degeneration such as would accompany senescence. They give rise by division to others of the same kind, which also, under favourable conditions, continue to live, to all appearance indefinitely. But these instances, although they indicate that in the simplest forms of organisation existence may be greatly extended without signs of decay, do not furnish conclusive evidence of indefinite prolongation of life. Most of the cells which constitute the body, after a period of growth and activity, sometimes more, sometimes less prolonged, eventually undergo atrophy and cease to perform satisfactorily the

Senescence and death.

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[27] Lord Lister was President at Liverpool in 1896.

functions which are allotted to them. And when we consider the body as a whole, we find that in every case the life of the aggregate consists of a definite cycle of changes which, after passing through the stages of growth and maturity, always leads to senescence, and finally terminates in death. The only exception is in the reproductive cells, in which the processes of maturation and fertilisation result in rejuvenescence, so that instead of the usual downward change towards senescence, the fertilised ovum obtains a new lease of life, which is carried on into the new-formed organism. The latter again itself ultimately forms reproductive cells, and thus the life of the species is continued. It is only in the sense of its propagation in this way from one generation to another that we can speak of the indefinite continuance of life: we can only be immortal through our descendants!

Average duration of life and possibility of its prolongation.

The individuals of every species of animal appear to have an average duration of existence.[28] Some species are known the individuals of which live only for a few hours, whilst others survive for a hundred years.[29] In man himself the average length of life would probably be greater than the three-score and ten years allotted to him by the Psalmist if we could eliminate the results of disease and accident; when these results are included it falls far short of that period. If the terms of life given in the purely mythological part of the Old Testament were credible, man would in the early stages of his history have possessed a remarkable power of resisting age and disease. But, although many here present were brought up to believe in their literal veracity, such records are no longer accepted even by the most orthodox of theologians, and the nine hundred odd years with which Adam and his immediate descendants are credited, culminating in the nine hundred and sixty-nine of Methuselah, have been relegated, with the account of Creation and the Deluge, to their proper position in literature. When we come to the Hebrew Patriarchs, we notice a considerable diminution to have taken place in what the insurance offices term the 'expectation of life.' Abraham is described as having lived only to 175 years, Joseph and Joshua to 110, Moses to 120; even at that age 'his eye was not dim nor his natural force abated.' We cannot say that under ideal conditions all these terms are impossible; indeed, Metchnikoff is disposed to regard them as probable; for great ages are still occasionally recorded, although it is doubtful if any as considerable as these are ever substantiated. That the expectation of life was

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[28] This was regarded by Buffon as related to the period of growth, but the ratio is certainly not constant. The subject is discussed by Ray Lankester in an early work: *On Comparative Longevity in Man and Animals*, 1870.

[29] The approximately regular periods of longevity of different species of animals furnishes a strong argument against the theory that the decay of old age is an accidental phenomenon, comparable with disease.

better then than now would be inferred from the apologetic tone adopted by Jacob when questioned by Pharaoh as to his age: 'The days of the years of my pilgrimage are a hundred and thirty years; few and evil have the days of the years of my life been, and have not attained unto the days of the years of the life of my fathers in the days of their pilgrimage.' David, to whom, before the advent of the modern statistician, we owe the idea that seventy years is to be regarded as the normal period of life,[30] is himself merely stated to have 'died in a good old age.' The periods recorded for the Kings show a considerable falling-off as compared with the Patriarchs; but not a few were cut off by violent deaths, and many lived lives which were not ideal. Amongst eminent Greeks and Romans few very long lives are recorded, and the same is true of historical persons in mediæval and modern history. It is a long life that lasts much beyond eighty; three such linked together carry us far back into history. Mankind is in this respect more favoured than most mammals, although a few of these surpass the period of man's existence.[31] Strange that the brevity of human life should be a favourite theme of preacher and poet when the actual term of his 'erring pilgrimage' is greater than that of most of his fellow creatures!

The end of life.

The modern applications of the principles of preventive medicine and hygiene are no doubt operating to lengthen the average life. But even if the ravages of disease could be altogether eliminated, it is certain that at any rate the fixed cells of our body must eventually grow old and ultimately cease to function; when this happens to cells which are essential to the life of the organism, general death must result. This will always remain the universal law, from which there is no escape. 'All that lives must die, passing through nature to eternity.'

Such natural death unaccelerated by disease—is not death by disease as unnatural as death by accident?—should be a quiet, painless phenomenon, unattended by violent change. As Dastre expresses it, 'The need of death should appear at the end of life, just as the need of sleep appears at the end of the day.' The change has been led gradually up to by an orderly succession of phases, and is itself the last manifestation of life. Were we all certain of a quiet passing—were we sure that there would be 'no moaning of the bar when we go out to sea'—we could anticipate the coming of death after a ripe old age without apprehension. And if ever the time shall arrive when man will have learned to regard this change as a simple physiological process, as natural as

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[30] The expectation of life of a healthy man of fifty is still reckoned at about twenty years.

[31] 'Hominis ævum cæterorum animalium omnium superat præter admodum paucorum.'—Francis Bacon, *Historia vitæ et mortis*, 1637.

the oncoming of sleep, the approach of the fatal shears will be as generally welcomed as it is now abhorred. Such a day is still distant; we can hardly say that its dawning is visible. Let us at least hope that, in the manner depicted by Dürer in his well-known etching, the sunshine which science irradiates may eventually put to flight the melancholy which hovers, bat-like, over the termination of our lives, and which even the anticipation of a future happier existence has not hitherto succeeded in dispersing.

British Association for the Advancement of Science.

SECTION A: DUNDEE, 1912.

ADDRESS

TO THE

MATHEMATICAL AND PHYSICAL SCIENCE SECTION

BY

PROFESSOR H. L. CALLENDAR, LL.D., F.R.S.,

PRESIDENT OF THE SECTION.

My first duty on taking the chair is to say a few words in commemoration of the distinguished members whom we have lost since the last meeting.

George Chrystal, Professor of Mathematics in the University of Edinburgh for more than thirty years, officiated as President of this section in the year 1885, and took a prominent part in the advancement of science as Secretary of the Royal Society of Edinburgh since 1901. Of his brilliant mathematical work and his ability in developing the school at Edinburgh, I am not competent to speak, but I well remember as a student his admirable article on 'Electricity and Magnetism' contributed to the 'Encyclopædia Britannica,' which formed at that time the groundwork of our studies at Cambridge under Sir J. J. Thomson. It would be difficult to find a more complete and concise statement of the mathematical theory at the time when that article was written. One can well understand the value of such a teacher, and sympathise with his University in the loss they have sustained.

John Brown, F.R.S., who acted as Local Secretary for the Association at Belfast in 1902, will be remembered for his work on the Volta contact effect between metals, which he showed to be in the main dependent on chemical action, and to be profoundly affected by the nature of the gas or other medium in which the plates were immersed. Although the theory of this difficult subject may not yet be completely elucidated, there can be little doubt that his work takes the first rank on the experimental side.

William Sutherland, D.Sc., who at one time acted as Professor of Physics at Melbourne, is best known for his familiar papers on the subject of molecular physics in the 'Philosophical Magazine.' His work was always remarkable for its wide range and boldness of imagination. Many of his hypotheses cannot yet be weighed in the balance of experiment, but some have already been substantiated. For instance, his theory of the variation of viscosity of gases with temperature has been generally accepted, and results are now commonly expressed in terms of Sutherland's constant.

Osborne Reynolds, the first Professor of Engineering at Owens College, was President of Section G in 1887, but belongs almost as much to mathematics and physics, in which his achievements are equally memorable. It would be hardly possible for me to enumerate his important contributions to the science of engineering, which will be more fittingly commemorated elsewhere. His mastery of mathematical and physical methods, while contributing greatly to his success as a pioneer in the engineering laboratory, enabled him to attack the most difficult problems in physics, such as the theory of the radiometer and the thermal transpiration of gases. His determination of the mechanical equivalent of heat is a most striking example of accurate physical measurement carried out on an engineering scale. His last great work, on the 'Submechanics of the Universe,' is so original in its ideas and methods that its value cannot yet be fully appreciated. While it differs so radically from our preconceived ideas that it fails to carry immediate conviction, it undoubtedly represents possibilities of truth which subsequent workers in the same field cannot afford to ignore.

The present year has been one of remarkable activity in the world of Mathematical and Physical Science if we may measure activity by the number and importance of scientific gatherings like the present for the interchange of ideas and the general advancement of science. The celebration of the 250th Anniversary of the Foundation of the Royal Society brought to our shores a number of distinguished delegates from all parts of the world, to promote the ever-growing fellowship among men of science which is one of the surest guarantees of international progress. The Congress of Universities of the Empire brought other guests from distant British dominions, and considered, as one of the principal points in its programme, the provision of facilities for the interchange of students between different universities, which will doubtless prove particularly advantageous to the scientific student in the higher branches of research. In the special branches of knowledge more particularly associated with this section, the International Congress of Mathematics at Cambridge, while it affords to Cambridge men like myself a most gratifying recognition of our *Alma Mater* as one of the leading schools of mathematics in the world, has given us the opportunity of meeting here a number of distinguished foreign mathematicians whose presence and personality cannot be otherwise than inspiring to our proceedings, and will compensate for any deficiency in our own mathematical programme. The Optical Convention held this year in London, by the importance of the papers contributed for discussion, and by its admirable exhibition of British instruments, has revealed the extent of our optical industry and talent, and has done much to dispel the impression, fostered by an unfortunate trade regulation, that the majority of optical instruments were 'made elsewhere.' The Radio-Telegraphic Conference, held under the auspices of the British Government, has formulated recommendations for regulating and extending the application of the discoveries of modern physics for saving life and property at sea. The work of this Conference will be fittingly supplemented on the scientific side by the discussion on wireless telegraphy which has been arranged to take place in this section in conjunction with Section G.

It would be impossible, even if it were not out of place, for me to attempt to review in detail the important work of these congresses, a full account of which will shortly be available in their several reports of proceedings now in course of publication. In the present age of specialisation and rapid publication it would be equally impossible to give any connected account in the time at my disposal of recent developments in those branches of science which come within the range of our section. The appropriate alternative, adopted by the majority of my predecessors in this chair, is to select some theory or idea, sufficiently fundamental to be of general interest, and to discuss it in the light of recent experimental evidence. It may sometimes be advantageous to take stock of our fundamental notions in this way, and to endeavour to determine how far they rest on direct experiment, and how far they are merely developments of some dynamical analogy, which may represent the results of experiment up to a certain point, but may lead to erroneous conclusions if pushed too far. With this object I propose to consider on the present occasion some of our fundamental ideas with regard to the nature of heat, and in particular to suggest that we might with advantage import into our modern theory some of the ideas of the old caloric or material theory which has for so long a time been forgotten and discredited. In so doing I may appear to many of you to be taking a retrograde step, because the caloric theory is generally represented as being fundamentally opposed to the kinetic theory and to the law of the conservation of energy. I would, therefore, remark at the outset that this is not necessarily the case, provided that the theory is rightly interpreted and applied in accordance with experiment. Mistakes have been made on both theories, but the method commonly adopted of selecting all the mistakes made in the application of the caloric theory and contrasting them with the correct deductions from the kinetic theory has created an erroneous impression that there is something fundamentally wrong about the caloric theory, and that it is in the nature of things incapable of correctly representing the facts. I shall endeavour to show that this fictitious antagonism between the two theories is without real foundation. They should rather be regarded as different ways of describing the same phenomena. Neither is complete without the other. The kinetic theory is generally preferable for elementary exposition, and has come to be almost exclusively adopted for this purpose; but in many cases the caloric theory would have the advantage of emphasising at the outset the importance of fundamental facts which are too often obscured in the prevailing method of treatment.

The explanation of the development of heat by friction was one of the earliest difficulties encountered by the caloric theory. One explanation, maintained by Cavendish and others, was simply that caloric was generated *de novo* by friction in much the same way as electricity. Another explanation, more commonly adopted, was that the fragments of solid, abraded in such operations as boring cannon, had a smaller capacity for heat than the original material. Caloric already existing in the substance was regarded as being squeezed or ground out of it without any fresh caloric being actually generated. The probability of the second explanation was negatived by the celebrated experiments of Rumford and Davy, who concluded that friction did not diminish the capacities of bodies for heat, and that it could not be a material substance because the supply obtainable by friction appeared to be inexhaustible. Rumford also showed that no increase of weight in a body when heated could be detected by the most delicate apparatus available in his time. Caloric evidently did not possess to any marked extent the properties of an ordinary ponderable fluid; but, if it had any real existence and was not merely a convenient mathematical fiction, it must be something of the same nature as the electric fluids, which had already played so useful a part in the description of phenomena, although their actual existence as physical entities had not then been demonstrated. Heat, as Rumford and Davy maintained, might be merely a mode of motion or a vibration of the ultimate particles of matter, but the idea in this form was too vague to serve as a basis of measurement or calculation. The simple conception of caloric, as a measurable quantity of something, sufficed for many purposes, and led in the hands of Laplace and others to correct results for the ratio of the specific heats, the adiabatic equation of gases, and other fundamental points of theory, though many problems in the relations of heat and work remained obscure.

The greatest contribution of the caloric theory to thermodynamics was the production of Carnot's immortal 'Reflections on the Motive Power of Heat.' It is one of the most remarkable illustrations of the undeserved discredit into which the caloric theory has fallen, that this work, the very foundation of modern thermodynamics, should still be misrepresented, and its logic assailed, on the ground that much of the reasoning is expressed in the language of the caloric theory. In justice to Carnot, even at the risk of wearying you with an oft-told tale, I cannot refrain from taking this opportunity of reviewing the essential points of his reasoning, because it affords incidentally the best introduction to the conception of caloric, and explains how a quantity of caloric is to be measured.

At the time when Carnot wrote, the industrial importance of the steam-engine was already established, and the economy gained by expansive working was generally appreciated. The air-engine, and a primitive form of the internal-combustion engine, had recently been invented. On account of the high value of the latent heat of steam, it was confidently expected that more work might be obtained from a given quantity of heat or fuel by employing some other working substance, such as alcohol or ether, in place of steam. Carnot set himself to investigate the conditions under which motive-power was obtainable from heat, how the efficiency was limited, and whether other agents were preferable to steam. These were questions of immediate practical importance to the engineer, but the answer which Carnot found embraces the whole range of science in its ever widening scope.

In discussing the production of work from heat it is necessary, as Carnot points out, to consider a complete series or cycle of operations in which the working substance, and all parts of the engine, are restored on completion of the cycle to their initial state. Nothing but heat, or its equivalent fuel, may be supplied to the engine. Otherwise part of the motive power obtained might be due, not to heat alone, but to some change in the working substance, or in the disposition of the mechanism. Carnot here assumes the fundamental axiom of the cycle, which he states as follows : '*When a body has undergone any changes, and, after a certain number of transformations, is brought back identically to its original state, considered relatively to density, temperature, and mode of aggregation, it must contain the same quantity of heat as it contained originally.*' This does not limit the practical application of the theory, because all machines repeat a regular series of operations, which may be reduced in theory to an equivalent cycle in which everything is restored to its initial state.

The most essential feature of the working of all heat-engines, considered apart from details of mechanism, is the production of motive power by alternate expansion or contraction, or heating and cooling of the working substance. This necessitates the existence of a difference of temperature, produced by combustion or otherwise, between two bodies, such as the boiler and condenser of a steam-engine, which may be regarded as the source and sink of heat respectively. Wherever a difference of temperature exists, it may be made a source of motive-power, and conversely, without difference of temperature, no motive-power can be obtained from heat by a cyclical or continuous process. From this consideration Carnot deduces the simple and sufficient rule for obtaining the maximum effect : '*In order to realise the maximum effect, it is necessary that, in the process employed, there should not be any direct interchange of heat between bodies at sensibly different temperatures.*' Direct transference of heat between bodies at sensibly different temperatures would be equivalent to wasting a difference of temperature which might have been utilised for the production of motive-power. Equality of temperature is here assumed as the limiting condition of thermal equilibrium, such that an infinitesimal difference of temperature will suffice to determine the flow of heat in either direction. An engine satisfying Carnot's rule will be reversible so far as the thermal operations are concerned. Carnot makes use of this property of reversibility in deducing his formal proof that an engine of this type possesses the maximum efficiency. If in the usual or direct method of working such an engine takes a quantity of heat Q from the source, rejects heat to the condenser, and gives a balance of useful work W per cycle, when the engine is reversed and supplied with motive-power W per cycle it will in the limit take the same quantity of heat from the condenser as it previously rejected, and return to the source the same quantity of heat Q as it took from it when working direct. All such engines must have the same efficiency (measured by the ratio W/Q of the work done to the heat taken from the source) whatever the working substance, provided that they work between the same temperature limits. For, if this were not the case, it would be theoretically possible, by employing the most efficient to drive the least efficient reversible engine backwards, to restore to the source all the heat taken from it, and to obtain a balance of useful work without the consumption of fuel; a result sufficiently improbable to serve as the basis of a formal proof. Carnot thus deduces his famous principle, which he states as follows : '*The Motive Power obtainable from Heat is independent of the agents set at work to realise it. Its quantity is fixed solely by the temperatures between which in the limit the transfer of heat takes place.*'

Objection is commonly taken to Carnot's proof, on the ground that the combination which he imagines might produce a balance of useful work without infringing the principle of conservation of energy, or constituting what we now understand as perpetual motion of the ordinary kind in mechanics. It has become the fashion to introduce the conservation of energy in the course of the proof, and to make a final appeal to some additional axiom. Any proof of this kind must always be to some extent a matter of taste; but since Carnot's principle cannot be deduced from the conservation of energy alone, it seems a pity to complicate the proof by appealing to it. For the particular object in view, the absurdity of a heat-engine working without fuel appears to afford the most appropriate improbability which could be invoked. The final appeal must be to experiment in any case. At the present time the experimental verification of Carnot's principle in its widest application so far outweighs the validity of any deductive proof, that we might well rest content with the logic that satisfied Carnot instead of confusing the issue by disputing his reasoning.

Carnot himself proceeded to test his principle in every possible way by comparison with experiment as far as the scanty data available in his time would permit. He also made several important deductions from it, which were contrary to received opinion at the time, but have since been accurately verified. He appears to have worked out these results analytically in the first instance, as indicated by his footnotes, and to have translated his equations into words in the text for the benefit of his non-mathematical readers. In consequence of this, some of his most important conclusions appear to have been overlooked or attributed to others. Owing to want of exact knowledge of the properties of substances over extended ranges of temperature, he was unable to apply his principle directly in the general form for any temperature limits. We still labour to a less extent under the same disability at the present day. He showed, however, that a great simplification was effected in its application by considering a cycle of infinitesimal range at any temperature t. In this simple case the principle is equivalent to the assertion that the work obtainable from a unit of heat per degree fall (or per degree range of the cycle) at a temperature t, is some function $F't$ of the temperature (generally known as Carnot's Function), which must be the same for all substances at the same temperature. From the rough data then available for the properties of steam, alcohol, and air, he was able to calculate the numerical values of this function in kilogrammetres of work per kilocalorie of heat at various temperatures between 0° and 100° C., and to show that it was probably the same for different substances at the same temperature within the limits of experimental error. For the vapour of alcohol at its boiling-point 78°·7 C., he found the value $F't = 1{\cdot}230$ kilogrammetre per kilocalorie per degree fall. For steam at the same temperature he found nearly the same value, namely, $F't = 1{\cdot}212$. Thus no advantage in point of efficiency could be gained by employing the vapour of alcohol in place of steam. He was also able to show that the work obtainable from a kilocalorie per degree fall probably diminished with rise of temperature, but his data were not sufficiently exact to indicate the law of the variation.

The equation which Carnot employed in deducing the numerical values of his function from the experimental data for steam and alcohol is simply the direct expression of his principle as applied to a saturated vapour. It is now generally known as Clapeyron's equation, because Carnot did not happen to give the equation itself in algebraic form, although the principle and details of the calculation were most minutely and accurately described. In calculating the value of his function for air, Carnot made use of the known value of the difference of the specific heats at constant pressure and volume. He showed that this difference umst be the same for equal volumes of all gases measured under the same temperature and pressure, whereas it had always previously been assumed that the ratio (not the difference) of the specific heats was the same for different gases. He also gave a general expression for the heat absorbed by a gas in expanding at constant temperature, and showed that it must bear a constant ratio to the work of expansion. These results were verified experimentally some years later, in part by Dulong, and more completely by Joule, but Carnot's theoretical prediction has generally been overlooked, although it was of the greatest interest and importance. The reason of this neglect is probably to be found in the fact that Carnot's expressions contained the unknown function $F't$ of the temperature, the form of which could not be deduced without making some assumptions with regard to the nature of heat and the scale on which temperature should be measured.

It was my privilege to discover a few years ago that Carnot himself had actually given the correct solution of this fundamental problem in one of his most important footnotes, where it had lain buried and unnoticed for more than eighty years. He showed by a most direct application of the caloric theory, that if temperature was measured on the scale of a perfect gas (which is now universally adopted) the value of his function $F't$ on the caloric theory would be the same at all temperatures, and might be represented simply by a numerical constant A (our 'mechanical equivalent') depending on the units adopted for work and heat. In other words, the work W done by a quantity of caloric Q in a Carnot cycle of range T to T_0 on the gas scale would be represented by the simple equation :

$$W = AQ\,(T - T_0).$$

It is at once obvious that this solution, obtained by Carnot from the caloric theory, so far from being inconsistent with the mechanical theory of heat, is a direct statement of the law of conservation of energy as applied to the Carnot cycle. If the lower limit T_0 of the cycle is taken at the absolute zero of the gas-thermometer, we observe that the maximum quantity of work obtainable from a quantity of caloric Q at a temperature T is simply AQT, which represents the absolute value of the energy carried by the caloric taken from the source at the temperature T. The energy of the caloric rejected at the temperature T_0 is AQT_0. The external work done is equal to the difference between the quantities of heat energy supplied and rejected in the cycle.

The analogy which Carnot himself employed in the interpretation of this equation was the oft-quoted analogy of the waterfall. Caloric might be regarded as possessing motive-power or energy in virtue of elevation of temperature just as water may be said to possess motive-power in virtue of its head or pressure. The limit of motive-power obtainable by a reversible motor in either case would be directly proportional to the head or fall measured on a suitable scale. Caloric itself was not motive-power, but must be regarded simply as the vehicle or carrier of energy, the production of motive-power from caloric depending essentially (as Carnot puts it) not on the actual consumption of caloric, but on the fall of temperature available. The measure of a quantity of caloric is the work done per degree fall, which corresponds with the measure of a quantity of water by weight, *i.e.*, in kilogrammetres per metre fall.

That Carnot did not pursue the analogy further, and deduce the whole mechanical theory of heat from the caloric theory, is hardly to be wondered at if we remember that no applications of the energy principle had then been made in any department of physics. He appears, indeed, at a later date to have caught a glimpse of the general principle when he states that 'motive-power [his equivalent for work or energy] changes its form but is never annihilated.' It is clear from the posthumous notes of his projected experimental work that he realised how much remained to be done on the experimental side, especially in relation to the generation of caloric by friction, and the waste of motive-power by conduction of heat, which appeared to him (in 1824) 'almost inexplicable in the present state of the theory of heat.'

One of the points which troubled him most in the application of the theoretical result that the work obtainable from a quantity of caloric was simply proportional to the fall of temperature available, was that it required that the specific heat of a perfect gas should be independent of the pressure. This was inconsistent with the general opinion prevalent at the time, and with one solitary experiment by Delaroche and Bérard, which appeared to show that the specific heat of a gas diminished with increase of pressure, and which had been explained by Laplace as a natural consequence of the caloric theory. Carnot showed that this result did not necessarily follow from the caloric theory, but the point was not finally decided in his favour until the experiments of Regnault, first published in 1852, established the correct values of the specific heat of gases, and proved that they were practically independent of the pressure.

Another point which troubled Carnot was that, according to his calculations, the motive-power obtainable from a kilocalorie of heat per degree fall appeared to diminish with rise of temperature, instead of remaining constant. This might have been due to experimental errors, since the data were most uncertain. But, if he had lived to carry out his projected experiments on the quantity of motive-power required to produce one unit of heat, and had obtained the result, 424 kilogrammetres per kilocalorie, subsequently found by Joule, he could hardly have failed to notice that this was the same (within the limits of experimental error) as the maximum work AQT obtainable from the kilocalorie according to his equation. (This is seen to be the case when the values calculated by Carnot per degree fall at different temperatures were multiplied by the absolute temperature in each case. *E.g.*, 1·212 kilogrammetre per degree fall with steam at 79° C. or 352° Abs. $1{\cdot}212 \times 352 = 426$ kilogrammetres.) The origin of the apparent discrepancy between theory and experiment lay in the tacit assumption that the quantity of caloric in a kilocalorie was the same at different temperatures. There were no experiments at that time available to demonstrate that the caloric measure of heat as work per degree fall, implied in Carnot's principle, or more explicitly stated in his equation, was not the same as the calorimetric measure obtained by mixing substances at different temperatures. Even when the energy principle was established its exponents failed to perceive exactly where the discrepancy between the two theories lay. In reality both were correct, if fairly interpreted in accordance with experiment, but they depended on different methods of measuring a quantity of heat, which, so far from being inconsistent, were mutually complementary.

The same misconception, in a more subtle and insidious form, is still prevalent in such common phrases as the following: 'We now know that heat is a form of energy and not a material fluid.' The experimental fact underlying this statement is that our ordinary methods of measuring quantities of heat in reality measure quantities of thermal energy. When two substances at different temperatures are mixed, the quantity remaining constant, provided that due allowance is made for external work done and for external loss of heat, is the total quantity of energy. Heat is a form of energy merely because the thing we measure and call heat is really a quantity of energy. Apart from considerations of practical convenience, we might equally well have agreed to measure a quantity of heat in accordance with Carnot's principle, by the external work done in a cycle per degree fall. Heat would then not be a form of energy, but would possess all the properties postulated for caloric. The caloric measure of heat follows directly from Carnot's principle, just as the energy measure follows from the law of conservation of energy. But the term *heat* has become so closely associated with the energy measure that it is necessary to employ a different term, *caloric*, to denote the simple measure of a quantity of heat as opposed to a quantity of heat energy. The measurement of heat as caloric is precisely analogous to the measure of electricity as a quantity of electric fluid. In the case of electricity, the quantity measure is more familiar than the energy measure, because it is generally simpler to measure electricity by its chemical and magnetic effects as a quantity of fluid than as a quantity of energy. The units for which we pay by electric meter, however, are units of energy, because the energy supplied is the chief factor in determining the cost of production, although the actual quantity of fluid supplied has a good deal to do with the cost of distribution. Both methods of measurement are just as important in the theory of heat, and it seems a great pity that the natural measure of heat quantity is obscured in the elementary stages of exposition by regarding heat simply as so much energy. The inadequacy of such treatment makes itself severely felt in the later stages.

Since Carnot's principle was adopted without material modification into the mechanical theory of heat, it was inevitable that Carnot's caloric, and his solution for the work done in a finite cycle, should sooner or later be rediscovered. Caloric reappeared first as the 'thermo-dynamic function' of Rankine, and as the 'equivalence-value of a transformation' in the equations of Clausius; but it was regarded rather as the quotient of heat energy by temperature than as possessing any special physical significance. At a later date, when its importance was more fully recognised, Clausius gave it the name of *entropy*, and established the important property that its total quantity remained constant in reversible heat exchanges, but always increased in an irreversible process. Any process involving a decrease in the total quantity of entropy was impossible. Equivalent propositions with regard to the possibility or impossibility of transformations had previously been stated by Lord Kelvin in terms of the dissipation of available energy. But, since Carnot's solution had been overlooked, no one at the time seems to have realised that entropy was simply Carnot's caloric under another name, that heat could be measured otherwise than as energy, and that the increase of entropy in any irreversible process was the most appropriate measure of the quantity of heat generated. Energy so far as we know must always be associated with something of a material nature acting as carrier, and there is no reason to believe that heat energy is an exception to this rule. The tendency of the kinetic theory has always been to regard entropy as a purely abstract mathematical function, relating to the distribution of the energy, but having no physical existence. Thus it is not a quantity of anything in the kinetic theory of gases, but merely the logarithm of the probability of an arrangement. In a similar way, some twenty years ago the view was commonly held that electric phenomena were due merely to strains in the ether, and that the electric fluids had no existence except as a convenient means of mathematical expression. Recent discoveries have enabled us to form a more concrete conception of a charge of electricity, which has proved invaluable as a guide to research. Perhaps it is not too much to hope that it may be possible to attach a similar conception with advantage to caloric as the measure of a quantity of heat.

It has generally been admitted in recent years that some independent measure of heat quantity as opposed to heat energy is required, but opinions have differed widely with regard to the adoption of entropy as the quantity factor of heat. Many of these objections have been felt rather than explicitly stated, and are therefore the more difficult to answer satisfactorily. Others arise from the difficulty of attaching any concrete conception of a quantity of something to such a vague and shadowy mathematical function as entropy. The answer to the question 'What is caloric?' must necessarily be of a somewhat speculative nature. But it is so necessary for the experimentalist to reason by analogy from the seen to the unseen, that almost any answer, however crude, is better than none at all. The difficulties experienced in regarding entropy as a measure of heat quantity are more of an academic nature, but may be usefully considered as a preliminary in attempting to answer the more fundamental question.

The first difficulty felt by the student in regarding caloric as the measure of heat quantity is that when two portions of the same substance, such as water, at different temperatures are mixed, the quantity of caloric in the mixture is greater than the sum of the quantities in the separate portions. The same difficulty was encountered by Carnot from the opposite point of view. The two portions at different temperatures represented a possible source of motive-power. The question which he asked himself may be put as follows: 'If the total quantity of caloric remained the same when the two portions at different temperatures were simply mixed, what had become of the motive-power wasted?' The answer is that caloric is generated, and that the quantity generated is such that its energy is the precise equivalent of the motive-power which might have been obtained if the transfer of heat had been effected by means of a perfect engine working without generation of caloric. The caloric generated in wasting a difference of temperature is the necessary and appropriate measure of the quantity of heat obtained by the degradation of available motive-power into the less available or transformable variety of heat energy.

The processes by which caloric is generated in mixing substances at different temperatures, or in other cases where available motive-power is allowed to run to waste, are generally of so turbulent a character that the steps of the process cannot be followed, although the final result can be predicted under given conditions from the energy principle. Such processes could not be expected *à priori* to throw much light on the nature of caloric. The familiar process of conduction of heat through a body, the parts of which are at different temperatures, while equally leading to the generation of a quantity of caloric equivalent to the motive-power wasted, affords better promise of elucidating the nature of caloric, owing to the comparative simplicity and regularity of the phenomena, which permit closer experimental study. The earliest measurements of the relative conducting powers of the metals for heat and electricity showed that the ratio of the thermal to the electric conductivity was nearly the same for all the pure metals, and suggested that, in this case, the carriers of heat and electricity were the same. Later and more accurate experiments showed that the ratio of the conductivities was not constant, but varied nearly as the absolute temperature. At first sight this might appear to suggest a radical difference between the two conductivities, but it results merely from the fact that heat is measured as energy in the definition of thermal conductivity, whereas electricity is measured as a quantity of fluid. If thermal conductivity were defined in terms of caloric or thermal fluid, the ratio of the two conductivities would be constant with respect to temperature almost, if not quite, within the limits of error of experiment. On the hypothesis that the carriers are the same for electricity and heat, and that the kinetic energy of each carrier is the same as that of a gas molecule at the same temperature, it becomes possible, on the analogy of the kinetic theory of gases, to calculate the actual value of the ratio of the conductivities. The value thus found agrees closely in magnitude with that given by experiment, and may be regarded as confirming the view that the carriers are the same, although the hypotheses and analogies invoked are somewhat speculative.

When the electrons or corpuscles of negative electricity were discovered it was a natural step to identify them with the carriers of energy, and to imagine that a metal contained a large number of such corpuscles, moving in all directions, and colliding with each other, and with the metallic atoms, like the molecules of a gas on the kinetic theory. If the mass of each carrier were 1/1700 of that of an atom of hydrogen, the velocity at 0° C. would be about sixty miles a second, and would be of the right order of magnitude to account for the observed values of the conductivities of good conductors, on the assumption that the number of negative corpuscles was the same as the number of positive metallic atoms, and that the mean free path of each corpuscle was of the same order as the distance between the atoms. The same hypothesis served to give a qualitative account of thermo-electric phenomena, such as the Peltier and Thomson effects, and of radiation and absorption of heat, though in a less satisfactory manner. When extended to give a consistent account of *all* the related phenomena, it would appear that the number of free corpuscles required is too large to be reconciled, for instance, with the observed values of the specific heat, on the assumption that each corpuscle possesses energy of translation equal to that of a gas molecule at the same temperature.

Sir J. J. Thomson has accordingly proposed and discussed another possible theory of metallic conduction, in which the neutral electric doublets present in the metal are supposed to be continually interchanging corpuscles at a very high rate. Under ordinary condition these interchanges take place indifferently in all directions, but under the action of an electric field the axes of the doublets are supposed to become more or less oriented, as in the Grotthus-chain hypothesis of electrolytic conduction, producing a general drift or current proportional to the field This hypothesis, though fundamentally different from the preceding or more generally accepted view, appears to lead to practically the same relations, and is in some ways preferable, as suggesting possible explanations of difficulties encountered by the first theory in postulating so large a number of free negative corpuscles. On the other hand, the second theory requires that each neutral doublet should be continually ejecting corpuscles at the rate of about 10^{15} per second. There are probably elements of truth in both theories, but, without insisting too much on the exact details of the process, we may at least assert with some confidence that the corpuscles of caloric which constitute a current of heat in a metal are very closely related to the corpuscles of electricity, and have an equal right to be regarded as constituting a material fluid possessing an objective physical existence.

If I may be allowed to speculate a little on my own account (as we are all here together in holiday mood, and you will not take anything I may say too seriously), I should prefer to regard the molecules of caloric, not as being identical with the corpuscles of negative electricity, but as being neutral doublets formed by the union of a positive and negative corpuscle, in much the same way as a molecule of hydrogen is formed by the union of two atoms. Nothing smaller than a hydrogen atom has yet, so far as I know, been discovered with a positive charge. This may be merely a consequence of the limitations of our experimental methods, which compel us to employ metals to so large an extent as electrodes. In the symmetry of nature it is almost inconceivable that the positive corpuscle should not exist, if only as the other end of the Faraday-tube or vortex-filament representing a chemical bond. Professor Bragg has identified the X or γ rays with neutral corpuscles travelling at a high velocity, and has maintained this hypothesis with brilliant success against the older view that these rays are not separate entities, but merely thin, spreading pulses in the æther produced by the collisions of corpuscles with matter. I must leave him to summarise the evidence, but if neutral corpuscles exist, or can be generated in any way, it should certainly be much easier to detach a neutral corpuscle from a material atom or molecule than to detach a corpuscle with a negative charge from the positive atom with which it is associated. We should therefore expect neutral corpuscles to be of such exceedingly common and universal occurrence that their very existence might be overlooked, unless they happened to be travelling at such exceptionally high velocities as are associated with the γ rays. According to the pulse theory, it is assumed that all γ rays travel with the velocity of light, and that the enormous variations observed in their penetrative power depend simply on the thickness of the pulse transmitted. On the corpuscular theory, the penetrative power, like that of the α and β rays, is a question of size, velocity, and electric charge. Particles carrying electric charges, like the α and β rays, lose energy in producing ions by their electric field, perhaps without actual collision. Neutral or γ rays do not produce ions directly, but dislodge either γ rays or β rays from atoms by direct collisions, which are comparatively rare. The β rays alone, as C. T. R. Wilson's photographs show, are responsible for the ionisation. Personally, I have long been a convert to Professor Bragg's views on the nature of X rays, but even if we regard the existence of neutral corpuscles as not yet definitely proved, it is, I think, permissible to assume their existence for purposes of argument, in order to see whether the conception may not be useful in the interpretation of physical phenomena.

If, for instance, we assume that these neutral corpuscles or molecules of caloric exist in conductors and metallic bodies in a comparatively free state of solution, and are readily dissociated into positive and negative electrons owing to the high specific inductive capacity of the medium, the whole theory of metallic conduction follows directly on the analogy of conduction in electrolytic solutions. But, whereas in electrolytes the ions are material atoms moving through a viscous medium with comparatively low velocities, the ions in metallic conductors are electric corpuscles moving with high velocities more after the manner postulated in the kinetic theory of gases. It is easy to see that this theory will give similar numerical results to the electronic theory when similar assumptions are made in the course of the work. But it has the advantage of greater latitude in explaining the vagaries of sign of the Hall effect, and many other peculiarities in the variation of resistance and thermo-electric power with temperature. For good conductors, like the pure metals, we may suppose, on the electrolytic analogy, that the dissociation is practically complete, so that the ratio of the conductivities will approach the value calculated on the assumption that all the carriers of heat are also carriers of electricity. But in bad conductors the dissociation will be far from complete, and it is possible to see why, for instance, the electric resistance of cast iron should be nearly ten times that of pure iron, although there is comparatively little difference in their thermal conductivities. The numerical magnitude of the thermo-electric effect, which is commonly quoted in explanation of the deviation of alloys from the electronic theory, is far too small to produce the required result; and there is little or no correspondence between the thermo-electric properties of the constituents of alloys and the variations of their electric conductivities.

One of the oldest difficulties of the material theory of heat is to explain the process of the production of heat by friction. The application of the general principle of the conservation of energy leads to the undoubted conclusion that the thermal energy generated is the equivalent of the mechanical work spent in friction, but throws little or no light on the steps of the process, and gives no information with regard to the actual nature of the energy produced in the form of heat. It follows from the energy principle that the quantity of caloric generated in the process is such that its total energy at the final temperature is equal to the work spent. If a quantity of caloric represents so many neutral molecules of electricity, one cannot help asking where they came from, and how they were produced. It is certain that in most cases of friction, wherever slip occurs, some molecules are torn apart, and the work spent is represented in the first instance by the separation of electric ions. Some of these ions are permanently separated as frictional electricity, and can be made to perform useful work; but the majority recombine before they can be effectively separated, leaving only their equivalent in thermal energy. The recombination of two ions is generally regarded simply as reconstituting the original molecule at a high temperature, but in the light of recent discoveries we may perhaps go a step further. It is generally admitted that X or γ rays are produced by the sudden stoppage of a charged corpuscle, and Lorentz, in his electron theory of radiation, has assumed that such is the case however low the velocity of the electron. A similar effect must occur in the sudden stoppage of a pair of ions rushing together under the influence of their mutual attraction. Rays produced in this way would be of an exceedingly soft or absorbable character, but they would not differ in kind from those produced by electrons except that their energy, not exceeding that of a pair of ions, would be too small to produce ionisation, so that they could not be detected in the usual way. If the X rays are corpuscular in their nature, we cannot logically deny the corpuscular character even to the slowest moving rays. We know that X rays continually produce other X rays of lower velocity. The final stage is probably reached when the average energy of an X corpuscle or molecule of caloric is the same as that of a gas molecule at the same temperature, and the number of molecules of caloric generated is such that their total energy is equal to the work originally spent in friction.

In this connection it is interesting to note that Sir J. J. Thomson, in a recent paper on 'Ionisation by Moving Particles,' has arrived, on other grounds, at the conclusion that the character of the radiation emitted during the recombination of the ions will be a series of pulses, each pulse containing the same amount of energy and being of the same type as very soft X rays. If the X rays are really corpuscular, these definite units or quanta of energy generated by the recombination of the ions bear a close resemblance to the hypothetical molecules of caloric.

It may be objected that in many cases of friction, such as internal or viscous friction in a fluid, no electrification or ionisation is observable, and that the generation of caloric cannot in this case be attributed to the recombination of ions. It must, however, be remarked that the generation of a molecule of caloric requires less energy than the separation of two ions; that, just as the separation of two ions corresponds with the breaking of a chemical bond, so the generation of one or more molecules of caloric may correspond with the rupture of a physical bond, such as the separation of a molecule of vapour from a liquid or solid. The assumption of a molecular constitution for caloric follows almost of necessity from the molecular theories of matter and electricity, and is not inconsistent with any well-established experimental facts. On the contrary, the many relations which are known to exist between the specific heats of similar substances, and also between the latent heats, would appear to lead naturally to a molecular theory of caloric. For instance, it has often been noticed that the molecular latent heats of vaporisation of similar compounds at their boiling-points are proportional to the absolute temperature. It follows that the molecular latent caloric of vaporisation is the *same* for all such compounds, or that they require the same number of molecules of caloric to effect the same change of state, irrespective of the absolute temperatures of their boiling-points. From this point of view one may naturally regard the liquid and gaseous states as conjugate solutions of caloric in matter and matter in caloric respectively. The proportion of caloric to matter varies regularly with pressure and temperature, and there is a definite saturation limit of solubility at each temperature.

One of the most difficult cases of the generation of caloric to follow in detail is that which occurs whenever there is exchange of heat by radiation between bodies at different temperatures. If radiation is an electro-magnetic wave-motion, we must suppose that there is some kind of electric oscillator or resonator in the constitution of a material molecule which is capable of responding to the electric oscillations. If the natural periods of the resonators correspond sufficiently closely with those of the incident radiation the amplitude of the vibration excited may be sufficient to cause the ejection of a corpuscle of caloric. It is generally admitted that the ejection of an electron may be brought about in this manner, but it would evidently require far less energy to produce the emission of a neutral corpuscle, which ought therefore to be a much more common effect. On this view, the conversion of energy of radiation into energy of caloric is a discontinuous process taking place by definite molecular increments, but the absorption or emission of radiation itself is a continuous process. Professor Planck, by a most ingenious argument based on the probability of the distribution of energy among a large number of similar electric oscillators (in which the entropy is taken as the logarithm of the probability, and the temperature as the rate of increase of energy per unit of entropy), has succeeded in deducing his well-known formula for the distribution of energy in full radiation at any temperature; and has recently, by a further extension of the same line of argument, arrived at the remarkable conclusion that, while the absorption of radiation is continuous, the emission of radiation is discontinuous, occurring in discrete elements or quanta. Where an argument depends on so many intricate hypotheses and analogies the possible interpretations of the mathematical formulæ are to some extent uncertain; but it would appear that Professor Planck's equations are not necessarily inconsistent with the view above expressed that both emission and absorption of radiation are continuous, and that his *elementa quanta*, the energy of which varies with their frequency, should rather be identified with the molecules of caloric, representing the conversion of the electro-magnetic energy of radiation into the form of heat, and possessing energy in proportion to their temperature.

Among the difficulties felt rather than explicitly stated, in regarding entropy or caloric as the measure of heat quantity, is its awkward habit of becoming infinite, according to the usual approximate formulæ, at extremes of pressure or temperature. If caloric is to be regarded as the measure of heat quantity, the quantity existing in a finite body must be finite, and must vanish at the absolute zero of temperature. In reality there is no experimental foundation for any other conclusion. According to the usual gas formulæ it would be possible to extract an infinite quantity of caloric from a finite quantity of gas by compressing it at constant temperature. It is true that (even if we assumed the law of gases to hold up to infinite pressures, which is far from being the case) the quantity of caloric extracted would be of an infinitely low order of infinity as compared with the pressure required. But, as a matter of fact, experiment indicates that the quantity obtainable would be finite, although its exact value cannot be calculated owing to our ignorance of the properties of gases at infinite pressures. In a similar way, if we assume that the specific heat as ordinarily measured remains constant, or approaches a finite limit at the absolute zero of temperature, we should arrive at the conclusion that an infinite quantity of caloric would be required to raise the temperature of a finite body from 0° to 1° absolute. The tendency of recent experimental work on specific heats at low temperatures, by Tilden, Nernst, Lindemann, and others, is to show, on the contrary, that the specific heats of all substances tend to vanish as the absolute zero is approached and that it is the specific capacity for caloric which approaches a finite limit. The theory of the variation of the specific heats of solids at low temperatures is one of the most vital problems in the theory of heat at the present time, and is engaging the attention of many active workers. Professor Lindemann, one of the leading exponents of this work, has kindly consented to open a discussion on the subject in our section. We are very fortunate to have succeeded in securing so able an exponent, and shall await his exposition with the greatest interest. For the present I need only add that the obvious conclusion of the caloric theory bids fair to be completely justified.

A most interesting question, which early presented itself to Rumford and other inquirers into the caloric theory of heat, was whether caloric possessed *weight*. While a positive answer to this question would be greatly in favour of a material theory, a negative answer, such as that found by Rumford, or quite recently by Professor Poynting and Phillips, and by Mr. L. Southerns working independently, would not be conclusively against it. The latter observers found that the change in weight, if any, certainly did not exceed 1 in 10^8 per 1° C. If the mass of a molecule of caloric were the same as that generally attributed to an electron, the change of weight, in the cases tested, should have been of the order of 1 in 10^7 per 1° C., and should not have escaped detection. It is generally agreed, however, that the mass of the electron is entirely electro-magnetic. Any such statement virtually assumes a particular distribution of the electricity in a spherical electron of given size. But if electricity itself really consists of electrons, an argument of this type would appear to be so perfectly circular that it is questionable how much weight should be attached to it. If the equivalent mass of an electron in motion arises solely from the electro-magnetic field produced by its motion, a neutral corpuscle of caloric should not possess mass or energy of translation as a whole, though it might still possess energy of vibration or rotation of its separate charges. For the purpose of mental imagery we might picture the electron as the free or broken end of a vortex filament, and the neutral corpuscle as a vortex ring produced when the positive and negative ends are united; but a mental picture of this kind does not carry us any further than the sphere coated with electricity, except in so far as either image may suggest points for experimental investigation. In our ignorance of the exact mechanism of gravity it is even conceivable that a particle of caloric might possess mass without possessing weight, though, with the possible exception of the electron, nothing of the kind has yet been demonstrated. In any case it would appear that the mass, if any, associated with a quantity of caloric must be so small that we could not hope to learn much about it by the direct use of the balance.

The fundamental property of caloric, that its total quantity cannot be diminished by any known process and that it is not energy but merely the vehicle or carrier of energy, is most simply represented in thought by imagining it to consist of some indestructible form of matter. The further property, that it is always generated in any turbulent or irreversible process, appears at first sight to conflict with this idea, because it is difficult to see how anything indestructible can be so easily generated. When, however, we speak of caloric as being generated, what we really mean is that it becomes associated with a material body in such a way that we can observe and measure its quantity by the change of state produced. The caloric may have existed previously in a form in which its presence could not be detected. In the light of recent discoveries we might suppose the caloric generated to arise from the disintegration of the atoms of matter. No doubt some caloric is produced in this way, but those corpuscles that are so strongly held as to be incapable of detection by ordinary physical methods require intense shocks to dislodge them. A more probable source of caloric is the æther, which, so far as we know, may consist entirely of neutral corpuscles of caloric. The hypothesis of a continuous æther has led to great difficulties in the electro-magnetic theory of light and in the kinetic theory of gases. A molecular, or cellular-vortex, structure appears to be required. According to the researches of Kelvin, Fitzgerald, and Hicks, such an æther can be devised to satisfy the requirements of the electro-magnetic theory without requiring it to possess a density many times greater than that of platinum. So far as the properties of caloric are concerned, a neutral pair of electrons would appear to constitute the simplest type of molecule, though without more exact knowledge of the ultimate nature of an electric charge it would be impossible to predict all its properties. Whether an æther composed of such molecules would be competent to discharge satisfactorily all the onerous functions expected from it, may be difficult to decide, but the inquiry, in its turn, would probably throw light on the ultimate structure of the molecule.

Without venturing too far into the regions of metaphysical speculation, or reasoning in vicious circles about the nature of an electric charge, we may at least assert with some degree of plausibility that material bodies under ordinary conditions probably contain a number of discrete physical entities, similar in kind to X rays or neutral corpuscles, which are capable of acting as carriers of energy, and of preserving the statistical equilibrium between matter and radiation at any temperature in virtue of their interchanges with electrons. If we go a step further and identify these corpuscles with the molecules of caloric, we shall certainly come in conflict with some of the fundamental dogmas of the kinetic theory, which tries to express everything in terms of energy, but the change involved is mainly one of standpoint or expression. The experimental facts remain the same, but we describe them differently. Caloric has a physical existence, instead of being merely the logarithm of the probability of a complexion. In common with many experimentalists, I cannot help feeling that we have everything to gain by attaching a material conception to a quantity of caloric as the natural measure of a quantity of heat as opposed to a quantity of heat energy. In the time at my disposal I could not pretend to offer you more than a suggestion of a sketch, an apology for the possibility of an explanation, but I hope I may have succeeded in conveying the impression that a caloric theory of heat is not so entirely unreasonable in the light of recent experiment as we are sometimes led to imagine.

British Association for the Advancement of Science.

SECTION B: DUNDEE, 1912.

ADDRESS

TO THE

CHEMICAL SECTION

BY

PROFESSOR A. SENIER, PH.D., M.D., D.SC.

PRESIDENT OF THE SECTION.

PART I.

The Nature and method of Chemistry.

PERHAPS there is no intellectual occupation which demands more of the faculty of imagination than the pursuit of chemistry, and perhaps also there is none which responds more generously to the yearnings of the inquirer. It is surely no commonplace occurrence that in experimental laboraties day by day the mysterious recesses of Nature are disclosed and facts previously quite unknown are brought to light. The late Sir Michael Foster, in his presidential address at the Dover meeting, said: 'Nature is ever making signs to us, she is ever whispering the beginnings of her secrets.' The facts disclosed may have general importance, and necessitate at once changes in the general body of theory; and happily, also, they may at once find useful application in the hands of the technologist. Recent examples are the discoveries in radio-activity, which have found an important place as an aid to medical and surgical diagnosis and as a method of treatment, and have also led to the necessity of our revising one of the fundamental doctrines of the theory of chemistry—the indivisibility of atoms. But the facts disclosed may not be general or even seem important; they may appear isolated and to have no appreciable bearing on theory or practice—our journals are crowded with such—but he would be a bold man who would venture to predict that the future will not find use for them in both respects. To be the recipient of the confidences of Nature; to realise in all their virgin freshness new facts recognised as positive additions to knowledge, is certainly a great and wonderful privilege, one capable of inspiring enthusiasm as few other things can.

While the method of discovery in chemistry may be described, generally, as inductive, still all the modes of inference which have come down to us from Aristotle, analogical, inductive and deductive, are freely made use of. A hypothesis is framed which is then tested, directly or indirectly, by observation and experiment. All the skill, all the resource the inquirer can command, is brought into his service; his work must be accurate; and with unqualified devotion to truth he abides by the result, and the hypothesis is established, and becomes part of the theory of science, or is rejected or modified. In framing or modifying hypotheses imagination is indispensable. It may be that the power of imagination is necessarily limited by what is previously in experience—that imagination cannot transcend experience; but it does not follow, therefore, that it cannot construct hypotheses capable of leading research. I take it that what imagination actually does is—it rearranges experience and puts it into new relations, and with each successive discovery it gains in material for this process. In this respect the framing of a hypothesis is like experimenting, wherein the operator brings matter and energy already existing in Nature into new relations, new circumstances, with the object of getting new results. The stronger the imaginative power the greater must be the chance of success. The 'Times,' in a recent leading article on Science and Imagination, says: 'It has often been said that the great scientific discoverers . . . see a new truth before they prove it, and the process of proof is only a demonstration of the truth to others and a confirmation of it to their own reason.' While never forgetting the essentially tentative nature of a hypothesis, still, until it has been tested and found wanting, there should be some confidence or faith in its truthfulness; for nothing but a belief in its eventual success can serve to sustain an inquirer's ardour when, as so often happens, he is met by difficulties well-nigh insuperable. In a well-known passage Faraday says: 'The world little knows how many of the thoughts and theories which have passed through the mind of a scientific investigator have been crushed in silence and secrecy by his own severe criticism and adverse examination; that in the most successful instances not a tenth of the suggestions, the hopes, the wishes, the preliminary conclusions have been realised.'

But a hypothesis to be useful, to be admitted as a candidate for rank as a scientific theory, must be capable of immediate, or at least of possible, verification. Many years ago, in the old Berlin laboratory in the Georgenstrasse, when our imaginations were wont, as sometimes happened, to soar too far above the working benches, our great leader used to say: 'I will listen readily to any suggested hypothesis, but on one condition—that you show me a method by which it can be tested.' As a rule, I confess we had to return to the workaday world, to our bench experiments. No one felt the importance of the careful and correct employment of hypotheses more than Liebig. In his Faraday lecture Hofmann says of Liebig: 'If he finds his speculation to be in contradiction with recognised facts, he endeavours to set these facts aside by new experiments, and failing to do so he drops the speculation.' Again, he gives an illustration of how on one occasion, not being able to divest himself of a hypothesis, he missed the discovery of the element bromine. While at Kreuznach he made an investigation of the mother-liquor of the well-known salt, and obtained a considerable quantity of a heavy red liquid which he believed to be a chloride of iodine. He found the properties to be different in many respects from chloride of iodine; still, he was able to satisfy all his doubts, and he put the liquid aside. Some months later he received Balard's paper announcing the discovery of bromine, which he recognised at once as the red liquid which he had previously prepared and studied. Thus, though imagination is indispensable to a chemist, and though I think chemists should be, and let us hope are, poets, or at least possess the poetic temperament, still, little can be achieved without a thorough laboratory training; and he who discovers an improved experimental method or a new differentiating reaction is as surely contributing to the advancement of science as he who creates in his imagination the most beautiful and promising hypothesis.

It may never be possible to trace in civilisation's early records the exact period and place of the origin and beginnings of our science, but the historical student has been led, it appears to me, by a sure instinct to search for this in such lands of imaginative story as ancient Egypt and Arabia. For is there anything more fittingly comparable with the marvellous experiences of a chemical laboratory than the wonderful and fascinating stories that have come down to us in 'The Arabian Nights'? Those monuments of poetical building of which Burton, in the introduction to his great translation, says that in times of official exile in less-favoured lands, in the wilds of Africa and America, he was lifted in imagination by the jinn out of his dull surroundings, and was borne off by him to his beloved Arabia, where under diaphanous skies he would see again 'the evening star hanging like a golden lamp from the pure front of the western firmament; the after-glow transfiguring and transforming as by magic the gazelle-brown and tawny-clay tints and the homely and rugged features of the scene into a fairyland lit with a light which never shines on other soils for seas. Then would appear, &c.' I cannot help thinking that the study of such books as this, the habit of exercising the imagination by reconstructing the scenes of beauty and enchantment which they describe, might do much to strengthen and sharpen the imaginative faculty, and greatly increase its efficiency as an indispensable tool in the hands of the pioneer who seeks to extend the boundaries of knowledge. The 'Times,' in the leading article already quoted, says that, as with a Shakespeare, 'it is the same with imaginative discoverers in science. . . . But the faculty is not merely a fairy gift that can be exercised without pains. As the sense of right is trained by right action, so the sense of truth is trained by right thinking and by all the labour which it involves. That is as true of the artist as of the man of science; and one of the greatest achievements of science has been to prove this fact and so to justify the imagination and distinguish it from fancy.'

Again, let it not be forgotten that chemistry in its highest sense—that is, in its most general and useful sense—is purely a world of the imagination, is purely conceptual. And in addition to this, moreover, it is based, like all science, on the underlying assumption of the uniformity of Nature, an assumption incapable of proof. If we think of the science as a body of abstract general theory, and exclude for the moment from our purview its innumerable practical applications, and also all special individual facts not yet known to be related to general theory, then what remains are the more or less general facts or laws. These it is which give the power of prediction in newly arising cases of a similar character; the power of foresight by which the claim of chemistry to its position as a science is justified. Chemistry, as such, is a complicated ideal structure of the imagination, a gigantic fairy palace, and, be it noted, can only continue to exist so long as there are minds capable of reproducing it. Think of all the speculation—and speculation too of the highest utility when translated into concrete applications—about the internal structure of molecules. I venture to say that the most magnificent creations of the world's greatest architects are not more elaborate or more beautiful or more fairylike than the chemist's conception of intramolecular structure and the magical transformations of which molecules are capable; and yet no one has had direct sensuous experience of any molecule or atom, or possibly ever will. It is well from time to time to recall these truths and realise where we are. But although the conceptual nature of science is unquestionable, it certainly contains truth in some form as tested by deductive concrete realisation, by correctness of prediction, and during the last century or two has undoubtedly given to man a mastery over Nature never before dreamt of.

A brief historical retrospect.

The foundations of chemistry, as we now know it, were laid under the influence, the guidance, of three great theories: first, the theory of the alchemists of the transmutation of metals by means of the philosopher's stone; second, the theory of phlogiston, connected so much with the names of Becher and Stahl, which held sway for some two centuries; third, the theory of combustion, the quantitative period of chemistry, inaugurated by the great Scottish chemist Black by his introduction of the balance. How this led to a veritable renascence of chemistry in the hands of Lavoisier and the other giants of that stirring period—the close of the eighteenth and commencement of the nineteenth centuries—is well known. Looking back at the warfare which was waged about these older theories, for and against them, one realises now that there were elements of truth on both sides; for have we not in the work of Sir William Ramsay and others the revival of transmutation, and does not the essential truth of phlogiston survive in the modern doctrine of heat? In one of Dr. Johnson's letters to Boswell there is a curious reference to transmutation. He says that a learned Russian had at last succeeded, but, fearing the consequences to society, he had died without revealing the secret.

After the discovery of oxygen and the beginnings of quantitative chemistry, the science was ready for Dalton's great discoveries respecting combination by weight; the corresponding discoveries by Gay-Lussac on combination of gases by volume, and, through the latter, for Avogadro's famous hypothesis. Dalton had indeed, by reviving an old Greek suggestion, proposed to explain his discoveries by his atomic theory, but neither this nor our molecular theory, though the latter was inherent in the laws of gaseous combination of Gay-Lussac and in Avogadro's hypothesis, were finally put upon their present basis until Cannizzaro took up the subject half a century later. Meanwhile Dulong and Petit had completed their studies of atomic heat, and Mitscherlich had pointed out the relation between isomorphism and molecular structure. When it is considered how little is known of solid or liquid structure, and that our present knowledge of molecules is only of gaseous molecules, it is fortunate that these methods of study of solids are available. The same may be said of the results of the work of Kopp and his successors on molecular volumes. Of other aids to fixing our conception of molecules and atoms I need only refer to the periodic law, the studies of the properties of dilute solutions, of electrolytic dissociation, and of surface tension of liquids.

Liebig, in his first inquiry, begun before he went to Gay-Lussac in Paris, proved that silver fulminate and silver cyanate, though distinct substances, had exactly the same composition; thus was opened that great chapter in the history of chemistry which Berzelius named isomerism. Perhaps nothing in chemistry has given rise in recent years to more intellectual and practical activity than isomerism. Wöhler's classical synthesis of urea, by the metastasis of ammonium cyanate, added another instance of isomerism, and Berzelius soon afterwards announced the isomerism of tartaric and racemic acids. Wöhler's synthesis of urea, followed, as it was, by numerous other laboratory syntheses, showed that substances which occur in living organisms are not different from those which may be prepared artificially, and the old distinction between inorganic and organic chemistry disappeared—there is, of course, only one chemistry. The words it is true have survived, but only for reasons of practical convenience.

After isomerism the next great step forward in the study of intra-molecular structure was the discovery of groups partially individualised which are capable of remaining intact through many reactions. Gay-Lussac had previously noticed the Cyanogen group as common to cyanides; but it was the celebrated paper by Wöhler and Liebig on 'The Radical of Benzoic Acid' which finally established the existence of compound radicals or groups such as benzoyl, and obtained for the theory of compound radicals the position in chemistry it now holds. Bunsen followed somewhat later with the discovery of cacodyl, and now such groups are almost innumerable. In many respects, by the experimental skill which it shows, the clearness of its logical method, and the beauty of its form and diction, this memoir is a model of what a scientific communication should be. I will read the opening paragraph, using Hofmann's translation: 'When a chemist is fortunate enough to encounter, in the darksome field of organic nature, a bright point affording him guidance to the true path by following which he may hope to explore the unknown region he has good reason to congratulate himself, even though he may be conscious of being still far from the desired goal.' Of this memoir Belzelius, in a letter quoted by Hofmann (Faraday lecture), says: 'The facts put forward by you give rise to such considerations that they may well be regarded as the dawn of a new day in vegetal (organic) chemistry.'

The history of the advance of chemistry since the days of the Giessen laboratory is bewildering in its extent. This has been largely due to the Giessen laboratory itself, which sent trained investigators, each carrying with him some touch of its master's magic, into all civilised lands. I cannot attempt to even catalogue the results here. One thing may be said, that chemistry is not worked out, as some have thought; but rather the opportunities of discovery seem greater and more promising than at any previous period.

PART II.

Sub-atoms, atoms, molecules, molecular aggregates; valency.

Whether in the light of recent researches it may become necessary to give up that portion of Dalton's theory of atoms in which he regards them as undecomposable and indivisible; or whether we may consider them, as Prout suggested a hundred years ago, as different aggregates of sub-atoms of a uniform kind of matter; or whether they must be regarded as complexes built in the manner supposed by the electron hypothesis; also what should be our attitude towards the related problem of transmutation—all this I pass over, the more willingly that these subjects were discussed so recently by so high an authority as Sir William Ramsay in his address to the Association last year at Portsmouth.

I assume that we are fairly satisfied with our present atoms and their respective weights, and this no matter how the atoms are constructed, and that we shall be satisfied with them so long as they disport themselves in chemical changes as indivisible entities. And further, I assume that we are satisfied with our molecules and their respective weights, as determined by the application of Avogadro's hypothesis. Whether the molecular weight is obtained by direct determination of gaseous density or by taking advantage of the properties of dilute solutions, in either case the molecular weight which results is the weight of a supposed gaseous molecule, for the latter method depends for its justification on the former. All our molecular weights are weights of molecules in the gaseous state or are supposed to be; they are not necessarily applicable to liquids, and much less to solids : solids and liquids may well consist of far more complex particles.

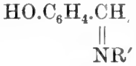
Gradually the central problem of chemistry has become more and more the study of the internal structure of molecules—of gaseous molecules. The enormous number and variety of the compounds of carbon, with which so many workers have enriched the science during the last hundred years, and the special adaptability of these compounds to the experimental study of molecular structure, has led investigators to make use of them rather than of the so-called inorganic compounds : thus out of inquiries into the intramolecular structure of these compounds arose and were developed the theories of types of Gerhardt, Williamson and Kekulé. These are now, however, looked upon more as aspects of the general problem. More fruitful has been the study of the compound radicals or individualised groups of Wöhler and Liebig. But gradually these molecular structures have been regarded, in agreement with the views of Dumas, as complete wholes; like fairy temples, which from different points of view show different parts in relief, accentuating, it may be, this or that column or frieze or pediment. Kekulé's brilliant and suggestive theory of chain compounds and ring compounds did more than any other theory to guide and stimulate research in chemistry in recent times. Like Gay-Lussac's theory of gaseous combination, though built in the first place only upon a few facts, this theory has proved true of the thousands of others with which we have since become acquainted; there seems indeed to be a need of a new psychology to account for such truly marvellous foresight as is here exhibited. The atoms forming these varied structures were, however, regarded as being arranged in a plane, until the great discoveries of Pasteur made it necessary for chemists to extend their conceptions and to frame hypotheses of three dimensions. Thus has arisen in the hands of Le Bel and van't Hoff and others our modern theories of stereo-chemistry. When isomerism occurs in an element Berzelius names it allotropy. It seems to me that now, when molecules of the elements do not differ essentially from molecules of compounds, there is no longer any distinctive meaning in the term, and that it might well be abandoned. I would like also to make another suggestion respecting nomenclature : that when we distinguish ring compounds as *cyclic* we might appropriately adopt the word *hormathic* (from the Greek word for a chain or a row) for chain compounds.

But in order to understand the linking of atoms in these molecular edifices some combining value had to be assigned to the different atoms. This idea of valency of the atoms was, no doubt, implied in Gerhardt's theory of types; but it did not gain much attention until later, when Frankland and Kolbé formulated an empirical theory of variable valency. Kekulé thought that atoms could not vary in their valency; but the alternative formulæ which he put forward to explain cases of difficulty would appear to be, rather, an attempted explanation of variable valency. It might be more correct to say that Kekulé's formulæ constitute an anticipation of Werner's theory of auxiliary valencies, the theory which seems to find most favour at the present day. Fixed valency can scarcely now be defended, in view of the existence of such compounds, for example, as the two fluorides and the two chlorides of phosphorus; the two oxides of carbon, ammonia and ammonium chloride; and, for example, the two series of compounds respectively of iron, mercury and copper. Variable valency of atoms is empirically, at least, an established fact.

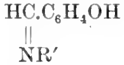
By the latest conceptions of variable atomic valency and its extension almost without limit—so that, for example, oxygen may be regarded as quadrivalent and even sexivalent—no doubt the existence of numerous compounds which previously presented difficulties can be explained. There are, however, others long known to chemists, such as double salts and the combination of water with salts, formerly called 'molecular compounds,' definite and individual, in which these views do not assist us. These compounds do not exist as gases, and unless they admit of experimental study by the methods of dilute solution, even their gaseous molecular weights cannot be ascertained.

It is noteworthy that in most of the instances recently investigated where variable valency has been assumed the compounds studied have been easily decomposable solids or liquids, and for one reason or another their gaseous molecular weights could not be determined. Many of these compounds, indeed, only exist at low temperatures. As instances of work of this kind I may mention Collie and Tickle on quadrivalent oxygen in dimethylpyrone derivatives; Gomberg on triphenylmethyl; Landolf on acetone di-hydrofluoride; Thiele and Peter on methyl-iodo-dichloride; and similar studies by Kehrmann, Willstätter and Iglauer, Bülow and Sicherer, Baeyer and Villiger, Archibald and McIntosh, Chattaway, Pfeiffer and Truskier, and others.

Another most interesting class of solids which are capable of existing in two isomeric forms distinguished from each other by such physical properties as density or colour are the Schiff's bases or anils. Some of these were studied by Hantzsch, who proposed to explain their existence by the Hantzsch-Werner stereo hypothesis :—



$$\begin{array}{r} HO.C_6H_4.CH \\ \| \\ NR' \end{array}$$



$$\begin{array}{l} HC.C_6H_4OH \\ \| \\ NR' \end{array}$$

But since only a few, and these not very satisfactory compounds, show this isomerism, which do not contain the hydroxyl group, other suggestions have been put forward to account for the isomerism, by Anselmino and by Manchot.

In my own laboratory, associated with Mr. F. G. Shepheard and also with Miss Rosalind Clarke, I have made a study of various Schiff's bases for the purpose of investigating the remarkable property which some of these bases exhibit of *phototropy*. By phototropy is meant the capability of reversible change of colour in solids depending upon the presence or absence of light. Incidentally, too, I wished to study another physical property which many Schiff's bases possess, in common with other substances, of reversible change of colour with raising or lowering of temperature. This property we have called *thermotropy*, and many old instances will be remembered of substances of simpler constitution which exhibit it : thus, when subjected to the temperature of solid carbon dioxide, ordinary sulphur becomes colourless, red oxide of mercury becomes yellow, vermilion becomes scarlet, and on return to the ordinary temperature the original colours reappear.

As has been pointed out in a recent communication by Biilman, it is most important in these discussions that we should be perfectly clear in the use of terms. I take it for granted that *isomerism* is a general term for compounds differing in some respect but having the same composition. If the molecules (gaseous) have the same weights they are *metamerides*; if of different weights they are *polymerides*. When solids crystallise in more than one form they are *polymorphous*. Now it does not seem reasonable to suppose that reversible colour changes such as those exhibited by phototropes or thermotropes involve such violent intra-molecular changes as the breaking and reconnecting of atomic linkages. For example, take the three bases, salicylidene-*m*-toluidine, which in the dark or immediately it is exposed to light is yellow, but on continued exposure to light quickly becomes orange, and changes back again to its original colour in the dark; salicylidene-*m*-aminophenol, which at ordinary temperatures is orange, but is much paler at the temperature of solid carbon dioxide, on raising the temperature to nearly the melting-point (128·9°) becomes orange red, and these changes take place in the reverse order again on cooling; salicylidene-*p*-aminobenzoic acid, studied by ourselves and by Manchot and Furlong independently, shows a wider range of thermotropic change between bright yellow and blood-red, and is also phototropic.

To explain such changes as these and the others of a similar nature previously referred to, I think some less drastic hypothesis should be sought than intra-molecular breaking, and consequent metastasis or polymerisation. Though doubtless the hypothesis of Hantzsch and Werner could be invoked, or the modified hypotheses of Manchot or Anselmino, I think there should be some simpler explanation. Someone suggests polymorphism. Now polymorphism means that a change of crystalline form takes place which might doubtless connote change of colour. If one watches phototropic crystals changing colour under the influence of light from yellow to red, and notices that after remaining in the dark that the same crystals have changed back to the original colour, and, remember, that these changes can be repeated with the same crystals apparently without limit, it will not be considered likely that this phenomenon depends on a reversible change of crystalline form. In a communication to the Chemical Society some three years ago Mr. Shepheard and I put forward the following suggestion: 'Evidence is accumulating of reversible isomeric reactions, like those described in this paper, which are indicated by physical differences, such as changes of colour. It is possible that these may be explained by hypotheses, similar to that of Hantzsch and Werner assuming intra-molecular rearrangement; but in the case of phototropy and thermotropy it should not be forgotten that the substances exhibiting these phenomena are solids. No one will doubt, however, that these differences of colour depend on isomeric change of some kind, but in the case of solids we know practically nothing of their molecules, not even of their relative molecular weights. The molecules of solids are probably far more complex than those of liquids or gases; indeed, they may be rather complex groups or aggregates of ordinary gaseous molecules, which would give rise to far more numerous possibilities of isomerism. It appears to us that phototropic and thermotropic reactions are more probably due to isomeric changes affecting the aggregation of molecules in solids than to intra-molecular change of molecules derived from a study of gases.'

It seems to me that just as atoms may be structures built of sub-atoms of some kind, and just as molecules of gases are built of atoms variously linked together, that it is reasonable to conceive that molecules might combine to form aggregates, particularly when constituting solids; that as the sub-atoms may be conceived to have a combining valency, and the atoms are already accredited with this property, and in addition, as is supposed with Thiele's partial or Werner's auxiliary valencies, that molecules may have valencies also whereby to combine into molecular aggregates. It may be presumed that such aggregates are more complicated in structure, and thus may give rise to greater variety of isomerides, and be more readily transmutable than gaseous molecules. If such aggregates of gaseous molecules exist they might explain not only the easily changed isomerides recently studied, but also the large class of 'molecular compounds' of the older chemists. I imagine someone saying that in suggesting this hypothesis—which by the way is not new, for it is mentioned in Ostwald's 'Outlines'—I am violating the canon to which I have myself subscribed, as a condition of a scientific hypothesis, that it should be verifiable. Perhaps we carry our critical faculty sometimes too far. It is most highly scientific to doubt, but doubt which is merely destructive has little value; rather, with Descartes, it should lead on to construction, for he who builds even imperfectly is better than he who simply destroys. And I do not doubt that some way will be found to study solids and obtain data that will lead to the determination of their molecular aggregate weights. The study of molecular volumes of solid solutions; the remarkable results obtained by Pope and Barlow; Tutton's work on crystallography, and much besides, induce the hope that some day solids like gases will find their Avogadro.

PART III.

Pursuit of Chemistry justified by its useful applicability.

In the pursuit of all this abstract theory, and still more so in the bewildering multitude of undigested individual facts, there is danger that important and fundamental, even moral, considerations may be lost sight of. For example take the fundamental question, Why should we pursue chemistry? No doubt it is considered by its votaries, those who seek in our laboratories to advance the science, that they are entitled to have provided for them, and will be rewarded by the provision of, the ordinary means of livelihood; but these, it will scarcely be denied, could generally be far better assured by other pursuits. It is suggested that intellectual discipline is a reason; but, I ask, for what purpose? Will anyone pretend that intellectual discipline without utilitarian object, without the possibility of using it for the betterment of society, is a worthy pursuit? I think not. But, in any case, none of us have devoted ourselves to chemistry merely for the sharpening of our wits. Again, someone suggests that chemistry and learning generally should be pursued for their own sake. In a recent most interesting and inspiring academic address[1] Professor Sir Walter Raleigh commends 'those who seek nothing from knowledge but the pleasure of understanding.' If such a statement bears its most obvious meaning then, I venture to think that, in common with intellectual discipline without the intention of applying to a useful object the intellect so trained, such a reason is selfish, inadequate, and unworthy, and does not justify the pursuit of anything. No: research in chemistry apart from the possibility of applying it to the advantage of humanity cannot be defended. The mastery of the seemingly unlimited resources of Nature which chemistry achieves more and more and its use to alleviate the misery and add to the happiness of mankind is the only worthy and effective defence. And that this is the underlying ideal, in point of fact, that leads the chemist onward, not necessarily that he is always conscious of it, but always when he reflects, I think cannot be doubted. But, of course, no narrow idea of utility must be aimed at. Practically any chemical inquiry may lead to results of material advantage. Certainly nothing could be more mischievous than to make a narrow immediate utility the test. It would be easy to illustrate all this from the records of science, but instances in point are so well known that it is unnecessary.

On the other hand, it should not be forgotten that in making use of the manifold advantages derived from the growth of science, humanity, on its part, owes a great debt to scientific inquirers, and ought to feel it a sacred duty to do in return all in its power by support and encouragement to further scientific research. As Sir Walter Raleigh, in the address already referred to, says: 'It is so easy to use the resources of civilisation that we fall into the habit of regarding them as if they were ours by right. They are not ours by right; they come to us by free gift from the thinkers.'

That this advantage to civilisation has been, and is, the result of the pursuit and consequent advance of chemistry is happily a truth that is well known.

Some concrete applications of the Science.

There is scarcely an industry or a profession that has not been materially influenced or even created by the discoveries of chemistry, and therefore the welfare of nations is most intimately concerned in promoting its advancement. Now, it is common knowledge that no country has appreciated this to the same degree as Germany. It will, therefore, be worth our while to consider a moment the inauguration in Berlin, a year ago, of an entirely new institution, the Kaiser Wilhelm Institut, for the promotion and organisation of chemical research. This research is to be effected throughout the German Empire, in the universities, the technical high schools, or in works, and it is supported mainly, at least at first, by subscriptions of the chemical manufacturers. An address of very great importance was delivered at its opening by Professor Emil Fischer, than whom, perhaps, no one living has added more to the progress of chemistry. A translation of this address appeared in 'Nature,' and, with additions, has since been published in a convenient book form.[2] In this address an authoritative account is given of the main contributions of chemistry to the national welfare, which even to those familiar with the subject must be astonishing in their importance, variety, and universality. It includes the applications of the science to problems of nourishment, to agriculture, and food supply; to engineering, metallurgy, cements; to clothing, artificial silk, or to colouring—dyes; to indiarubber production, both natural and artificial; to perfumery—artificial violet and other artificial floral perfumes, even that of the rose; to synthetic camphor; to drugs and synthetic materia medica, including the recent arsenic and selenium organic compounds which promise so much in the treatment of cancer and other fatal diseases; to radio-activity, to therapeutics, to the destruction of pathogenic microbes; to methods of sewage disposal; to the preparation of efficient explosives; and to

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[1] 'The Meaning of a University.' Clarendon Press, 1911.

[2] 'Chemical Research in its Bearings on National Welfare.' London, 1912.

many other useful objects. In connection with the manufacture of explosives the public should know that the ability to wage war is becoming more and more dependent on the work of chemists. When the supply of mineral nitrates is exhausted, or even before that event, the requisite nitrogen compounds will have to be provided in some other way, and almost certainly they will be obtained synthetically from the atmospheric gases which even now are becoming a commercial source.

The Time-spirit and Science.

But students of history know that there are certain periods that for some unexplained reason are specially fruitful in certain departments of intellectual or artistic development. Professor Sir Walter Raleigh, for instance, a high authority on this subject, says : 'The human body, so far as we know, has not been improved within the period recorded by history; nor has the human mind, so far as we can judge, gained anything in strength or grace.' Further, regarding literature : 'The question is not by how much we can excel our fathers, but whether with toil and pains we may make ourselves worthy to be ranked with them.' Again : 'In the beautiful art which models the human figure in stone or some other enduring material, who can hope to match the Greeks? In the art of building who can look at the crowded confusion of any great modern city, with all its fussy and meaningless wealth of decoration, like a pastrycook's nightmare, and not marvel at the simplicity, the gravity, the dignity and the fitness of the ancient classic buildings? How can the seasoned wisdom of life be better or more searchingly expressed than in the words of Virgil or Horace, not to speak of more ancient teachers?' Thus all things are not progressing. The time-spirit now, and for some two centuries past, seems to have chosen to take under its particular guardianship the physical and natural sciences, their cultivation and applications, rather than philosophy or architecture or sculpture, or painting or literature. We shall do well to recognise this, and not waste our resources in striving to fight against it.

Present indiscriminate elementary teaching and neglect of research.

Large sums of money are expended in this country on the diffusion of some knowledge of chemistry among all classes of scholars and students; in fact, scarcely anyone escapes from a smattering, largely undigested if not indigestible, either forced on them by regulations or by the allurement of bribes in the form of prizes, scholarships, or academic laurels. And if this is not good for scholars or students, it is worse for masters or professors. Our professors work 'whole time' at this 'stall-feeding' process, and if they happen to be strong men mentally and physically they may be able when weary with work to devote any overtime to—what I submit is far the more important matter for the State—the advancement of science by research. But this pursuit requires, for its successful prosecution, for resource and initiative to be at their best, that all the faculties should be in readiness in their fullest strength, freedom, and adaptability. How many, alas! are not strong men, and in their praiseworthy endeavours, notwithstanding, to contribute something to the achievements of their time succumb as martyrs to their devotion. The truth of this statement, I fear, is too well known to many of us here. In Germany this strain of elementary teaching is more recent, and is only now being felt. Professor Emil Fischer in his address (*loc. cit.*) says of it : 'During the last ten years a scheme of practical education of the masses has developed.' 'But this very education of the masses tends mentally to exhaust the teacher, and to a great extent, certainly to a higher degree than is desirable or indeed compatible with the creative power of the investigator, there prevails in modern educational laboratories a condition of overstrained activity.' And again, 'In the harassing cares of the day the teacher too readily loses that peace of mind and broad view of scientific matters necessary for tackling the larger problems of research.' Laboratories, he says, are wanted 'which should permit of research in absolute tranquillity, unencumbered by the duties of teaching.' I have given these quotations from Professor Fischer's address as indicating the matured judgment of a highly competent authority, communicated in the presence of the German Emperor on an historic occasion. His words are words of great weight, and no country which regards its future welfare can afford to ignore them.

Sir Walter Raleigh (*loc. cit.*) says that every university is bound to help the poor . . . but that does not mean that a university is doing good if it helps those who have no special bent for learned pursuits to acquire with heavy labour and much assistance—just so much as may enable them to pass muster; on the contrary, it is doing harm. I would like to invite the attention of all who are seriously interested in the country's welfare to reconsider the present policy in the teaching of chemistry; and this applies also to other sciences. For the advancement of civilisation, for the increased welfare of the race by the technical applications of our science, it is not the indiscriminate teaching of the masses and the multiplication of examinations that is wanted, but the training of the few, of capable investigators. I do not propose necessarily that we should interfere with, or much less abandon, much of our present elementary teaching, and I know that elementary, largely technical, training in chemistry is needed for medicine and engineering; but I do propose that our first endeavour should be to secure under present conditions in the present college or works laboratories, or in laboratories to be specially provided, that capable men, of whom we have many, should be able to devote themselves to research without the worry of teaching and examining or of providing the ways and means of livelihood. There is, happily, reason to believe that this vital need is to some extent becoming known; for there have been several recent instances where a particular investigator has been afforded the means, financially, of prosecuting his particular researches in tranquillity. The diversion of endowments to such purposes, instead of their going to the foundation of additional school or undergraduate scholarships, cannot be too highly commended.

We may learn a lesson which bears on this from that remarkably prolific period of our science, the close of the eighteenth and the beginning of the nineteenth centuries. It was then no easy matter to pass the precincts of a chemical laboratory; only the fittest survived the ordeal. At the beginning of the nineteenth century the traditions of Berthollet and Lavoisier in Paris were kept alive by Gay-Lussac; in England those of Cavendish and Priestley by Davy; and Berzelius in Sweden worthily maintained the older school of Bergmann and Scheele. By a happy fate the interest of Alexander v. Humboldt was the means of both Liebig and Dumas being admitted to the intimacy of Gay-Lussac; and in Sweden Wöhler was fortunate to gain the confidence of Berzelius; and in London, Faraday that of Davy. The achievements of these men—Liebig, Dumas, Wöhler, and Faraday—is part of the history of science. To me it contains a lesson, in point, of great importance. The opportunity offered them was beset with difficulties. No bribes such as scholars or students expect to-day were offered them; they knew no examinations, and their available apparatus and laboratory equipment was of the smallest and crudest description; but they were eager students with whom the master was in sympathy, and it is common knowledge that they completed the foundations of our science. Now I ask, considering the thousands of students whom we teach and examine to-day, are we doing as well in the interest of the country as our predecessors a century ago? Who can confidently answer in the affirmative? No; whatever else is done, the country needs the provision of men whose untrammelled energy should be devoted to original chemical research. Even as intellectual discipline the value of research is of the highest importance. In his address to the British Association at Winnipeg, Professor Sir J. J. Thomson bears testimony to this. He says: 'I have had considerable experience with students beginning research in experimental physics, and I have always been struck by the quite remarkable improvement in judgment, independence of thought, and maturity produced by a year's research. Research develops qualities that are apt to atrophy when the student is preparing for examinations, and, quite apart from the addition of new knowledge to our store, is of the greatest importance as a means of education.'

And the object and ideal is wrong also in our system of technical training. We aim too much at giving elementary instruction to artizans, which, though important in itself, can never take the place of the higher education of leaders or managers of industrial works. This is different in Germany, where, although the training of artizans is by no means neglected, the chief energy is directed to the training and teaching of the smaller class of managers. There is, too, in Germany a far more intimate relation between academic and industrial work, and the leaders in each often interchange posts. In one respect we have an advantage over Germany; it is important that this should be understood. The higher technical instruction across the Rhine has not been undertaken by the universities, but is carried out in separate institutions. With us the universities have gradually undertaken, in addition to the older technical subjects, theology, medicine, and law, the various branches of engineering and agriculture, and even commerce. This, it is to be hoped, will be extended so that the highly trained technologist may have the advantage of the undoubted humanising influence of the university.

Conclusion.

I have not attempted in this Address any complete survey of chemistry, either its growth in the past or its present condition, but I have endeavoured to give some account of the sort of thing chemistry is—of its method—and to maintain three theses: (1) That the logical method by which chemistry advances is not a simple one, and requires as one essential element the use of a highly developed imagination. To render this more efficient I have advocated special training. (2) Without violating, I hope, the canons of the proper use of hypothesis, I have proposed, in order to account for certain isomeric and other phenomena, the conception of solid molecular aggregates, although I am not able at present to indicate precise methods for its further investigation. These molecular aggregates are supposed to be formed by the combination of gaseous molecules just as the latter are formed by the combination of atoms. (3) As a matter of vital interest to the continued well-being of this country I have insisted strongly that our educational resources devoted to chemistry should be directed, in the first place and chiefly, to the highest possible training of promising students in the prosecution of research, and that the giving to the many of elementary instruction should be at least a secondary consideration.

Now I do not wish to dictate how this last proposition could be best carried into effect. I think we should distinguish three classes of chemists, or technical chemists, whose domains would more or less overlap. Occasionally there will be a man, like the late Sir William Perkin, who would combine all three. The three classes are: first, the pure chemist, devoted to scientific discovery only; second, the technical chemist, who prepares the discoveries of the pure chemist for the technologist, and has to determine such questions as economical production and, for example, the conversion of colours into dyes; third, the technologist or works manager. These three classes should be in close relation to one another. By such a scheme we should probably overcome by education one of our most serious present difficulties—the ignorance of owners of works of the value of science.

It is a matter deserving most earnest consideration whether, under the propitious influence of our own time-spirit, it would be possible to organise research and develop it without interfering with its essential freedom and initiative, and this in each of the three classes I have mentioned, either by means of some of our existing institutions, or by the inauguration here of such an organisation as the Kaiser Wilhelm Institut in Berlin.

British Association for the Advancement of Science.

SECTION C: DUNDEE, 1912.

ADDRESS

TO THE

GEOLOGICAL SECTION

BY

B. N. PEACH, LL.D., F.R.S.,

PRESIDENT OF THE SECTION.

THE RELATION BETWEEN THE CAMBRIAN FAUNAS OF SCOTLAND AND NORTH AMERICA.

CONTENTS.

Introduction.

EVER since the announcement made by Salter in 1859 that the biological affinities of the fossils found in the Durness Limestone are more closely linked with American than with European forms, the relation between the older palæozoic faunas of Scotland and North America has been a subject of special interest to geologists. The subsequent discovery of the *Olenellus* fauna in the North-West Highlands furnished striking confirmation of Salter's opinion. This intimate relationship raises questions of prime importance bearing upon the sequence and distribution of life in Cambrian time in North America and North-West Europe, on the probable migration of forms from one life-province to another, and on the palæogeographical conditions which doubtless affected these migrations.

On this occasion, when the British Association revisits the border of the Scottish Highlands, it seems appropriate to refer to some of these problems. With this object in view I shall try to recapitulate briefly the leading features of the life history of Cambrian time in Scotland and North America, to indicate the relation which these life-provinces bear to each other, and, from these data, to draw some inferences regarding the probable distribution of land and sea which then obtained in those regions.

The two great rock groups in Scotland that are universally admitted to be older that Cambrian time are the Lewisian Gneiss and the Torridon Sandstone. The Lewisian Gneiss, as mapped by the Geological Survey, consists mainly of igneous rocks, or of gneisses and schists of igneous origin. But, in addition to these materials, we find, in the Loch Maree region, schists of sedimentary origin, comprising siliceous schist, mica-schist, graphite-schist, limestone, chert, and other sediments. The association of graphite-schist with limestone and chert suggests that we are here dealing with rocks that were formed at or near the extreme limit of sedimentation, where the graphite, the limestone, and the chert were probably accumulated from the remains of plankton. But this assemblage has been so completely altered into crystalline schists that all traces of original organic structure in them have been destroyed.

The Torridonian strata were evidently accumulated under desert or continental conditions, and could therefore furnish little or no evidence bearing upon the development of marine life. That life existed, however, is clear from the presence of phosphatic nodules, containing remains of cells and fibres of organic origin, in the upper division of the system, and from the presence of worm burrows and casts in the Diabaig beds (Lower Torridon).

Geologists are familiar with the fact that the Cambrian faunas all over the globe present highly specialised types belonging to most of the great groups of marine invetebrate life. Scotland is no exception to this general rule. For the fossils prove that their ancestors must have had a long history in pre-Cambrian time.

The Cambrian Fauna of Scotland.

Beginning with the false-bedded quartzites forming the basal sub-division of the Cambrian strata in the North-West Highlands, we find no traces of organic remains in them, except at one locality, where worm casts (*Scolithus linearis*) were obtained. In the upper subdivision of the quartzites—the pipe-rocks—the cylinders of sand are so numerous that the beds have been arranged in five subzones, based on a definite order of succession of different forms probably of specific value. One of them, *Arenicolites* of Salter, may be of generic importance. Worms of this habit are confined to comparatively shallow water, and therefore near the shore line. Their occurrence helps to confirm the belief that the quartzites were laid down on an ancient shelving shore line during a period of gentle subsidence. Their presence also indicates the existence of plankton, from which they derived nourishment. Besides the relics of these burrowing annelids, one of the subzones of the pipe-rock has yielded specimens of *Salterella* (*Serpulites Maccullochii*)—a tubicolar annelid, which becomes more abundant in the overlying fucoid beds, serpulite grit, and basal limestone, where it is associated with *Olenellus* and other typical Lower Cambrian forms.

The fucoid beds, which immediately overlie the pipe-rocks, consist chiefly of shales and brown dolomitic bands, with intercalations of grit locally developed. This type of sedimentation indicates that the mud line was superimposed on the shore line by subsidence. With this change of conditions there is a change of organisms, for though the burrowing forms (*Scolithus*) are still to be found in the sandy layers, the most characteristic types are those occurring along the bedding planes, known under the name of *Planolites* (Nicholson). They are very varied forms, and were probably produced by many types of errant annelids. The tubicolar annelids are represented by *Salterella, Coleoloides,* and *Hyolithes*—an organism which perhaps links the worms with the hingeless brachiopods. This suggestion gains additional support from the researches of Dr. Walcott in the Middle Cambrian rocks of Canada. It is interesting to note that small annelids seem to have bored the spines of dead trilobites. Walcott has found similar borings in the chetæ of annelids in the Middle Cambrian rocks of Canada.[1]

The researches of Dr. Walcott have proved beyond doubt that representatives of nearly all the divisions of the annelids are entombed in the Middle Cambrian rocks of Mount Stephen, in British Columbia. We may therefore reasonably infer that the worm casts of *Scolithus* type found in the North-West Highlands are due to annelids. He has also shown that worm-like holothurians are to be found in the same beds.[2] In this connection it may be observed that some of the recent holothurians have much the same habit of obtaining nourishment from the sands and silts containing organic matter.

Fragments showing the characteristic microscopic structures of the plates and ossicles of echinoderms have been found in the fucoid beds. These are probably Cystidean. Hingeless forms of brachiopods also occur, among which may be mentioned *Paterina labradorica* and *Acrothele subsidua.* The type of *Acrothele* suggests a genetic descent from such a tubicolar worm as *Hyolithes.* Of the gasteropods, only one specimen, belonging to a subgenus of *Murchisonia,* has been obtained at one locality in Skye. *Helenia bella,* a curved calcareous tube, open at both ends, doubtfully referred to the *Dentalidae* by Walcott, is comparatively plentiful. It occurs also in the *Olenellus* zone in Newfoundland.

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[1] *Smithsonian Miscell. Collect.*, vol. 57, No. 5, p. 125, 1911.

[2] *Ibid.*, No. 3, 1911.

But the organic remains that render the fucoid beds of exceptional interest and importance are the trilobites, because they clearly define the horizon of this zone in the Cambrian system and display strong affinities with American types. They are represented by five species and varieties of *Olenellus*, very closely resembling the forms in the Georgian terrane, or *Olenellus* zone, on the east and west sides of the North American continent. The genus *Olenelloides* has also been recorded from these beds. The crustacea are represented by phyllocarids, among which we find *Aristozoe rotundata*, likewise characteristic of the *Olenellus* zone of North America.

Next in order comes the serpulite grit, which indicates a recrudescence of the pipe-rock conditions of deposition, and presents the *Scolithus* type of annelid borings. From the diameter of the pipe and the depth of the burrow it is probable that the worm may have belonged to a different species from any of those whose casts are to be found in lower horizons. This large variety is associated with smaller and more irregular worm casts which have often weathered out and leave the rock honeycombed with hollow casts. The characteristic form from which the zone takes its name is *Salterella* (*Serpulites Maccullochii*). It occurs abundantly along certain calcareous layers that mark pauses in the deposition of the sand. This calcareous type culminates at the top of the zone, where there is a thick, carious, weathering band, crowded with specimens of *Salterella*, forming a passage bed into the calcareous shales at the base of the Durness dolomites. At one locality near Loch an Nid, Dundonnell Forest, Ross-shire, thin shales, intercalated in the serpulite grit, yielded a fine carapace of *Olenellus Lapworthi*—a form of frequent occurrence in the underlying fucoid beds. Professor Lapworth recorded the finding of *Orthoceras* and linguloid shells in the top part of this zone at Eireboll.[3]

Immediately above the serpulite grit in Eireboll and Assynt we find a few feet of dark calcareous shale, with iron pyrites, probably deposited at the limit of sedimentation. This layer, which is singularly devoid of organisms, ushers in the great succession of dolomites and limestones, upwards of 1,500 feet in thickness—perhaps the most remarkable type of sedimentation among the Cambrian rocks of the North-West Highlands. The Geological Survey has divided this calcareous sequence into seven well-marked groups, some of which have as yet yielded no fossils beyond worm casts. Attention will presently be directed to the absence of calcareous forms in many of the bands of dolomite and to the probable cause of their disappearance.

The thin calcareous shale just referred to is followed by dark blue dolomite limestone, forming the basal portion of the Ghrudhaidh group. It contains sparsely scattered, well-rounded sand grains, with a bed about three feet thick, near the bottom, charged with *Salterella pulchella* and *S. rugosa*. In the overlying twenty feet of dolomite the sand grains gradually disappear, and the rock assumes a mottled character, due to innumerable worm casts of the *Planolites* type. Here a second layer, yielding *Salterella pulchella* and *S. rugosa*, supervenes, both forms occurring in the *Olenellus* zone of North America.

The brief summary of the palæontological evidence which has just been given clearly shows that the strata ranging from the middle of the pipe-rock zone to the upper *Salterella* band of the Durness dolomites represent in whole or in part the *Olenellus* zone of North America. Owing to the absence of fossils we have no means of deciding more definitely the base and top of the Lower Cambrian rocks of the North-West Highlands. All the quartzites lying below the middle of the pipe-rock, notwithstanding the absence of zonal forms, have been included in the Lower Cambrian division. This correlation receives some support from the remarkable discovery of Dr. Walcott, who found primitive trilobites several thousand feet beneath the beds yielding *Olenellus Gilberti*, the form closely allied to the Highland trilobites.

On the other hand, when we pass upwards for a certain distance from the *Salterella* bands the evidence is insufficient to establish the stratigraphical horizon of the beds. For in the overlying strata, comprising the remainder of the Ghrudhaidh group, the whole of the Eilean Dubh group, and the lower part of the Sail Mhor group, and consisting of dolomites, limestones, and cherts, with little or no terrigenous material, the only fossils that can be shown to be due to

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[3] *Geol. Mag.*, vol. x., new series p. 126. 1883.

organisms are worm casts of the nature of *Planolites*, although the limestone and chert may have originated from the débris of the calcareous and siliceous organisms of the plankton. A noticeable feature of the Ghrudhaidh and Eilean Dubh groups is the occurrence in them of bands of brecciated dolomite on several horizons, which do not imply any break in the continuous sequence of deposits. The total thickness of this portion of the Durness dolomites and limestones, yielding no fossils beyond worm casts, amounts to 350 feet.

But in the upper part of the Sail Mhor group siliceous and calcareous organisms of a higher grade make their appearance. Among the former we find the *Rhabdaria* of Billings. The calcareous forms are represented by (1) gasteropods, including a single specimen of a murchisonid, two species of a pleurotomarid (*Euconia Ramsayi and E. Etna*) of a type occurring in the calciferous rocks of Newfoundland and Canada; (2) cephalopods, comprising two slightly bent forms with closely set septa and wide endogastric siphuncles, showing affinities with those of *Endoceras* and *Piloceras*; (3) arthropods, represented by the epitome of a large asaphoid trilobite resembling that of *Asaphus canalis* of Conrad. This evidence is insufficient to determine the exact horizon of these beds, but clearly indicates that we are no longer dealing with Lower Cambrian strata. The cephalopods are like those found in the Ozarkic division of Ulrich (Upper Cambrian), in North America. According to Schuchert, the cephalopods with closely set septa are of Cambrian type and older than those of the Beekmantown terrane of American geologists. On the other hand, the asaphoid type of trilobite is suggestive of a somewhat higher horizon.

No fossils have been found in the overlying Sangomore group, about 200 feet thick, which consists mainly of granular dolomite, with bands of chert, some being oölitic, together with thin fine-grained limestones near the top.

Above this horizon, at a height of over 800 feet above the top of the *Olenellus* zone, we encounter the great home of the fossils peculiar to the Durness limestone in the Balnakeil and Croisaphuill groups. The former consists mostly of dark limestones, with nodules of chert, and, with a few alternations, of white limestone bands. A few thin layers are charged with worm casts. The overlying group is more varied, the lower part being composed of dark grey limestones full of worm casts, and with some small chert nodules arranged in lines; the middle portion, of dark granular and unfossiliferous dolomite; and the upper part, of massive sheets of fossiliferous limestone full of worm casts. The total thickness of these two groups in Durness is about 550 feet.

These two subdivisions have yielded over twenty genera and about one hundred species. In Durness sixty-six species have been obtained from the Balnakeil group alone, fifteen of which have not as yet been found in the overlying Croisaphuill group, thus leaving fifty-one species common to both divisions. The Ben Suardal limestones in Skye, which were mapped by the Geological Survey as one division, are regarded, on palæontological grounds, as the equivalents of both these groups. Owing to the number of species common to both subdivisions, the fauna will be here referred to as a whole.

Both siliceous and calcareous organisms are present in this fauna. Among the former we find *Archæoscyphia* (Hinde), described by Billings as *Archæocyathus*, an early Cambrian coral, but shown by Hinde to be a siliceous sponge.[4] The genus *Calathium* is represented by four species. Other genera and species of sponges occur, so that the siliceous nodules, which are very common in both groups, may be in great part due to them. In this connection it may be mentioned that Hinde obtained sponge spicules from some of the nodules. Hinged brachiopods have also been collected from these beds and include *Nisusia* (*Orthosina*) *festinata*, *N. grandæva*, and Camarella.

But the characteristic feature of the fauna is the assemblage of calcareous mollusca comprising lamellibranchs, gasteropods, and cephalopods, showing a wide range of variation, and consequently a long ancestry. The lamellibranchs, though represented only by two genera, *Euchasma* and *Eopteria* of Billings, with several intermediate forms, are of extreme interest, as they are only known to occur elsewhere in Newfoundland and Eastern Canada. The gasteropods, however, furnish the largest number of species—about 48 per cent. of the whole. The primitive euomphalids, *Maclurea* and *Ophileta*, are most characteristic.

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[4] *Quart. Jour. Geol. Soc.*, vol. xlv., p. 125, 1889.

The former genus has a large number of species, many of which are to be found in the Beekmantown limestone of Newfoundland and Eastern North America. Only one of the species (*Maclurea Peachi*) is peculiar to Durness. Several species of *Ophileta* are found, some of which likewise occur in the Beekmantown limestone. Euomphalus has also been recorded, while several forms belonging to the nearly allied family of the Turbinidæ, and placed in Lindström's genus *Oriostoma*, are also met with in the Beekmantown limestone.

Murchisonids and Pleurotomarids number twenty-seven species and show a very wide range of variation. The chief subgenera of the former are *Hormotoma* and *Ectomaria*, many species of which occur with remarkable variations. All the types of variation found in Durness are to be found in North America, and several of the species are common to both regions. The pleurotomarids vary in a similar manner, the chief genera being *Raphistoma* and *Euconia*, and a form resembling *Hormotoma*, only with a shorter spire. Species belonging to each of these subgenera are likewise common to both areas, while some are only known from the North-West Highlands.

The cephalopods are of equal interest. They are also of primitive type and, at the same time, show a wide range in form. The prominent feature in the straighter specimens is the great width of the laterally placed siphuncle, which is generally furnished with endocones and organic deposits. The genus *Piloceras* is the most characteristic type and shows this peculiar feature best. It has only been recorded from Scotland, Newfoundland, Canada, and the eastern States of North America. The following additional genera are represented—viz., *Endoceras*, chiefly by siphuncles in great variety; *Actinoceras*, *Cyrtoceras*, and, doubtfully, *Orthoceras*. Several forms have been attributed to *Orthoceras*, which, on re-examination, have been found to be the siphuncles of other genera, resembling American types described by Hall and Whitfield.

The whorled nautiloids provisionally classed with the genus *Trocholites* of Conrad are represented by several distinct forms as yet unnamed.

The trilobites are of rare occurrence in these two groups of dolomite and limestone. They are fragmentary and poorly preserved. This is doubtless one of the disappointing features connected with this remarkable assemblage of organic remains, for the presence of a zonal form would have helped to define the horizon of these beds. Only one species, *Bathyurus Nero* (Billings) has been identified, which also occurs in the Beekmantown limestone of Newfoundland. The other trilobite remains, though poorly preserved, leave a Cambrian facies characteristic of North America.

In connection with this fauna certain features have been observed which throw some light on the absence of calcareous organisms from thick zones of the Durness dolomite and limestone. In my detailed description of the palæontology of the Cambrian rocks of the North-West Highlands in the *Geological Survey Memoir* I stated that 'in most cases the septa and walls of chambered shells have been wholly or in part dissolved away, so as to leave only the more massive structures of the siphuncles, and worm castings are often found within the chambers where the septa have been preserved. These features seem to indicate that the accumulation of the calcareous mud in which the fossils were embedded was so slow that there was time for the solution of part of an organism before the whole of it was covered up.'[5] There is good reason to believe that many organisms wholly disappeared by this process, so that it is reasonable to conclude that the fossils obtained from the Durness dolomites cannot be regarded as furnishing a complete life-history of the forms that originally existed in that sequence of deposits. Attention has already been called to the fact that beneath the two subdivisions now under consideration there are groups of dolomite and limestone which so far have yielded no organic remains beyond worm castings. And even in the important Croisaphuill group, with its fossiliferous zones, there are thick groups of dolomite which have furnished no calcareous organic remains. Obviously the palæontological record in this instance is glaringly incomplete, for we have no reason to suppose that the life of the time flourished in some of the calcareous zones and not in others.

The highest subdivision of the Durness limestone, measuring about 150 feet

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[5] 'Geological Structure of the North-West Highlands,' *Geol. Sur. Mem.*, 1907, p. 380.

in thickness (Durine group), has yielded two species of *Hormotoma*—viz., *H. gracilis* and *H. gracillima*—both of which occur in the two underlying groups. *H. gracilis* occurs in the Beekmantown, the Chazy, and the Trenton limestones of America.

Before assigning any stratigraphical horizons to the fauna of the Durness dolomites, it is desirable, owing to the American facies of the fossils, to recapitulate the evidence bearing upon the life of Cambrian time in North America. But the Cambrian life-history of Scotland would be incomplete without a brief reference to the recent discovery of fossils along the eastern border of the Highlands.

In 1911 Dr. Campbell announced in the *Geological Magazine* that fossils had been found in the Highland border series north of Stonehaven, and, during this year, Dr. Jehu made a similar discovery in rocks belonging to this series near Aberfoyle. Papers on these subjects will be communicated to this section. For my present purpose it will be sufficient to indicate the nature of the fossils and the lithological characters of the rocks containing them.

The Highland border series north of Stonehaven and near Aberfoyle includes sheared igneous rocks, both lavaform and intrusive, with black shales, cherts, and jaspers. North of Stonehaven the fossils occur in thin, dark, flinty pyritous shale, while at Aberfoyle they have been found in shaly films at the edge of the chert bands. Several years ago radiolaria were detected in the cherts between Aberfoyle and Loch Lomond. From time to time these Highland border rocks have been carefully searched for fossils, but until recently with little success, owing to the intense movement to which they have been subjected, resulting in marked flaser structure in all except the most resistant bands.

The fossils consist chiefly of horny, hingeless brachiopods, phyllocarid crustacea, worm tubes, and the jaws and chetæ of annelids. The genera of brachiopods comprise *Lingulella, Obolus, Obolella, Acrotreta,* and *Linarssonia.* The association of these brachiopods with phyllocarid crustaceans resembling *Hymenocaris* and *Lingulocaris* is suggestive of an Upper Cambrian horizon—an inference which is supported by the absence of graptolites.

In the published Geological Survey maps these Highland border rocks are queried as of Lower Silurian age. This correlation was based partly on their resemblance to the Arenig volcanic rocks and radiolarian cherts of the Southern Uplands, and partly because, as shown by Mr. Barrow, they are overlain by an unconformable group of sediments, termed by him the Margie series. The cherts, the green schists, and the Margie series have shared in a common system of folding, and are unconformably surmounted by Downtonian strata near Stonehaven. Though the original correlation may not be strictly correct, it is probable, in my opinion, that representatives of both the Arenig and Upper Cambrian formations may occur in the Highland border series, and, further, that Upper Cambrian strata may yet be found in the Girvan area, as originally suggested by Professor Lapworth in correspondence with Dr. Horne.

The Cambrian Fauna of North America.

The classification of the Cambrian fauna found in North America is based on the researches of a band of distinguished palæontologists, comprising among the older investigators Billings, Hall, and Whitfield, and among modern workers Walcott, Ulrich, Schuchert, Brainerd, Seely, Ruedemann, Matthew, Clarke, and Grabau. Prominent among these investigators stands Dr. Walcott, alike for his original and exhaustive contributions to this branch of inquiry and for his complete mastery of the sequence and distribution of life in Cambrian time in North America. Indeed, geologists all over the world owe him a deep debt of gratitude for the services which he has rendered to Cambrian palæontology.

Throughout the greater part of Cambrian time there existed in North America two distinct life provinces. The eastern one ran along the Atlantic coast from the north of Newfoundland to a point south of New York, extending only a short distance inland, with a faunal facies resembling that of North-West Europe, exclusive of the North-West Highlands of Scotland. The western province lay to the north-west of that just described, and ranged from Northern Newfoundland, south-westwards to Central North America and the Pacific Ocean. On the east side of the Rocky Mountains it swept northwards to British

Columbia, perhaps as far as the Arctic Ocean. The remarkable feature of the life of the western province is its essentially American facies.

Geologists are familiar with the triple classification of the Cambrian system by means of the trilobites in North America, as in Europe. The Lower Cambrian division represents the *Olenellus* epoch of Walcott, characterised by some form of Olenellid, or, to use the name now given to the family by that investigator, the *Mesonacidæ*. The western life-province contains the true *Olenellus* of which *O. Thompsoni* is the type. The strata yielding this fauna extend over such a wide area of North America that within this same province we find a western and an eastern facies. The western facies is found in Nevada and California, where *Olenellus* is represented by such specific forms as *O. Gilberti* and *O. Freemonti*. But it is noteworthy that these forms occur near the top of the Lower Cambrian series, and are soon followed by *Zacanthoides* and *Crepicephalus*, trilobites of middle Cambrian affinities. Towards the lower part of the sequence of deposits, which there consist mainly of limestone, and extend downwards for a distance of over 4,000 feet beneath the beds containing the true *Olenellus*, Walcott found specimens of *Holmia Rowei* and *Nevadia Weeksi*. The latter form is regarded by him as the most primitive of all the *Mesonacidæ* yet known. Near the base the limestones have yielded the primitive corals, *Archæocyathus* and *Ethmophyllum*; and the brachiopods *Mickwitzia* and *Trematobolus*. The other forms found on this horizon belong to the following genera: (trilobites) *Protypus* and *Microdiscus* (brachiopods) *Kutorgina, Swantonia, Nisusia, Billingsella*, and (tubicolar annelids) *Hyolithellus* and *Salterella*. The eastern facies of the western life province is best known from the region of Georgia, in Vermont. It is the home of the type species of *Olenellus* (*O. Thompsoni*). It is associated with *Mesonacis vermontana*, which has now given the name to the whole family, with *Elliptocephalus asaphoides*, one of the earliest known trilobites of the family, and with other forms such as *Bathynotus, Holopygia, Protypus*, and *Microdiscus*. The tubicolar worms are represented by *Hyolithellus* and *Salterella*, the brachiopods by *Nisusia, Swantonia, Kutorgina cingulata*, and *Paterina labradorica*. There can be no doubt that the assemblage of organic remains found in this Georgian terrane is merely the counterpart of that found in the *Olenellus* zone of the North-West Highlands.

Proceeding now to the eastern life-province, we find that the Lower Cambrian rocks are characterised by the trilobite genus *Callavia*, belonging to the family of the *Mesonacidæ*, and bearing a close resemblance both to *Holmia* and *Nevadia*. In Southern Newfoundland two species of *Callavia* occur, of which *C. Bröggeri* is the type. It is accompanied by *Microdiscus, Hyolithellus, Paterina labradorica*, and *Helenia bella*. In New Brunswick the *Protolenus* fauna, with *Protolenus* as the characteristic trilobite, probably represents the upper part of the *Olenellus* zone. In this connection the recent discovery of the *Protolenus* fauna by Mr. Cobbold, in Shropshire, in strata associated with *Callavia*, and overlain by beds yielding *Paradoxides*, is of special importance, as it shows the close relation between the Lower Cambrian fauna of Wales and that of the Atlantic or eastern province of North America.[6]

The Middle Cambrian division of the western life-province is characterised chiefly by the trilobite genus *Olenoides*; indeed, the western part of it is the home of *Olenoides* and the large tailed trilobites. The characteristic genera of this group to be found in that region are *Kootenia, Zacanthoides, Bathyuriscus, Asaphiscus, Neolenus, Dorypygella, Dorypyge, Damesella*, and *Ogygopsis*.

In this region the Middle Cambrian limestones and shales occurring on Mount Stephen, in British Columbia, have yielded a magnificent series of trilobites, eurypterids, limuloids, crustacea ranging from congeners of the brine shrimps to phyllocarid nebalids, annelids belonging to most of the still extant families, holothurians, medusae, and other organic remains. For the most part many of these forms are so fragile that only their tracks remain as indications of their existence in palæozoic deposits. Not till we reach the Solenhofen slates in Jurassic time do we find similar favourable conditions for the entombment and preservation of their highly modified successors. The remarkable evidence bearing on the evolution of groups of organisms furnished by this assemblage of fossils from Mount Stephen has been admirably described and illustrated by

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[6] *Quart. Jour. Geol. Soc.*, vol. lxvii., p. 296, 1911.

Walcott in his series of papers published in the Smithsonian Miscellaneous Collections.

In the New Brunswick portion of the eastern or Atlantic life-province the strata yielding *Paradoxides* follow those bearing the *Protolenus* fauna. Six species of Paradoxides have been obtained from this horizon, including *P. davidis*, together with the following genera: *Agnostus, Agranlos, Liostracus, Conocoryphe,* and *Ctenicephalus.* Schuchert points out that this fauna is 'closely allied to the *Paradoxides* faunas of Wales and Sweden, but less so with that of Bohemia.'[7]

In Southern Newfoundland Walcott showed that the base of the Middle Cambrian division is marked in Manuel's Brook by a conglomerate containing fossils of the lower or Georgian terrane, thus indicating elevation and erosion of the Lower Cambrian rocks. Higher up the strata yielded *Paradoxides davidis* and *P. bennetti.*

Important evidence pointing to the conclusion that the *Paradoxides* fauna of the eastern or Atlantic province encroached to some extent on the eastern part of the western life-province has been obtained by Walcott at St. Albans, Vermont. But the suggestion has been made by Schuchert that their present position is there due to north-westerly thrusting.[8]

It should be borne in mind that in Middle Cambrian time the eastern and western parts of the western life-province were evidently separated from each other by a land barrier, owing to crustal movement, which was probably connected with the elevation of the Lower Cambrian rocks in the region where they were subjected to erosion.

In the upper division of the Cambrian system in North America there is a marked change in the fauna. Its characteristic features are thus clearly summarised by Schuchert: 'In a general way it may be said that the Ozarkic period of Ulrich (Upper Cambrian) begins with the trilobite genus *Dikelocephalus* and the first distinct molluscan fauna. . . . The trilobites and inarticulate brachiopods (greatly reduced in species) are still Cambrian in aspect, while the new faunal feature consists in a rapid evolution, in form and size, of the coiled gasteropods, and of both straight and coiled cephalopods. The latter are distinguished from those of subsequent periods by the exceedingly close arrangement of the septa.'[9]

The distinctive trilobite genus of the Upper Cambrian strata of the western life-province is *Dikelocephalus,* where it is associated with an American facies of fossils. The eastern or Atlantic province is characterised by Olenids, though *Dikelocephalus* also occurs, and by typical European forms. In Minnesota and Wisconsin, where the strata consists of sandstones, dolomites, and shales, two species of *Dikelocephalus* have been obtained, together with other genera of trilobites such as *Agnostus,* and *Illænurus*; the limuloid *Aglaspis;* and the gasteropods, *Holopea, Ophileta,* and *Raphistoma.*

In certain areas this period is characterised by a great succession of calcareous deposits, comprising parts of the Shenandoah limestone and Kittatinny dolomite in New Jersey, portions of the Knox dolomite in Tennessee, and of the dolomite and limestone in Oklahoma. In some of these localities, at least, the lower portions of this calcareous series are grouped with the Upper Cambrian sediments, while the upper parts are classed with Lower Silurian or Ordovician strata. The researches of American palæontologists have shown that in certain areas there is a mixed Cambrian and Ordovician fauna in some of the beds, as in the Tremadoc rocks of Wales. This commingling of faunas is exemplified in the case of the Beekmantown limestone, which is grouped with the Ordovician (Lower Silurian) rocks by most American geologists. Ulrich and Schuchert, on the other hand, regard it as a formation (the Canadic) distinct from the overlying Ordovician system.

The type areas of the Beekmantown limestone are Lake Champlain, the Mingan Islands, and Newfoundland, where the strata consist mainly of a succession of limestones and dolomites over 1,000 feet thick. The fossils are chiefly molluscan, comprising lamellibranchs, gasteropods, and cephalopods. The lamellibranchs are represented, among others, by the genera *Euchasma,*

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[7] *Bull. Geol. Soc. of Amer.,* vol. xx. (1910), p. 522.

[8] *Ibid.*

[9] *Op. cit.,* p. 524.

and *Eopteria;* the gasteropods, by *Ophileta, Maclurea, Euomphalus, Holopea, Hormotoma, Ectomaria, Murchisonia, Lophospira, Euconia, Raphistoma, Helicotoma*; the cephalopods, by *Orthoceras, Cyrtoceras, Gomphoceras, Piloceras, Trocholites.* Of the foregoing genera many of the species are common to this region and the North-West Highlands of Scotland.

The trilobites associated with this fauna comprise the genera, *Dikelocephalus, Bathyurus, Asaphus, Harpes,* and *Nileus.*

In Northern Newfoundland, in zones F to N of Billings, this fauna, with localised species, is found in great development, in limestones and dolomites resembling those of Durness. Its upper limit is there clearly defined, for the limestones and dolomites are overlain by dark shales containing graptolites of undoubted Arenig age.

A careful comparison of the faunas of the Durness and Beekmantown limestones shows that the assemblage of fossils in the Balnakiel and Croisaphuill groups of Durness is practically identical with that in the zones F to N of Billings, as developed in Newfoundland. These groups must therefore be older than the Arenig rocks of Wales, and must represent at least the Welsh Tremadoc strata, if not part of the Lingula Flags, both of which, according to the English classification, are grouped with the Cambrian system.

But even in the purely European province of North America, in New Brunswick, where the Beekmantown calcareous fauna is entirely absent, and where the faunal sequence and type of sedimentation are almost identical with those of North Wales, the basal Ordovician or Lower Silurian rocks of American geologists include the *Peltura scarabaeoides* and the *Parabolina spinulosa* zones, which, in Wales, are classed with the Lingula Flags. It is obvious, therefore, that the boundary-line between the Cambrian and Ordovician (Lower Silurian) systems is not drawn at the same stratigraphical horizon by American and British geologists. In fixing the age of the Durness dolomites and limestones the English classification has been adopted.

The palæontological evidence now adduced regarding the relation of the Cambrian fauna of the North-West Highlands to that of North America leads to the following conclusions :—

1. The Lower Cambrian fauna of the North-West Highlands, distinguished by the genus *Olenellus* and its associates, is almost identical in character with that of the Georgian terrane of the western life-province of North America, and essentially different from the Lower Cambrian fauna of the rest of Europe.
2. No forms characteristic of the Middle Cambrian division, either of Europe or North America, have as yet been found in the North-West Highlands; but this division may be represented by the unfossiliferous dolomites and limestones of the Ghrudhaidh, Eilean Dubh, and the lower Sail Mhor groups.
3. The fossiliferous bands of the Sail Mhor group may be the equivalents of the lower part of the Upper Cambrian formation.
4. The Balnakeil and Croisaphuill groups of the Durness dolomites and limestones contain a typical development of the molluscan fauna of the Beekmantown limestone, belonging to the western life-province of North America. As the Beekmantown limestone is succeeded by shales, with Arenig graptolites, it follows, in accordance with British classification, that these groups must be of Upper Cambrian age.
5. The highest subdivision of the Durness limestone (Durine) has not yielded fossils of zonal value, and the members of this group are not overlain in normal sequence by graptolite-bearing shale or other sediments.

Cambrian Palæogeography between North America and North-West Europe.

In attempting to restore in outline the distribution of land and sea in Cambrian time between North America and North-West Europe reference must be made to various investigators whose researches in palæogeography are more or less familiar to geologists. Among these may be mentioned Suess, Dana, De Lapparent, Frech, Walcott, Ulrich, Schuchert, Bailey Willis, Grabau, Hull, and Jukes Browne. The views now presented seem to me to be reasonable inferences from the palæontological evidence set forth in this address.

In the North-West Highlands there is still a remnant of the old land surface upon which the Torridonian sediments were laid down. There is conclusive evidence that the pre-Torridonian land was one of high relief. As the Torridonian sediments form part of a continental deposit it may be inferred that the Archæan rocks had a great extension in a north-westerly direction. The increasing coarseness of the deposits towards the north-west suggests that the land may have become more elevated in that direction. At any rate, the pile of Torridonian sediments points to a subsidence of the region towards the south-east, and probably to a correlative movement of elevation towards the north-west.

The sparagmite of Scandinavia is an arkose resembling the dominant type of the Torridon sandstone; is of the same general age, and has evidently been derived from similar sources in the Scandinavian shield. In eastern North America coarse sedimentary deposits from part of the newer Algonkian rocks, which are still to be found rising from underneath the Cambrian strata in the region of the great lakes. These materials were obtained from the great Canadian shield, which must have formed a large continental area during their deposition.

It is reasonable to infer that these isolated relics of old land surfaces were united in pre-Torridonian time, thus forming a continuous belt from Scandinavia to North America. During the period which elapsed between the deposition of the Torridon sandstone and the basement members of the Cambrian system a geosyncline was established which gave rise to a submarine trough, trending in an east-north-east and west-south-west direction, both in the British and North American areas. In the latter region it extends from Newfoundland to Alabama, its south-eastern limit being defined by the old land surface of Appalachia. The extension of this Appalachian land area in a north-east direction beyond the limits of Nova Scotia and Newfoundland was postulated by Dana and other American writers. This geosyncline remained a line of weakness throughout palæozoic time, both in Britain and North America, which resulted in the Caledonian system of folding in Britain, and in the Taconic, Appalachian, and Pennsylvanian systems in North America. Hence it is manifest that the original shore-lines of this trough are now much nearer each other than they were in Cambrian time.

The Cambrian rocks of the North-West Highlands were laid down along the north-west side of this trough during a period of subsidence, for the great succession of Durness dolomite and limestone, with little or no terrigenous material, is superimposed on the coarser sediments of that formation. On the other hand, the Cambrian strata of Wales seem to have been deposited along the southern limit of this marine depression. The Archæan rocks that now constitute the central plateau of France may have formed part of its southern boundary. The extension of this land area towards the north-east may have given rise to the barrier that separated the Baltic life-province from that of Bohemia, Sardinia, and Spain. In my opinion, this southern land area in Western Europe was continuous across the Atlantic with Appalachia. For the life sequence found in the Cambrian rocks of New Brunswick is practically identical with that of Wales and the Baltic provinces, thus showing that there must have been continuous intercourse between these areas. Along this shoreline the migration of forms seems to have been from Europe towards America. On the other hand, along the northern shore the tide of migration seems to have advanced from America towards the North-West Highlands. The question naturally arises, what cause prevented the migration of the forms from one shore of this trough to the other? American geologists are of opinion that this is probably due to the existence of land barriers; but, in my opinion, it can be more satisfactorily accounted for by clear and open sea, aided by currents.

The south-western extremity of the American trough in Lower Cambrian time opened out into the Mississippian sea, which was connected with the Pacific Ocean, and stretched northwards towards the Arctic regions. Reference has already been made to Walcott's discovery in Nevada of the primitive trilobite *Nevadia Weeksi*, from which he derives both branches of the *Mesonacidæ*, one branch linking *Nevadia*, through *Callavia*, *Holmia*, and *Wanneria*, with *Paradoxides*, the other connecting *Nevadia* with *Olenellus*, through *Mesonacis*, *Elliptocephalus* and *Pædumias*.

In Nevada the genus *Holmia*, as already shown, is associated with the primitive type *Nevadia*. *Wanneria* is found in Nevada, in Alabama, and in Pennsylvania, thus showing that this genus is common to the Mississippian sea and to the long trough north-east of Alabama. *Mesonacis* has been obtained in the submarine depression at Lake Champlain, at Bonne Bay, Newfoundland, and at the north side of the Straits of Belle Isle. *Elliptocephalus* has been recorded from the New York State. *Olenellus* has been found in Nevada, in Vermont, and in the North-West Highlands. All the genera now referred to may have migrated along the north-western shore of this trough.

As regards the distribution of the genus *Callavia*, this form has been met with in Maine, in Newfoundland, and in derived pebbles in a conglomerate in Quebec. Two species have been recorded in Shropshire. These forms probably moved along the southern shore of this sea from Wales to North America.

Reference has already been made to the fact that, in the interval between Lower and Middle Cambrian time, in certain areas in North America the Lower Cambrian rocks were locally elevated and subjected to erosion. During this interval the southern end of the trough seems to have had no connection with the Mississippian sea, for in Middle Cambrian time, as already indicated, the *Paradoxides* fauna is found in the trough on the east side of North America, whereas on the west side it is represented by the *Olenoides* fauna.

In Upper Cambrian time a great transgression of the sea towards the north supervened. The *Dikelocephalus* fauna is found on both sides of America, thus showing that the previous land barrier had been submerged. While this genus occurs in Wales and the Baltic provinces, it has not as yet been recorded from the North-West Highlands, but I quite expect that this discovery may be made at some future time.

Along the northern side of the American trough clear water conditions prevailed, owing to the northward recession of the shore-line, which led to the accumulation of a great succession of calcareous deposits, including the Beekmantown limestone, to which reference has already been made. Schuchert, as already stated, has pointed out that, in the lower part of the Ozarkic (Upper Cambrian) system, in Minnesota and Wisconsin, the gasteropod genera, *Holopea*, *Ophileta*, and *Raphistoma*, are associated with two species of *Dikelocephalus*. This molluscan fauna is evidently the precursor of that of the Beekmantown limestone. It was probably from this central region of America that the calcareous fauna of Beekmantown migrated to the submarine trough in the typical Champlain region, and through Newfoundland to the North-West Highlands of Scotland.

The section at St. John, New Brunswick, where the Baltic and Welsh types of the *Olenus* fauna occurs, shows that the southern shore line of the trough must then have occupied much the same relative position as in Lower and Middle Cambrian time. In the same region the strata containing this fauna, with *Peltura scarabœoides*, and *Dictyonema flabelliforme* are overlain by dark shales with Arenig graptolites. These graptolite-bearing terrigenous deposits eventually extended across the trough northwards, till, in Newfoundland, they came to rest on the Beckmantown limestones.

In the Lake Champlain region, in the Chazy limestone, which there immediately succeeds the Beekmantown limestone without the intervention of the Arenig graptolite shale, there is a survival of the Beekmantown molluscan fauna with only such slight modifications as to indicate genetic descent. In the same trough the descendants of this fauna are to be found in the Trenton limestone.

In this connection it is worthy of note that the molluscan fauna and the corals of the Stinchar and Craighead limestones of Upper Llandeilo age in the Girvan district of the Southern Uplands, have an American facies, as first suggested by Nicholson. The appearance of American types in these limestones may be accounted for in the following manner: Attention has already been called to the divergent types of sedimentation presented by the Upper Cambrian strata of the North-West Highlands, and of the South-East Highlands, at Stonehaven and Aberfoyle. In the former case there is a continuous sequence of dolomites and limestones, while in the latter we find a group, comprising radiolarian cherts and black shales, associated with pillowy spilitic lavas and intrusive igneous rocks, indicating conditions of deposition at or near the limit of sedimentation. But, notwithstanding the different types of sedimentation and the divergent faunas in the two areas, I believe that during the Upper Cambrian period, and probably for some time thereafter, continuous sea extended from the North-West Highlands to beyond the Eastern Highland border. The Upper Cambrian terrigenous sediments which we now find at Stonehaven and Aberfoyle must have been derived from land to the south. In Llandeilo time the Arenig and Lower Llandeilo rocks of the Girvan area were elevated and subjected to extensive denudation. On this highly eroded platform, as first proved by Professor Lapworth, coarse conglomerates, composed of the underlying materials, were laid down in association with the Stinchar and Craighead limestones. In my opinion the appearance of the American forms in these limestones is connected with the movement that produced this unconformability in the Girvan area. This local elevation was probably associated in some form with the great crustal movements that culminated in the overthrusts of the North-West Highlands and caused the intense folding and flaser structure of the rocks along the Highland border. By these movements shore-lines may have been established between the north side of the old Palæozoic sea and the Girvan area, which permitted the southern migration of the American forms.

Note.—Since writing the above my attention has been directed to the recent work of Bassler on 'The Early Palæozoic Bryozoa of the Baltic Provinces,' published by the Smithsonian Institution in 1911. In his introduction the author has shown that the Ordovician (Lower Silurian) and Gothlandian (Upper Silurian) rocks of the Baltic Provinces contain a large percentage of bryozoan species, in common with the Black River, Trenton, and Niagara limestones of the same relative age in Eastern North America. This fact suggests that during Lower and Upper Silurian time the old lines of migration were still open, and that the Bryozoa, being of clear-water habit, were able to cross the old trough from side to side.

British Association for the Advancement of Science.

SECTION D: DUNDEE, 1912.

ADDRESS

TO THE

ZOOLOGICAL SECTION

BY

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PRESIDENT OF THE SECTION.

Zoological Gardens and the Preservation of Fauna.

In thinking over possible subjects for this Presidential Address, I was strongly tempted to enter on a discussion of the logical methods and concepts that we employ in Zoology. The temptation was specially strong to a Scot speaking in Scotland, that he should devote the hour when the prestige of the presidential chair secured him attention, to putting his audience right on logic and metaphysics. But I reflected that Zoology is doing very well, however its logic be wavering, and that as all lines subtend an equal angle at infinity, it would be of small moment if I were to postpone my remarks on metaphysics. And so I am to essay a more modest but a more urgent theme, and ask you to consider the danger that threatens the surviving land-fauna of this globe. A well-known example may serve to remind you how swift is the course of destruction. In 1867, when the British Association last met at Dundee, there were still millions of bison roaming over the prairies and forests of North America. In that year the building of the Union Pacific, the first great trans-continental railway, cut the herd in two. The Southern division, consisting itself of several million individuals, was wiped out between 1871 and 1874, and the practical destruction of the Northern herd was completed between 1880 and 1884. At present there are only two herds of wild bison in existence. In the Yellowstone Park only about twenty individuals remained in 1911, the greater part of the herd having been killed by poachers. A larger number, over three hundred, still survive near the Great Slave Lake, and there are probably nearly two thousand in captivity, in various Zoological Gardens, private domains and State Parks. It is only by the deliberate and conscious interference of man that the evil wrought by man has been arrested.

A second example that I may select is also taken from the continent of North America, but it is specially notable because it is sometimes urged, as in India, that migratory birds require no protection. Audubon relates that just a century ago Passenger Pigeons existed in countless millions, and that for four days at a time the sky was black with the stream of migration. The final extinction of this species has taken place since the last meeting of the Association in Dundee. In 1906 there were actually five single birds living, all of which had been bred in captivity, and I understand that these last survivors of a prolific species are now dead, although the birds ranged in countless numbers over a great continent.

It would be futile to discuss in detail the precise agencies by which the destruction of animal life is wrought, or the pretexts or excuses for them. The most potent factors are the perfection of the modern firearm and the enormous increase in its use by civilised and barbarous man. Sometimes the pretext is sport, sometimes wanton destructiveness rules. The extermination of beasts-of-prey, the clearing of soil for stock or crops, the securing of meat, the commercial pursuit of hides and horns and of furs and feathers, all play their part. Farmers and settlers on the outskirts of civilisation accuse the natives, and allege that the problem would be solved were no firearms allowed to any but themselves. Sportsmen accuse other sportsmen, whom they declare to be no real sportsmen, and every person whose object is not sport. The great museums, in the name of science, and the rich amateur collectors press forward to secure the last specimens of moribund species.

But even apart from such deliberate and conscious agencies, the near presence of man is inhospitable to wild life. As he spreads over the earth, animals wither before him, driven from their haunts, deprived of their food, perishing from new diseases. It is part of a general biological process. From time to time, in the past history of the world, a species, favoured by some happy kink of structure or fortunate accident of adaptability, has become dominant. It has increased greatly in numbers, outrunning its natal bounds, and has radiated in every possible direction, conquering woodland and prairies, the hills and the plains, transcending barriers that had seemed impassable, and perhaps itself breaking up into new local races and varieties. It must be long since such a triumphant progress was unattended by death and destruction. When the first terrestrial animals crept out of their marshes into the clean air of the dry land, they had only plants and the avenging pressure of physical forces to overcome. But when the Amphibians were beaten by the Reptiles, and when from amongst the Reptiles some insignificant species acquired the prodigious possibility of transformation to Mammals, and still more when amongst the Mammals Eutherian succeeded Marsupial, Carnivore the Creodont, and Man the Ape, it could have been only after a fatal contest that the newcomers triumphed. The struggle, we must suppose, was at first most acute between animals and their nearest inferior allies, as similarity of needs brings about the keenest competition, but it must afterwards have been extended against lower and lower occupants of the coveted territory.

The human race has for long been the dominant terrestrial species, and man has a wider capacity for adaptation to different environments, and an infinitely greater power of transcending geographical barriers than have been enjoyed by any other set of animals. For a considerable time many of the more primitive tribes, especially before the advent of firearms, had settled down into a kind of natural equilibrium with the local mammalian fauna, but these tribes have been first driven to a keener competition with the lower animals, and then, in most parts of the world, have themselves been forced almost or completely out of existence. The resourceful and aggressive higher races have now reached into the remotest parts of the earth and have become the exterminators. It must now be the work of the most intelligent and provident amongst us to arrest this course of destruction and to preserve what remains.

In Europe, unfortunately, there is little left sufficiently large and important to excite the imagination. There is the European bison which has been extinct in Western Europe for many centuries, whilst the last was killed in East Prussia in 1755. There remains a herd of about seven hundred in the forests of Lithuania, strictly protected by the Tsar, whilst there are truly wild animals, in considerable numbers, in the Caucasus, small captive herds on the private estates of the Tsar, the Duke of Pless and Count Potocki, and a few individuals in various Zoological Gardens. There is the beaver, formerly widespread in Europe, now one of the rarest of living mammals, and lingering in minute numbers in the Rhone, the Danube, in a few Russian rivers and in protected areas in Scandinavia. The wolf and the bear have shrunk to the recesses of thick forests and the remotest mountains, gluttons to the most barren regions of the north. The chamois survives by favour of game-laws and the vast inaccessible areas to which it can retreat, but the mouflon of Corsica and Sardinia and the ibex in Spain are on the verge of extinction. Every little creature, from the otter, wild cat and marten to the curious desman is disappearing.

India contains the richest, the most varied, and, from many points of view, the most interesting part of the Asiatic fauna. Notwithstanding the teeming human population it has supported from time immemorial, the extent of its area, its dense forests and jungles, its magnificent series of river valleys, mountains, and hills have preserved until recent times a fauna rich in individuals and species. The most casual glance at the volumes by sportsmen and naturalists written forty or fifty years ago reveals the delight and wonder of travel in India so comparatively recently as the time when the Association last met in Dundee. Sir H. H. Johnston has borne witness that even in 1895 a journey 'through almost any part of India was of absorbing interest to the naturalist.' All is changed now, and there seems little doubt but that the devastation in the wonderful mammalian fauna has been wrought chiefly by British military officers and civilians, partly directly, and partly by their encouragement of the sporting instincts of the Mohammedan population and the native regiments, although the clearing of forests and the draining of marshlands have played an important contributory part. The tiger has no chance against the modern rifle. The one-horned rhinoceros has been nearly exterminated in Northern India and Assam. The magnificent gaur, one of the most splendid of living creatures, has been almost killed off throughout the limits of its range—Southern India and the Malay Peninsula. Bears and wolves, wild dogs and leopards are persecuted remorselessly. Deer and antelope have been reduced to numbers that alarm even the most thoughtless sportsmen, and wild sheep and goats are being driven to the utmost limits of their range.

When I speak of the fauna of Africa, I am always being reminded of the huge and pathless areas of the Dark Continent, and assured that lions and leopards, elephants and giraffe still exist in countless numbers, nor do I forget the dim recesses of the tropical forests where creatures still lurk of which we have only the vaguest rumour. But we know that South Africa, less than fifty years ago, was a dream that surpassed the imagination of the most ardent hunter. And we know what it is now. It is traversed by railways, it has been rolled over by the devastations of war. The game that once covered the land in unnumbered millions is now either extinct, like the quagga and the black wildebeeste, or its scanty remnant lingers in a few reserves and on a few farms. The sportsman and the hunter have been driven to other parts of the Continent, and I have no confidence in the future of the African fauna. The Mountains of the Moon are within range of a long vacation holiday. Civilisation is eating into the land from every side. All the great European countries are developing their African possessions. There are exploring expeditions, punitive expeditions, shooting and collecting expeditions. Railways are being pushed inland, water-routes opened up. The land is being patrolled and policed and taxed, and the wild animals are suffering. Let us go back for a moment to the Transvaal and consider what has happened since the Rand was opened, neglecting the reserves. Lions are nearly extinct. The hyæna has been trapped and shot and poisoned out of existence. The eland is extinct. The giraffe is extinct. The elephant is extinct. The rhinoceros is extinct. The buffalo is extinct. The bontebok, the red hartebeeste, the mountain zebra, the oribi, and the grysbok are so rare as to be practically extinct. And the same fate may at any time overtake the rest of Africa. The white man has learned to live in the tropics; he is mastering tropical diseases; he has need of the vegetable and mineral wealth that lie awaiting him, and although there is yet time to save the African fauna, it is in imminent peril.

When we turn to Australia with its fauna of unique zoological interest, we come to a more advanced case of the same disease. In 1909 Mr. G. C. Shortridge, a very skilled collector, working for the British Museum, published in the 'Proceedings of the Zoological Society of London' the results of an investigation he had carried out on the fauna of Western Australia south of the tropics, during the years 1904-1907. He gave a map showing the present and comparatively recent distribution for each of the species of Marsupials and Monotremes indigenous to that locality. West Australia as yet has been very much less affected by civilisation than Queensland, New South Wales or Victoria, and yet in practically every case there was found evidence of an enormous recent restriction of the range of the species. Marsupials and Monotremes are, as you know, rather stupid animals, with small powers of adaptation to new conditions, and they are in the very gravest danger of complete extinction. In the island of Tasmania, the thylacine, or marsupial wolf, and the Tasmanian devil have unfortunately incurred the just hostility of the stock raiser and poultry farmer, and the date of their final extermination is approaching at a pace that must be reckoned by months rather than by years.

The development of the continent of North America has been one of the wonders of the history of the world, and we on this side of the Atlantic almost hold our breath as we try to realise the material wealth and splendour and the ardent intellectual and social progress that have turned the United States into an imperial nation. But we know what has happened to the American bison. We know the danger that threatens the pronghorn, one of the most isolated and interesting of living creatures, the Virginian deer, the mule-deer, and the bighorn sheep. Even in the wide recesses of Canada, the bighorn, the caribou, the elk, the wapiti, the white mountain goat, and the bears are being rapidly driven back by advancing civilisation. In South America less immediate danger seems to threaten the jaguar and maned wolf, the tapirs and ant-eaters and sloths, but the energy of the rejuvenated Latin races points to a huge encroachment of civilisation on wild nature at no distant date.

You will understand that I am giving examples and not a catalogue even of threatened terrestrial mammals. I have said nothing of the aquatic carnivores, nothing of birds, or of reptiles, or of batrachians and fishes. And to us who are zoologists, the vast destruction of invertebrate life, the sweeping out, as forests are cleared and the soil tilled, of innumerable species that are not even named or described, is a real calamity. I do not wish to appeal to sentiment. Man is worth many sparrows; he is worth all the animal population of the globe, and if there were not room for both, the animals must go. I will pass no judgment on those who find the keenest pleasure of life in gratifying the primeval instinct of sport. I will admit that there is no better destiny for the lovely plumes of a rare bird than to enhance the beauty of a beautiful woman. I will accept the plea of those who prefer a well-established trinomial to a moribund species. But I do not admit the right of the present generation to careless indifference or to wanton destruction. Each generation is the guardian of the existing resources of the world; it has come into a great inheritance, but only as a trustee. We are learning to preserve the relics of early civilisations, and the rude remains of man's primitive arts and crafts. Every civilised nation spends great sums on painting and sculpture, on libraries and museums. Living animals are of older lineage, more perfect craftsmanship and greater beauty than any of the creations of man. And although we value the work of our forefathers, we do not doubt but that the generations yet unborn will produce their own artists and writers, who may equal or surpass the artists and writers of the past. But there is no resurrection or recovery of an extinct species, and it is not merely that here and there one species out of many is threatened, but that whole genera, families and orders are in danger.

Now let me turn to what is being done and what has been done for the preservation of fauna. I must begin by saying, and this was one of the principal reasons for selecting the subject of my Address, that we who are professional zoologists, systematists, anatomists, embryologists, and students of general biological problems, in this country at least, have not taken a sufficiently active part in the preservation of the realm of nature that provides the reason for our existence. The first and most practical step of world-wide importance was taken by a former President of the British Association, the late Lord Salisbury, one of the few in the long roll of English statesmen whose mind was attuned to science. In 1899 he arranged for a convention of the Great Powers interested in Africa to consider the preservation of what were curiously described as the 'Wild Animals, Birds and Fish' of that continent. The convention, which did most important pioneer work, included amongst its members another President of this Association, Sir Ray Lankester, whom we hold in high honour in this Section as the living zoologist who has taken the widest interest in every branch of zoology. But it was confined in its scope to creatures of economic or of sporting value. And from that time on the central authorities of the Great Powers and the local Administrators, particularly in the case of tropical possessions, seem to have been influenced in the framing of their rules and regulations chiefly by the idea of preserving valuable game animals. Defining the number of each kind of game that can be killed, charging comparatively high sums for shooting-permits, and the establishment of temporary or permanent reserved tracts in which the game may recuperate, have been the principal methods selected. On these lines, narrow although they are, much valuable work has been done, and the parts of the world where unrestricted shooting is still possible are rapidly being limited.

I may take the proposed new Game Act of our Indian Empire, which has recently been explained, and to a certain extent criticised, in the 'Proceedings of the Zoological Society of London,' by Mr. E. P. Stebbing, an enlightened sportsman-naturalist, as an example of the efforts that are being made in this direction, and of their limitations.

The Act is to apply to all India, but much initiative is left to Local Governments as to the definition of the important words 'game' and 'large animal.' The Act, however, declares what the words are to mean in the absence of such local definitions, and it is a fair assumption that local interpretations will not depart widely from the lead given by the central Authority. Game is to include the following in their wild state: Pigeons, sandgrouse, peafowl, jungle-fowl, pheasants, partridges, quail, spurfowl, florican and their congeners; geese, ducks and their congeners; woodcock and snipe. So much for Birds. Mammals include hares and 'large animals' defined as 'all kinds of rhinoceros, buffalo, bison, oxen; all kinds of sheep, goats, antelopes and their congeners; all kinds of gazelle and deer.'

The Act does not affect the pursuit, capture, or killing of game by non-commissioned officers or soldiers on whose behalf regulations have been made, or of any animal for which a reward may be claimed from Government, of any large animal in self-defence, or of any large animal by a cultivator or his servants, whose crops it is injuring. Nor does it affect anything done under licence for possessing arms and ammunition to protect crops, or for destroying dangerous animals, under the Indian Arms Act. Then follow prohibitory provisions all of which refer to the killing or to the sale or possession of game or fish, and provisions as to licences for sportsmen, the sums to be paid for which are merely nominal, but which carry restrictions as to the number of head that may be killed. I need not enter upon detailed criticism as to the vagueness of this Act from the zoological point of view, or as to the very large loopholes which its provisions leave to civil and military sportsmen; these have been excellently set forth by Mr. Stebbing, who has full knowledge of the special conditions which exist in India. What I desire to point out is that it conceives of animals as game rather than as animals, and that it does not even contemplate the possibility of the protection of birds-of-prey and beasts-of-prey, and still less of the enormous numbers of species of animals that have no sporting or economic value.

Mr. Stebbing's article also gives a list of the very large number of reserved areas in India, which are described as 'Game Sanctuaries.' His explanation of them is as follows: 'With a view to affording a certain protection to animals of this kind (the elephant, rhinoceros, ruminants, &c.) and of giving a rest to species which have been heavily thinned in a district by indiscriminate shooting in the past, or by anthrax, drought, &c., the idea of the Game Sanctuary was introduced into India (and into other parts of the world) and has been accepted in many parts of the country. The sanctuary consists of a block of country, either of forest or of grassland, &c., depending on the nature of the animal to which sanctuary is required to be given; the area has rough boundaries such as roads, fire lines, nullahs, &c., assigned to it, and no shooting of any kind is allowed in it, if it is a sanctuary pure and simple; or the shooting of carnivora may be permitted, or of these latter and of everything else save certain specified animals.'

Mr. Stebbing goes on to say that sanctuaries may be formed in two ways. The area may be automatically closed and reopened for certain definite periods of years, or be closed until the head of game has become satisfactory, the shooting on the area being then regulated, and no further closing taking place, save for exceptional circumstances. The number of such sanctuary blocks, both in British India and in the Native States, will cause surprise and pleasure to most readers, and it cannot be doubted but that they will have a large effect on the preservation of wild life. The point, however, that I wish to make is that in the minds of those who have framed the Game Act, and of those who have caused the making of the sanctuaries—as indeed in the minds of their most competent critics—the dominant idea has been the husbanding of game animals, the securing for the future of sport for sportsmen. I do not forget that there is individual protection for certain animals; no elephant, except a rogue elephant, may be shot in India, and there are excellent regulations regarding birds with plumage of economic value. The fact remains that India, a country which still contains a considerable remnant of one of the richest faunas of the world, and which also is probably more efficiently under the autocratic control of a highly educated body of permanent officials, central and local, than any other country in the world, has no provision for the protection of its fauna simply as animals.

The conditions in Africa are very different from those in India. The land is portioned out amongst many Powers. The settled population is much less dense and the hold of the white settler and the white ruler is much less complete. The possibility of effective control of native hunters and of European travellers and sportsmen is much smaller, and as there are fewer sources of revenue, the temptation to exploit the game for the immediate development of the struggling colonies is much greater. Still, the lesson of the extinction of the South African fauna is being taken to heart. I have had the opportunity of going through the regulations made for the shooting of wild animals in Africa by this country, by our autonomic colonies, by France, Germany, Italy, Portugal, and Belgium, and, with the limitation that they are directed almost solely towards the protection of animals that can be regarded as game, they afford great promise for the future. But this limitation is still stamped upon them, and even so enthusiastic a naturalist as Major Stevenson-Hamilton, the Warden of the Transvaal Government Game Reserves, who has advocated the substitution of the camera for the rifle, appears to be of the opinion that the platform of the convention of 1900 is sufficient. It included the sparing of females and immature animals, the establishment of close seasons and game sanctuaries, the absolute protection of rare species, restrictions on the export for trading purposes of skins, horns and tusks, and the prohibition of pits, snares and game traps. Certainly the rulers of Africa are seeing to the establishment of game reserves. As for British Africa, there are two in Somaliland, two in the Sudan, two in Uganda and two in British East Africa (with separate reserves for eland, rhinoceros and hippopotamus), two in Nyasaland, three in the Transvaal, seven in Rhodesia, several in Natal and in Cape Colony, and at least four in Nigeria. These are now administered by competent officials, who in addition are usually the executive officers of the game laws outside the reserved territory. Here again, however, the preservation of game animals and of other animals of economic value, and of a few named species is the fundamental idea. In 1909 I had the honour of being a member of a deputation to the Secretary of State for the Colonies, arranged by the Society for the Preservation of the Wild Fauna of the Empire, one of the most active and successful bodies engaged in arousing public opinion on the subject. Among the questions on which we were approaching Lord Crewe was that of changes in the locality of reserves. Sometimes it had happened that for the convenience of settlers or because of railway extension, or for some other reason, proposals were made to open or clear the whole or part of a reserve. When I suggested that the substitution of one piece of ground for another, even of equivalent area, might be satisfactory from the point of view of the preservation of large animals, but was not satisfactory from the zoological point of view, that in fact pieces of primeval land and primeval forest contained many small animals of different kinds which would be exterminated once and for all when the land was brought under cultivation, the point was obviously new not only to the Colonial Secretary, who very courteously noted it, but to my colleagues.

This brings me to the general conclusion to which I wish to direct your attention and for which I hope to engage your sympathy. We may safely leave the preservation of game animals, or rare species if these are well known and interesting, and of animals of economic value, to the awakened responsibility and the practical sense of the Governing Powers, stimulated as these are by the enthusiasm of special Societies. Game laws, reserves where game may recuperate, close seasons, occasional prohibition and the real supervision of licence holders are all doing their work effectively. But there remains something else to do, something which I think should interest zoologists particularly, and on which we should lead opinion. There exist in all the great continents large tracts almost empty of resident population, which still contain vegetation almost undisturbed by the ravages of man, and which still harbour a multitude of small animals, and could afford space for the larger and better-known animals. These tracts have not yet been brought under cultivation, and are rarely traversed except by the sportsman, the explorer and the prospector. On these there should be established, in all the characteristic faunistic areas, reservations which should not be merely temporary recuperating grounds for harassed game, but absolute sanctuaries. Under no condition should they be open to the sportsman. No gun should be fired, no animal slaughtered or captured save by the direct authority of the wardens of the sanctuaries, and for the direct advantage of the denizens of the sanctuaries, for the removal of noxious individuals, the controlling of species that were increasing beyond reason, the extirpation of diseased or unhealthy animals. The obvious examples are not the game reserves of the Old World, but the National Parks of the New World and of Australasia. In the United States, for instance, there are now the Yellowstone National Park with over two million acres, the Yosemite in California with nearly a million acres, the Grand Cañon Game Preserve with two million acres, the Mount Olympus National Monument in Washington with over half a million acres, and the Superior Game and Forest Preserve with nearly a million acres, as well as a number of smaller reserves for special purposes, and a chain of coastal areas all round the shores for the preservation of birds. In Canada, in Alberta, there are the Rocky Mountains Park, the Yoho Park, Glacier Park, and Jasper Park, together extending to over nine million acres, whilst in British Columbia there are smaller sanctuaries. These, so far as laws can make them, are inalienable and inviolable sanctuaries for wild animals. We ought to have similar sanctuaries in every country of the world, national parks secured for all time against all the changes and chances of the nations by international agreement. In the older and more settled countries the areas selected unfortunately must be determined by various considerations, of which faunistic value cannot be the most important. But certainly in Africa, and in large parts of Asia, it would still be possible that they should be selected in the first place for their faunistic value. The scheme for them should be drawn up by an international commission of experts in the geographical distribution of animals, and the winter and summer haunts of migratory birds should be taken into consideration. It is for zoologists to lead the way, by laying down what is required to preserve for all time the most representative and most complete series of surviving species without any reference to the extrinsic value of the animals. And it then will be the duty of the nations, jointly and severally, to arrange that the requirements laid down by the experts shall be complied with.

And now I come to the last side of my subject, that of Zoological Gardens, with which I have been specially connected in the last ten years. My friend M. Gustave Loisel, in his recently issued monumental ‘Histoire des Ménageries’ has shown that in the oldest civilisations of which we have record, thousands of years before the Christian Era, wild animals were kept in captivity. He is inclined to trace the origin of the custom to a kind of totemism. Amongst the ancient Egyptians, for instance, besides the bull and the serpent, baboons, hippopotami, cats, lions, wolves, ichneumons, shrews, wild goats and wild sheep, and of lower animals, crocodiles, various fishes and beetles were held sacred in different towns. These animals were protected, and even the involuntary killing of any of them was punished by the death of the slayer, but besides this general protection, the priests selected individuals which they recognised by infallible signs as being the divine animals, and tamed, guarded and fed in the sacred buildings, whilst the revenues derived from certain tracts of land were set apart for their support. The Egyptians were also famous hunters and kept and tamed various wild animals, including cheetahs, striped hyænas, leopards, and even lions which they used in stalking their prey. The tame lions were sometimes clipped, as in ancient Assyria, and used both in the chase and in war. The rich Egyptians of Memphis had large parks in which they kept not only the domestic animals we now know, but troops of gazelles, antelopes, and cranes which were certainly tame and were herded by keepers with wands. So also in China at least fifteen centuries before our era, wild animals were captured in the far north by the orders of the Emperor and were kept in the Royal Parks. A few centuries later the Emperor Wen-Wang established a zoological collection between Pekin and Nankin, his design being partly educational, as it was called the Park of Intelligence. In the valley of the Euphrates, centuries before the time of Moses, there were lists of sacred animals, and records of the keeping in captivity of apes, elephants, rhinoceroses, camels and dromedaries, gazelles and antelopes, and it may well be that the legend of the Garden of Eden is a memory of the Royal Menagerie of some ancient king. The Greeks, whose richest men had none of the wealth of the Egyptians or of the princes of the East, do not appear to have kept many wild animals, but the magnates of imperial Rome captured large numbers of leopards, lions, bears, elephants, antelopes, giraffes, camels, rhinoceroses and hippotami, and ostriches and crocodiles, and kept them in captivity, partly for use in the arena, and partly as a display of the pomp and power of wealth. In later times royal persons and territorial nobles frequently kept menageries of wild animals, aviaries and aquaria, but all of these have long since vanished.

Thus, although the taste for keeping wild animals in captivity dates from the remotest antiquity, all the modern collections are of comparatively recent origin, the oldest being the Imperial Menagerie of the palace of Schönbrunn, Vienna, which was founded about 1752, whilst some of the most important are only a few years old. These existing collections are of two kinds. A few are the private property of wealthy landowners, and their public importance is due partly to the opportunity they have afforded for experiments in acclimatisation on an extensive scale, and still more to the refuge they have given to the relics of decaying species. The European bison is one of the best-known cases of such preservation, but a still more extraordinary instance is that of Pére David's deer, a curious and isolated type which was known only in captivity in the Imperial Parks of China. The last examples in China were killed in the Boxer war, and the species would be absolutely extinct but for the small herd maintained by the Duke of Bedford at Woburn Abbey. In 1909 this herd consisted of only twenty-eight individuals; it now numbers sixty-seven. The second and best-known types of collections of living animals are in the public Zoological Gardens and Parks maintained by Societies, private companies, States and municipalities. There are now more than a hundred of these in existence, of which twenty-eight are in the United States, twenty in the German Empire, five in England, one in Ireland, and none in Scotland. But perhaps I may be allowed to say how much I hope that the efforts of the Zoological Society of Scotland will be successful, and that before many months are over there will be a Zoological Park in the capital of Scotland. There is no reason of situation or of climate which can be urged against it. The smoke and fog of London are much more baleful to animals than the east winds of Edinburgh. The Gardens of North Germany and the excellent institution at Copenhagen have to endure winters much more severe than those of lowland Scotland, whilst the arctic winter and tropical summer of New York form a peculiarly unfortunate combination, and none the less the Bronx Park at New York is one of the most delightful menageries in existence. The Zoological Society of Scotland will have the great advantage of beginning where other institutions have left off; it will be able to profit by the experience and avoid the mistakes of others. The Zoological Society of London would welcome the establishment of a Menagerie in Scotland, for scientific and practical reasons. As I am speaking in Scotland, I may mention two of the practical reasons. The first is that in Great Britain we labour under a serious disadvantage as compared with Germany with regard to the importation of rare animals. When a dealer in the tropics has rare animals to dispose of, he must send them to the best market, for dealing in wild animals is a risky branch of commerce. If he send them to this country, there are very few possible buyers, and it often happens that he is unable to find a purchaser. If he send them to Germany, one or other of the twenty Gardens is almost certain to absorb them, and failing Germany, Belgium and Holland are near at hand. Were there twenty prosperous Zoological Gardens in Great Britain, they could be better stocked, at cheaper rates, than those we have now. The second practical reason is that it is a great advantage to menageries to have easy opportunities of lending and exchanging animals; for it often happens that as a result of successful breeding or of gifts on the one hand, or of deaths on the other, a particular institution is overstocked with one species or deficient in another.

One of the ideas strongly in the minds of those who founded the earlier of modern Zoological Gardens was the introduction and acclimatisation of exotic animals that might have an economic value. It is curious how completely this idea has been abandoned and how infertile it has proved. The living world would seem to offer an almost unlimited range of creatures which might be turned to the profit of man and as domesticated animals supply some of his wants. And yet I do not know of any important addition to domesticated animals since the remotest antiquity. A few birds for the coverts, fancy water-fowl for ponds and lakes, and brightly plumaged birds for cages or for aviaries have been introduced, chiefly through zoological societies, but we must seek other reasons for their existence than these exiguous gains.

Menageries are useful in the first place as educational institutions, in the widest sense of the word. Every new generation should have an opportunity of seeing the wonder and variety of animated nature, and of learning something that they cannot acquire from books or pictures or lectures about the chief types of wild animals. For that reason Zoological Gardens should be associated in some form with elementary and secondary education. We in London admit the children from elementary schools on five mornings in the week at the nominal charge of a penny for each child, and in co-operation with the Educational Committee of the London County Council, we conduct courses of lectures and demonstrations for the teachers who will afterwards bring their children to visit the Gardens.

Menageries provide one of the best schools for students of art, for nowhere else than amongst living animals are to be found such strange fantasies of colour, such play of light on contour and surface, such intricate and beautiful harmonies of function and structure. To encourage art the London Society allows students of recognised schools of drawing and painting, modelling and designing, to use the Gardens at nominal rates.

Menageries provide a rich material for the anatomist, histologist, physiologist, parasitologist and pathologist. It is surprising to note how many of the animals used by Lamarck and Cuvier, Johannes Müller and Wiedersheim, Owen and Huxley were obtained from Zoological Gardens. At all the more important gardens increasing use is being made of the material for the older purposes of anatomical research and for the newer purposes of pathology and physiology.

There remains the fundamental reason for the existence of Menageries, that they are collections of living animals and therefore an essential material for the study of zoology. Systematic zoology, comparative anatomy, and even morphology, the latter the most fascinating of all the attempts of the human intellect to re-create nature within the categories of the human mind, have their reason and their justification in the existence of living animals under conditions in which we can observe them. And this leads me to a remark which ought to be a truism but which, unfortunately, is still far from being a truism. The essential difference between a zoological museum and a menagerie is that in the latter the animals are alive. The former takes its value from its completeness, from the number of rare species of which it has examples, and from the extent to which its collections are properly classified and arranged. The value of a menagerie is not its zoological completeness, not the number of rare animals that at any moment it may contain, not even the extent to which it is duly labelled and systematically arranged, but the success with which it displays its inhabitants as living creatures under conditions in which they can exercise at least some of their vital activities.

The old ideal of a long series of dens or cages in which representatives of kindred species could mope opposite their labels is surely but slowly disappearing. It is a museum arrangement, and not an arrangement for living animals. The old ideal by which the energy and the funds of a Menagerie were devoted in the first place to obtaining species 'new to the collection' or 'new to science' is surely but slowly disappearing. It is the instinct of a collector, the craving of a systematist, but is misplaced in those who have the charge of living animals. Certainly we like to have many species, to have rare species, and even to have new species represented in our Menageries. But what we are learning to like most of all is to have the examples of the species we possess, whether these be new or old, housed in such a way that they can live long, and live happily, and live under conditions in which their natural habits, instincts, movements, and routine of life can be studied by the naturalist and enjoyed by the lover of animals.

Slowly the new conditions are creeping in, most slowly in the older institutions hampered by lack of space, cumbered with old and costly buildings, oppressed by the habits of long years and the traditions established by men who none the less are justly famous in the history of zoological science. Space, open air, scrupulous attention to hygiene and diet, the provision of some attempt at natural environment are receiving attention that they have never received before. You will see the signs of the change in Washington and New York, in London and Berlin, in Antwerp and Rotterdam, and in all the Gardens of Germany. It was begun simultaneously, or at least independently, in many places and under the inspiration of many men. It is, I think, part of a general process in which civilised man is replacing the old hard curiosity about nature by an attempt at sympathetic comprehension. We no longer think of ourselves as alien from the rest of nature, using our lordship over it for our own advantage; we recognise ourselves as part of nature, and by acknowledging our kinship we are on the surest road to an intelligent mastery. But I must mention one name, that of Carl Hagenbeck of Hamburg, to be held in high honour by all zoologists and naturalists, although he was not the pioneer, for the open-air treatment and rational display of wild animals in captivity were being begun in many parts of the world while the Thier-Park at Stellingen was still a suburban waste. He has brought a reckless enthusiasm, a vast practical knowledge and a sympathetic imagination to bear on the treatment of living animals, and it would be equally ungenerous and foolish to fail to recognise the widespread and beneficent influence of his example.

However we improve the older menageries and however numerous and well-arranged the new menageries may be, they must always fall short of the conditions of nature, and here I find another reason for the making of zoological sanctuaries throughout the world. If these be devised for the preservation of animals, not merely for the recuperation of game, if they be kept sacred from gun or rifle, they will become the real Zoological Gardens of the future, in which our children and our children's children will have the opportunity of studying wild animals under natural conditions. I myself have so great a belief in the capacity of wild animals for learning to have confidence in man, or rather for losing the fear of him that they have been forced to acquire, that I think that man, innocent of the intent to kill, will be able to penetrate fearlessly into the sanctuaries, with camera and notebook and field-glass. In any event all that the guardians of the future will have to do will be to reverse the conditions of our existing menageries and to provide secure enclosures for the visitors instead of for the animals.

I must end as I began this Address by pleading the urgency of the questions I have been submitting to you as an excuse for diverting your attention to a branch of zoology which is alien from the ordinary avocations of most zoologists, but which none the less is entitled to their fullest support. Again let me say to you that I do not wish to appeal to sentiment; I am of the old school, and, believing that animals are subject and inferior to man, I set no limits to human usufruct of the animal kingdom. But we are zoologists here, and zoology is the science of the living thing. We must use all avenues to knowledge of life, studying the range of form in systematic museums, form itself in laboratories, and the living animal in sanctuaries and menageries. And we must keep all avenues to knowledge open for our successors, as we cannot guess what questions they may have to put to nature.

British Association for the Advancement of Science.

SECTION E: DUNDEE, 1912.

ADDRESS

TO THE

GEOGRAPHICAL SECTION

BY

COLONEL SIR C. M. WATSON, K.C.M.G., C.B., M.A., R.E.,

PRESIDENT OF THE SECTION.

THE last occasion upon which the City of Dundee extended its hospitality to the members of the British Association was in 1867, forty-five years ago, and at that meeting the President of the Geographical Section was Sir Samuel Baker, who had then recently returned from his explorations on the Upper Nile, for which he had been awarded the Patron's Medal of the Royal Geographical Society, and which were of the greatest importance as regards that then little-known river.

In the Address which he gave to Section E, Sir Samuel Baker naturally referred at considerable length to the geography of the Sudan, and to the question of the sources of the Nile, which had been discovered a few years previously by Captain Speke and Captain Grant, when they visited the great lake, named by them the Victoria Nyanza, out of which flows the main branch of the river, the fertiliser of Egypt, which, after a course of more than 3,500 miles, pours its waters into the Mediterranean. He also spoke of the second great lake, the Albert Nyanza, which he had himself discovered, after a long and very arduous journey, though, perhaps naturally, he did not dwell so much on what he had himself accomplished, as another speaker might have done. The words he spoke are well worth calling to remembrance, and, on reading them over, one is struck by the fact that hardly anything was then known of the country through which he travelled, but that, thanks to him and his predecessors, Speke and Grant, the first steps were taken which led to half-a-century of steady progress in geographical knowledge, until now the basin of the Upper Nile is fairly well known and fairly well mapped.

To-day I propose to take up the tale where Sir Samuel Baker had to stop, and to give a short *résumé* of the story of the Sudan since those days, more especially from the geographical point of view; but it will be necessary briefly to allude to its history also, for, in this case, as in all others, history and geography are closely united, and it is difficult to understand one without knowing something of the other.

There is a considerable amount of uncertainty in the minds of some people as to what the Sudan is, an uncertainty not without reason, as the word has an ethnological rather than a geographical meaning. The complete word, Balad-es-Sudan, is an Arabic expression for the country of the black people, and therefore includes, theoretically, all those parts of Africa which are inhabited by negro or negroid races. There has, however, been such a mingling of different races that it would be difficult to say to what part of the great continent the word Sudan should properly be applied. But, of recent years, changing from its original ethnological meaning, it has come to be regarded as the name of a more limited area; and perhaps the simplest definition is that it includes all the country watered by the Nile and its tributaries, as far north as the twentieth degree of latitude, and excluding the Sahara, and the basins of Lake Chad and the Congo on the west, and the districts watered by the river systems which terminate in the Red Sea and Indian Ocean on the east. Such a definition does not, of course, altogether agree with the existing political divisions, as it includes the eastern part of Abyssinia, Uganda, and part of the Congo State territory; but these divisions are in no sense geographical, whereas the basin of the Nile is a well-defined region which contains the greater portion of what may be regarded as the real Sudan.

There is one point as regards the geography of the Sudan which is remarkable and perhaps unique. In former times it was to a certain extent known, and, in the maps of Ptolemy, and of the Middle Ages, the great lakes, the ranges of mountains, and the rivers flowing from them, are indicated in a distinct, if not very accurate manner. But, owing to various causes, this geographical knowledge was completely lost, and the natural features disappeared from the maps. Look, for example, at Keith Johnston's Atlas, published in 1843, and you will see that there are no lakes shown, while the Nile to the south of 10° North latitude is indicated as an insignificant stream. The Sudan had relapsed into the position of a *terra incognita*, just as it had been in the days of Herodotus, and Ptolemy and the other ancient geographers were regarded as victims of their imaginations.

The revival of the knowledge of geography of the Sudan may be said to commence with the travels of James Bruce, who visited Abyssinia in 1770, explored Lake Tsana, and found what he believed to be the true source of the Nile in the River Abai, which ran into the lake from the south. He examined the place where the Blue Nile flowed out of Lake Tsana, but was not able to follow its course through the western mountains of Abyssinia, and rejoined it at Sennaar, about 220 miles above the junction with the White Nile. Travelling along the south bank of the Blue Nile, he crossed it at the ferry of El Efun, and then went on to Halfaya, north of the site of the present town of Khartum, which at that time did not exist. Of the White Nile he says: 'At half-past eight, about four miles further, we came to the village Wad Hogali. The river Abiad, which is larger than the Nile, joins it there. Still the Nile preserves the name of Bahr-el-Azrek, or the Blue River, which it got at Sennaar. The Abiad is a very deep river; it runs dead, and with little inclination; because, rising in latitudes where there are continual rains, it therefore suffers not the decrease the Nile does by the six months' dry weather.' This is all he says of the White Nile, and he does not seem even to have taken the trouble to look at it, as he reports the point of junction of the two rivers as four miles north of Halfaya, whereas it is to the south of that place. He was so convinced that the Blue River was the one and only Nile that he regarded the investigation of the White Nile as unimportant, and shows it on his map as a comparatively insignificant river. Bruce's action in this matter is a warning to explorers not to neglect to examine something that does not fit in with their preconceived ideas.

At the time of Bruce's visit the origin of the White Nile seems to have been unknown to the inhabitants of the kingdom of Sennaar, a kingdom which had been established in 1504 by the Fung dynasty, which had taken possession of what had been the Christian kingdom of Alwah. Soba, the capital of Alwah, was abandoned, and a new town built at Sennaar, which was made the seat of government. The Fungs were partly of Arab and partly of negro descent, and their kingdom extended east of the Blue Nile to the foot of the Abyssinian Mountains, and westward as far as the White Nile, beyond which were the independent kingdoms of Kordofan and Darfur. At that time there appears to have been little or no traffic on the White Nile, and the marshes of the tenth degree, inhabited by the powerful Shilluk tribes, formed an impenetrable barrier to the south.

But, although after Bruce's expedition to Lake Tsana the majority of people seem to have accepted the Blue River as the true Nile, there were some wider-minded people who felt that there was a secret hidden behind the marsh barrier. One of these was a certain Mr. W. G. Browne, who made an interesting journey to Darfur in 1793, and who records in the account of his travels that he had the conviction that the river, of which Bruce had discovered the source, was not the true Nile, and that he considered it a matter of great importance that the course of the more western river, *i.e.* the White Nile, should be investigated, as he could not believe that its source was only two hundred leagues south of Sennaar.

Starting from Egypt, Browne travelled with a merchant's caravan from Assiut, by way of the oases of Khargeh and Selima, to El Fasher, in Darfur. Here he remained for three years, but was not able to do much in the way of exploration, as he was thwarted by the king and people, and was not allowed to go to Sennaar or to explore the White Nile. He collected, however, from the accounts given him by the natives, a good set of itineraries in Darfur and Kordofan, the first, so far as I know, compiled for the Sudan. But his efforts to obtain information as to the source of the White Nile were not successful, and all he was able to learn was that ten days' journey south of a place called Abu Telfan, the Bahr-el-Abiad had its source in forty rivers, which came from the hills of Kumr. It seems probable that these numerous rivers were those that form the head waters of the Bahr-el-Ghazal, and that the people of Darfur knew as little about the Bahr-el-Gebel, as the southern part of the White Nile is called, as the people of Sennaar.

But although Browne was not able himself to solve the mystery, his name should not be forgotten, as being one of the first in modern times to realise the fact that the White Nile was the longer of the two rivers. His views, however, seem to have met with no support, and Bruce was supposed to have settled the question of the sources of the Nile. The great lakes, shown by Ptolemy and the mediæval geographers, were, as I have already mentioned, erased from the map, and the White Nile was left in peace.

During the visit of Browne to Darfur the kingdom of Sennaar had fallen upon evil times, as an insurrection, which had commenced during the reign of Bady, ended with the death of King Adlan in 1789, when the Fung dynasty came to an end, and all authority fell into the hands of the tribal chiefs, who made and removed the kings of Sennaar at their pleasure. The internecine wars continued up to the time of the arrival of the Egyptians in the Sudan, and greatly facilitated the advance of the latter.

This advance of the Egyptians was due to the policy of Mahomed Ali Pasha, the Turkish Governor of Egypt, who had greatly increased his power by a successful campaign in Arabia in 1812-18, when he succeeded in capturing Mecca and Medina, and made himself master of the country. He then turned his attention to the Sudan, and decided to take advantage of the local troubles and to add Sennaar and Kordofan to the Egyptian dominions. In 1820 he sent an army up the Nile, under his son Ismail, who took possession of Dongola and the country adjacent to the river, as far as the junction of the Blue and White Niles, and, after seizing Sennaar, marched up the Blue Nile to Fazokl, on the Abyssinian frontier. Kordofan was also occupied, and the capital of the new Egyptian province was placed at Khartum, the point where the two Niles met, which took the place of the old capital of Sennaar; but no attempt was made to take possession of the country along the White Nile to more than about one hundred miles south of Khartum. So little was that river known beyond this that when Linant Pasha succeeded in sailing up the river as far as the Island of Aba he was supposed to have arrived at the furthest point reached by a European since the first century.

No further advance was made for a few years, but, in 1838, Mahomed Ali decided to try to open up the White Nile, and an expedition under Major Selim, of the Egyptian Army, succeeded in making its way through the marsh district, and in reaching a point about 6° 30′ North latitude on the Bahr-el-Gebel, while another expedition in 1842 got as far as Gondokoro. It was thus proved that the marshes were not impenetrable, and trading stations began to be opened up, both on the Bahr-el-Gebel and the Bahr-el-Ghazal. On the former river, however, the traders could not at first proceed further than Gondokoro, as the rapids, which commenced a few miles south of that place, made navigation by sailing vessels impracticable, so the merchants had to establish their depots at Gondokoro and depend upon the natives bringing ivory from the south. To these natives the opening of the river proved a great evil, as the legitimate traders were soon followed by slave-hunters, who carried thousands into captivity, while killing many others. By the ill-will thus created the difficulty of exploration was increased. In the end, the source of the White Nile was discovered not from the north, but from the south, when Captain Speke, who, in company with Captain Burton, was exploring Central Africa from the east coast, heard of a great lake lying to the north, and succeeded in reaching the south end of the Victoria Nyanza in 1858. Convinced that he had found the long-desired source of the Nile, he started on another expedition, accompanied by Captain Grant, in 1860, and, after marching round the Victoria Lake, reached Gondokoro in 1863. Here he met Sir Samuel Baker, who had started from Khartum in 1862, in the hope of discovering the Nile sources. The information given by Speke and Grant showed that they had forestalled him; but he continued his journey, and in 1864 succeeded in reaching the Albert Nyanza, the second great lake from which the White Nile derives its water.

Thus, at length, after a lapse of many centuries, the truth of the statements made by Ptolemy and other ancient geographers was justified, and the lakes shown by them were restored to the map of Africa, while the White Nile was proved to be the real Nile, and the Blue Nile was relegated to the position of being the most important tributary.

During the period of the travels of Speke and Baker the slave trade had been rapidly increasing, and the traders had practically taken possession of the country, and made themselves independent of the Egyptian authorities in Khartum. These slave traders cared nothing for geography, and had matters remained as they were at that time, it is probable that a State hostile to Europeans would have been established, and all chance of further exploration would have been lost.

But in 1869 the Khedive Ismail, who had succeeded as ruler of Egypt in 1863, and had obtained largely increased powers from the Sultan, decided to restore his authority on the White Nile, and appointed Sir Samuel Baker as Governor of the country south of Gondokoro, with instructions to establish Egyptian rule as far as he could to the south of that point. But nature fought against Baker, and the difficulty of sailing up the White Nile had been enormously increased by the formation of the sudd, that strange vegetable barrier which at times completely closes the river channel, and he did not reach Gondokoro until two years had elapsed from the time of his departure from Khartum. There he hoisted the Egyptian flag, and then proceeded to occupy the country to the south. But he was not successful, as the force at his disposal was quite insufficient, and, though he established a few stations on the road from Gondokoro to Foweira, on the Upper Nile, little effective had been done when he returned to Gondokoro in April 1873. Neither was he able to do much in the way of geographical research, and, greatly to his regret, was unable to revisit the lake which he had discovered on his first journey.

In 1874 Colonel Gordon was appointed to succeed Baker, and, leaving Khartum in March, reached Gondokoro in twenty-four days, the sudd, fortunately for him, having been cut through by the Egyptian officials only a month before his arrival in the Sudan. Gordon ruled the equatorial provinces until October 1876, and during that time did much to tranquillise the country, as he had a remarkable influence over the natives. He moved the headquarters of the government from Gondokoro to Lado, and established a chain of posts along the Nile to Duflé, and thence to Nyamyongo, in Uganda, about eighty miles below the Ripon Falls. He also placed two steamers and two sailing-boats on the Albert Lake to facilitate communication. Gordon devoted much attention to the geography of the district, and prepared a map of the White Nile from Khartum to Urondogani, superior to any that had preceded it. This map included a plan of the Albert Nyanza, based on surveys made by Gessi and Mason, both of whom circumnavigated the lake. Mason reported the existence of the river, now called the Semliki, entering the lake from the south, but was unable to enter it, as the water was too shallow for his vessel.

Soon after his arrival at Gondokoro Gordon fully realised the difficulty of keeping up communication with Egypt by the Nile, and requested the Khedive to send an expedition to Formosa Bay, about a hundred miles north of Mombasa, on the east coast of Africa, with the view of opening up a road towards the Nile. The route he thought of was a little north of that now followed by the Uganda railway; but at the time he made the proposal the country was entirely unknown, and the difficulties would have been much greater than he anticipated. The idea, however, came to nothing, first, because the expedition was sent to the

River Juba, on the border of Somaliland, which was much too far to the north, and, secondly, because it was ordered away by the British Government, which considered that it was encroaching on the territories of the Sultan of Zanzibar.

At the time that Gordon was establishing Egyptian authority in the equatorial provinces the Khedive's dominions were being extended by the conquest of Darfur, and the occupation of the province of Harrar, with its port at Zeila, in the Gulf of Aden. An excellent reconnaissance of Kordofan was carried out by Colonel Prout, of the United States Survey Department, in 1875, and a reconnaissance of Darfur was made by Colonel Purdy, another American in the Egyptian service, so that considerable additions were made at this period to the geographical knowledge of the Sudan.

But soon afterwards there was a serious setback to the Khedive Ismail's projects of conquest. Having acquired Massowah, Tajurra, and Zeila, on the Red Sea, he sent an expedition into Abyssinia in 1875, which was cut to pieces at Gundet, on the road to Adua, and another larger force sent in the following year was utterly defeated by the Abyssinians and had to retreat, with great loss, to Massowah. Some surveys were made by the American officers on the staff of the Egyptian Army, but these expeditions did but little for geography, and their fate was the precursor of the destruction of Egyptian power in the Sudan.

Colonel Gordon returned to Egypt in December 1876, and early in the following year was appointed Governor-General of the whole Sudan, a post he held for nearly three years, years of incessant labour, during which, much to his regret, he was able to do little for geography; as, though he travelled many thousands of miles through his vast territories, his whole time was occupied with questions of administration. He was wonderfully successful in his dealings with the inhabitants, and had he been left alone for a few years, the history of the Sudan would have been different; but he was constantly urged to send money to Cairo, money which he could not obtain without following the example of his predecessors and oppressing the inhabitants. This he would not do, and resigned in August 1879, when he was succeeded by an Egyptian Pasha, who revived the old bad customs of the country. His appointment led to the result that might have been anticipated, and in 1881 the revolt led by Mahomed Achmed, the Mahdi, broke out, and the Egyptians were driven out of the Sudan. Then the country was completely closed to Europeans, and nothing further could be done in the way of geographical discovery until the defeat of the rebels at Omdurman in 1898. Now, fortunately, peace is restored, a peace which, it may be hoped, will be a lasting one.

To geographers, of course, the existing state of affairs is very satisfactory, as it will undoubtedly lead to an increase in our knowledge of the Sudan and its resources. That knowledge is still very limited, much more so than many people are aware, and there are vast regions which still stand in need of careful examination. Maps, especially small scale maps, are misleading, and convey the impression that more is known than is really known. Take, for example, the case of the Blue Nile, one of the most important tributaries of the great river. Of this, the head waters, Lake Tsana, first carefully examined by James Bruce, are fairly well known, and a good reconnaissance of this lake was made by Mr. C. Dupuis, of the Egyptian Irrigation Department, in 1903, a copy of whose interesting report is attached to the valuable Report on the Basin of the Nile, made by Sir W. Garstin in 1904.

The course of the Blue Nile from Famaka on the Abyssinian frontier to Khartum is also fairly well known, although not yet accurately surveyed. But of the river between Lake Tsana and Famaka, and of its course through the mountains of Abyssinia, our knowledge is most elementary, and it is doubtful whether the line as marked upon maps is correct. Here is a chance for a resolute explorer to distinguish himself by making a really good reconnaissance of this part of the river, and following it carefully from Lake Tsana to Famaka. But it would probably be rather an arduous task, and there would be many difficulties, natural and human, to overcome.

The question of the Blue Nile is only one of the many geographical problems to be solved in the Sudan. The upper waters of other tributaries, such as the Atbara, the Rahad, the Dinder, and the Sobat, and the mountains from which they flow, are also little known, and will require years of exploration, while great areas of the level country of the Nile basin remain unvisited and unsurveyed. This can be well realised by anyone reading Sir W. Garstin's excellent report already mentioned, in which he gives an admirable summary of the hydrography, and deals with the important question as to the manner in which the water of the different tributaries of the Nile can best be utilised for improving the agricultural capacity both of the Sudan and of Egypt. Among other projects with this object he proposes the cutting of an entirely new channel of more than two hundred miles in length, so as to allow the waters of the Bahr-el-Gebel to leave the existing channel at Bor, eighty miles north of Gondokoro, and to rejoin the Nile near the mouth of the Sobat below the sudd district; but, as he justly points out, the country through which this new channel would pass is practically unknown, as the whole of the area lying between the Bahr-el-Gebel, the Bahr-ez-Zaraf, and the Sobat is a *terra incognita.*

Sir W. Garstin points out that there is a great loss of water from the Bahr-el-Gebel between Gondokoro and Bor, for which he cannot account, and this is another point requiring to be investigated. Reading his remarks upon this subject reminds me of the time when I was assisting in General Gordon's survey of the Nile, when on this part of the river, at a point about fifty miles north of Gondokoro. I noticed a considerable branch leaving the Bahr-el-Gebel, and going apparently in a north-easterly direction. The native pilot told me that it was reported by the inhabitants to join the Sobat. It was impossible to investigate the truth of this statement, which, at the time, seemed rather doubtful, but it is interesting to note that a high authority like Sir W. Garstin records that the Nile loses a considerable volume of water near this place.

Whether the proposal of Sir W. Garstin to make this great canal will ever be carried out is doubtful; for my own part, I am inclined to think that, having regard to the amount of work to be done in the Sudan, it would be better to leave the Bahr-el-Gebel alone for the present. The cost of a canal such as that suggested would be very large, and if funds were available it would be better to spend them on a railway from the Sobat southwards. Sooner or later the railway, which now runs some distance south of Khartum to the point where it crosses the White Nile into Kordofan, will be extended, and in process of time will reach the Sobat. Meanwhile it might be worth while to select a point on the Sobat suitable for a bridge, and to make that point the northern terminus of a line of railway, leading southwards to Gondokoro, and later, on to Uganda. Communication between Khartum and this terminus would, for the present, be kept up by the White Nile, which, with the exception of one or two places, is navigable for the whole year.

Looking at the question of the Sudan from the geographical point of view, there has been a wonderful increase of knowledge since the last meeting of the British Association in Dundee; but, on the other hand, there is a larger amount of work yet to be done before the whole of the vast area will have been satisfactorily surveyed, and it must be remembered that the Sudan Government has claims of greater importance at present than that of carrying out a complete trigonometrical survey. But exploration will no doubt be carried on year by year, and the blank spaces on the map will gradually be filled up. Meanwhile we must wish Godspeed to the British officers in the Sudan, who are carrying out a great work of civilisation, and, at the same time, adding to the geographical knowledge of the world.

Leaving the Sudan, I would like to allude to a very important geographical undertaking which has made considerable progress during the past year. This is the production of the international Map of the World on the scale of $\frac{1}{1000000}$, a project which has been under the consideration of the leading geographers of the important countries for more than twenty years, since it was first proposed at the International Geographical Congress held at Berne in 1891. The question was discussed at succeeding Geographical Congresses, but did not take definite shape until the meeting held at Geneva in 1908, when a series of resolutions dealing with the subject were drawn up by a Committee composed of distinguished men of many nations, which was appointed to formulate rules for the production of the maps, so as to ensure that they should be prepared upon a uniform system.

These resolutions were approved at a general meeting of the Geneva Congress, and were forwarded by the Swiss Government to the British Government for consideration, whereupon the latter issued invitations to the Governments of

Austria-Hungary, France, Germany, Japan, Russia, Italy, Spain, and the United States of North America, asking them to nominate delegates to act as the members of an International Committee to meet in London and debate the question. This Committee assembled at the Foreign Office in November 1909, and Colonel S. C. N. Grant, C.M.G., then Director-General of the British Ordnance Survey, was appointed President. The proceedings were opened by the Under-Secretary of State for Foreign Affairs, Sir Charles Hardinge, G.C.M.G., now Lord Hardinge, who, in his address, referred to the progress that had already been made with regard to the International Map, and expressed the hope, on behalf of the British Government, that the great undertaking might be brought to a satisfactory conclusion.

The main business before the Committee was to settle on the mode of execution of the map, especially as regards the size of the sheets, so as to ensure that adjacent sheets, published by different countries, should fit together; and also to settle upon the symbols, printing, and conventional signs to be used, in order that these should be uniform throughout. A series of resolutions, embodying the decisions arrived at concerning these various points, was approved and drawn up in English, French, and German, the first of these languages being taken as the authoritative text. As the map was to embrace the whole surface of the globe, the method of projection to be adopted was, of course, a very important consideration, and, after due deliberation, it was decided that a modified polyconic projection, with the meridians shown as straight lines, and with each sheet plotted independently on its central meridian, would prove the most satisfactory.

The surface of the sphere was divided into zones, each containing four degrees of latitude, commencing at the equator, and extending to 88° North, and 88° South latitude. There were thus twenty-four zones on each side of the equator, and these were distinguished by the letters A to V north, and A to V south. This fixed the height of each sheet. For the width of the sheets, the surface of the sphere was divided into sixty segments, each containing six degrees of longitude, and numbered consecutively from one to sixty, commencing at longitude 180°. This arrangement made each sheet contain six degrees of longitude by four degrees of latitude; but, as the width of the sheets diminished as they approached the poles, it was decided that, beyond 60° North, or 60° South, two or more sheets could be combined. Each sheet could thus be given a clear identification number defining its position on the surface of the globe, without it being necessary to mention the country included in it, or the latitude and longitude. For example, the sheet containing the central part of England is called North, N. 30.

In order to ensure that the execution of all the maps should be identical, a scheme of lettering and of conventional topographical signs was drawn up and attached to the resolutions; and it was decided that a scale of kilometres should be shown on each sheet, and also a scale of the national measure of length of the country concerned. As regards the representations of altitude it was arranged that contours should be shown at vertical intervals of a hundred metres, or at smaller intervals in the case of very flat, and larger in the case of steep ground, the height being measured from mean sea-level, as determined in the case of each country; while the levels of the surface of the country were to be indicated by a scale of colour tints, the colours being green from 0 to 300 metres, brown from 300 to 2,500 metres, and purple above 2,500 metres. In the same manner the depths of the ocean and of large lakes were to be indicated by varying tints of blue, so as to show intervals of 100 metres. In order to ensure uniformity in the scale of colours to be used, a copy of it, as approved by the Committee, was included in the plate of topographical symbols.

The whole scheme was thoroughly well worked out, and great credit is due to the members of the International Committee for the manner in which they carried out their difficult task. Since the meeting of the Committee in 1909 the preparation of the sheets, in accordance with the principles decided upon, has been taken in hand in several countries, and a number of these have been issued, which give a good idea of what this great map, the largest ever contemplated, will be like. These sheets deserve to be carefully studied, and will doubtless be the subject of considerable criticism, as there are several points which seem worthy of examination.

In the first place, it is for consideration whether it would not have been better if the colour scheme for representing differences of altitude had been omitted, as it is doubtful whether the advantage of the result gained is commensurate with the increased cost of printing the colours. And one naturally asks for what purpose is the map intended. Is it for the use of skilled geographers, of whom there are a comparatively small number in each country, or is it for the instruction of ordinary people? If it is for the latter, it is to be feared that the colour scheme will give rise to erroneous impressions. Compare, for example, Sheet North, M 31, of France, with Sheet South, H 34, of part of South Africa. In the former, as the greater part of the country shown is less than 300 metres above the sea, the general colour of the sheet is green, while in the latter, as nearly the whole of the country included has an altitude of more than 300 metres, the map is for the most part brown. This to the less educated man will probably convey the idea that, while France is a fertile country, South Africa is a desert. The fact, too, that the darker tint of green represents the lower level and the lighter the higher, while, in the case of the brown, the lighter represents the lower and the darker the higher, and, in the case of the purple, the relative strength of the tints is again reversed, is rather confusing.

There is another point as regards the colour scheme which might be noticed, that is, that it is not the same on different sheets. For example, the scale of tints adopted in Sheet North, O 30 (Scotland), North, M 31 (France), and North, K 35 (Turkey), do not correspond. In the Scotch map the brown colour commences at an altitude of 200 metres, in the French at 300 metres, and in the Turkish at 400 metres. There may be some reason for this, but it appears not to be in accord with the resolutions of the Committee. Another reason for omitting the colour scheme for altitudes is that it might be better to keep colour work for other purposes, such as indicating political divisions, as there can be little doubt that so good a map as this, when completed, will be largely used for many purposes. It might be better that on a map of this small scale only the horizontal features, such as coast lines, river courses, railways, roads, and the position of towns should be shown, while to represent height graphically tends to obscure the former.

Another criticism I would venture to make is that the resolutions of the Committee appear to have been drawn up on the supposition that the whole world has been accurately surveyed, and no attempt seems to have been made to distinguish between those regions of which the maps are based on triangulation, such as England and parts of Europe, and the countries of which complete surveys have not yet been made. As the construction of the map proceeds and sheets are prepared of parts of the world our knowledge of which is imperfect, this want will become more pressing, but it is noticeable even with regard to the sheets already published. It is one of the evils of cartography that where anything is shown on a carefully engraved map it comes to be regarded as true, and, if it afterwards turns out to be erroneous, it is not easy to get it altered.

The scale of the map, $\frac{1}{1000000}$, appears to have been wisely chosen, as it is sufficiently large to give an adequate amount of detail, while, at the same time, the sheets will not be unduly numerous. Of course, for an international map a cadastral scale was essential, although for national maps a scale based upon the national system of measures is more convenient, as, for example, in the United Kingdom, where the scales of one inch and six inches to the mile are better than scales of $\frac{1}{500000}$ and $\frac{1}{10000}$ would have been. They are more suited for the majority of individuals, and an ordinary foot-rule can be used for measuring distances, instead of having to take them off with a pair of dividers from the printed scale on the map.

Looked at from the general point of view, there can be no doubt that the International Map is a most important and valuable undertaking. It is satisfactory that such a leading part in the matter has been taken by the British officers of the Royal Engineers and by the Royal Geographical Society.

In speaking of this map I have referred to the advisability, if not the necessity, of distinguishing between what is accurately and what is inaccurately known, and this brings me to another matter of considerable interest, the preparation of maps based upon the observations and information collected by explorers in unknown or little known countries. To these explorers, some of whom have not been trained in geographical science, a large amount of detail shown upon modern maps is due, and it is only a small proportion of the land surface of the globe that has, up to the present, been surveyed in a scientific manner.

It is therefore of the greatest importance that the best value possible should be obtained from the work done by explorers, and this in the past has not always been sufficiently attended to, though during the last few years it is better understood. The people who stop at home in comfortable ease do not sufficiently realise the difficulties under which the conscientious traveller works and gathers together information about the country he passes through. Formerly, he generally had to work out his own observations and compile his own maps, but now conditions in this respect have greatly improved, and when he brings home his observations, notes, and sketches he can hand them over to some body, such as the Royal Geographical Society, by whom they will be put in shape in a better manner than he could do it for himself. One has heard of an explorer in a little-known country sitting up all night after a hard day's work, working out his astronomical observations, and trying to put his rough surveys into shape. He would have done better to have gone to sleep and prepared himself by a good rest for the next day's journey. In fact, it would be better if an explorer never looked at the figures of an observation after he had recorded them, or read over the notes of his past work, confining himself to recording what he has actually seen day by day as accurately as circumstances permitted, and carefully distinguishing what he really saw from what he thought he had seen, or what he had heard.

It would be easy to adduce instances of the errors which have arisen from the neglect of such precautions. Perhaps one of the best known is that I have already alluded to, when James Bruce, a careful explorer, because he had made up his mind that the Blue Nile was the real Nile, passed the White Nile without taking the trouble to examine it, and recorded it as being a comparatively insignificant river. Then, there was the case of Sir Samuel Baker, who, having reached the shores of the Albert Nyanza with great difficulty, relied too much on what he was told by the natives, and showed it on his map as extending many miles to the south of the equator. But great responsibility rests also upon those who have the task of compiling a map from the notes of an explorer, and the greatest care has to be taken to show only what is really known, and not what is uncertain. Geographers, whether in the field or in the drawing office, should always hold up before themselves a standard of accuracy higher than it is always easy to live up to.

Geography under its more ancient name of geometry is, of course, the mother of all sciences, although at the present time geometry has got a more narrow meaning, and is perhaps regarded by some as independent of geography, although really only a branch of it. The study of the earth upon which they lived was to the ancient nations the most important of all studies, and it is interesting to trace how astronomy, mathematics, geology, and ethnology are all so interspersed with geography that it is difficult to separate them. It is satisfactory to note how from the very first the British Association has always recognised the great importance of geography, since the first meeting of the Association at Oxford in 1832, when Sir Roderick Murchison, so well known to fame, acted as President of the Geographical and Geological Section. These two sciences remained united in the same section until the meeting at Edinburgh in 1850, when Sir R. Murchison was again the President. But, at the next meeting at Ipswich in 1851, they were separated, and while Geology remained as the subject of Section C, Geography, on account of its great importance, was made the subject of Section E, and the science of ethnology was united with it. Sir R. Murchison was the first President of the new Geographical Section, and was afterwards President no fewer than six times of Section E, showing the great importance attached by him to the study of the science of Geography. May I express the hope that the Presidents of the Section will endeavour in future to follow, however humbly, in the footsteps of that leader of science.

British Association for the Advancement of Science.

SECTION F: DUNDEE, 1912.

ADDRESS

TO THE

ECONOMIC SCIENCE AND STATISTICAL SECTION

BY

Sir HENRY H. CUNYNGHAME, K.C.B.,

PRESIDENT OF THE SECTION.

Although the theories of Auguste Comte as to the progress of the sciences are in many respects open to question, yet he made two contributions of especial value to our ideas on that subject. In the first place, he was one of the earliest writers who maintained that the social and political sciences are subject to laws just as exact, though more complicated, as the laws which govern the physical sciences; and, in the next place, he formulated the celebrated principle of the three phases of thought. According to this view all sciences commence with a theological stage, they pass through a metaphysical stage, and end by becoming positive.

In a primitive state of civilisation man attributes all phenomena to the exercise of volition; in a more advanced stage of thought he endeavours to attribute them to 'virtues' or 'agencies.' The third stage is reached when he ceases to speculate, and uses general principles rather as modes of classifying phenomena than of explaining them. An example may be taken from theories regarding the nature of fire. At first, fire both celestial and terrestrial wherever it occurred was believed to be due to the direct action of a god. Under Aristotle and the Greeks the phenomena of heat, burning, and dryness were attributed to a principle, one of the characteristics of which was a tendency to fly up to the circle of the stars. But in modern times, owing to the labours of the chemists and physicists, it has been explained as a violent motion of molecules. Of its ultimate character we are still ignorant, but the study of heat has passed into a positive stage, in which great progress has been made in classifying its properties and extending our knowledge of them.

The history of ontology is an example of a study which for centuries was in the theological stage, but which emerged from that condition and entered the metaphysical stage chiefly through the labours of the schoolmen. From their time onwards it steadily evolved along the lines laid down by the realists on the one hand, and the conceptualists on the other, until an attempt at a union of their systems was made by Hegel. But even Hegelism is only metaphysical. We know nothing of what he means by his absolute, which might be the god of Averrhoes or Spinoza on the one hand, or the matter of La Mettrie on the other. The philosophy of the absolute is mere metaphysics.

Positive philosophy or science is at best a classification of phenomena; of ultimate causes we can know nothing. Our knowledge, as is finely said by Byron, is but an exchange of ignorance for that which is another kind of ignorance; though immense progress in knowledge of phenomena is made by the transaction.

One of the signs that a science has passed into the positive stage is that it has been subjected to the laws of mathematics. Mechanics, physics, chemistry, and electricity have long since been treated mathematically.

Biology has only recently begun to receive mathematical treatment. Politics, economic sciences, sociology, anthropology, and language have, however, hitherto firmly resisted attempts to bring them under mathematical guidance. In some cases attempts have been made, as, for example, when that great mathematician Professor Sylvester endeavoured to formulate a mathematical poetry. Unfortunately he put his theories into practice, but the mathematical poems which he composed were not such as to encourage the adoption of his methods. The above sciences have, indeed, passed out of the theological stage. We no longer ascribe political maxims to the direct commands of God, nor social phenomena to direct Divine interposition. But all the social sciences are for the most part still in the metaphysical stage. The doctrine of the divine right of kings has only disappeared in order to be replaced by the doctrine of the divine right of majorities. Yet from a positive point of view neither of these stands on a footing much firmer than that of the other. The 'duty of obedience to authority' and the 'right of resistance' are in the same condition. The 'right to work,' 'the right to live,' 'the right to a living wage,' 'the right to the vote' are all metaphysical propositions assumed as axiomatic by various energetic writers and speakers, and which are usually advanced with a dogmatism proportioned to the uncertainty of their foundation. Yet on what basis do they rest? One might with equal cogency declare for 'the right of the stronger to destroy the weaker,' 'the duty to improve the race by permitting and encouraging the forcible elimination of the unfit.' Or again, we might argue that animals have a right to be protected against attempts made upon their life or property, and to be considered in any scheme for the promotion of the greatest happiness of the greatest number. This problem is said to have perplexed Bentham in his later years. For by a negation of the doctrine of the immortality of the soul it was difficult for him to see why they were not to be put on a par with man. Or, again, take Proudhon's aphorism: 'Everyone has a right to that which he has made. Who made the land?—God, then, proprietor, begone.' Even if the major premiss were granted, it is easy to see that the proprietor might logically refuse to give up the land till God came Himself to ask for it, and decline to surrender it to one who had no more share in making it than the person actually in possession. Another example is the metaphysical aphorism that every right involves a corresponding duty, so that if I have a right to do a thing it is the duty of others concerned in the action to let me do it. This axiom seems at first sight to have a certain amount of plausibility. But does it follow, from the fact that I have a right to kill my ducks, that it is their duty to come and be killed? Nor in this case does the reason addressed to the ducks by the girl in the nursery rhyme appear likely to be very convincing to them. Thus also the right to individual property, the right to an equality of enjoyment, the right to an equality of opportunity, the right of an individual to be considered as an end in himself, the duty of an individual to be considered only as part of an organised society, are all metaphysical assumptions having no firm positive basis. Equally baseless is the axiom that wherever the State enjoins a duty, as on a parent to educate his children, the State ought to pay for it. Or that a local authority contributing funds to an object has a right in every case to interfere with their administration. Yet these are mere chance specimens of the political dogmas that have for years been flying about, and which emphasise the undoubted fact that politics and social science have not yet entered the positive stage of thought.

In what stage is political economy? It appears still the battle-ground of opposite schools. Some there are who tell us that it has 'gone to Saturn.' But this only raises the question what is meant by going to Saturn? Is it meant that the so-called laws of economics are not laws at all, and that the whole pretended science is built on false foundations? Or is it meant that those engaged in the practical politics of the country have resolved to legislate in defiance of the laws of economics, and to settle the problems of free trade and protection, the taxation of fixed and movable property, and the regulation of wages, as though these problems were not subjected to any natural laws at all?

The latter position would, of course, be particularly dangerous if it turned out that there were laws, and that those laws were being ignored. For example, there is a school of biologists who contend that acquired characteristics are never inherited, and that therefore all education and improvement of environment can only be useful inasmuch as they promote the generation of the best types—but that, unscientifically used, these and other means of improvement may, by promoting the survival of the most unfit, only damage humanity. The truth or falsehood of this statement is no concern of this Section of the Association; only one may be permitted the remark that, if it is really a law applicable to the human race, and if it is ignored, it seems most probable that the law will remain here on earth, and that it is not the law, but the race that ignores it, which will go to Saturn. Or, to take an example more directly connected with Political Economy, it is alleged by the students of that science that there are certain laws which regulate wages. They are not altogether in agreement upon the laws, still less upon the mode of expressing them, or upon the modifications which are necessary to make them true. This is only natural in a science that is only just entering upon a positive stage. But I suppose most economists would agree that (provided suitable meanings are given to the words) 'Wages in a free market depend upon the demand and supply of labour.'

A Legislature, wishing to remedy social inequalities and evils, might resolve to render the market no longer free—to impose a minimum wage, or to put a tax upon wages, or in other ways to regulate them by statute. And legislation might go so far as to render wages wholly dependent on scales fixed by authority or by custom. Or, again, a powerful combination, either of employers or of workmen, might unite to fix rates of wages and render it impossible in practice for any other rates to be paid. Systems established by these means might be wise or foolish, beneficial or injurious. But could it be said that the authors of them had succeeded in sending political economy to Saturn? Certainly not. They might have rendered inapplicable that chapter of Political Economy which deals with price as fixed by exchange in a free market, but only to bring the case under the next chapter, entitled 'Price as fixed under conditions of monopoly.' The Political Economy would be there surely enough, with its laws, and with their consequences for those who ignore its teachings.

I am not, mind, arguing against such attempts. Man is a social animal, not merely an individual unit, and it may be wise and desirable that certain of his dealings should be regulated by conditions depending on legal regulations rather than on the free play of demand and supply. I only point out that if political action be taken in the field of economics such action will, whether the authors of it wish it or not, be governed by the laws of economics, and those who purpose such action must consider what effect it will have on the flow and investment of capital, the demand for commodities, and, in fact, duly take into account the whole problem. For, if they do not, it is not the laws of supply and demand that will go to Saturn. Again, before settling a scheme of taxation we ought to study the cases in which a tax levied on one class falls on another, as, for instance, a tax intended to be paid by landlords, which really falls upon their tenants; of taxes levied on tenants which fall on their landlords; of taxes levied on producers that can be shown ultimately to fall on consumers, and of taxes levied upon commodities that can be shown to fall on the workmen by whom they are produced. For a tax often resembles an arrow shot into the air; though apparently aimed in a definite direction it may fall one knows not where, obedient to the laws of the incidence of taxation, just as an arrow in its flight is subject to the inflexible laws of air resistance, friction, and gravity. Or, again, when we prohibit work of children or young persons, to whom such work is detrimental, we must consider not only one side of the question; but we must also take into account the loss that may ensue in wages, and the consequences to the nutrition of the family, and also indirectly to the growth of population.

In fact, in all these and similar cases, unless we possess the power of making the sun and moon stand still in their courses, we must have regard to the operation of natural laws, from which we can no more escape than we can from the air in which we breathe.

I may perhaps pass by the criticisms or even attacks on Political Economy by those whose schemes of action appear to contravene its principles. It has been called a 'dismal science.' But to a bankrupt, arithmetic is a dismal science, while to a successful trader it may be a source of daily satisfaction. Sciences cannot be dismal or wicked; it is only men that can be joyful or desponding, or good or bad.

Having thus endeavoured to the best of my ability to protest against the idea that Economics is not a science, but a mere collection of copybook aphorisms that may be used at random like quack medicines, I should like, with your leave, to endeavour to establish its claim to come among the exact sciences by the surest test that can be applied—namely, its capability of being demonstrated by means of geometry and mathematics.

I know here that I touch on delicate ground. I fear that there are many to whom the very name of geometry is repellent. The cause of this generally is that in their youth mathematics was presented to them in a totally indigestible form. It was like a vegetable diet is to a cat—the intestine was unfitted to assimilate it. I would, however, ask such persons, if any of them be here, to exercise their sense of fairness. How many boys who are totally incapable of comprehending any poetic idea are subjected to a steady course of English poetry in the Board Schools, and of Latin poetry in the public schools. The process is painful, but it is believed to do them good. Seeing then that I am, so to speak, in the pulpit for a short time, I will ask those who dislike mathematical reasonings patiently to listen in their turn while I try to expound the doctrines of supply and demand in a geometrical form—a form familiar, I have no doubt, to many of my audience, but very useful to illustrate my present theme; a form first designed by Cournot, but subsequently developed by other workers.

I will commence by saying that for the comprehension of this method no previous acquaintance with mathematics or geometry is necessary. One can work straight from first principles, and this mode of considering the problem has been so helpful to many persons that I believe it will find favour in the eyes even of opponents. Moreover, in so far as it is correct, it certainly helps to prove the proposition with which I started, that economics may claim to have entered upon the positive stage.

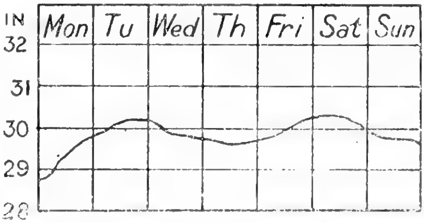

FIG. 1.

Everyone in this room is no doubt acquainted with the machine known as a barograph or registering barometer. There is one on the table. It is constructed as follows: A vertical cylinder covered with white paper revolves once in a week. A light arm is hinged on to a series of hollow elastic circular chambers, from which the air has been pumped out. As the pressure of the atmosphere varies, the air chambers dilate and contract, carrying the arm with them. The arm carries a pen which marks with a dot on the paper the height of the barometer at any time. As the paper moves the dot is drawn out into a line, which gives a continuous record of barometric variations. This diagram is a picture of one of the records.

Now, a little consideration will show what a useful diagram we have here. If we were to attempt to give the information contained in it in words we should have to say something like this. On Monday at 0 A.M. the barometer stood at 28.8 inches; during the morning of Monday it rose until about 2 P.M., when it remained stationary for three hours. It again steadily rose in the evening, until at midnight it stood at 29.9 inches (fig. 1). On Tuesday it still continued to rise till midday, when it again experienced a fall, &c., &c. Or, if the same results were put into arithmetical form, we should have quite a column of figures.

But this diagram shows us the height of the barometer at any time, and all its fluctuations. Its life history for the week and the law of its variations are obvious at a glance, in a way which no words could convey to us. So great are the advantages of this method that barographs are printed in many of the newspapers.

But the use of such curves is not confined to the registration of atmospheric pressure or temperature. They may be used for all purposes. Thus, for example, we might have a curve indicating the variation in successive years of the number of marriages per head of the population.

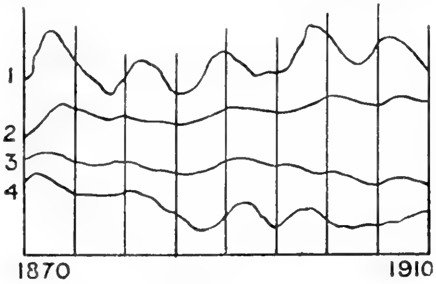

FIG. 2.

Line 1 (fig. 2) shows the proportion of marriages to population from 1870 to 1910. The advantages of this synoptic view are obvious. But they become more obvious still when we add other curves. For instance, line 4 shows the price of wheat in various years, line 3 the price of coal, line 2 the average of money wages, and line 1 the number of marriages per head of the population. A simple inspection shows that these curves rise and fall sympathetically, and proves beyond doubt that the facts they represent are causally connected.

How eloquently this diagram represents on a space that in a printed book may be three inches square, a series of relations which would take three or four pages to describe even imperfectly in words. And would any description in words enable us to follow the changes like this diagram? The diagram, in fact, plays the part that maps play in geography and when duly appreciated becomes as valuable as maps of countries.

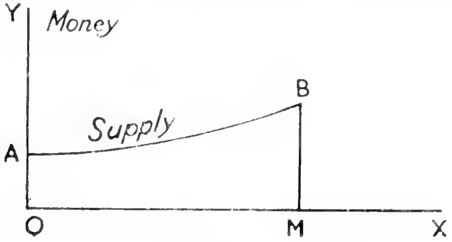

FIG. 3.

We may use similar diagrams in the exposition of economic facts. It was, however, reserved for Cournot to show that the use of curves might go further still. Not only might they be used to display statistical facts, but they might also be used to solve problems. I will endeavour to illustrate this very ingenious and interesting development.

It is a well-known fact that in certain departments of industry the cost of making an article increases in proportion to the number produced. The growth of corn is a familiar example of this principle. The principle depends on two facts: (1) that corn can be grown in some places with a less expenditure of capital and labour than in others; and (2) that the quantity of the more favourable land is limited. Whence it follows that growers will first have recourse to the most fertile land; afterwards to that which is less fertile. If we were acquainted exactly with the economics of corn-growing we could represent this state of things in any country at any given time by a curve like a barograph.

Along the line OX (fig. 3), instead of the progressive days of the week, we should mark off successive quantities of corn, and the vertical height of the curve above any given quantity would represent the price per quarter of production of that part which was produced at greatest expense. Thus, the cost of production of the first and most easily grown quarter would be, say 18*s*., of the next 18*s*. 1*d*., and so on. And it would be evident that the total cost of the whole of the wheat grown would be obtained by adding all these prices together, that is to say, by the area of the curve OMBA; for an area is but the sum of all its constituent parallel lines, just as the total of a bill for goods is an addition of all its items.

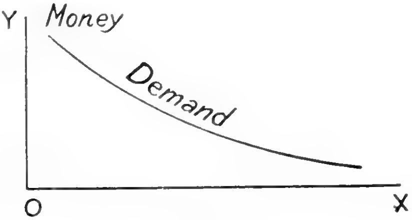

FIG. 4.

Let us now dismiss this corn-growing graph from our minds and turn to another side of the question. Let us consider the various prices which consumers would give for various quantities of corn if they could get these and no more. I do not mean the market prices of the quantities, but what might be called the famine prices, which they would give rather than not have the corn. If we draw a corn-consumers' graph it will obviously be a descending curve, for the more they can get the less they will value successive portions. In fact, if the supply of corn were unlimited the surplus would be used first to feed animals, then to consume as fuel, then as manure, and at last have to be destroyed as a nuisance.

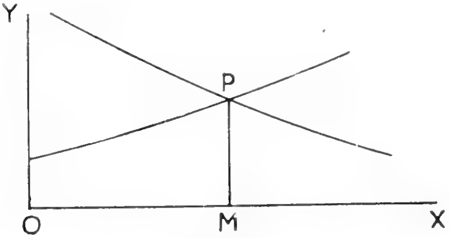

FIG. 5.

The curve would be of the form shown in fig. 4.

The contemplation of these curves of corn will no doubt suggest the question whether if we had them both we could tell what the market price would be. For it seems obvious that if we know all the conditions, both of demand and supply, we ought to be able to foretell the market price. This is the case and can be easily done. All that is necessary is to superpose the curves, as is done in fig. 5.

We then see at once that PM must represent the market price of corn per quarter at a given epoch, and OM the quantity produced in a standard time. For if more than OM were grown it could only be sold at a loss; if less the growing of corn would produce an abnormal profit, which would soon cause an expansion, so as to bring the quantity grown and sold up to the maximum that could be profitably produced.

These diagrams have therefore done more than present a state of facts; they have solved a problem, just as could be done by a pair of algebraic equations.

Moreover, other illustrations can be derived from fig. 5. By drawing the series of lines shown in fig. 6 meanings can be given to various parts of the diagram. The area NPMO represents the total price paid for the corn; the area APMO represents the total cost of growing; the area APN, which is the difference between them, represents the surplus profit obtained from the use of the better lands, or, in other words, rent; the area BPMO represents the total enjoyment the consumer derives from the corn, expressed in terms of money; and since NPMO is the price he pays for it, BNP is the surplus enjoyment he gets by obtaining corn for less than he would have given for it had there been a famine.

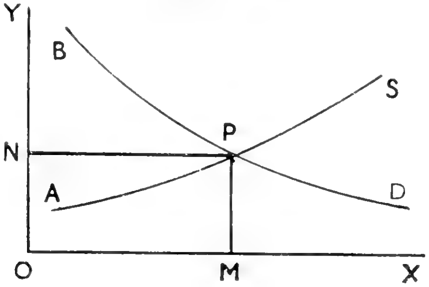

FIG. 6.

Let us go a little further. Suppose that a tax were laid on corn, and that all corn grown in a country were subject to an excise duty like that now levied on the manufacture of spirits. Suppose the duty were 5*s*. a quarter, and to simplify the problem suppose no corn came in from the outside. Then the curve APS (fig. 7) would be pushed upwards all along its length by 5*s*., and assume a position A′P′S′. And notice that the price would rise not by 5*s*., but by some amount rather less than 5*s*. For M′P′—MP must always be less than the upward movement of the curve APS. Again, the rent would be decreased, for the area N′P′A′ is less than NPA. The amount grown would decrease from OM to OM′. The proceeds of the tax would be OM′ times five shillings, and the consumers' surplus of enjoyment would have considerably diminished. This is all obvious enough if you look at the curves. But I want to ask whether,

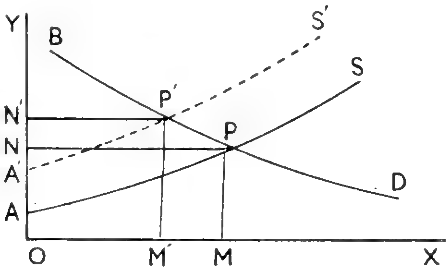

FIG. 7.

without a curve, you could have got all that so quickly by logical cogitation? I agree it could have been done by hard thought, but what a help the diagram has been in thinking it out. It is like drawing a genealogical tree when you are thinking out some complex problems of family relationship. A simple inspection of the figure also shows that an *ad valorem* tax on rent would not increase price or diminish production.

Again, what is a monopoly? A monopoly is simply a power of stopping production at a point short of that which it would reach under conditions of free production, sale and distribution. You can stop production by means of statutes regulating quantities produced, or by combinations to limit production, or to limit the supply of labour produced, or by statutes regulating the employment of capital, or by statutes fixing minima of wages, or in various other ways. If you exercise the power, then the state of things shown in fig. 8 comes into play. The quantity produced is reduced to OM′. The price rises from PM to P′M′, the surplus producers' profit (including rent) rises from ANP to AQP′N′. So that profits, interest, and wages increase, but the consumers' surplus enjoyment goes down from NPB to N′P′B. The limitation of output plays a far larger part in the regulation of prices than is commonly supposed. Those who are engaged in the manipulation of the meat trade, and the bread trade, and the petroleum industry, the supply of machinery or other articles, do not usually advertise the means they have taken to limit supply, nor do trade unions publicly descant upon the means they adopt to limit the labour of adults or apprentices. It is no part of our business here to discuss the necessity or the legitimate limits of such limitations. All that I am here to do is to show how useful diagrams are in explaining their effects.

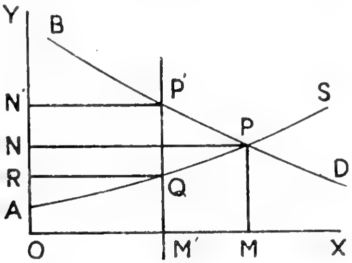

Fig. 8.

The monopoly controller seeks, of course, to make the area AQP′N′ a maximum, arranging his price just in the way a milliner would do who had to cut the biggest square she could out of a remnant of cloth. How much reduction of output and increase of price will the market bear? is the question that all monopolists present to themselves.

I could go on with these curves through a great variety of questions. They become especially interesting where applied to show the effects of tariffs upon export and import trade, but I must forbear.

My principal object has not been to introduce to the notice of the audience a subject already known to many of them, but rather to use it as an illustration of the truth that national economics is subject to laws—laws which, though complicated, are as exact and unfailing as the laws of physics, chemistry, or engineering, and which, if neglected by political engineers, will as certainly bring the State to ruin as the miscalculation of a mechanical engineer in designing a boiler, or of a civil engineer in designing a bridge. Whence, then, instead of consigning economics to Saturn, let us study it, not in a metaphysical or Aristotelian manner, using question-begging epithets, or, on the other hand, in the manner of some moderns, as for example Ruskin, by replacing reason by sentiment; but let us approach it in the spirit of positive science.

British Association for the Advancement of Science.

SECTION G: DUNDEE, 1912.

ADDRESS

TO THE

ENGINEERING SECTION

BY

PROFESSOR ARCHIBALD BARR, D.SC.,

PRESIDENT OF THE SECTION.

ONE of the great engineers of the past, Leonardo da Vinci, prefaced a collection of observations on various themes, including the Mechanical Arts, with the remark: 'Seeing that I cannot choose any subject of great utility or pleasure, because my predecessors have already taken as their own all useful and necessary themes, I will do like one who, because of his poverty, is the last to arrive at the fair, and not being able otherwise to provide himself, chooses all the things that others have already looked over and not taken, but refused as being of little value. With these despised and rejected wares—the leavings of many buyers—I will load my modest pack and therewith take my course.' These words describe, with some approach to exactitude, the position in which I find myself, and may form a fitting introduction to an Address that will be discursive rather than systematic, and perhaps more critical than constructive.

It may be less true to-day, than it was four hundred years ago, to say that all important matters concerning the existing state of the mechanical arts have been dealt with in spoken or written addresses. Each year there might be found sufficient subject-matter for a general survey of the ground that has been covered or a sketch of what lies before us. But each important advance is nowadays recorded as soon as it is made, and I do not feel that I have any special call to assume the rôle of the historian, nor can I claim any right to don the mantle of the prophet.

A President of this Section, who is not disposed to deal with the general aspects of the progress being made in the department of science allotted to us, can usually find a large enough subject for his address within the limits of that part of our wide field with which his own work has been more particularly identified, and it might be expected that I would devote my address to a discussion of the conclusions at which I have arrived during thirty-six years of practice and experience in the teaching of mechanical science. But so much has been said of late on the training of engineers, and so many divergent and even irreconcilable opinions have been expressed regarding the lines such training should follow, that I feel sure I shall be relieving the apprehensions of some of my audience if I begin by stating that I do not propose to inflict upon you a discourse on that threadbare theme. There are limits to the endurance even of those who practise a profession well calculated to inculcate the virtues of patience and forbearance.

When we have as President of the Section one who has broken new paths in the exploration of the territory assigned to us, or to whose labours the fruitfulness of some corner of the domain may be chiefly attributed, we would hardly be disposed to tolerate the omission from his Address of an account of his own special work, in investigation or in practice, and the developments to which it is leading. But while, no doubt, every worker is the chief authority on something or other, the plot he cultivates may be so restricted in area, and its products may bulk so little in the general harvest, as to form no suitable topic to engage the attention of his fellow-workers on such an occasion as this.

When an engineer leaves practice in the great, and takes to the devising and production of what are usually referred to specifically as 'scientific instruments' (though all machines and mechanical appliances may properly be classed as such), his colleagues in the profession may be disposed to look upon the change as a degeneration of species. Naturally I am not disposed to accept such a verdict. Remembering the careers of those who did most in the founding of the various branches of present-day practice, I am quite prepared to accept as applicable another phase borrowed from the language of the biologist, and to let it be called a 'reversion to a more primitive type.' But instead of dealing with the narrow branch of applied science with which my own practice is chiefly connected, I prefer to utilise the short time at my disposal to make some observations upon a larger and more general theme. The thesis which I propose to uphold may not fall very obviously within the scope of the original aims of the British Association, but it has, at least, an intimate bearing on the work of those who are concerned with the applications of mechanical science.

Tredgold's oft-quoted definition of engineering as 'the art of directing the great sources of power in nature for the use and convenience of man' may well be taken, and often has been taken, as a text upon which to hang a discourse on the importance of the profession to which many of us belong, the leading part it has played in the process of civilisation, and the dependence of the world to-day on its activities. But the words suggest failures as well as achievements, and responsibilities no less than privileges. The definition suggests that the engineer not only fails in his vocation if he does not accomplish something for the use and convenience of man, but further, that he acts contrary to the spirit of his profession if he directs the sources of power in Nature to the unuse[1] or inconvenience of man; and surely we must understand by 'man' not the engineer's immediate client but mankind in general. The works of the engineer are to be used by some people; they have to be endured by all.

Taking the highest view of our calling—and surely we do not hold that ours is in any sense a sordid or selfish vocation—the engineer fails in the fulfilment of his duty in so far as his works are detrimental to the health, or destructive to the property of the community, or in so far as they are unnecessarily offensive to any of the senses of those who are compelled to live with them. There has been too great a neglect of such considerations. The medical practitioner is held to be negligent of his duty if he acts solely in the immediate interests of his patient, and does not take due precaution to guard against the spread of disease or the offence of the community by the exhibition of unsightly forms. We should take as high a view of our responsibilities.

In his Presidential Address to the Association last year, Sir Wm. Ramsay said that the question for the engineer has come to be not 'can it be done?' but 'will it pay to do it?' The answer to this question, in respect to any particular proposal, depends on the width of view we take in answering two preliminary questions: whose interests are we to consider? and, what do we mean by paying? Of course, there are limits that must be set in answering each of these; my present contention is that these limits are usually much too narrowly drawn. A road surveyor may save a few pence or shillings to his county council by leaving a piece of newly metalled road unrolled—because the clock strikes the hour for retiring—and may thereby cause expense, amounting to pounds, it may be to hundreds of pounds, through damage to motor-cars or the laming of horses (not to speak of loss of life or limb), to the users of the road, who are, after all, the clientèle he is there to serve. Does it pay? The authorities of a city will spend large sums on the adornment of the streets with stately and ornate buildings, and on the purchase of works of art—and rightly so, though comparatively few of the citizens can appreciate or even give themselves the chance

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[1] We have no word to denote very clearly the negative of *use*, as the term is here applied; *unuse* may serve for the present.

of appreciating them—while they will tolerate or even be directly responsible for the running on these same streets of quite unnecessarily ugly and noisy tramcars, and congratulate themselves on the drawing of a paltry income from the display of hideous advertisements that are constantly before the eyes of the whole community. Does it pay thus to separate æsthetic from utilitarian demands and interests?

It is too much to assume that engineers could meet all the reasonable demands of their immediate clients without producing, at least temporarily, secondary effects that may be of inconvenience to some members of the community. Bacon, indeed, said that 'The introduction of new Inventions seemeth to be the very chief of all human actions. Inventions make all men happy without either Injury or Damage to any one single Person,' but Bacon was a philosopher, and dealt with ideals rather than with hard facts, and in his times inventors had not yet begun to dominate all the elements of our physical environment. Had he lived to-day beside one of our country roads he might have had something to say, in another key, regarding motor-cars and dust; or had his lot been cast in the proximity of a great centre of industry he might have modified his conviction of the universality of the benefits conferred by the inventor. He might even have been disposed to agree with a literary man of to-day who is reported as asserting that 'The universal and blatant intrusion of Science into our lives has resulted in a total disappearance of repose.' Isolated and unqualified statements such as those I have quoted are like proverbs—you can always find two that are directly opposed. The truth lies about midway between these extremes, or rather there are aspects of the facts in regard to which one is an approach to the truth, and aspects in which the other has some justification. Our aim should be to make Bacon's dictum have more of truth and Mr. Stephen Coleridge's assertion have less foundation in fact. And the outlook seems to me to be a very hopeful one, though to be able to take an altogether favourable view of the tendencies of the present time, one must be an optimist of the true order—'One who can scent the harvest while the snow is on the ground.'

When we examine into the immediate causes of the injuries and inconveniences that result from our activities we find that they are due in all, or almost all, cases to failures rather than to successes. The more completely the engineer achieves the primary end of his work the less is the damage or injury that can be laid to his charge. If it can be shown that this is a very general law, as I think it can be, we may look forward to the elimination, as a direct result of progress in the mechanical arts, of the nuisances and inconveniences for which, in some measure at least, we must accept responsibility. And not only so, but the converse will be equally true—the more we keep in view the removal or avoidance of anything that can cause offence, the more rapidly we shall advance in the attainment of the primary ends at which we aim. Consider, by way of example, the nuisance to which I have referred, and of which we hear so much—the raising of dust by motor-cars. I shall not discuss the debated question as to how far the motor-car produces dust, or only distributes it, nor shall I deal in detail with the possible remedies. We hope to have a paper on the subject at this meeting from one of our leading authorities. For my present purpose it suffices to point out that it is no part of the function of a road surface to fritter itself down into dust under traffic of any kind. The ideal road would be one that would not wear at all, and the nearer we approach this ideal of a permanent road surface, the less will be the inconvenience caused, not only to those responsible for the upkeep of the road, but to the general public. And conversely, the more attention we give to the devising of a dustless road the more rapid will be our advance towards the provision of one best suited for all the purposes which a road is intended to serve. We had dusty roads before the motor-car came into being, but the demand that is being forced upon the engineer to eliminate this nuisance is leading to an improvement of our roads for all users. The inventors of the automobile will yet merit the thanks even of those who, bemoaning the blatant intrusion of science into our lives, may discard the railway train and the motor-car and take to the stage-coach of their grandfathers with a view to the recovery of some of the lost repose.

Again, the combustion of fuel does little harm to anyone; it is the imperfection of the combustion that is the main cause, almost the sole cause, of injury to health, to property, and to the amenity of populous centres. Of course one knows that smokeless combustion is not necessarily, nor always, the most economical, but that is only because we have not yet learned how to use fuel in anything like a perfect manner. But all the tendencies at the present time are towards improvement, and the more attention we pay to the elimination of the smoke nuisance the more rapid will be our progress in the economical use of one of the most valuable of our inheritances. It is therefore clearly the duty of every engineer who has to do with power or heat production—for the credit of his profession and even in the interests of his immediate clients—to consider the use and convenience of all who can be affected by the work for which he is responsible. The time is not far distant when the direct burning of bituminous coal in open grates will be looked upon as not only a source of serious harm but as a culpably wasteful practice. Great progress has been made in processes for the partial distillation of coal by which a free burning and quite smokeless fuel is prepared and valuable by-products (so-called) are conserved. If all engineers concerned with the design and application of plants in which coal is used had a due sense of their responsibilities to the community, progress would have been, and would to-day be, much more rapid; and economies would be effected that would, in themselves, amply justify the application of more scientific methods of utilising the constituents of a very complex material, which we are too apt to look upon as merely a convenient source of heat—plentiful enough and cheap enough, as yet, to be used in a most wasteful manner. It will not be to the credit of our profession if it should require restrictive legislation not only to prevent a gross interference with the health and comfort of the community and the amenities of our centres of industry or of population, but to effect economies in the utilisation of the chief of the sources of power which it is our function to direct to the best advantage of all concerned.

In other directions also we see that progress towards economy is leading to a reduction, and possibly to the entire elimination, of all the nuisances associated with the older methods of power and heat production. The great improvements that have recently been made in producer plants and gas engines have rendered out of date, as regards economy, at least the smaller sizes of steam plants which are so fruitful a source of injury and inconvenience to the community; and we now have engines of the Diesel, and the so-called semi-Diesel, types that can utilise natural oils, and oils obtained in the distillation or partial distillation of coal, not only with an efficiency hitherto unattained in heat-engines, but 'without injury or damage to any one single person'—except possibly the maker of inferior[2] plants.

Present indications point to the coming of a time, in the near future, when the power and heat required for industrial and domestic purposes will be distributed electrically, in a perfectly inoffensive manner, from large central stations; and even at these stations there will be no pollution of the atmosphere that could give the most sensitive of critics any just grounds of complaint against the intrusion of science into our lives. In his Presidential Address to the Institution of Electrical Engineers in November 1910, Mr. Ferranti dealt in a most masterly way with this, which is undoubtedly the greatest of the many schemes at present before the engineering profession. That address reads like a chapter from a Romance of Utopia, but unlike most of the forecasts that have been presented to us of ideal conditions in a world of the future, the system which Mr. Ferranti sketches out, and advocates with so much knowledge and convincing argument, does not depend for its reasonableness on the postulation of a perfected humanity. It would not only provide vastly improved conditions of life for the community as a whole, but it would satisfy the more selfish aims of the users of power and the makers of machinery, by increasing the economy of production and stimulating the demand for mechanical appliances. No doubt there may be some who will hold that to commend any worthy scheme, to those who might carry it out, by an appeal to their selfish interests is an altogether immoral kind of argument. I do not think so. Advancement of the race through

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[2] My typist in transcribing a rather illegible draft of this passage substituted for the adjective I have here used the less restrained, but perhaps equally appropriate one, 'infernal,' but I noticed this in time to amend the emendation. I had no intention to speak so candidly of any of the works of members of my own profession.

benefits to the individual is, at least, not inconsistent with nature's method of securing progress. However much we may desire to develop a purely altruistic spirit in men of all classes we must meantime make the best of human nature as it is, and recognise that the rapidity of our progress toward better conditions of life will be in proportion to the advantages that each advance can promise to those who would be immediately concerned in its realisation.

It is just a hundred years since passengers were first carried on the Clyde in a mechanically propelled ship, and to-day—when they are not too completely obscured by smoke—we can see the successors of the 'Comet' plying on that river with power plants of greatly superior overall efficiency but showing little advance in regard to the combustion of the fuel. Had the emission of smoke from river craft been prohibited years ago, there is little doubt that engineers would have let few days pass without arriving at some solution of the problem of inoffensive power production, and the demand for economy would have looked after itself. How much better it would be were engineers to take the wider view of their duties and responsibilities to which I have referred, and realise that they are acting contrary to the true spirit of their profession when they produce appliances that pollute the atmosphere for miles around to the hurt and inconvenience of those whose 'use' they are intended to serve. But this year a ship has left the Clyde that we hope may be the forerunner of a new race which will attain a higher efficiency that any of the direct descendants of the 'Comet,' and that will ply their trade without inconvenience to man or beast, who can claim some right to be permitted to enjoy an unpolluted atmosphere and the measure of sunshine which Nature—sparingly enough in those regions—intended to provide.

But there are injuries which we may inflict upon the community other than those to health and physical comfort. Every one, even the least cultured, has some sense of the beautiful and the comely, and is affected by the aspects of his environment more than he himself can realise. The engineer, then, whose works needlessly offend even the most fastidious taste is acting contrary to the spirit of his profession, at its best. There has been far too great a disregard of æsthetic considerations in the everyday work of the engineer—we usually take a too exclusively utilitarian view of our calling. We should not be prepared to accept, as referring to the arts we practise at their best, the distinction drawn by a philosophical writer between 'the *mechanical* arts which can be efficiently exercised by mere trained habit, rote, or calculation,' and 'the fine arts which have to be exercised by a higher order of powers.'[3] And I think it can be shown that a greater regard for artistic merit in our designs would not necessarily lead to extravagance, but, in many cases, would conduce to economy and efficiency. It is at least true—and much less than the whole truth—that greater artistic merit than is commonly found in our works could be attained with no sacrifice of structural fitness, or of suitability for the purposes they are designed to serve.

There was a time when engineers made desperate attempts to secure artistic effects by the embellishment (?) of their productions with features which they believed to be ornamental. Fortunately the standard of taste has risen above and beyond this practice in the case of most members of our profession and most of our clients. We are all familiar with illustrations of philosophical instruments, and other mechanical contrivances, of the early times, that vied in lavishness of adornment—though not in artistic merit—with those wonderful astronomical appliances that were carried—as trophies of war!—from Pekin to Sans Souci. Many of us can remember a time when the practice had not altogether disappeared, even in the design of steam engines, lathes, and other products of the mechanical engineer's workshop. I well remember in my apprenticeship days, the building of a beam engine that was a triumph of ingenuity in the misapplication of decorative features. In place of the mildly ornamented pillars and entablature of Watt's design, there was provided, for the support of the journals of the beam, a pair of A frames constructed in the form of elaborately moulded Gothic arches flanked by lesser arches on each side, while the beam itself, and many other parts, were plentifully provided with even less

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[3] *Enc. Brit.*, eleventh edition, article 'Art.'

appropriate embellishments borrowed from the art of the stone-mason. It is some consolation to remember that the clients for whom the engine was built were not of this country, and that the design itself was not a product of the workshop that was favoured with the contract to produce this amazing piece of cast-iron architecture. We have all seen wrought-iron bridges the inattractive features of which were concealed by cast-iron masks—in the form of panelling, or of sham pillars and arches with no visible means of support—that not only have no connection with the structural scheme, but suggest types of construction that could not, by any possibility, meet the requirements. Structures of this kind remind one of the pudding which the White Knight (with good reason when we remember the characteristics of his genius) considered the cleverest of his many inventions. It began, he explained, with blotting-paper, and when Alice ventured to express the opinion that that would not be very nice, he assured her that though it might not be very nice *alone* she had no idea what a difference it made mixing it with other things—such as gunpowder and sealing-wax.

There are, and must always be, wide differences of opinion regarding what is good or bad in matters of taste, but we may go so far in generalisation as to say that we can admire the association of elements we *know* to be incongruous only in compositions that are intended to be humorous. 'All human excellence has its basis in reason and propriety; and the mind, to be interested to any efficient purpose, must neither be distracted nor confused.'[4] But to be able to judge of the propriety or reasonableness of any composition we must have some knowledge of the essential qualities and relationships of its component parts, and excellence cannot depend upon an appeal to ignorance. We can quite imagine that the White Knight's pudding would appeal as an admirable and most ingenious concoction to one who lacked a knowledge of the dietetic value of blotting-paper and was willing to take for granted the excellence of gunpowder as a spice and of sealing-wax as a flavouring. No artist would be bold enough to include a polar bear or a walrus in the composition of a picture of the African desert, nor be prepared to consider as a legitimate exercise of the artistic imagination the depicting an Arab and his camel wending their weary way across the Arctic snows. He would recognise the incongruity, and might even realise that it is only a lack of imagination or of true inventive power that could lead anyone to resort to such measures for the securing of a desired colour scheme. These are lengths to which even artists will not go in the arrangement of elements in a composition. But an artist *will* secure a colour scheme at which he aims by the introduction into his landscape of a rainbow in an impossible position, or of impossible form or dimensions, or with colours arranged according to his own fancy, though in this there is a much more essential unreasonableness. A polar bear might be transported to the desert, and an Arab might conceivably find his way to the regions of snow and ice, but a rainbow cannot wander from the place assigned to it by Nature, nor can it have other than the ordained form or dimensions or sequence of colours. No artist would paint a figure holding a candle and make the light fall on the side of the face remote from the source; but he will, and usually does, paint the moon illuminated on the side remote from the sun. Why? Simply because he has not before his mind the essential absurdity of the scheme, if indeed he knows why the moon shines. Artists who deal with nature in any of its aspects, may be commended to 'mark, learn, and inwardly digest' Whistler's definition of their calling: 'Nature contains the elements in colour and form of all pictures . . . but the artist is born to pick and choose, and group with science, these elements, that the result may be beautiful.' Whether or not we are to understand that Whistler intended to include an accurate knowledge of physical facts and phenomena in what he calls *science*, he cannot have meant anything less than *sense*.

So in regard to the arts of construction, we may say that Mechanical Science provides the elements of all structures, and the craftsman—be he called engineer or architect—is born to pick and choose, and group with science, these elements, that the result may be useful and not devoid of grace.

The only valid excuse for such departures from the fit and rational in painting

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[4] Mr. Duppa's 'Life of Michelangelo.'

or in structural design, as those which I have instanced, is ignorance on the part of the designer of the nature of the elements he employs, or a lack of skill to devise a possible or reasonable arrangement of details that will secure the general effect he desires.

It may almost savour of sacrilege to quote, in this connection, from the writings of that 'Wild, wilful, fancy's child' the story of whose eight short years of life and literary work Dr. John Brown has given in his charming 'Pet Marjorie' —a record of perhaps the shortest human life that has formed the subject of a biography. But the lines are too pertinent to my purpose to be withheld, and the frankness of the confessions they contain, of a childlike limitation of artistic power, may be commended to those who practise either the fine arts or the arts of construction, and feel compelled to 'trust to their imagination for their facts,' or to resort to the association of incompatible details for lack of knowledge, or of ability to attain their ends by more reasonable means.

Marjorie writes of the death of James II. :—

'He was killed by a common splinter,
  Quite in the middle of the Winter;
Perhaps it was not at that time,
  But I could find no other rhyme!'

'Quite in the middle of the winter,' describes August 3, 1460 A.D., with no wider licence than we find assumed in the works of more experienced, if less candid, artists and craftsmen. Again in her sonnet to a monkey—written, we must remember, when she was six or seven years of age—she acknowledges the compelling power of an artistic aim :—

'His nose's cast is of the Roman:
  He is a very pretty woman.
I could not get a rhyme for Roman
  So was obliged to call him woman.'

It may seem that I have wandered widely from my text : those who found discourses on texts usually do! But there is, or ought to be, a closer connection than is usually recognised between the work of the engineer and that of those to whom we usually restrict the title of artist. There was no great gulf fixed between the fine arts and the utilitarian arts in earlier times. Some at least of those to whom we owe the greatest advances in the fine arts were eminent also in the arts of construction. We may claim such men as Michelangelo, Raphael, and Leonardo da Vinci as masters in the arts of construction as well as in those with which their names are usually associated. The separation of the beautiful and the useful is quite a modern vice. But much that I have ventured to say in the digression—if such it be—is applicable, with little or no alteration of terms, to the work of our own profession. The architect or engineer who, for the sake of effect, fills the space between the flanges of a beam or girder with slabs of stone, or cast-iron pillars and arches, that could not fulfil the function of a web, exhibits just the same lack of skill as Pet Marjorie owns up to—shall I say?—like a *man*. Such practices have no 'basis in reason and propriety,' and the employment of such 'decorative features' is certainly not a 'grouping of elements with science.' It is said that 'The highest art is to conceal art'; the lowest in matters pertaining to our profession is to conceal ill-devised construction with false and senseless masks. But what I have said has, I think, a sufficiently obvious bearing on the mechanical arts—I need not further point the moral.

There is an old maxim to the effect that 'the designer should ornament his construction and not construct his ornament.' This is an admirable rule so far as it goes, but it should be subordinated to a higher rule, that he should ornament his structure only if he lacks the skill to make it beautiful in itself. A structure of any kind that is intended to serve a useful end should have the beauty of appropriateness for the purpose it is to serve. It should tell the truth, and nothing but the truth, and if its character be such that it can be permitted to tell the whole truth, so much the better. It should be beautiful in the sense in which we commonly use the term with respect to a machine—we call a mechanical device beautiful only if it strikes us as accomplishing the end for which it is designed in the simplest and most direct way. Our works—like the highest creations in nature—should be beautiful and not beautified. 'Beautified' should be considered a vile phrase when applied to a work of construction, no less than when used to characterise a fair Ophelia. Artists accept the human form, at its best, as the highest embodiment of grace and beauty, but there is not a curve in the figure that is not the contour of some structural detail that is there for a definite purpose. The practice of resorting to extraneous adornments to minimise crudities of structural scheme had its rise—if I mistake not—in the comparatively recent times when culture and taste were at their lowest. It is specially characteristic not only of earlier times, but of the earlier stages of the design of any particular product. It has already disappeared in some cases and will continue to disappear from the practice of the arts of construction as skill and taste develop. I have already alluded to the abandonment of ornament in the design of machines, and I think there can be no one, with any sense of the fit and pleasing, who does not approve this change in practice. The stage coach and horses of former times were lavishly decorated—the carriage of to-day is more graceful and pleasing in virtue of the simple elegance of its lines. In the best domestic architecture of to-day we see the same tendency to trust for effect, more and more, to an artistic grouping of the lines and masses of essential parts and the gradual abandonment of purely decorative features, without and within. There was a time when the hulls and riggings and sails of ships were lavishly ornamented; now even the figurehead—the last remnant of barbaric taste—has disappeared; and do we not find in a full-rigged ship of to-day (or yesterday, perhaps one should say) a grace and dignity that no extraneous embellishments would enhance? From the racing yacht the designer has been forced, by the demand for efficiency, to cast off every weight and the adornments that so beset the craft of earlier times, with the result that there is left only a beautifully modelled hull, plain masts, and broad sweeps of canvas, and we can hardly imagine any more beautiful or graceful product of the constructive arts. These examples will serve to illustrate the contention that the attainment of the highest efficiency brings with it the greatest artistic merit. But in the development of the yacht of to-day, through many stages, the designer has been forced, from time to time, to strive to combine grace with efficiency. Selection on the part of clients must have eliminated ungraceful forms when more beautiful ones could be found, and therefore the advance has been rapid. I think I may appeal to this illustration to support the further contention that advance in efficiency may be helped and not hindered by keeping in view an æsthetic as well as a utilitarian aim. Further illustrations will occur to anyone who has studied the development of design of structures or machines.

It is a matter of constant remark, and with justice, that steel bridges, as a class, are much less pleasing to the eye than those of stone. The reasons for the contrast in artistic merit are not far to seek. The building of stone bridges is an ancient art, and survival of the fittest, and selection—even with little creative skill on the part of the designers—would have led to the development of types having, of necessity, at least the elegance of fitness. But further, this art has come down through the times to which I have referred when artistic and utilitarian aims had not yet been divorced, in the practice of the crafts; and further still, the practice of building in stone has been in the hands of architects, as well as of engineers, and architects are expected to be artists, and are trained as such. On the other hand, construction in steel is a very modern art, and it has been in the hands of engineers who usually neglect, if they do not despise, the study of the fine arts. But why have architects, with their artistic training, not succeeded in producing structures in steel as admirably as those they design in stone? Partly, no doubt, because they are hampered by tradition. They have not yet fully realised the difference in spirit that must characterise fit designs in the newer and the older materials. No one can be an artist in any material, the possibilities and limitations of which he has not fully mastered. Again—if a common engineer may venture the criticism—the architect, as a rule, has not sufficiently mastered the *science* of construction, and has been too much addicted to taking the easy course of adopting a decorated treatment instead of striving to secure elegance of structural scheme as such; and decoration, at least on anything like traditional lines, is wholly incompatible with the best possibilities of steel as a structural material. Progress is being made in the art of designing efficient and graceful structures in metal, but the best results can only be attained by a designer who has a thorough scientific and technical knowledge of the properties of steel and the processes of its manipulation, on the one hand, and cultured artistic sense and capacity on the other. These should not be considered as appropriate equipments for separate professions.

There are many, however, who have a rooted conviction that structures in steel can never be so beautiful as those in stone. This I believe to be altogether wrong. It arises partly from the crudity of design that characterises most of the steel structures that have yet been erected, and partly from preconceived notions as to what is fitting in proportions and massiveness. We can quite imagine that a native of the Congo region whose notions of the proportions suitable and comely for a quadruped were founded on his familiarity with the hippopotamus would, at first sight, consider the racehorse sadly lacking in substance and solidity, but, in time, he might come to recognise some measure of gracefulness in a creature that has been developed to meet requirements that hitherto he had not fully considered.

Mr. Wells has said in his 'New Utopia,' 'the world still does not dream of the things that will be done with thought and steel when the engineer is sufficiently educated to be an artist, and the artistic intelligence has been quickened to the accomplishment of an engineer.' But we need not postpone, till the advent of a complete Utopia, the full realisation of our duty to practise our profession as far as in us lies, with due regard for the material interests and the æsthetic susceptibilities of all who can be affected by the works for which we are responsible.

British Association for the Advancement of Science.

SECTION H: DUNDEE, 1912.

ADDRESS

TO THE

ANTHROPOLOGICAL SECTION

BY

PROFESSOR G. ELLIOT SMITH, M.A., M.D., CH.M., F.R.S.,

PRESIDENT OF THE SECTION.

IN a recent address Lord Morley referred to 'evolution' as 'the most overworked word in all the language of the day'; nevertheless, he was constrained to admit that, even when discussing such a theme as history and modern politics, 'we cannot do without it.' But to us in this Section, concerned as we are with the problems of man's nature and the gradual emergence of human structure, customs, and institutions, the facts of evolution form the very fabric the threads of which we are endeavouring to disentangle; and in such studies ideas of evolution find more obvious expression than most of us can detect in modern politics. In such circumstances we are peculiarly liable to the risk of 'overworking' not only the word evolution, but also the application of the idea of evolution to the material of our investigations.

My predecessor in the office of President of this Section last year uttered a protest against the tendency, to which British anthropologists of the present generation seem to be peculiarly prone, to read evolutionary ideas into many events in Man's history and the spread of his knowledge and culture in which careful investigation can detect no indubitable trace of any such influences having been at work.

I need offer no apology for repeating and emphasising some of the points brought forward in Dr. Rivers' deeply instructive Address; for his lucid and convincing account of the circumstances that had compelled him to change his attitude toward the main problems of the history of human society in Melanesia first brought home to me the fact, which I had not clearly realised until then, that in my own experience, working in a very different domain of anthropology on the opposite side of the world, I had passed through phases precisely analogous to those described so graphically by Dr. Rivers. He told us that in his first attempts to trace out 'the evolution of custom and institution' he started from the assumption that 'where similarities are found in different parts of the world they are due to independent origin and development, which in turn is ascribed to the fundamental similarity of the workings of the human mind all over the world, so that, given similar conditions, similar customs and institutions will come into existence and develop on the same lines.' But as he became more familiar with the materials of his research he found that such an attitude would not admit of an adequate explanation of the facts, and he was forced to confess that he 'had ignored considerations arising from racial mixture and the blending of cultures.'

I recall these statements to your recollection now, not merely for the purpose of emphasising the far-reaching significance of an Address which is certain to be looked back upon as one of the most distinctive and influential utterances from this presidential chair; nor yet with the object of telling you how, in the course of my investigations upon the history of the people in the Nile Valley,[1] I also started out to search for evidences of evolution, but gradually came to realise that the facts of racial admixture and the blending of cultures were far more obtrusive and significant. My intention is rather to investigate the domain of anthropology in which unequivocal evolutionary factors have played, and are playing, a definite rôle; I refer to the study of Man's genealogy and the forces that determined the precise line of development his ancestors pursued and ultimately fashioned man himself.

I suppose it is inevitable in these days that one trained in biological ways of thought should approach the problems of anthropology with the idea of evolution as his guiding principle; but the conviction must be reached sooner or later, by everyone who conscientiously, and with an open mind, seeks to answer most of the questions relating to Man's history and achievements—certainly the chapters in that history which come within the scope of the last sixty centuries—that evolution yields a surprisingly small contribution to the solution of the difficulties which present themselves. Most of the factors that call for investigation concerning the history of Man and his works are unquestionably the direct effects of migrations and the intermingling of races and cultures.

But I would not have you misunderstand my meaning. The current of evolution is running at least as strongly and moving mankind onward no less quickly than it did when it brought his ancestors to human rank; it is as potent as ever to alter his structure, even though the way in which 'selection' has been modified has deflected the stream. Those who imagine that the strength and influence of evolutionary forces have waned forget the enormous length of time it has taken to fashion the human body. It has been amply sufficient for slowly developing changes, such as are taking place at present, to have transformed an Ape into human form. Environment, however it may act, whether directly or indirectly, is still helping to shape the human form, and is affecting the development of Man's customs and achievements at least as powerfully as, if not more so than, ever before. The effects of selection—not only what Darwin understood by the term 'sexual' selection, but also what we have learned to call 'organic' and 'social' selection—are certainly emphasized by the heightened powers of discrimination which the intelligence and the fashions of civilised Man create. We have every reason for believing, therefore, that the forces of evolution are still operating with undiminished vigour; but all the evidence that I have to bring before you goes to show that such forces act as a rule very slowly and imperceptibly, and need vast spans of time for the production of their effects. In studying Man's past history we find no clear evidence, or even suggestion, of any such sudden jumps or 'mutations' as students of other branches of biology are calling to their aid to solve their difficulties, as a sort of magic carpet to convey them across awkward chasms in their evolutionary route.

The Negro was quite as definitely negroid when we first meet with his sixty-centuries-old remains as he is now: the narrow-headed brunet of small stature, who has dwelt around the shores of the Mediterranean since the dawn of history, was almost, if not quite, as definitely differentiated from the round-headed Armenoid of Western Asia at the end of the Stone Age as are their modern representatives; and all the millennia of exposure of their scattered descendants to vastly different climates and conditions of life have produced amazingly little effect upon their physical characteristics. Further evidence may perhaps lend some measure of confirmation to the contention of Professor Boas,[2] that the uprooting of European people and their transference to America leads to an immediate effect upon the physical characters of such of their progeny as may happen to be born in the new environment; but while not denying the possibility that such an influence may be exerted, no anthropologist, however strongly he may be inclined to accept evidence in support of such a contention, can seriously take Professor Boas's data or the inferences he draws from them at his valuation. Professor Boas would have us believe that the forces of environment which produce little or no effect upon the growing child (of aliens in New York), who happened to be born in Europe immediately before his parents emigrated,

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[1] 'The Ancient Egyptians,' 1911.

[2] 'Changes in Bodily Form of Descendants of Immigrants,' *United States Government Reports*, 1910, 1911.

can, nevertheless, so influence the germ plasm of his parents that their American-born progeny will be instantaneously modified. I know that it is easy to find parallels from biology, and especially from botany, to justify such an influence on the germ-plasm in the case of sudden changes of temperature, climate, soil, and other conditions of life; but until Professor Boas has dealt more exhaustively than he has, even in his second report, with the possibility of racial admixture as the obvious explanation of his statistics, and excluded it definitely, anthropologists must continue to view his data and the inferences from them with the most profound suspicion.

I for one am quite prepared, if not to admit, at any rate to recognise the possibility that a new environment might produce immediate changes in the physical characteristics of the human body. The multiplicity of internal secretions that recent research in physiology has shown to influence the growth of the various tissues of the body, and the immediate effects of a slight increase or diminution in the activity of the glands providing such secretions, which *may* perhaps be caused by new dietetic, climatic, or other conditions, are quite sufficient to suggest that the observer must keep his mind open impartially to view new observations concerning the immediate effect of a new environment on individuals, such as might conceivably afford a handle for the forces of natural selections to seize hold of and produce changes in the progeny of the altered parents; but there is a vast difference between admitting the *possibility* and recognising the *proof* of such an hypothesis; and I am still entirely sceptical of Professor Boas's so-called proofs. One is certainly not the more disposed to accept such hypotheses when the attempt is made to bolster them up with a tissue of statements intended to minimise or even deny those physical, mental, and moral distinctions between different races of mankind,[3] the results of many millennia of years of differentiation, the reality of which is substantiated by the whole history of the world and the experience of those who have watched the intercourse of the various peoples.

Difference of race implies a real and deep-rooted distinction in physical, mental, and moral qualities; and the contrasts in the achievements of the various peoples cannot be explained away by lack of opportunities, in face of the patent fact that among the most backward races of the present day are some that first came into contact with, or even were the founders of, civilisation, and were most favourably placed for acquiring culture and material supremacy.

It is not, however, with such contentious matters as the precise mode of operation of evolution at the present day that I propose to deal; nor yet with the discussion of when and how the races of mankind became specialised and differentiated the one from the other. It is the much older story of the origin of Man himself and the first glimmerings of human characteristics amidst even the remotest of his ancestors to which I invite you to give some consideration to-day.

In a recently published book[4] the statement is made that 'the uncertainties as to Man's pedigree and antiquity are still great, and it is undeniably difficult to discover the factors in his emergence and ascent.' There is undoubtedly the widest divergence of opinion as to the precise pedigree; nevertheless, there seems to me to be ample evidence now available to justify us sketching the genealogy of Man and confidently drawing up his pedigree as far back as Eocene times—a matter of a million years or so—with at least as much certainty of detail and completeness as in the case of any other recent mammal; and if all the factors in his emergence are not yet known, there is one unquestionable, tangible factor that we can seize hold of and examine—the steady and uniform development of the brain along a well-defined course throughout the Primates right up to Man—which must give us the fundamental reason for 'Man's emergence and ascent,' whatever other factors may contribute toward that consummation.

We have this advantage over most of our predecessors in approaching the consideration of the problems of the gradual emergence of human traits from the uncouth simian features of our ancestors, that the main contention, the fact of the 'Descent of Man,' is now generally admitted; and it is no part of our task to discuss more or less irrelevant side issues, born of prejudice, superstition, and

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[3] 'The Mind of Primitive Man,' 1911.

[4] J. A. Thomson and P. Geddes, 'Evolution,' 1912, p. 102.

ignorance. Moreover, we are able to command a vast army of newly discovered facts and attack the difficulties at issue in ways that were not open to those who went before us.

In these circumstances it seems to me that I may perform a useful service by setting forth the views which my studies—both of the facts of Nature and of the writings of contemporary biologists—have led me to adopt concerning Man's genealogy, from the remote period when his ancestral line branched off from those of the other mammalian orders, and to make the attempt to appreciate the nature of the factors that determined each upward step in his march toward the supreme position among intelligent beings.

In spite of all the precise knowledge, not only of the structure and functions, but even of the 'blood relationships,' using that term in its literal as well as its metaphorical sense, of the Apes and Men, it is surprising that there should be so little agreement among leading authorities as to the precise line of Man's ancestry. Biologists do not seem to be exempt from that spirit of unrest which is abroad at the present time; and there seems to be a strange reluctance to admit the obvious. Some zoologists try to persuade us that Man is not nearly related to the Old-World Apes, and seek for closer affinities with the New-World Apes, or even the Lemurs; others, again, exclude the Lemurs altogether from the Order Primates, or, on the other hand, would eliminate the Platyrrhine Monkeys from Man's genealogy; yet others claim a diphyletic origin for Man from the Apes. I do not propose to enter into the discussion of these problems[5] here, but rather, taking for granted the genealogical line which I, in agreement with many zoologists, believe to be a close approximation to the real line of descent,[6] attempt to find an explanation of how each of the more significant advances was brought about in the course of Man's evolution.

This theme, in one form or another, has often formed the subject of presidential addresses before this Section. The last time the Association met in Scotland the late Professor Cunningham, whose death since then we all so deeply deplore, presided over this Section, and dealt with[7] certain highly technical aspects of the subject in his own characteristically lucid manner. But though only eleven years have elapsed since then, the additions to our knowledge of comparative anatomy, especially that relating to the brain, have been so great and so fundamental that we can regard the problems discussed by him from a very different and, I think, more intimate and instructive point of view.

We no longer look, as he did, to that small patch of cortex in the left cerebral hemisphere, which has long been supposed to be the storehouse of the motor memories of articulate speech, as being the likeliest region to supply the key to the secret of Man's mental pre-eminence. Since Pierre Marie[8] expressed his disbelief in any such so-called motor speech centre, many physiologists and physicians have become sceptical as to the ability of the left inferior frontal convolution to control the mere muscular movements that produce speech. But even if we grant the basal contention upon which Professor Cunningham's argument was founded (as I think we are justified in doing, in spite of the writings of Marie and his followers), we cannot be said to explain the evolution of the modern steamship when we describe the steering-wheel which directs its course.

Speech is a manifestation of the intelligence that depends upon the activity and co-operation of most parts of a large and highly organised cerebral cortex, acting as a whole; and what we have to discover is not so much how a particular series of groups of muscles bring about the mere motor acts of phonation and articulation, but what is the nature of the living mechanism which enables the mind to appreciate the multiplicity of sounds and other sensations, and to record such sensation-factors so that they may be recalled in memory and brought into relation as percepts with other sensation-factors in conscious experience, and made use of as guides to the realisation of the nature of causes and effects in the

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[5] A critical examination of some of these views will be found in Prof. Sollas' Presidential Address to the Geological Society of London, February 1910.

[6] The simian stages of this genealogy are admirably expressed in a diagram made by Prof. Arthur Keith for his Hunterian lectures 'On Certain Phases in the Evolution of Man,' *Brit. Med. Journ.*, April 6, 1912, p. 788.

[7] *Brit. Assoc. Report*, 1901, p. 776.

[8] 'L'Aphasie,' *Semaine Médicale*, 1906, p. 241.

events taking place around the individual : it is the evolution of the cortex which makes possible this wide association of sounds and ideas, and this learning by experience that is important, rather than the mere instrument which regulates the emission of sounds that we learn to associate each with its appropriate idea. Charles Darwin recognised this fact quite definitely, and expressed it with his usual directness,[9] when he said, 'It is not the mere articulation which is our distinguishing character, for parrots and other birds possess this power. Nor is it the mere capacity of connecting definite sounds with definite ideas; for it is certain that some parrots which have been taught to speak connect unerringly words with things and persons with events. The lower animals differ from Man solely in his almost infinitely larger power of associating together the most diversified sounds and ideas; and this obviously depends on the high development of his mental powers.'

What I propose to attempt is to put into serial order those vertebrates which we have reason to believe are the nearest relatives to Man's ancestors now available for examination, and to determine what outstanding changes in the structure of the cerebral hemispheres have taken place at each upward step that may help to explain the gradual acquirement of the distinctively human mental faculties, which, by immeasurably increasing the power of adaptation to varying circumstances and modifying the process of sexual selection, have made Man what he is at present.

A numerous band of zoologists and psychologists have collected a large mass of accurate information relating to the behaviour and intelligence of animals, with which Darwin, Huxley, Tylor, and many recent writers have familiarised students of anthropology. It is not my intention to attempt to summarise or deal directly with these problems of comparative psychology.

But there has been accumulating during the last few years, thanks largely to the efforts of Oskar and Cécile Vogt, Karl Brodmann, and many others,[10] the material which will eventually enable us to correlate these differences in habits and behaviour in different mammals with structural differences in their brains; and in this Address I propose to discuss with you what light such investigations can throw upon the problems of Man's origin and the evolution of the instrument of his intelligence. If in my attempts to interpret the significance of the progressive modifications, which we can demonstrate in the brains of the series of mammals I have selected, I shall give utterance to the crudest psychological conceptions, you must not credit this entirely to my ignorance of the teachings of psychology, but partly to the fact that so far we have been able only dimly to realise the nature of the processes that are taking place in the cerebral cortex; and it is safer to use the crudest forms of expression, so as not to delude you into the belief, which more precise terms might suggest, that our knowledge had yet attained to any degree of completeness or exactitude.

We know something of the differences in behaviour of a series of Primates and of the variations in their responses to electrical stimulation of their brains. How far can these differences be correlated with structural distinctions in their brains? And how far can such information be used to explain the evolution of our own brain, which supplies to each of us the only real knowledge of consciousness we possess? These are the questions that we are striving to answer.

The class Mammalia, to which Man belongs, is distinguished from all other vertebrates by the size and high development of the brain, and by the fact that, to a much greater degree than in any other class, a progressive increase in the size of the brain, and more especially of the cerebral cortex, becomes imperative in each successive epoch if its possessor is to maintain itself in free and open competition with its fellows. It was the advance in brain structure in far greater measure than anything else that determined the evolution of mammals;[11] and it

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[9] 'The Descent of Man,' p. 130 in the 1901 reprint.

[10] C. and O. Vogt, 'Zur Kenntnis der elektrisch erregbaren Hirnrinden-Gebiete bei den Saugetieren,' *Journ. f. Psych. u. Neur.*, Bd. VIII., 1907, p. 280. K. Brodmann. 'Vergleichende Lokalisationslehre der Grosshirnrinde,' Leipzig, 1909.

[11] In opening the discussion on the 'Origin of Mammals' at the meetings of Section D. last year, I developed this argument (*Brit. Assoc. Reports*, 1911, p. 424).

has been wholly responsible for their dominant position, their world-wide distribution, and the plasticity which has manifested itself in the marvellous variety of adaptations to every mode of life which the mammals have undergone.

If we search for the new feature in the brain which has made possible all their achievements it will be found in a cortical area to which eleven years ago I gave the name 'neopallium.'[12]

In the lowlier vertebrates each of the avenues of the senses leads to a special part of the brain, and although there are free communications between the regions allotted to the olfactory, visual, auditory, tactile, and other senses, there is no instrument for the adequate blending of impressions reaching the brain through these different portals, or for the storing of impressions, so as to awaken in consciousness the different properties of an object which appeals to several different senses. The lowlier vertebrates do not see, hear, or feel an object in the sense that we associate with these terms. A biologically adequate stimulus, such, for example, as a splash in a pond to a frog, or the croaking of its fellows, will call forth an appropriate response; but an excitation such as would not normally come within the range of experience of the creature, however intense it may be, such as a loud noise from a gong, will leave it quite indifferent, because it lacks any mechanism to enable it to compare the novel stimulation either with former impressions or with its immediate effect upon other sense organs, and to judge it in the light of such standards of comparison.

This must not be confused with the complex reflexes found in all vertebrates, where a response, or a whole train of complicated acts, is excited only by a series of stimuli entering the nervous system through various avenues of the senses, and elaborating in the central nervous system what Professor Sherrington would call a 'common path' to the nuclei of the motor nerves that excite the muscles to produce the appropriate response.

Now it must be evident that unless there is some mechanism (*a*) for storing impressions so that they may be revived—*i.e.*, recalled in associative memory at some later time, and (*b*) for blending in consciousness the sensory impulses entering the sensorium by different portals, there can be no learning by experience. A fish that has swallowed a fly on a hook and got rid of the latter will repeat the process immediately, presumably because there is no intimate association in the brain between the receptor of the visual impression of the fly in the mid-brain and the receptor of the painful impression of the hook in the hind-brain; and therefore nothing to inhibit the normal response of the animal to the adequate biological stimulus provided by the visual image of the fly, whenever it occurs again.

Some faint glimmering of an elementary kind of judgment makes its appearance in reptiles, in which the tactile paths have made their way into the hitherto almost exclusively olfactory cerebral hemispheres, and established some definite representation for the sense of touch in this dominant part of the brain.[13] The snake which smells out some food-material, and then tests it by feeling it with its tongue, is checking by means of one sensory perception the information gained through another sense : it thus displays the germ of the power of contrasting the impressions and the memories of sensations reaching the brain by two distinct avenues of sense, from which eventually the power of instituting deliberate conscious judgments is developed.

It is able to do so (and this is the important point for us to remember in this discussion) in virtue of the fact, which can be demonstrated by the comparative study of the structure of the brain, that both the senses of smell and touch are able not only to pour their impressions into contiguous and intimately associated areas of the cerebral cortex, but also because they are represented there by a mass of material serving as a storehouse for the impressions of these senses, which can be revived in memory.

But such potentialities can be said to become first definitely established for

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[12] 'The Natural Subdivision of the Cerebral Hemisphere,' *Journ. Anat. and Phys.*, vol. xxxv., 1901, p. 431.

[13] Arris and Gale Lectures on the Evolution of the Brain, *Lancet*, January 15, 1910, p. 153.

all the senses in mammals. The neopallium of the mammal[14] provides a receptive organ for impressions of all the senses—touch, vision, and hearing, as well as taste and smell, among the rest—which enables the effects of all such perceptions to manifest themselves unified in consciousness, and to become recorded in some way so that they can be revived again, each discrete sensory impression or the one idea excited by all of them, in associative memory. Moreover, it is the instrument by means of which all these perceptions, past and present, can be freely blended in consciousness, so that the animal is able to appreciate all the properties of any object, to whatever sense they may appeal, and can benefit by past experience, and so be educated.

In spite of the opinion of Prof. C. J. Herrick,[15] I maintain that the neopallium is a feature distinctive of the mammalian brain, and that it represents in itself the unity of the apparatus concerned with psychical phenomena, the *sensorium commune*, which Aristotle postulated as the counterpart of the unity of consciousness.[16] I am well aware that psychologists may consider this the rankest heresy, to judge from the writings of my friend Dr. William McDougall[17]; but the anatomical evidence is quite definite and unequivocal that in the neopallium of the lowlier Mammalia we have a 'unitary organ the physical processes of which might be regarded as corresponding to the unity of consciousness.' Moreover, it fulfils Aristotle's claim for his *sensorium commune* of possessing 'especially the perceptual functions that are common to the several senses.'

Nothing that happens in this area in the course of its enormous expansion and differentiation in the higher mammals materially affects this fundamental purpose of the neopallium, which continues to remain a unifying organ that acts as a whole, though each part is favourably placed to receive and transmit to the rest its special quota to the sum-total of what we may call the materials of conscious life.

Thus the area in which the tract from the eyes ends in the neopallium naturally becomes the mechanism for visual perception, but it also serves as a means of union between visual and other perceptual parts of the cortex; for if the eyes are destroyed, the visual area does not wholly atrophy; or again, in the intact individual the visual impression of an apple is capable of awakening memories of perceptions of its 'feel,' its weight, its smell and its taste, all of which were originally acquired by impressions made upon the receptive area of each of the senses concerned.

The consciousness which resides, so to speak, in this neopallium, and is fed by the continual stream of sensory impressions pouring into it and awakening memories of past sensations, can express itself directly in the behaviour of the animal through the intermediation of a part of the neopallium itself, the so-called motor area, which is not only kept in intimate relation with the muscles, tendons, and skin by sensory impressions, but controls the voluntary responses of the muscles of the opposite side of the body.

The possession of this higher type of brain enormously widened the scope for the conscious and intelligent adaptation of the animal to varying surroundings; a sensory impression once received no longer remained only half an experience, which left no lasting impression behind it to influence behaviour in the future, or at most a perception uninfluenced by those simultaneously received by other sense organs, and thus incapable of being checked, so to speak; and in the exercise of this newly acquired power of choice the way was opened for adaptations to varying environments entailing manifold structural modifications, in which the enhanced plasticity of the new type of animal found expression.

Nature tried innumerable experiments with the new type of brain almost as soon as the humble Therapsid-like mammal felt the impetus of its new-found

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[14] I am omitting all reference to birds, in which a new formation makes its appearance in the cerebral hemisphere and performs functions in a sense analogous to those of the neopallium in mammals. It is, however, a specialisation incapable of great extension, just as the structure and functions of a bird are so highly specialised for flight as to lose the high degree of plasticity of the primitive mammal.

[15] 'The Morphology of the Forebrain in Amphibia and Reptilia,' *Journ. Comp. Neur. and Psych.*, October 1910, p. 439.

[16] See *op. cit.*, *Journ. Anat. and Phys.*, vol. xxxv., p. 453.

[17] 'Body and Mind,' 1911, p. 286.

power of adaptation, and from its South African home began to wander throughout the world. In turn the Prototherian and Metatherian types of brain were tried before the more adaptable scheme of the Eutherian brain was evolved.

The group of lowly Insectivora, including such creatures as Moles, Hedgehogs, and Shrews, persists to the present time to reveal the nature of the earliest Eutherian brain.

The Insectivora have been able to persist in competition with more advanced mammals by reason of their small size, and the development of manifold varieties of protective specialisations and the adoption of habits to ensure their safety. In their spread throughout the world they eventually came to occupy practically the whole earth, with the remarkable exception of the Australasian and, except for a short time, South American regions, where their predecessors, the Metatheria, found a haven of refuge, which saved them from the extinction that would inevitably have been their fate if they had had to struggle for existence in competition with the more nimble-witted Eutheria, endowed with a superior type of brain.[18]

Among the Insectivora there is one group—perhaps worthy of ordinal distinction, the Menotyphla of Haeckel—which is of peculiar interest to the student of Primate and human genealogies.

This group includes the Oriental Tree-Shrews and the African Jumping-Shrews. The latter (Macroscelididæ), living in the original South African home of the Mammalia, present extraordinarily primitive features, linking them by close bonds of affinity to the Marsupials. The Tree-Shrews (Tupaiidæ), however, which range from India to Java, while presenting very definite evidence of kinship to their humble African cousins, also display in the structure of their bodies positive evidence of relationship to the stem of the aristocratic Primate phylum.

I need not discuss the evidence for these views, because it has recently been summarised most excellently by Dr. W. K. Gregory[19] of New York.

It will suffice to point out that, quite apart from the striking similarities produced by identical habits and habitats, there are many structural identities, not directly associated with such habits, which can be interpreted only as evidences of affinity.

These Tree-Shrews are small squirrel-like animals which feed on 'insects and fruit, which they usually seek in trees, but also occasionally on the ground. When feeding they often sit on their haunches, holding the food, after the manner of squirrels, in their forepaws.'[20] They are of 'lively disposition and great agility.'[21] These vivacious, large-brained, little insectivores, linked by manifold bonds of relationship to some of the lowliest and most primitive mammals, present in the structure of their skull, teeth, and limbs undoubted evidence of a kinship, remote though none the less sure, with their compatriots the Malaysian Lemurs; and it is singularly fortunate for us in this inquiry that side by side there should have been preserved from the remote Eocene times, and possibly earlier still, these insectivores, which had almost become Primates, and a little primitive lemuroid, the Spectral Tarsier, which had only just assumed the characters of the Primate stock, when Nature fixed their types and preserved them throughout the ages, with relatively slight change, for us to study at the present day.

Thus we are able to investigate the influence of an arboreal mode of life in stimulating the progressive development of a primitive mammal, and to appreciate precisely what changes were necessary to convert the lively, agile *Ptilocercus*-like ancestor of the Primates into a real Primate.

In the forerunners of the Mammalia the cerebral hemisphere was predominantly olfactory in function; and even when the true mammal emerged, and all the other senses received due representation in the neopallium, the animal's behaviour was still influenced to a much greater extent by smell impressions than by those of the other senses.

This was due not only to the fact that the sense of smell had already installed

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[18] *Vide* Arris and Gale Lectures, *op. cit.*, *supra*.

[19] 'The Orders of Mammals,' *Bull. Amer. Mus. Nat. Hist.*, vol. xxvii., 1910, p. 321.

[20] Flower and Lydekker, 'Mammals, Living and Extinct,' 1891, p. 618.

[21] W. K. Gregory, *op. cit.*, p. 269, and pp. 279, 280.

its instruments in, and taken firm possession of, the cerebral hemisphere, long before the advent in this dominant part of the brain of any adequate representation of the other senses, but also, and chiefly, because to a small land-grubbing animal the guidance of smell impressions, whether in the search for food or as a means of recognition of friends or enemies, was much more serviceable than all the other senses. Thus the small creature's mental life was lived essentially in an atmosphere of odours, and every object in the outside world was judged primarily and predominantly by its smell; the senses of touch, vision, and hearing were merely auxiliary to the compelling influence of smell.

Once such a creature left the solid earth and took to an aquatic or an arboreal life all this was changed, for away from the ground the guidance of the olfactory sense lost much of its usefulness; and, in the case of aquatic mammals, the whole smell apparatus atrophied, and in some cases vanished. We need not stop to consider the aquatic mammal, because a life in the water calls for such marked specialisation of structure that such creatures disappear from the race for mammalian supremacy. But the case is very different with arboreal mammals. Life amidst the branches limits the usefulness of olfactory organs, but it is favourable to the high development of vision, touch, and hearing. Moreover, it demands an agility and quickness of movement that necessitates an efficient motor cortex to control and co-ordinate such actions as an arboreal mode of life demands (and secures, by the survival only of those so fitted), and also a well-developed muscular sensibility to enable such acts to be carried out with precision and quickness. In the struggle for existence, therefore, all arboreal mammals, such as the Tree-Shrews, suffer a marked diminution of their olfactory apparatus, and develop a considerable neopallium, in which relatively large areas are given up to visual, tactile, acoustic, kinæsthetic and motor functions, as well as to the purpose of providing a mechanism for mutually blending in consciousness the effects of the impressions pouring in through the avenues of these senses.

Thus a more equable balance of the representation of the senses is brought about in the large brain of the arboreal animal; and its mode of life encourages and makes indispensable the acquisition of agility. Moreover, these modifications do not interfere with the primitive characters of limb and body. These small arboreal creatures were thus free to develop their brains and maintain all the plasticity of a generalised structure, which eventually enabled them to go far in the process of adaptation to almost any circumstances that presented themselves.

Towards the close of the Cretaceous period some small arboreal Shrew-like creature took another step in advance, which was fraught with the most far-reaching consequences; for it marked the birth of the Primates and the definite branching off from the other mammals of the line of man's ancestry.

A noteworthy further reduction in the size of the olfactory parts of the brain, such as is seen in that of *Tarsius*,[22] quite emancipated the creature from the dominating influence of olfactory impressions, the sway of which was already shaken, but not quite overcome, when its Tupaioid ancestor took to an arboreal life. This change was associated with an enormous development of the visual cortex in the neopallium, which not only increased in extent so as far to exceed that of *Tupaia*, but also became more highly specialised in structure. Thus, in the primitive Primate, vision entirely usurped the controlling place once occupied by smell; but the significance of this change is not to be measured merely as the substitution of one sense for another. The visual area of cortex is part of the neopallium, and when its importance thus became enhanced the whole of the neopallium felt the influence of the changed conditions. The sense of touch also shared in the effects, for tactile impressions and the related kinæsthetic sensibility, the importance of which to an agile tree-living animal is obvious, assist vision in the conscious appreciation of the nature and the various properties of the things seen and in learning to perform agile actions which are guided by vision.

An arboreal life also added to the importance of the sense of hearing; and the cortical representation of this sense exhibits a noteworthy increase in the

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[22] 'On the Morphology of the Brain in the Mammalia, with Special Reference to that of the Lemurs, Recent and Extinct,' *Trans. Linn. Soc. Lond.*, second series; *Zoology*, vol. viii., Part 10, February 1903.

Primates, the significance of which it would be difficult to exaggerate in the later stages, when the simian are giving place to the distinctively human characteristics.

The high specialisation of the sense of sight awakened in the creature the curiosity to examine the objects around it with closer minuteness, and supplied guidance to the hands in executing more precise and more skilled movements than the Tree-Shrew attempts. Such habits not only tended to develop the motor cortex itself, trained the tactile and kinæsthetic senses, and linked up their cortical areas in bonds of more intimate associations with the visual cortex, but they stimulated the process of specialisation within or alongside the motor cortex of a mechanism for regulating the action of that cortex itself—an organ of attention which co-ordinated the activities of the whole neopallium so as the more efficiently to regulate the various centres controlling the muscles of the whole body. In this way not only is the guidance of all the senses secured, but the way is opened for all the muscles of the body to act harmoniously so as to permit the concentration of their action for the performance at one moment of some delicate and finely adjusted movement.

In some such way as this there was evolved from the motor area itself, in the form of an outgrowth placed at first immediately in front of it, a formation, which attains much larger dimensions and a more pronounced specialisation of structure in the Primates than in any other order; it is the germ of that great prefrontal area of the human brain which is said to be 'concerned with attention and the general orderly co-ordination of psychic processes,'[23] and as such is, in far greater measure than any other part of the brain, deserving of being regarded as the seat of the higher mental faculties and the crowning glory and distinction of the human fabric.

But the high development of certain other parts of the cortex was necessary to minister to these high functions of the prefrontal cortex and to supply the materials, if the term can be applied to anything so immaterial as perceptions and memories, upon which its own activities are expended. For before an animal like *Tarsius* could attempt to concentrate its attention upon the performance of some delicately skilled action, its large and highly specialised visual, tactile, and motor areas must have had impressed upon them countless numbers of records of things seen and felt and of memories of the experience acquired in performing innumerable simpler acts.

Whether the exceedingly primitive *Tarsius*-like Primates, of which I have been speaking, originated in the extreme south-west of Asia, where its modern representative now lives, or in North America, it is not possible to say with certainty; but the fact that the Eocene beds of North America contain remains of both the *Tarsius*-like *Anaptomorphus* and *Tupaia*-like Insectivores[24] seems to suggest that North America may have been the original home of the Primate phylum, and the centre from which, quite early in Eocene times, there radiated to South America, Asia, Europe, and Africa the ancestors not only of the Tarsier itself, but of the Lemurs also, as well as specialised varieties of lemuroids, such as *Adapis*, which may perhaps be related to the progenitors of the Lemurs. What remained of the original undifferentiated Primate stock in North America became transformed into primitive monkeys of Platyrrhine type, from which in turn sprang not only the later New World apes, but also the Catarrhine or Old World apes, which were undoubtedly derived from primitive Platyrrhines, possibly after their migration into the Old World in Oligocene times, or perhaps even earlier.

In the present state of our knowledge it is not possible to give the precise history of the wanderings of the early Primates. For not only are there vast ages of time in which we lose sight of them altogether, but in addition we are still far from an agreement as to the intercontinental land bridges along which our simian ancestors must have travelled in their wanderings. How great are the discrepancies between the conclusions reached by leading authorities on this subject is revealed only too clearly in the recent monographs by Professor

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[23] J. S. Bolton, 'The Functions of the Frontal Lobes,' *Brain*, 1903.

[24] H. F. Osborn, 'The Age of Mammals,' 1910, p. 155.

Osborn [25] and Dr. Scharff,[26] and in the opinions of other writers which they set forth in these two books.

The extremely primitive and Metatheroid characters of the Jumping Shrews mark them out as the nearest approach to the original Eutherian mammal now living; and it is probable that South Africa, their present habitat, was not only the original home of mammals, but also the place where the Eutherian mammals were evolved. In Upper Cretaceous times there was a direct land bridge [27] from this South African home of the Jumping Shrews to the Southern Asiatic habitat of their Primate-like kinsmen, the Tree-Shrews; and also another broad tract linking Eastern Asia to North America. So that there is no difficulty in realising how the Anaptomorphid Lemuroids or their immediate ancestors reached North America.

It is now generally admitted that *Tarsius* is not only the most primitive Prosimian now living, but that it is much more intimately related to the monkeys than the true Lemurs are. The fact that the earliest known fossil Primate, the Early Eocene *Anaptomorphus* of North America, so nearly resembles *Tarsius* further strengthens this view, and convinces us that in the Anaptomorphidæ we have the real progenitors of the Apes and Man.

Now, in the middle of the Eocene period these Lemuroids, and in fact all trace of the Primates, disappear from North America; and for the rest of the Eocene and Oligocene periods neither the Tarsioids nor true monkeys, according to some leading authorities, can be traced, until, in the Miocene, Platyrrhine Apes suddenly make their appearance in Patagonia, and Catarrhine and Anthropoid Apes in Europe. Even if we side with those who disagree with Professor W. B. Scott's opinion that the earliest monkeys in South America belong to the Miocene Age, and look upon them as Oligocene or Late Eocene,[28] there still remain a great many lacunæ in the fabric of our story.

We are not concerned here with the problem of how *Tarsius* and the Lemurs came to the Old World. It may have been the case that the original habitat of the Tarsioids ranged from North America to South-eastern Europe, and that the Tupaioid Insectivores, whose past and present representatives were distributed much in the same way, shared a similar fate. The Lemurs also may have sprung from some Protarsioid form and spread into Asia; or, seeing that their earliest allies [29] are found in the Middle and Late Eocene beds of Europe, it is not impossible that they may have made their way from North America into Europe by means of Scharff's hypothetical Atlantis,[30] which according to him linked Mexico and the Antilles to Europe.

But this is a problem that does not concern us in this inquiry. For the Lemurs, even in the Eocene, had left the path that leads to the Apes; and, however interesting *Tarsius* itself may be, once its Eocene progenitors gave birth to true monkeys we become interested in them, and not in the wanderings of the unmodified Lemuroids themselves.

As the facts of Comparative Anatomy point quite definitely to a kinship between the more primitive Platyrrhine monkeys of South America and the Catarrhines of the Old World, and also suggest that the latter must have passed through a Platyrrhine stage in the course of their evolution, we must first consider the nature of the wanderings that such kinships involve.

In the Lower and Middle Eocene of North America we find primitive Tarsioids, then in the Miocene [or late Eocene] in South America true Platyrrhines occur, and in the European Miocene Catarrhine and Anthropoid Apes. What working hypothesis can be framed to fill in the extensive lacunæ in this story? Palæontology tells us little more than this of the evolution of the Apes. Hence we must fall back upon the teaching of Comparative Anatomy.

The appearance in the Egyptian Fayoum as early as the Oligocene, according to Schlosser,[31] of monkeys presenting primitive traits suggestive of the

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[25] H. F. Osborn, 'The Age of Mammals,' New York, 1910.

[26] R. F. Scharff, 'Distribution and Origin of Life in America,' London, 1911.

[27] See Dr. Ortmann's map in Scharff, *op. cit.*, facing p. 292.

[28] Dr. von Ihering, quoted by Scharff, *op. cit.*, p. 393.

[29] *Adapis* and the *Tarsius*-like *Necrolemur*.

[30] *Op. cit.*, see Map 14, facing p. 280.

[31] Max Schlosser, *Zool. Anzeiger*, March 1, 1910, p. 500.

New World Platyrrhines, in association with others that seem to suggest a very early Anthropoid, the *Propliopithecus Haeckelii* (Schlosser), is a most suggestive discovery. It accords, however, with the scheme of Primate evolution and migrations which is based upon the other evidence at our disposal; so that some such hypothesis as I shall now sketch naturally shapes itself in our minds.

As the Tarsioids entirely disappeared from North America by the Middle Eocene and are not known to occur anywhere else in the world, either in past or present times, except as the Spectral Tarsier of the South-eastern corner of Asia, we must assume that some Tarsioids took refuge in Asia, which they reached, if they were not there before, by Scharff's hypothetical trans-Pacific land bridge in the Early Eocene, before their brethren disappeared from North America.

But it seems probable that some of the Tarsioids that lived in North America in Early Eocene times became transformed from Prosimiæ into true monkeys of a very primitive Platyrrhine type, distinguished, among other things, from their Lemuroid ancestors by a much higher development of the power of skilled movement, of which tangible evidence is forthcoming in the considerably larger and more highly specialised motor area of their modern descendants.[32]

The true monkey seizes its food with its hands, and not with its jaws, as the Lemur does.

A noteworthy increase occurred also in the visual cortex, and especially in those outlying parts of it which do not receive impressions directly from the optic tracts, but presumably are concerned with the storing of visual memories and associating them with tactile and acoustic impressions.

There is a corresponding, and perhaps relatively greater, change in the auditory centres.

Although the fossil beds of America have not yet yielded up the intermediate stages in these transformations, we have in the Middle Eocene Notharctids, those curiously half-formed Platyrrhines, and others of their contemporaries,[33] some evidence of the experiments Nature was performing in the process of creating Apes. And if it be objected that it is mere conjecture to say that the Tarsioids which went southwards in America were transformed into Platyrrhine monkeys by the time they reached Patagonia, it must be remembered that the vast continent which formed the link between California and Patagonia in those days, if we follow Scharff,[34] is now submerged, with all its relics of the birth and early history of the Apes. It is highly probable that some of the primitive Platyrrhines which were thus evolved spread from America into the Old World. Those that remained in the former continent spread south, leaving North America entirely without Primates; and in the relative seclusion of the South American forests they retained much of their primitive structure, but some of them sacrificed part of the advantage, from the point of view of mental evolution, gained when they took the upward step from the Prosimian to the Simian status, by devoting the special skill they had acquired to developing the prehensile powers of their tails, rather than concentrating such potentialities in the more serviceable culture of their hands.

Their brethren who crossed into the Old World took the higher course, and eventually achieved the greater distinction, when they became especially expert in performing the most delicate movements with their hands.

Which of two land bridges, trans-Pacific and trans-Atlantic, available in the Early Tertiary Age from Central America to Asia and Europe respectively,[35] the primitive monkeys traversed there is no definite evidence to indicate. For, while most writers assume that they went directly from North America to Western Asia, it must be borne in mind that the earliest remains of monkeys in the Old World occur in Egypt and Europe; and thus it is not altogether improbable that the Platyrrhine ancestors of the Catarrhines set out on their journey to the Old World by the trans-Atlantic route, and perhaps underwent the earlier stages of their further development in North Africa. For in the

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[32] C. and O. Vogt, *op. cit.*, p. 394.

[33] H. F. Osborn, 'The Age of Mammals,' p. 161; also *Bull. Amer. Mus. Nat. Hist.*, Vol. xvi., 1902; Workman, *Amer. Journ. Sci.*, vol. xv., 1903; and Max Weber, 'Saugetière,' 1904, p. 763.

[34] Scharff, *op. cit.*, fig. 14.

[35] Scharff, *op. cit.*, fig. 14.

Fayoum monkeys, described by Schlosser as belonging to the Oligocene Age, there is a strange association of Platyrrhine and Catarrhine traits.

In their new environment these primitive Catarrhines felt the impetus of their newly-acquired faculties; and an army of variously specialised Apes set out to invade the rest of the warmer regions of the Old World. In the course of these differentiations some of the Catarrhines fell away in some respects from the high standard their ancestors had attained; the Baboons, for example, took to quadrupedal progression, and thereby sacrificed all chance of further advance in the Anthropoid direction. But in Early Oligocene times certain of the Catarrhines probably developed still further the powers of walking erect, which their remote Tarsioid ancestor possessed in some measure, and became modified in structure so as to be able to walk upright upon their hind limbs, and use their hands and arms for other purposes. Thus, one group of Catarrhines became transformed into Anthropoid Apes very soon after the evolution of the first monkey of the Old World.

The assumption of the erect attitude is not the simple matter that most anthropologists suppose it to be : it is not a question merely of learning to balance the body on the hinder extremities : but, as Professor Keith has well shown,[36] it entailed profound structural changes for the fixation of the contents of the body so that the organs would not fall, and also the loss of the tail, so that the tail muscles could be turned to other uses as visceral supports; moreover, the freeing of the arms, which in a pronigrade animal, as Dr. Wood Jones has shown, are fixed supports for the muscles of respiration, interfered with this function of the extrathoracic muscles, and made the diaphragm the chief respiratory muscle. Thus was effected the most far-reaching changes in the mode of breathing. Such fundamental modifications of the structure and functions of the body must have been fashioned when the primitive Catarrhine was far more plastic than any of the modern Old World Apes we are familiar with; and I should not have been at all surprised, even without any knowledge of Schlosser's Oligocene *Propliopithecus*, to find that an erect Anthropoid Ape had already emerged from the Proto-Catarrhine stock soon after the close of the Eocene period.

In the modern Gibbon, which is a true Anthropoid, Nature has preserved for us with relatively only slight changes the type of the original ancestor of the phylum common to Man and the giant Apes.

The additional freedom which the degree of erectness of the Gibbon affords to the arms gave an immense impetus to the development of more highly skilled movement and a phenomenal agility; and such potentialities are the expression of still further stages of growth and elaboration of the brain. In all probability it is not strictly accurate to speak of the freeing of the arms as supplying the stimulus to the brain by permitting the acquisition of more highly skilled movements, expressed in a higher state of cultivation and growth of the motor cortex. There can be no question that the primary stimulus to the fuller use of the arms as organs, not of mere progression, but of prehension and the more skilled acts came from the steady growth and specialisation of the brain itself; but the consequences of the acquisition of this skill were the freeing of the arms and the possibility of still further cultivation of the powers of the hands, which no doubt reacted so as to call for yet further growth and specialisation of the brain. Thus, there was a reciprocal influence of brain and the erect attitude, both helping to achieve the result which various writers are too apt to attribute exclusively to one factor only.

That this explanation is the correct one is shown by the gradual increase in the size of the motor area, the degree of specialisation of the movements that become possible, and especially in the progressive expansion of the prefrontal area found at each step, when we compare Lemurs with Platyrrhines, the latter with Catarrhines, and these, in turn, with Anthropoids.[37]

This is especially seen in the increase in the control of the independent movements of the fingers. Such actions are very poorly developed in Lemurs, a little

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[36] Keith, Hunterian Lectures on Certain Phases in the Evolution of Man, *Brit. Med. Journ.*, April 6, 1912, p. 788.

[37] C. and O. Vogt, *op. cit.*, pp. 392, 393, and 394; also Brodmann, *op. cit.*, *supra.*

better in the New World Apes; but they became well developed in the Old World Apes, and very highly skilled and controlled in all the Anthropoid Apes. It was this gradual increase in skill of hand and arm which made it advantageous for the ancestors of the Gibbon to adopt, more definitely than *Tarsius* had done, the erect attitude and for their structure to become 'set' to make the best use of such skill.

But, marked as are the changes that can be detected in the behaviour of the erect Gibbon, in the increased size and specialisation of its motor and prefrontal areas, of which the latter is an expression of an improved control over the functions of the cortex as a whole, there is a significant increase in the size of the area which intervenes between the visual, tactile, and auditory centres. The growth and increased functional value of this parietal area, which from its position is obviously the place for storing the records of the complex states of consciousness, blended of visual, tactile, and auditory sensations, lie at the root of all the other changes in the brain; and the perfecting of the mechanism for supplying consciousness with these more perfect materials of experience, explains the increased ability to perform more highly skilled movements which, in turn, led to the adoption of the erect attitude, and thus freed the hands for the work they had for the first time become skilled to perform.

So far in this address I have been attempting to sketch certain outstanding features of Man's ancestry with the object of offering some explanation of the factors which made possible the emergence of such a creature as Man. We have seen that the adoption of an arboreal life by some small Insectivore-like creature, shortly before the dawn of the Tertiary period, and its subsequent cultivation of the sense of vision until it became a highly specialised Anaptomorphid, enabled Man's remotest Primate ancestor to escape from the domination of the sense of smell as the guiding influence of its life, and to cultivate its other senses, so as immensely to widen the sensory avenues by which the outside world could affect its conscious activities. The arboreal life, which demanded great activity and agility, led to the special cultivation of skilled movements of the limbs, and such an acquirement was clearly facilitated in this group by the perfection of visual control, without which finely adjusted actions of the hands and feet could not easily be learned. The acquisition of such skill in movement necessitated the increased perfection of the tactile and other sensory areas of the brain, so as the more nicely to control the adjustments and correlations of muscles essential for any precise action. Thus, we have a chain of linked influences that follow on the specialisation of the visual apparatus in the brain of our primitive arboreal ancestor—the perfecting of touch and the acquirement of skill in action. The heightened acuity of vision and the expansion of the cortical area for storing visual impressions, together with the growing importance of touch, and in a less measure, perhaps, of hearing, immensely widened the psychical content of the life of the Eocene Tarsioid in comparison with that of its contemporaries; but there is yet another factor which its mode of life called into play which was fraught with the most far-reaching possibilities in the creation of Man. The co-ordination of large groups of muscles for the purpose of performing some precise action, which must be controlled during the stage of learning by tactile, kinæsthetic and visual impressions and memories—the fruits of experience—necessitated the formation of some cortical apparatus which would control and harmonise the activities of the various centres, regulating the muscular actions, and bringing the total sum of consciousness at any one moment to bear upon the performance of a given act. Out of such a necessity as this there sprang in the early ancestor of Man—and, though in much less degree, in certain other phyla also—an outgrowth of the motor cortex, which became the mechanism for attention and the orderly regulation of the psychical processes.

Thus, at the very dawn of the Tertiary period there were developed the germs of all the psychical greatness which, in the million or so of years that have followed, culminated in the human mind.

But the early Primate stem attained this distinctive position of relative isolation, not merely by the development of its own members, but also by the rapid and divergent specialisation of other mammalian groups. The enhanced plasticity conferred upon mammals by the development of a neopallial cortex, which enabled them to exercise to an immeasurably greater degree than any of their predecessors had enjoyed, what we may without inaccuracy term intelligent choice, found immediate expression in a bewildering variety of specialisations of structure adapted to different modes of life. Some mammals became fleet of foot, and developed limbs specially adapted to enhance their powers of rapid movement. They attained an early pre-eminence, and were able to grow to large dimensions in the slow-moving world at the dawn of the age of mammals. Others developed limbs specially adapted for swift attack and habits of stealth, successfully to prey upon their defenceless relatives. Others took to the water or the air, and acquired the modifications in structure and manners of life necessary to accommodate themselves to their new environments.

Most of these groups attained the immediate success that often follows upon early specialisation: but they also paid the inevitable penalty. They became definitely committed to one particular kind of life; and in so doing they had sacrificed their primitive simplicity and plasticity of structure, and in great measure their adaptability to new conditions. The retention of primitive characters, which so many writers upon biological subjects, and especially upon anthropology, assume to be a sign of degradation, is not really an indication of lowliness. We should rather look upon specialisation of limbs and the narrowing of the manner of living to one particular groove as confessions of weakness, the renunciation of the wider life for one that is sharply circumscribed. It may be asked why all these other non-Primate mammals, leading an active life, many of them in open competition with their fellows, did not develop brains as highly organised as those of the Primates. The Ungulates and Carnivores, which most people who put such queries have in mind when they do so, develop large brains, although they are relatively very small in comparison with those of Primates of similar size. There are many reasons for this. Such creatures, even at the present day, always remain more or less under the influence of the sense of smell; and even though the visual, auditory, and tactile senses become well represented in the neopallium, these are secondary and relatively late modifications which have produced much less effect than the earlier usurpation of the dominant position by vision exerted in the Primates. Nevertheless, visual and auditory centres and the interposed parietal area become well developed, especially in Carnivora like the Dog, Cat, Bear, and Seal—an anatomical fact which explains their high degree of educability. But the growth of other cortical areas is subject to inevitable limitations in these creatures. The specialisation of limbs, and this is peculiarly the case in Ungulates, to perform only a very limited range of more or less automatic movements, does away with all possibility of developing further either an area to preside over skilled movements or a controlling prefrontal 'area of attention,' the usefulness of which is restricted by the impossibility of performing many acts that call for such guidance. Further progress along these lines was barred for all time by early specialisation of structure of the limbs; and hence these creatures lack one of the chief means which has led the Primate to the position of mental supremacy.

The Primates at first were a small and humble folk, who led a quite unobtrusive and safe life in the branches of trees, taking small part in the fierce competition for size and supremacy that was being waged upon the earth beneath them by their Carnivorous, Ungulate, and other brethren. But all the time they were cultivating that equable development of all their senses and limbs, and that special development of the more intellectually useful faculties of the mind which, in the long run, were to make them the progenitors of the dominant mammal—the mammal who was to obtain the supremacy over all others, while still retaining much of the primitive structure of limb that his competitors had sacrificed. It is important, then, to keep in mind that the retention of primitive characters is often to be looked upon as a token that their possessor has not been compelled to turn aside from the straight path and adopt protective specialisations, but has been able to preserve some of his primitiveness and the plasticity associated with it, precisely because he has not succumbed or fallen away in the struggle for supremacy. It is the wider triumph of the individual who specialises late, after benefiting by the all round of experience of early life, over him who in youth becomes tied to one narrow calling.

The Primates found in the branches of trees the asylum and protection necessary for the cultivation of brain and limbs during the period of their obscurity as an insignificant tribe; but when they became powerful enough to hold their own and wax great, both in size and power, they had maintained sufficient of their primitive characters, and the plasticity that goes with them, to be able gradually to give up the arboreal mode of living and to re-establish themselves once more as dwellers on the solid earth, competent to hold their own against all comers.

This principle extends not only to the Primate phylum itself, but to its different branches, and especially to Man.

I should be inclined to look upon the Orang, the Chimpanzee, and the Gorilla not as ancestral forms of Man; but as the more unenterprising members of Man's family, who were not able to maintain the high level of cerebral development of the feeble-bodied human, but saved themselves from extinction by the acquisition of great strength and a certain degree of specialisation of structure. The feebler Man was able to overcome his enemies and maintain himself in the struggle for existence by his nimbleness of wit and his superior adaptability to varying circumstances.

In many respects Man retains more of the primitive characteristics, for example, in his hands, than his nearest Simian relatives; and in the supreme race of mankind many traits, such as abundance of hair, persist to suggest pithecoid affinities, which have been lost by the more specialised Negro and other races. Those anthropologists who use the retention of primitive features in the Nordic European as an argument to exalt the Negro to equality with him are neglecting the clear teaching of Comparative Anatomy, that the persistence of primitive traits is often a sign of strength rather than of weakness. This factor runs through the history of the whole animal kingdom.[38] Man is the ultimate product of that line of ancestry which was never compelled to turn aside and adopt protective specialisations either of structure or mode of life, which would be fatal to its plasticity and power of further development.

Having now examined the nature of the factors that have made a Primate from an Insectivore and have transformed a Tarsioid Prosimian into an Ape, let us turn next to consider how Man himself was fashioned.

It is this aspect of the problem of the origin of Man which has always excited chief interest and has been the subject of much speculation, as the addresses of my predecessors in this presidency bear ample witness.

These discussions usually resolve themselves into the consideration of such questions as whether it was the growth of the brain, the acquisition of the power of speech, or the assumption of the erect attitude that came first and made the Ape into a human being. The case for the erect attitude was ably put before the Association in the address delivered to this Section by Dr. Munro in 1893. He argued that the liberation of the hands and the cultivation of their skill lay at the root of Man's mental supremacy. In Professor Sollas' address to the Geological Society, to which I have had occasion to refer before in this address, the same view seems to be favoured, for we find the statement that 'the first change which started the Ape on the path towards Man was probably the assumption of the erect attitude.' But this expression of opinion follows immediately upon what Professor Sollas calls a 'confession of faith' stated in these terms: 'My belief that the really fundamental change, underlying all the rest, was the increasing growth of the intellectual powers, and this I regard as an ultimate fact as difficult of explanation as any other ultimate fact, such as the origin of variations or even of life itself. But, having made this admission, I shall not introduce it into any of the following speculations, which will be more in accordance with the prevailing philosophy of the day.'[39] But if the increasing growth of the intellectual powers underlies all the rest, why depose this factor in favour of the influence of the erect attitude merely out of deference to fashion? Moreover, if the nature of the intellect is 'an ultimate fact as difficult of explanation as any other fact,' it does not imply that we must give up all hope of understanding some at least of the factors that explain the increasing growth of the intellectual powers or the obvious physical manifestations of growth and specialisation in the cerebral cortex, which unquestionably make such increase of intellectual powers possible.

If the erect attitude is to explain all, why did not the Gibbon become a Man in Miocene times? The whole of my argument has aimed at demonstrating that the steady growth and specialisation of the brain has been the fundamental

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[38] Elliot Smith, 'The Brain in the Edentata,' *Trans. Linn. Soc.*, 1899.

[39] *Proc. Geol. Soc.*, May 1910, p. 1, xxxii.

factor in leading Man's ancestors step by step right upward from the lowly Insectivore status, nay, further, through every earlier phase in the evolution of mammals—for Man's brain represents the consummation of precisely those factors which throughout the vertebrata have brought their possessors to the crest of the wave of progress. The external conditions of life have supplied the stimulus to this cerebral growth, as well as the sieve to strain off those individuals who do not adequately respond to the stimulus, and thus become 'unfit' to survive. But such advances as the assumption of the erect attitude are brought about simply because the brain has made skilled movements of the hands possible: yet once such a stage has been attained the very act of liberating the hands for the performance of more delicate movements opens the way for a further advance in brain development to make the most of the more favourable conditions and the greater potentialities of the hands.

I have already referred to the common fallacy of supposing that the erect attitude is Man's distinctive prerogative, and of regarding the assumption of that position and mode of progression as the determining factor in the evolution of Man. It is a fact beyond dispute that the divergent specialisation of the human limbs, one pair for progression, and the other for prehension and the more delicately adjusted skilled actions, has played a very large part in preparing the way for the emergence of the distinctively human characteristics; but it would be a fatal mistake unduly to magnify the influence of these developments. The most primitive living Primate, the Spectral Tarsier, frequently assumes the erect attitude, and uses its hands for prehension rather than progression in many of its acts, and many other Lemurs, such as the Indrisinæ of Madagascar, can and do walk erect.

In the remote Oligocene, a Catarrhine Ape, nearly akin to the ancestors of the Indian Sacred Monkey, *Semnopithecus*, became definitely specialised in structure in adaptation for the assumption of the erect attitude; and this type of early Anthropoid has persisted with relatively slight modifications in the Gibbon of the present day. But if the earliest Gibbons were already able to walk upright, how is it, one might ask, that they did not begin to use their hands, thus freed from the work of progression on the earth, for skilled work, and at once before men? The obvious reason is that the brain had not yet attained a sufficiently high stage of development to suggest appropriate occupation for these competent hands to do, to the exclusion of their function in climbing: those areas of the neopallium, upon the integrity and normal functioning of which consciousness relies for its experience, based upon the memories of visual, auditory, tactile, and all other kinds of sensations, as well as of the recollection of what effects follow upon certain lines of action—the cortical areas which, as we know by processes of inference and exclusion, must be responsible for some such functions as these—are still small and insignificant in the Gibbon in comparison with those of the human brain; and in the first Gibbon-like Anthropoid ancestor of Man they were no doubt equally in their infancy. If one considers, for instance, what a vast number of memories not only of sensations and relations between sensation factors, but also of conscious experience of the nature and results of innumerable acts, must be acquired before an animal can learn to anticipate what will happen as the direct effect of some action, before it can venture, for example, upon even so simple a purposive act as chipping a flint to produce a cutting edge, it must be evident that a vast neopallial storehouse for recording, and, so to speak, classifying, such experiences must be provided before the animal can begin to make the simplest inferences, compounded as these are of thousands of experiences, to make the conception of such an act possible.

The Ape is tied down absolutely to his experience, and has only a very limited ability to anticipate the results even of relatively simple actions, because so large a proportion of his neopallium is under the direct influence of the senses, directly or indirectly, *i.e.*, is concerned with the memories of mere sensations.

Psychologists tell us that 'so long as animals were absorbed in direct responses to the demands of their environment, their mental complexes were of a direct, primitive type, and stimulations issued into direct motor channels with relatively little possibility of ideational organisation.' But 'as soon as a type of response developed which was indirect there was a complete change in the general mode of bodily and conscious organisation.'[40] This distinction between the behaviour

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[40] C. H. Judd, 'Psychology, General Introduction,' 1907, p. 253.

of Man and other mammals is obviously correlated with the great expansion of the temporo-parietal area, which is the fundamental distinctive feature of the human brain.

In other Primates, even in the Anthropoid Apes, there is relatively only a small area intervening between the great visual, tactile, and auditory territories, and the fringing bands intimately associated with them, so that practically the whole of this part of the brain is under the direct influence of sensations that are constantly pouring in to keep the animal 'absorbed in direct responses to the demands of its environment'; but the part of the temporo-parietal area which is not under such direct influences undergoes a steady increase in size as we ascend the Primate series, until in the Anthropoid Apes the three sensory territories are definitely being pushed asunder, the visual to the occipital pole, the auditory to the temporal region, and the sensory to the central. The time eventually arrives when a sufficiently large area is formed where the functions of correlation of sensory images can go on undisturbed by the new stream of incoming impressions, and provide the physical mechanism for storing up records of the consequences of actions excited by the frontal areas, which must be the materials out of which the anticipation of what the result of any given action must be compounded.

It may seem wildly speculative to speak in this manner of the way in which these great temporo-parietal and frontal areas perform their functions, when we are so profoundly ignorant of the precise nature of their working. But they present the outstanding features of contrast in the brains of Men and Anthropoid Apes: we know that these two areas show a progressive and unbroken increase in size in the series of Primates, with which we can associate the growing power of skill in manipulation, of intelligent action, and the faculty of learning to perform most complex movements; and the records of clinical medicine and psychiatry have given us some faint glimmerings of the way in which they perform their functions in Man.

Thus there seems to be some justification for framing a definite working hypothesis to cover the factors that are known to us. The temporo-parietal area is the storehouse for the memories of the states of consciousness compounded of visual, auditory, and tactile sensations, and its progressive growth and specialisation is the measure of the efficiency with which it performs these functions. The central area is the storehouse for the memories of actions and the feelings associated with them. The prefrontal area is concerned with attention and the orderly control of the psychical activities of the whole cortex; and its great expansion and high differentiation in Man may be taken to mean, among other things, that it correlates the actions of the central and temporo-parietal areas, and supplies the mechanism for recording the experience of the casual relationship between the states of consciousness, causes, and effects, with which the central and the temporo-parietal areas respectively are more immediately concerned.

I am aware that this is a rather clumsy and crude attempt at psychological analysis; but the idea I am striving to express is this: that the Gibbon and the other Anthropoid Apes are 'strictly bound down to experience' and have not learned 'to anticipate to any extent what is going to happen,' because the parts of the brain, temporo-parietal and prefrontal areas, which provide the mechanisms for correlating causes and effects and making such experience available to regulate conduct, are still small and relatively undifferentiated. They are not yet sufficiently large to be removed far from the great sensory avenues along which a constant stream of traffic is surging and overflowing into these areas, compelling the latter to attune their activities to the immediate demands of the animal's environment.

And so the Gibbon, not yet mentally endowed to anticipate the consequences of its acts and to do any great variety of useful things with its hands, developed its arms in size and cunning, and became the most expert gymnast the world has known. Nor, again, when Man's nearer relatives, the Chimpanzee and the Gorilla, branched off from his ancestral line, were they any the more able to use their arms for the highest skilled work: but they developed great strength, which enabled them to hold their own upon the ground and wax in size when pitted in competition with the animals of their African forest home. In these specialisations of their limbs they lost something of the mechanisms that were essential to Man, which his ancestors retained when those of the giant Anthropoids chose the lesser part of relying upon their strength rather than their intelligence.

From the study of the brain in a series of Apes the fact emerges that in this progression there is taking place in the cerebral cortex a gradual extension and development of certain areas, which eventually made it possible for the Ape that achieved most in this way to make a fuller use of its erect attitude and employ its liberated hands and arms for a higher purpose than the rest of its tribe.

Even in the lowliest Catarrhine Apes the cortical area which is set apart directly to receive visual impressions is as large and as highly developed as it ever will be; and perhaps we may assume, though our evidence is not yet so satisfactory, that the tactile and acoustic areas are not very much inferior, so far as the mere perception of touch and hearing is concerned, to those of Man. But in the series of Primates we can detect fringes of cortex surrounding, perhaps growing out from, these great sensory areas, and others linking these fringing bands the one to the other, which show a progressive increase in size and complexity of structure, as we proceed from Prosimian to Platyrrhine, Platyrrhine to Catarrhine Ape, and through the series Gibbon, Chimpanzee, and Gorilla.

The progressive increase in size of these areas presumably connotes the growth and perfecting of the apparatus for recording sensory impressions and the complex states of consciousness which are awakened by the blending of various impressions, visual, acoustic, tactile, and the rest, and the memories of similar or contrasted forms of consciousness that the individual may have experienced in the past.

In Sir Edward Tylor's classical book on 'Anthropology' there is an admirable illustration of this in his comparison of the action of children and monkeys scrambling for nuts. 'Knowing a nut by sight, or having an idea of a nut, means that there are grouped together in the child's mind memories of a number of past sensations, which have so become connected by experience that a particular form and colour, feel and weight, lead to the expectation of a particular flavour. Of what here takes place in the child's mind we can judge, though by no means clearly, from what we know about our own thoughts and what others have told us about theirs. What takes place in the monkeys' minds we can only guess by watching their actions, but these are so like the human as to be most readily explained by considering their brain-work also to be like the human, though less clear and perfect.'

In Man there is relatively an enormous increase in the extent and degree of differentiation of the (temporo-parietal) region of the cerebral cortex which we must associate with this particular category of function. Presumably this implies not only that there is provided in the human brain a much more extensive cortical area in which the records of experience can become imprinted, but also that this larger territory will be free from the disturbance of the constant traffic upon the main great sensory avenues, such as is inevitable in brains in which the temporo-parietal area is little more than a narrow strip sandwiched in between the great sensory areas, and liable to be flooded and overwhelmed by the sensory impressions streaming into the cortex through them. In the Apes, even the most highly endowed Anthropoids, the conduct of the animal is attuned to the sensory impressions of the moment (modified in some degree only by the experience of the past), because the cerebral cortex is so flooded with impressions from the outside world.

But the time comes in the gradual development of the brain along the lines we see exemplified in the series of Apes, when those cortical areas not immediately concerned with the reception of sensory impressions become so large and so stored with the fruits of experience that they come to exercise an influence upon conduct more potent than that of direct sensory stimulation. Undisturbed by the stream of impressions constantly pouring into the sensorium they provide a mechanism for recording to an almost unlimited extent not only the mere visual, tactile, and other qualities of objects, and the states of consciousness each awakens, but also the recollection of acts and their consequences, so that Man is endowed with such a wealth of experience of the consequences of certain lines of action that he is able to foresee the results of his behaviour and modify it accordingly.

In comparison with the Apes, Man has an enormously enhanced faculty of profiting by experience, and of controlling the impulse to respond to every sensory stimulus in his environment by recalling the consequences of such responses on previous occasions. We may correlate these contrasts in behaviour with the striking differences in the cerebral cortex. In the Ape the activity of the greater part of the neopallium is to a large extent controlled by impressions streaming into its various parts from one or other of the sense organs or other sensitive structures in the body. In the course of evolution of the human brain there is added to this cortex of Man's Simian progenitor a mass of tissue, roughly, about five hundred cubic centimetres, bigger than the whole of the Gorilla's brain; and as the sensory areas of the human brain are practically equal to those of the Gorilla, all this enormous increase goes to swell the dimensions of those parts of the cortex which do not receive sensory impressions directly. These neopallial areas are at least six times as large in the human brain as they are in the Gorilla's. To put these facts into a slightly different form, in the Simian brain the sensory areas predominate, and the behaviour of the animal is to be looked upon as the response to the immediate sensory impressions of the moment: in the human brain the great association areas have grown far beyond the dimensions of the sensory areas, and experience, the effects of education, and knowledge assume the dominant rôle in influencing conduct.

In the series of Primates it is found that the size of the cortical area controlling skilled movements increases as we ascend the scale, and the variety, complexity, and skilled nature of the movements become markedly increased. The extent to which the Anthropoid Apes can be trained to remember and perform the most complex actions must be familiar to everyone who has visited the modern 'music-hall' or travelling menagerie.

Thus it happens when the brain reaches the stage in its evolution to impel its possessor to attempt complex purposive acts directed toward the accomplishment of some intelligent aim, the hand and arm are not only ready and free from the duty of progression, but they already have attained in great measure the skill and the cunning to perform what the intelligent will requires of them.

What more favourable conditions could be imagined than these for the forces of natural selection to seize hold of, to fix and establish more definitely the erect attitude, to make the hand a more delicate and exact instrument capable of performing infinitely more complex and varied skilled actions, to make the leg a more efficient support, and thereby simultaneously to give still more freedom to the all-important hand, while the growing brain is all the time becoming more richly endowed with the potentialities of the conscious memory, reciprocally stimulating and being stimulated by the motor centres, which, by directing the performance of new manœuvres, add to the storehouse of consciousness new elements of experience of cause and effect?

The erect attitude, infinitely more ancient than Man himself, is not the real cause of man's emergence from the Simian stage; but it is one of the factors made use of by the expanding brain as a prop to still further extend its growing dominion, and by fixing and establishing in a more decided way this erectness it liberates the hand to become the chief instrument of Man's further progress.

In learning to execute movements of a degree of delicacy and precision to which no Ape could ever attain, and which the primitive Ape-man could only attempt once his arm was completely emancipated from the necessity of being an instrument of progression, that cortical area which seemed to serve for the phenomena of attention became enhanced in importance. Hence the prefrontal region, where the activities of the cortex as a whole are, as it were, focussed and regulated, began to grow until eventually it became the most distinctive characteristic of the human brain, gradually filling out the front of the cranium and producing the distinctively human forehead. In the diminutive prefrontal area of *Pithecanthropus*,[41] and, to a less marked degree, Neanderthal man,[42], we see illustrations of lower human types, bearing the impress of their lowly state in receding foreheads and great brow-ridges. However large the brain may be

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[41] Eug. Dubois, 'Remarks upon the Brain-cast of Pithecanthropus,' *Proc. Fourth Internat. Cong. Zool.*, August 1898, published Camb., 1899, p. 81.

[42] Boule and Anthóny, 'L'encéphale de l'homme fossile de la Chapelle-aux-Saints,' *L'Anthropologie*, tome xxii., No. 2, 1911, p. 50.

in *Homo primigenius*, his small prefrontal region, if we accept Boule and Anthony's statements, is sufficient evidence of his lowly stage of intelligence and reason for his failure in the competition with the rest of mankind.

Once the Simian ancestor of Man began to anticipate the consequences of his acts and put this knowledge and the growing appreciation of the powers of his hands to useful purpose, for using weapons, or even making them, the erect attitude would become a regular habit, so as to emancipate the hands entirely for their new duties. The realisation of his ability to defend himself upon the ground, once he had learned the use of sticks and stones as implements, would naturally have led the intelligent Ape to forsake the narrow life of the forest and roam at large in search of more abundant and attractive food and variety of scene. Like most creatures who live in the open, the adoption of social habits is one of the surest means of protection; for the eyes and ears of each individual thus become the servants of the whole community, giving warning of danger, and thus adding to the safety of the herd. The development of the legs then became a necessary condition of survival: for warnings of danger to animals living in the open are useless without fleetness of foot to escape or skill of arm to ward off the threatened danger. Thus we have come to realise the steps by which a growing brain makes it possible and desirable for the most intelligent of the Apes to forsake the purely arboreal life and seek a wider sphere of activity upon the earth: they emerged from their original forest home, and in troops invaded the open country, led on no doubt by the search for a more plentiful supply or a more appetising variety of food. Such an existence, demanding an ever-increasing skill to use implements of defence and to specialise the arms in using them, and at the same time fleeter limbs, better adapted for progression on the earth, would rapidly transform the limbs and specialise them each for its separate functions.

Thus it easy to conceive how it came to happen, once the evolution of the brain made it possible for the Ape to appreciate its ability to perform, and anticipate the results of skilled actions, that he at once began to avail himself of the larger life that was opened before him. He already possessed the skill to use his hands, and this became emphasised with their added usefulness and value to their possessor, the more efficient brain, and increased delicacy of the hands themselves, once they ceased to be mere prehensile instruments. And the emancipation of the hands from progression threw the whole responsibility upon the legs, which became more efficient for their purpose as supports once they lost their prehensile powers and became elongated and specialised for rapid progression. Thus the erect attitude became stereotyped and fixed and the limbs specialised, and these upright Simians emerged from their ancestral forests in societies, armed with sticks and stones, and with the rudiments of all the powers that eventually enabled them to conquer the world. The greater exposure to danger which these more adventurous spirits encountered once they emerged in the open, and the constant struggles these first semi-human creatures must have had in encounters with definite enemies, no less than with the forces of Nature, provided the factors which rapidly weeded out those unfitted for the new conditions, and by natural selection made real Men of the survivors.

The growth in intelligence and in the powers of discrimination no doubt led to the dawning of a definite æsthetic sense, which, operating through sexual selection, brought about a gradual refinement of the features, added grace to the general build of the body, and demolished the greater part of its hairy covering. It also intensified the sexual distinctions, especially by developing in the female localised deposits of fatty tissue, not found in the Apes, which produced profound alterations in the general form of the body.

To one who considers what precisely it means to fix the attention and attempt the performance of some delicately adjusted and precise action it must be evident that one hand only can be usefully employed in executing the consciously skilled part in any given movement. The other hand, like the rest of the muscles of the whole body, can be only auxiliary to it, assisting, under the influence of attention, either passively or actively, in steadying the body or helping the dominant hand. Moreover, it is clear that if one hand is constantly employed for doing the more skilled work, it will learn to perform it more precisely and more successfully than either would if both were trained, in spite of what ambidextral enthusiasts may say. Hence it happened that when Nature was fashioning Man the forces of natural selection made one hand more apt to perform skilled movements than the other. Why precisely it was the right hand that was chosen in the majority of mankind we do not know, though scores of anatomists and others are ready with explanations. But probably some slight mechanical advantage in the circumstances of the limb, or perhaps even some factor affecting the motor area of the left side of the brain that controls its movements, may have inclined the balance in favour of the right arm; and the forces of heredity have continued to perpetuate a tendency long ago imprinted in Man's structure when first he became human.

The fact that a certain proportion of mankind is left-handed, and that such a tendency is transmitted to some only of the descendants of a left-handed person, might perhaps suggest that one half of mankind was originally left-handed and the other right-handed, and that the former condition was recessive in the Mendelian sense, or that some infinitesimal advantage may have accrued to the right-handed part of the original community, which in time of stress spared them in preference to left-handed individuals; but the whole problem of why right-handedness should be much more common than left-handedness is still quite obscure. The superiority of one hand is as old as mankind, and is one of the factors incidental to the evolution of Man.

It is easily comprehensible why one hand should become more expert than the other, as I have attempted to show; and the fact remains that it is the right hand, controlled by the left cerebral hemisphere, which is specially favoured in this respect. This heightened educability of the (left) motor centre (for the right hand) has an important influence upon the adjoining areas of the left motor cortex. When the Ape-Man attained a sufficient degree of intelligence to wish to communicate with his fellows other than by mere instinctive emotional cries and grimaces, such as all social groups of animals employ, the more cunning right hand would naturally play an important part in such gestures and signs; and, although the muscles of both sides of the face would be called into action in such movements of the features as were intended to convey information to another (and not merely to express the personal feelings of the individual), such bilateral movements would certainly be controlled by the left side of the brain, because it was already more highly educated.

Up to this stage the means of communication with other individuals was practically confined to signs and gestures, controlled by the left brain of the signaller and appreciated by the visual apparatus of the receiver: and no doubt a special bond was established between the visual areas, in which the memories of such signs and their meanings were recorded, and the area in which the memories of the particular movements of arms and face were stored: and as the latter were controlled in the left hemisphere, the bond between the visuo-psychic and arm-head motor centres would be specially intimate in the left cerebral hemisphere.

The increased control acquired by the left motor centres (over the right hand and both sides of the face) also extended to the left centres that regulate the muscles of the tongue, palate, and larynx; and the skill that the primitive Ape-Man had acquired to perform delicately adjusted actions with the right hand and face naturally became extended to include these other muscles, the movements of which are regulated by the adjoining cortical area, and are also used to aid in expressing the ideas conveyed by the movements of the hand and face. Then he learned to make a much greater variety of sounds than he has inherited from his Gorilla-like and Gibbon-like ancestors. To the memories of the sounds of other animals and of the noises that occur in Nature, which had already become stored up in the sensorium of Apes, the primitive Ape-Man added a collection of records of the expressive sounds deliberately emitted by his fellows; and in course of time the consciousness of these sounds was recorded, along with the memories of his gestures and grimaces, associating each with some meaning, which became a new way of communicating with his fellows.

The perfection of the cortical mechanism for appreciating sounds and detecting a very wide range of qualities is associated in the human brain with a remarkable growth and differentiation of the auditory area of the cortex.[43] As intercommunication between members of a social group became a matter of

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[43] Brodmann, *op. cit.*, p. 144.

vital importance to the individuals composing it, this acuity in recognising sounds of different pitch, tone, and timbre, and in detecting their precise emotional significance, would grow *pari passu* with the acquisition of speech.

From the time the early Primate ancestor of Man took to an arboreal mode of life, the sense of hearing has always been keen and especially well-represented, though not to the degree that vision is, in the neopallium; but this normal acuity of the Primate hearing became enormously heightened, or rather, attuned to perceive a much greater variety of sounds, when it came to be the chief means of communication between Men.

It must be quite evident that the first essential condition of speech must be the evolution of an area (usually in the left brain) in which there can be added to the vast collection of visual, auditory, tactile, and other complex states of consciousness already stored in it, not only the memories of the visual impressions of gestures and their meanings, but also of sounds and their associated ideas—that is, the state of consciousness which each particular sound awakens, through being linked up in memory with the auditory sensation; and the second essential must be a motor cortex sufficiently skilled to produce similar gestures, grimaces, and sounds. But just as a child must learn the meaning of words long before he attempts to reproduce the sounds himself, so in the dawn of human existence the Ape-Man educated his acoustic cortex to associate definite meanings with the sounds that occurred in Nature around him, and no doubt learned to imitate them before he began to invent new sounds to express new meanings, or to imitate those emitted by his fellow-Men. If it was the precocious high development of the sense of sight that started the Primates on their career, the high development of the cortical mechanism for discriminating sounds played a great part in making Man from an Ape. I think that most anthropologists who approach the study of speech from the physical or biological side have concentrated too much attention upon the supposed motor centres, and not enough on the great temporo-parietal areas, upon the education of which the faculty of speech, as Pierre Marie is rightly insisting, so largely depends. In other words, our Simian ancestor must have had something to say before he attempted to find means of expressing it.

I do not propose to discuss the tremendous impetus that the invention of speech must have given to human progress and intellectual development, in enabling the knowledge acquired by each individual to become the property of the community and be handed on to future generations, as well as by supplying in words the very symbols and the indispensable elements of the higher mental processes. This theme has been frequently discussed by many great thinkers: it has been expounded by several of my predecessors in this chair, and its influence pictured much more graphically and eloquently than I am capable of doing. For as Huxley has well said: Man 'alone possesses the marvellous endowment of intelligible and rational speech, whereby, in the secular period of his existence, he has slowly accumulated and organised the experience which is almost wholly lost with the cessation of life in other animals; so that now he stands raised upon it as on a mountain top, far above the level of his humble fellows, and transfigured from his grosser nature by reflecting here and there a ray from the infinite source of truth.'[44]

We are apt to forget the immensity of the heritage that has come down to us from former generations of Men, until we begin dimly to realise that for the vast majority of mankind almost the sum-total of their mental activities consists of imitation or acquiring and using the common stock of knowledge. For this accumulation of knowledge and its transmission to our generation we are almost wholly indebted to the use of speech. In our forgetfulness of these facts we marvel at the apparent dulness of early Man in being content to use the most roughly chipped flints for many thousands of years before he learned to polish them, and eventually to employ materials better suited for the manufacture of implements and weapons. But is the more highly cultured and civilised population of our own day so much more fertile of ideas? Is it not the fact that no really new idea ever enters the mind of the vast majority of mankind, and even much that seems new is really compounded of the knowledge gained by others? When we consider how slowly and laboriously primitive Man acquired new ideas,

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[44] Quoted by Sollas, *op. cit.*, p. lxxxvii.

and how such ideas—even those which seem childishly simple and obvious to us—were treasured as priceless possessions and handed on from tribe to tribe, what becomes of the current theories of independent evolution of customs and culture? What room is there for hypotheses of 'similar workings of the human mind leading to the development of similar inventions'? Really original ideas were far too rare in the youth, and in fact also in the full flush of adult age, of mankind, for us seriously to consider the possibility that merely similar circumstances would tend to call forth similar ideas? This brings us back to the place from which we started. The very essence of intelligence is the uncertainty of the response it gives to 'similar circumstances'; the blind forces of environment working in two organisms may lead to similar results, but who can predict the final issue when intelligence interferes?

The modern problems of anthropology that we have to solve, those which relate to Man and his inventions since the time of his world-wide distribution and differentiation into distinct races, are not so much questions of independent evolution, but rather those concerning the migrations, the intermixtures and the blendings of different races and cultures. The hypothesis of the 'fundamental similarity of the working of the human mind' is no more potent to explain the identity of customs in widely different parts of the world, the distribution of megalithic monuments, or the first appearance of metals in America, than it is to destroy our belief that one man, and one only, originally conceived the idea of the mechanical use to which steam could be applied, or that the electric battery was not independently evolved in each of the countries where it is now in use.

In these discursive remarks I have attempted to deal with old problems in the light of newly-acquired evidence; to emphasise the undoubted fact that the evolution of the Primates and the emergence of the distinctively human type of intelligence are to be explained primarily by a steady growth and specialisation of certain parts of the brain; that such a development could have occurred only in the Mammalia, because they are the only plastic class of animals with a true organ of intelligence; that an arboreal mode of life started Man's ancestors on the way to pre-eminence, for it gave them the agility, and the specialisation of the higher parts of the brain incidental to such a life gave them the seeing eye, and in course of time also the understanding ear; and that all the rest followed in the train of this high development of vision working on a brain which controlled ever-increasingly agile limbs.

If I have made these general principles clear, however clumsily set forth, and with whatever crudities of psychological statement they may be marred, I shall feel that I have not laboured in vain.

British Association for the Advancement of Science.

SECTION I: DUNDEE, 1912.

ADDRESS

TO THE

PHYSIOLOGICAL SECTION

BY

LEONARD HILL, M.B., F.R.S.,

PRESIDENT OF THE SECTION.

Last year the distinguished President of this Section raised us to the contemplation of the workings of the soul. I ask you to accompany me in the consideration of nothing higher than a stuffy room. Everyone thinks that he suffers in an ill-ventilated room owing to some change in the chemical quality of the air, be it want of oxygen, or excess of carbon dioxide, the addition of some exhaled organic poison, or the destruction of some subtle property by passage of the air over steam-coils, or other heating or conducting apparatus. We hear of 'devitalised' or 'dead' air, and of 'tinned' or 'potted' air of the battleship. The good effects of open-air treatment, sea and mountain air, are no less generally ascribed to the chemical purity of the air. In reality the health-giving properties are those of temperature, light, movement, and relative moisture of the surrounding atmosphere, and leaving on one side those gross chemical impurities which arise in mines and in some manufacturing processes, and the question of bacterial infection, the alterations in chemical composition of the air in buildings where people crowd together and suffer from the effects of ill-ventilation have nothing to do with the causation of these effects.

Satisfied with the maintenance of a specious standard of chemical purity, the public has acquiesced in the elevation of sky-scrapers and the sinking of cavernous places of business. Many have thus become cave-dwellers, confined for most of their waking and sleeping hours in windless places, artificially lit, monotonously warmed. The sun is cut off by the shadow of tall buildings and by smoke—the sun, the energiser of the world, the giver of all things which bring joy to the heart of man, the fitting object of worship of our forefathers.

The ventilating and heating engineer hitherto has followed a great illusion in thinking that the main objects to be attained in our dwellings and places of business are chemical purity of the air and a uniform draughtless summer temperature.

Life is the reaction of the living substance to the ceaseless play of the environment. Biotic energy arises from the transformation of those other forms of energy—heat, light, sound, &c.—which beat upon the transformer—the living substance (B. Moore). Thus, when all the avenues of sense are closed, the central nervous system is no longer aroused and consciousness lapses. Laura Bridgeman, paralysed in almost all her avenues of sense, fell asleep whenever her remaining eye was closed. The patient who lost one labyrinth by disease and, to escape unendurable vertigo, had the other removed by operation, was quite unable to guide his movements or realise his position in the dark. Rising from bed one night, he collapsed on the floor and remained there helpless till succour arrived.

A sense organ is not stimulated unless there is a change of rate in the transference of energy; and this to be effectual must occur in most cases with considerable quickness. If a weak agent is to stimulate, its application must be abrupt (Sherrington). Thus the slow changes of barometric pressure on the body-surface originate no skin sensations, though such changes of pressure if applied suddenly, are much above the threshold value for touch. A touch excited by constant mechanical pressure of slight intensity fades quickly below the threshold of sensation. Thus the almost unbearable discomfort which a child feels on putting on for the first time a 'natural' wool vest fades away, and is no longer noticed with continual wear. Thomas à Beckett soon must have become oblivious to his hair-shirt, and even to its harbingers. It is not the wind which God tempers to the shorn lamb, but the skin of the lamb to the wind. The inflow of sensations keeps us active and alive and all the organs working in their appointed functions. The cutaneous sensations are of the highest importance. The salt and sand of wind-driven sea air particularly act on the skin and through it braces the whole body. The changing play of wind, of light, cold, and warmth stimulate the activity and health of mind and body. Monotony of sedentary occupation and of an overwarm still atmosphere endured for long working hours destroys vigour and happiness and brings about the atrophy of disuse. We hear a great deal of the degeneration of the race brought about by city life, but observation shows us that a drayman, navvy, or policeman can live in London, or other big city, strong and vigorous, and no less so than in the country. The brain-worker, too, can keep himself perfectly fit if his hours of sedentary employment are not too long and he balances these by open-air exercise. The horses stabled, worked, and fed in London are as fine as any in the world; they do not live in windless rooms heated by radiators.

The hardy men of the North were evolved to stand the vagaries of climate—cold and warmth—a starved or full belly have been their changing lot. The full belly and the warm sun have expanded them in lazy comfort; the cold and the starvation have braced them to action. Modern civilisation has withdrawn many of us from the struggle with the rigours of Nature; we seek for and mostly obtain the comfort of a full belly and expand all the time in the warm atmosphere afforded us by clothes, wind-protected dwellings, and artificial heat—particularly so in the winter, when the health of the business man deteriorates. Cold is not comfortable, neither is hunger, therefore we are led to ascribe many of our ills to exposure to cold, and seek to make ourselves strong by what is termed good living. I maintain that the bracing effect of cold is of supreme importance to health and happiness, that we become soft and flabby and less resistant to the attacks of infecting bacteria in the winter not because of the cold but because of our excessive precautions to preserve ourselves from cold; that the prime cause of 'cold' or 'chill' is not really exposure to cold but to the over-heated and confined air of rooms, factories, and meeting-places. Seven hundred and eleven survivors were saved from the *Titanic* after hours of exposure to cold. Many were insufficiently clad and others wet to the skin. Only one died after reaching the *Carpathia*, and he three hours after being picked up. Those who died perished from actual cooling of the body. Exposure to cold did not cause in the survivors the diseases commonly attributed to cold.

Conditions of city and factory life diminish the physical and nervous energy, and reduce many from the vigorous health and perfectness of bodily functions which a wild animal possesses to a more secure, but poorer and far less happy form of existence. The ill chosen diet, the monotony and sedentary nature of daily work, the windless uniformity of atmosphere, above all, the neglect of vigorous muscular exercise in the open air and exposure to the winds and light of heaven—all these, together with the difficulties in the way of living a normal sexual life, go to make the pale, undeveloped, neurotic, and joyless citizen. Nurture in unnatural surroundings, not Nature's birth-mark, moulds the criminal and the wastrel. The environment of childhood and youth is at fault rather than the stock: the children who are taken away and trained to be sailors, those sent to agricultural pursuits in the Colonies, those who become soldiers, may develop a physique and bodily health and vigour in striking contrast to their brothers who become clerks, shop assistants, and compositors.

Too much stress cannot be put on the importance of muscular exercise in regard to health, beauty, and happiness. Each muscle fills with blood as it relaxes, and expels this blood on past the venous valves during contraction. Each muscle together with the venous valves forms a pump to the circulatory system. It is the function of the heart to deliver the blood to the capillaries, and the function of the muscles—visceral, respiratory, and skeletal—to bring it back to the heart. The circulation is contrived for a restless mobile animal; every vessel is arranged so that muscular movement furthers the flow of blood.

The pressure of the blood in the veins and arteries under the influence of gravity varies with every change of posture. The respiratory pump, too, has a profound influence on the circulation. Active exercise, such as is taken in a game of football, entails endless changes of posture, varying compressive actions —one with another struggling in the rough and tumble of the game—forcible contractions and relaxations of the muscles, and a vastly increased pulmonary ventilation: at the same time the heart's action is accelerated and augmented and the arterial supply controlled by the vaso-motor system. The influence of gravity, which tends to cause the fluids of the body to sink into the lower parts, is counteracted: the liver is rhythmically squeezed like a sponge by the powerful respiratory movements, which not only pump the blood through the abdominal viscera but thoroughly massage these organs, and kneading these with the omentum clean the peritoneal cavity and prevent constipation. At the same time the surplus food metabolic products, such as sugar and fat, stored in the liver, are consumed in the production of energy, and the organs swept with a rapid stream of blood containing other products of muscular metabolism which are necessary to the inter-relation of chemical action. The output of energy is increased very greatly: a resting man may expend two thousand Calories per diem: one bicycling hard for most of the day expended eight thousand Calories, of which only four thousand was covered by the food eaten.

Such figures show how fat is taken off from the body by exercise, for the other four thousand Calories comes from the consumption of surplus food products stored in the tissues. While resting a man breathes some 7 litres of air, and uses 300 c.c. of oxygen per minute, against 140 litres and 3000 c.c. while doing very hard labour. The call of the muscles for oxygen through such waste products as lactic acid impels the formation of red corpuscles and hæmoglobin. The products of muscular metabolism in other ways not yet fully defined modify the metabolism of the whole body.

Exposure to cold, cold baths, and cold winds has a like effect, accelerating the heart and increasing the heat production, the activity of the muscles, the output of energy, the pulmonary ventilation, and intake of oxygen and food. In contrast with the soft pot-bellied, over-fed city man the hard, wiry fisherman trained to endurance has no superfluity of fat or tissue fluid. His blood volume has a high relative value in proportion to the mass of his body. His superficial veins are confined between a taut skin and muscles, hard as in a racehorse trained to perfection. Thus the adequacy of the cutaneous circulation and loss of heat by radiation rather than by sweating is assured. His fat is of a higher melting-point, hardened by exposure to cold. In him less blood is derived to other parts such as adipose tissue, skin, and viscera. He uses up the oxygen in the arterial blood more completely and with greater efficiency: for the output of each unit of energy his heart has to circulate much less blood (Kreogh): his blood is sent in full volume by the well-balanced activity of his vaso-motor system to the moving parts. Owing to the perfect co-ordination of his muscles, trained to the work, and the efficient action of his skin and cutaneous circulation—the radiator of the body—he performs the work with far greater economy and less fatigue. The untrained man may obtain 12 per cent. of his energy output as work, against 30 per cent. or perhaps even 50 per cent. obtained by the trained athlete. Hence the failure and risk suffered by the city man who rushes straight from his office to climb the Alps. On the other hand, the energetic man of business or brain worker is kept by his work in a state of nervous tension. He considers alternative lines of action, but scarcely moves. He may be intensely excited, but the natural muscular response does not follow. His heart is accelerated and his blood pressure raised, but neither muscular movements and accompanying changes of posture, nor the respiratory pump materially aid the circulation. The activity of his brain demands a rapid flow of blood, and his heart has to do the circulatory work, as he sits still or stands at his desk, against the influence of gravity. Hence a high blood pressure is maintained for long periods at a time by vaso-constriction of the arteries in the lower parts of the body and increased action of the heart; hence, perhaps, arise those degenerative changes in the circulatory system which affect some men tireless in their mental activity. We know that the bench-worker, who stands on one leg for long hours a day, may suffer from degeneration and varicosity of the veins in that leg. Long continued high arterial pressure, with systolic and diastolic pressures approximately the same, entails a stretched arterial wall, and this must impede the circulation in the vaso vasorum, the flow of tissue lymph in, and nutrition of, the wall. Since his sedentary occupation reduces the metabolism and heat production of his body very greatly, the business man requires a warmer atmosphere to work in. If the atmosphere is too warm it reduces his metabolism and pulmonary ventilation still further; thus he works in a vicious circle. Exhausting work causes the consumption of certain active principles, for example, adrenalin, and the reparation of those must be from the food. To acquire certain of the rarer principles expended in the manifestation of nervous energy more food may have to be eaten by the sedentary worker than can be digested and metabolised. His digestive organs lack the kneading and massage, the rapid circulation and oxidation of foodstuffs which is given by muscular exercise. Hence arise the digestive and metabolic ailments so common to brain workers.

Mr. Robert Milne informs me that of the thousands of children which have passed through Barnardo's Homes—there are 9,000 in the homes at any one time—not one after entering the institution and passing under its regimen and the care of his father, Dr. Milne, has developed appendicitis. Daily exercise and play, adequate rest, a regular simple diet have ensured their immunity from this infection. It pays to keep a horse healthy and efficient; it no less pays to keep men healthy. I recently investigated the case of clerks employed in a great place of business, whose working hours are from 9 to 6 on three days, and 7 to 9 on the other three days of each week, and, working such overtime, they make 1*l.* to 2*l.* a week; these clerks worked in a confined space—forty to fifty of them in 8,200 cubic feet, lit with thirty electric lamps, cramped for room, and overheated in warm summer days. It is not with the chemical purity of the air of such an office that fault is to be found, for fans and large openings ensured this sufficiently. These clerks suffered from their long hours of monotonous and sedentary occupation, and from the artificial light, and the windless, overwarm and moist atmosphere. Many a girl cashier has worked from 8 to 8.30, and on Saturdays from 8 to 10, and then has had to balance her books and leave perhaps after midnight on Sunday morning. Her office is away in the background—confined, windless, artificially lit. The Shops Act has given a little relief from these hours. What, I ask, is the use of the State spending a million a year on sanatoria and tuberculin dispensaries, when those very conditions of work continue which lessen the immunity and increase the infection of the workers?

The jute industry in this town of Dundee is carried out almost wholly by female and boy labour. 'The average wages for women are below 12*s.* in eight processes, and above 12*s.*, but under 18*s.*, for the remaining five processes.' The infant mortality has been over 170 per 1,000. The Social Union of Dundee reported in 1905 that of 885 children born to 240 working mothers no fewer than 520, or 59 per cent., died—and almost all of them were under five years of age. The life of these mothers was divided between the jute factory and the one-roomed tenement. Looking such conditions squarely in the face, I say it would be more humane for the State to legalise the exposure of every other newborn infant on the hillside rather than allow children to be slowly done to death. The conditions, as given in the Report, contravene those rights of motherhood which the meanest wild animal can claim.

Isolation hospitals, sputum-pots, and anti-spitting regulations will not stamp out tuberculosis. Such means are like shutting the door of the stable when the horse has escaped. Flügge has shown that tubercle bacilli are spread by the droplets of saliva which are carried out as an invisible spray when we speak, sing, cough, sneeze. Sputum-pots cannot control this. The saliva of cases of phthisis may teem with the bacilli. The tuberculin reaction tests carried out by Hamburger and Monti in Vienna show that 94 per cent. of all children aged eleven to fourteen have been infected with tubercle. In most the infection is a mere temporary indisposition. I believe that the conditions of exhausting work, and amusement in confined and overheated atmospheres, together with ill-regulated feeding, determine largely whether the infection, which almost none can escape, become serious or not. Karl Pearson suggests that the death statistics afford no proof of the utility of sanatoria or tuberculin dispensaries, for during the very years in which such treatment has been in vogue, the fall in the mortality from tuberculosis has become less relatively to the fall in general mortality. He opines that the race is gradually becoming immune to tubercle, and hence the declination in the mortality curve is becoming flattened out —that Nature is paramount as the determinant of tuberculosis, not nurture. From a statistical inquiry into the incidence of tuberculosis in husband and wife and parent and child Pearson concludes that exposure to infection as in married couples is of little importance, while inborn immunity or diathesis is a chief determinant. Admitting the value of his critical inquiries and the importance of diathesis, I would point out that in the last few years the rush and excitement of modern city life has increased, together with the confinement of workers to sedentary occupations in artificially lit, warm, windless atmospheres. The same conditions pertain to places of amusement, eating-houses, tube railways, &c.

Central heating, gas-radiators, and other contrivances are now displacing the old open fire and chimney. This change greatly improves the economical consumption of coal and the light and cleanliness of the atmosphere. But in so far as it promotes monotonous, windless, warm atmospheres, it is wholly against the health and vigour of the nation. The open fire and wide chimney ensure ventilation, the indrawing of cold outside air, streaky air—restless currents at different temperatures, which strike the sensory nerves in the skin and prevent monotony and weariness of spirit. By the old open fires we were heated with radiant heat. The air in the rooms was drawn in cool and varied in temperature. The radiator and hot-air system give us a deadly uniformly heated air—the very conditions we find most unsupportable on a close summer's day.

In Labrador and Newfoundland, Dr. Wakefield tells me, the mortality of the fisherfolk from tuberculosis is very heavy. It is generally acknowledged to be four per 1,000 of the population per annum, against 1·52 for England and Wales. Some of the Labrador doctors talk of seven and even eight per 1,000 in certain districts. The general death-rate is a low one. The fishermen fish off shore, work for many hours a day in the fishing season, and live with their families on shore in one-roomed shanties. These shanties are built of wood, the crannies are 'stogged' with moss, and the windows nailed up, so that ventilation is very imperfect. They are heated by stoves and kept at a very high temperature, *e.g.*, 80° F. Outside in the winter the temperature may be 30 degrees below freezing. The women stay inside the shanties almost all their time, and the tuberculosis rate is somewhat higher in them. The main food is white bread, tea stewed in the pot till black, fish occasionally, a little margarine and molasses. The fish is boiled and the water thrown away. Game has become scarce in recent years; old, dark-coloured flour—spoken of with disfavour—has been replaced by white flour. In consequence of this diet beri-beri has become rife to a most serious extent, and the hospitals are full of cases. Martin Flack and I have found by our feeding experiments that rats, mice, and pigeons cannot be maintained on white bread and water, but can live on wholemeal, or on white bread in which we incorporate an extract of the sharps and bran in sufficient amount. Recent work has shown the vital importance of certain active principles present in the outer layers of wheat, rice, &c., and in milk, meat, &c., which are destroyed by heating to 120° C. A diet of white bread or polished rice and tinned food sterilised by heat is the cause of beri-beri. The metabolism is endangered by the artificial methods of treating foods now in vogue. As to the prevalency of tuberculosis in Labrador, we have to consider the intermarriage, the bad diet, the over-rigorous work of the fishermen, the overheating of, and infection in the shanties. Dr. Wakefield has slept with four other travellers in a shanty with father, mother, and ten children. In some there is scarce room on the floor to lie down. The shanties are heated with a stove on which pots boil all the time; water runs down the windows. The patients are ignorant, and spit everywhere, on bed, floor, and walls. In the schools the heat and smell is most marked to one coming in from the outside air. In one school 50 cubic feet per child is the allowance of space. The children are eating all day long, and are kept in close hot confinement. They suffer very badly from decay of the teeth. Whole families are swept off with tuberculosis, and the child who leaves home early may escape, while the rest of a family dies. Here, then, we have people living in the wildest and least populated of lands with the purest atmosphere suffering from all those ill-results which are found in the worst city slums—tuberculosis, beri-beri, and decayed teeth.

The bad diet probably impels the people to conserve their body heat and live in the over-warm, confined atmosphere, just as our pigeons fed on white bread sit, with their feathers out, huddled together to keep each other warm.

The metabolism, circulation, respiration, and expansion of the lung are all reduced. The warm, moist atmosphere lessens the evaporation from the respiratory tract, and therefore the transudation of tissue lymph and activity of the ciliated epithelium. The unexpanded parts of the lung are not swept with blood. Everything favours a lodgment of the bacilli, and lessens the defences on which immunity depends. In the mouth, too, the immune properties of the saliva are neutralised by the continual presence of food, and the temperature of the mouth is kept at a higher level, which favours bacterial growth. Lieutenant Siem informs me that recently in Northern Norway there has been the same notable increase in tuberculosis. The old cottage fireplaces with wide chimneys have been replaced with American stoves. In olden days most of the heat went up the chimney, and the people were warmed by radiant heat. Now the room is heated to a uniform moist heat. The Norwegians nail up the windows and never open them during the winter. At Lofoten, the great fishing centre, motor-boats have replaced the old open sailing and row boats. The cabin in the motor-boat is very confined, covered in with watertight deck, heated by the engine, crowded with a dozen workers. When in harbour the fishermen used to occupy ill-fitted shanties, through which the wind blew freely; now, to save rent, they sleep in the motor-boat cabins.

Here, again, we have massive infection, and the reduction of the defensive mechanisms by the influence of the warm, moist atmosphere.

The Norwegian fishermen feed on brown bread, boiled fish, salt mutton, margarine, and drink, when in money, beer and schnapps; there is no gross deficiency in diet, as in Labrador, and beri-beri does not attack them. They return home to their villages and longshore fishing when the season is over. The one new condition which is common to the two districts is confinement in stove-heated, windless atmospheres. In old days the men were crowded together, but in open boats or in draughty shanties, and had nothing but little cooking-stoves.

The conditions of great cities tend to confine the worker in the office all day, and to the heated atmosphere of club, cinema show, or music-hall in the evening. The height of houses prevents the town dweller from being blown upon by the wind, and, missing the exhilarating stimulus of the cool, moving air, he repels the dull uniformity of existence by tobacco and by alcohol, or by indulgence in food, *e.g.*, sweets, which are everywhere to his hand, and by the nervous excitement of business and amusement. He works, he eats, and is amused in warm, windless atmospheres, and suffers from a feeble circulation, a shallow respiration, a disordered digestion, and a slow rate of metabolism.

Many of the employments of modern days are detestable in their long hours of confinement and monotony. Men go up and down in a lift all day, and girls in the bloom of youth are set down in tobacco stalls in underground stations, and their health and beauty there fade while even the blow-flies are free to bask in the sun. In factories the operatives feed machines, or reproduce the same small piece of an article day after day. There is no art, or change, no pleasure in contrivance and accomplishment. The miner, the fisherman, even the sewerman, face difficulties, changing risks, and are developed as men of character and strength. Contrast the sailor with the steward on a steamer, the drayman outside with the clerk inside who checks the goods delivered at some city office, the butcher and the tailor, the seamstress and the market woman, and one sees the enormous difference which a confined occupation makes. Monotonous sedentary employment makes for unhappiness because the inherited functional needs of the human body are neglected, and education—when the outside field of interest is narrowed—intensifies the sensitivity to the bodily conditions. The sensations arising within the body—proprioceptive sensations—come to have too large a share in consciousness in comparison with exteroceptive. In place of considering the lilies how they grow, or musing on the beauty and motions of the heavenly bodies, the sedentary worker in the smoke-befouled atmosphere, with the limited activity and horizon of an office and a disturbed digestion, tends to become confined to the inward consideration of his own viscera and their motions.

Many of the educated daughters of the well-to-do are no less confined at home; they are the flotsam and jetsam cast up from the tide in which all others struggle for existence—their lives are no less monotonous than the sweated sempstress or clerk. They become filled with 'vapours' and some seek excitement not in the cannon's mouth but in breaking windows, playing with fire, and hunger strikes. The dull monotony of idle social functions, shopping and amusement no less than that of sedentary work and an asexual life, impels to a simulated struggle—a theatrical performance, the parts of which are studied from the historical romances of revolution. Each man, woman, and child in the world must find the wherewithal for living, food, raiment, warmth, and housing, or must die or get some other to find it for him. It seems to me as if the world is conducted as if ten men were on an island—a microcosm—and five sought for the necessaries of life, hunted for food, built shelters and fires, made clothes of skins, while the other five strung necklaces of shells, made loin-cloths of butterfly wings, gambled with knuckle-bones, drew comic pictures in the sand, or carved out of clay frightening demons, and so beguiled from the first five the larger share of their wealth. In this land of factories, while the many are confined to mean streets and wretched houses, possessing no sufficiency of baths and clean clothing, and are ill-fed, they work all day long, not to fashion for themselves better houses and clothing, but to make those unnecessaries such as 'the fluff' of women's apparel, and a thousand trifles which relieve the monotony of the idle and bemuse their own minds.

The discovery of radium and its disintegration as a source of energy has enabled the physicist to extend Lord Kelvin's estimate of the world's age from some thirty to a thousand million years. Arthur Keith does not hesitate to give a million of these years to man's evolution. Karl Pearson speaks of hundreds of thousands of years. The form of the human skull, the brain capacity of man, his skill as evidenced by stone implements and cave drawings of animals in action, was the same tens of thousands of years ago as now. For ages primitive man lived as a wild animal in tropical climes, discovered how to make fire, clothe himself in skins, build shelters, and so enable himself to wander over the temperate and arctic zones. Finally, in the last few score of years, he has made houses draughtless with glass windows, fitted them with stoves and radiators, and every kind of device to protect himself from cold, while he occupies himself in the sedentary pursuits and amusements of a city life. How much better, to those who know the boundless horizon of life, to be a frontiers-man and enjoy the struggle, with body hardened, perfectly fit, attuned to Nature, than to be a cashier condemned to the occupation of a sunless, windless pay-box. The city child, however, nurtured and educated in confinement, knows not the largeness and wonders of Nature, is used to the streets with their ceaseless movement and romantic play of artificial light after dark, and does not need the commiseration of the country mouse any more than the beetle who lives in the dark and animated burrows of his heap. But while outdoor work disciplines the body of the countryman into health, the town man needs the conscious attention and acquired educated control of his life to give him any full measure of health and happiness.

Experimental evidence is strongly in favour of my argument that the chemical purity of the air is of no importance. Analyses show that the oxygen in the worst-ventilated school-room, chapel, or theatre is never lessened by more than 1 per cent. of an atmosphere; the ventilation through chink and cranny, chimney, door, and window, and the porous brick wall, suffices to prevent a greater diminution. So long as there is present a partial pressure of oxygen sufficient to change the hæmoglobin of the venous blood into oxyhæmoglobin there can arise no lack of oxygen.

At sea-level the pressure of oxygen in the pulmonary alveolar air is about 100 mm. Hg. Exposed to only half this pressure the hæmoglobin is more than 80 per cent. saturated with oxygen.

In noted health-resorts of the Swiss mountains the barometer stands at such a height that the concentration of oxygen is far less than in the more ventilated room. On the high plateau of the Andes there are great cities : Potosi with a hundred thousand inhabitants is at 4,165 metres, and the partial pressure of oxygen there is about 13 per cent. of an atmosphere in place of 71 per cent. at sea-level; railways and mines have been worked up to altitudes of 14,000 to 15,000 feet. At Potosi girls dance half the night, and toreadors display their skill in the ring. On the slopes of the Himalayas shepherds take their flocks to altitudes of 18,000 feet. No disturbance is felt by the inhabitants or those who reach these great altitudes slowly and by easy stages. The only disability to a normal man is diminished power for severe exertion, but a greater risk arises from want of oxygen to cases of heart disease, pneumonia, and in chloroform anæsthesia at these high altitudes. The newcomer who is carried by the railway in a few hours to the top of Pike's Peak or the Andes may suffer severely from mountain sickness, especially on exertion, and the cause of this is want of oxygen. Acclimatisation is brought about in a few days' time. The pulmonary ventilation increases, the bronchial tubes dilate, the circulation becomes more rapid. The increased pulmonary ventilation lowers the partial pressure of carbon dioxide in the blood and pulmonary air, and this contributes to the maintenance of an adequate partial pressure of oxygen. Haldane and Douglas say that the percentage of red corpuscles and total quantity of the hæmoglobin increases, and maintain that the oxygen is actively secreted by the lung into the blood, but the C method by which their determinations have been made has not met with unqualified acceptance. If waste products, which arise from oxygen want, alter the combining power of hæmoglobin, this alteration may not persist in shed blood; for these products may disappear when the blood is exposed to air. Owing to the combining power of hæmoglobin the respiratory exchange and metabolism of an animal within wide limits is independent of the partial pressure of oxygen. On the other hand, the process of combustion is dependent not on the pressure but on the percentage of oxygen. Thus the aeroplanist may become seized with altitude sickness from oxygen want, while his gas engine continues to carry him to loftier heights.

The partial pressure of oxygen in a mine at a depth of 3,000 feet is considerably higher than at sea-level, and if the percentage is reduced to 17, while the firing of fire-damp and coal dust is impossible, there need be in the alveolar air of the lungs no lower pressure of oxygen than at sea-level. Thus the simplest method of preventing explosions in coal mines is that proposed by J. Harger, viz., to ventilate them with air containing 17 per cent. of oxygen.[1] There is little doubt that all the great mine-explosions have been caused by the enforcement of a high degree of chemical purity of the air. In the old days when ventilation was bad there were no great dust explosions. Mr. W. H. Chambers, general manager of the Cadeby mine, where the recent disastrous explosion occurred, with the authority of his great and long practical experience of fiery mines, told me that the spontaneous combustion of coal and the danger of explosion can be wholly met by adequate diminution in ventilation. The fires can be choked out while the miners can still breathe and work. The Coal Mines Regulation Act enforces that a place shall not be in a fit state for working or passing therein, if the air contains either less than 19 per cent. of oxygen, or more than $1\frac{1}{4}$ per cent. of carbon dioxide. A mine liable to spontaneous combustion of coal may be exempted from this regulation by order of the Secretary of State.

The regulations impel the provision of such a ventilation current that the percentage of oxygen is sufficient for the spread of dust explosions along the intake airways, with the disastrous results so frequently recorded. If the mine were ventilated with air containing 17 per cent. of oxygen in sufficient volume to keep the miners cool and fresh, not only would explosions be prevented but the mines could be safely worked and illuminated with electricity, and miners'

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[1] *Trans. Inst. of Mining Engineers*, 1912.

nystagmus prevented, for this is due to the dim light of the safety lamp. The problem possibly may be solved by purifying and cooling the return air, and mixing and circulating this with a sufficiency of fresh air.

Owing to the fact that the percentage of CO_2 is the usual test of ventilation and that only a very few parts per 10,000 in excess of fresh air are permitted by the English Factory Acts, it is generally supposed that CO_2 is a poison and that any considerable excess has a deleterious effect on the human body. No supposition could be further from the truth.

The percentage of CO_2 in the worst ventilated room does not rise above 0·5 per cent., or at the outside 1 per cent. It is impossible that any excess of CO_2 should enter into our bodies when we breathe such air, for whatever the percentage of CO_2 in the atmosphere may be, that in the pulmonary air is kept constant at about 5 to 6 per cent. of an atmosphere—by the action of the respiratory centre. It is the concentration of CO_2 which rules the respiratory centre, and to such purpose as to keep the concentration both in the lungs and in the blood uniform (Haldane); the only result from breathing air containing 0·5 to 1 per cent. of CO_2 is an inappreciable increase in the ventilation of the lungs. The very same thing happens when we take gentle exercise and produce more CO_2 in our bodies.

At each breath we rebreathe into our lungs the air in the nose and large air-tubes (the dead-space air), and about one-third of the air which is breathed in by a man at rest in dead-space air. Thus, no man breathes in pure outside air into his lungs. When a child goes to sleep with its head partly buried under the bed-clothes, and in a cradle confined by curtains, he rebreathes the expired air to a still greater extent, and so with all animals that snuggle together for warmth's sake. Not only the new-born babe sleeping against its mother's breast, but pigs in a sty, young rabbits, rats, and mice clustered together in their nests, young chicks under the brooding hen, all alike breathe a far higher percentage than that allowed by the Factory Acts.

To rebreathe one's own breath is a natural and inevitable performance, and to breathe some of the air exhaled by another is the common lot of men who, like animals, have to crowd together and husband their heat in fighting the inclemency of the weather.

In the Albion Brewery we analysed on three different days the air of the room where the CO_2 generated in the vats is compressed and bottled as liquid carbonic acid. We found from 0·14 to 0·93 per cent. of CO_2 in the atmosphere of that room. The men who were filling the cylinders and turning the taps on and off to allow escape of air must often breathe more than this. The men engaged in this occupation worked twelve-hour shifts, having their meals in the room. Some had followed the same employment for eighteen years, and without detriment to their health. It is only when the higher concentrations of CO_2 are breathed, such as 3 to 4 per cent. of an atmosphere, that the respiration is increased, so that it is noticeable to the resting individual; but percentages over 1 per cent. diminish the power to do muscular work, for the excess of CO_2 produced by the work adds its effect to that of the excess in the air, and the difficulty of co-ordinating the breathing to the work in hand is increased.

Haldane and Priestley found that with a pressure of 2 per cent. of an atmosphere of CO_2 in the inspired air the pulmonary ventilation of a man at rest was increased 50 per cent., with 3 per cent. about 100 per cent., with 4 per cent. about 200 per cent., with 5 per cent. about 300 per cent., and with 6 per cent. about 500 per cent. With the last, panting is severe, while with 3 per cent. it is unnoticed until muscular work is done, when the panting is increased 100 per cent. more than usual. With more than 6 per cent. the distress is very great, and headache, flushing, and sweating occur.

Divers who work in diving dress and men who work in compressed air caissons constantly do heavy and continuous labour in concentrations of CO_2 higher than 1 per cent. of an atmosphere, and so long as the CO_2 is kept below 2 to 3 per cent. they are capable of carrying out efficient work. In the case of workers in compressed air it is important to bear in mind that the effect of the CO_2 on the breathing depends on the partial pressure and not on the percentage of this gas in the air breathed.

By a series of observations made on rats confined in cages fitted with small, ill-ventilated sleeping-chambers, we have found that the temperature and humidity of the air—not the percentage of carbon dioxide or oxygen—determines whether the animals stay inside the sleeping-room or come outside. When the air is cold, they like to stay inside, even when the carbon dioxide rises to 4 to 5 per cent. of an atmosphere. When the sleeping-chamber is made too hot and moist they come outside.

The sanitarian says it is necessary to keep the CO_2 below 0·01 per cent., so that the organic poisons may not collect to a harmful extent. The evil smell of crowded rooms is accepted as unequivocal evidence of the existence of such. He pays much attention to this and little or none to the heat and moisture of the air. The smell arises from the secretions of the skin, soiled clothes, &c. The smell is only sensed by and excites disgust in one who comes to it from the outside air. He who is inside and helps to make the 'fugg' is both wholly unaware of, and unaffected by it. Flügge points out, with justice, that while we naturally avoid any smell that excites disgust and puts us off our appetite, yet the offensive quality of the smell does not prove its poisonous nature. For the smell of the trade or food of one man may be horrible and loathsome to another not used to such.

The sight of a slaughterer and the smell of dead meat may be loathly to the sensitive poet, but the slaughterer is none the less healthy. The clang and jar of an engineer's workshop may be unendurable to a highly strung artist or author, but the artificers miss the stoppage of the noisy clatter. The stench of glue-works, fried-fish shops, soap and bone-manure works, middens, sewers, become as nothing to those engaged in such, and the lives of the workers are in no wise shortened by the stench they endure. The nose ceases to respond to the uniformity of the impulse, and the stench clearly does not betoken in any of these cases the existence of a chemical organic poison. On descending into a sewer, after the first ten minutes the nose ceases to smell the stench; the air therein is usually found to be far freer from bacteria than the air in a schoolroom or tenement.

If we turn to foodstuffs we recognise that the smell of alcohol and of Stilton or Camembert cheese is horrible to a child, while the smell of putrid fish—the meal of the Siberian native—excites no less disgust in an epicure, who welcomes the cheese. Among the hardiest and healthiest of men are the North Sea fishermen, who sleep in the cabins of trawlers reeking with fish and oil, and for the sake of warmth shut themselves up until the lamp may go out from want of oxygen. The stench of such surroundings may effectually put the sensitive, untrained brain worker off his appetite, but the robust health of the fisherman proves that this effect is nervous in origin, and not due to a chemical organic poison in the air.

Ventilation cannot get rid of the source of a smell, while it may easily distribute the evil smell through a house. As Pettenkofer says, if there is a dung-heap in a room, it must be removed. It is no good trying to blow away the smell.

Flügge and his school bring convincing evidence to show that a stuffy atmosphere is stuffy owing to heat stagnation, and that the smell has nothing to do with the origin of the discomfort felt by those who endure it. The inhabitants of reeking hovels in the country do not suffer from chronic ill-health, unless want of nourishment, open-air exercise, or sleep come into play. Town workers who take no exercise in the fresh air are pale, anæmic, listless. Sheltered by houses they are far less exposed to winds, and live day and night in a warm, confined atmosphere.

The widespread belief in the presence of organic poisons in the expired air is mainly based on the statements of Brown Sequard and D'Arsenval, statements wholly unsubstantiated by the most trustworthy workers in Europe and America. These statements have done very great mischief to the cause of hygiene, for they led ventilating engineers and the public to seek after chemical purity, and neglect the attainment of adequate coolness and movement of the air. It was stated that the condensation water obtained from expired air is poisonous when injected into animals. The evidence on which this statement is based is not only not worthy of credence but is absurd, *e.g.*, condensation water has been injected into a mouse in a quantity equivalent to injecting 5 kilogrammes into a man weighing 60 kilogrammes. No proper controls were carried out. It is recognised now that any distilled water contaminated by bacterial products may have a toxic effect. Flack and I have for fourteen weeks kept guinea-pigs and rats confined together in a box and poorly ventilated, so that they breathed air containing 0·5 to 1·0 per cent. of CO_2. The guinea-pigs proved wholly free from anaphylactic shock on injecting rats' serum. Therefore they were not sensitised by breathing the exhaled breath of the rats for many weeks, and we are certain that no foreign protein substance is absorbed in this way. It has been proved by others, and by us, that animals so confined do well so long as they are well fed and their cages kept clean, light, cool, and dry. It is wholly untrue that they are poisoned by breathing each other's breath. The only danger arises from droplet contagion in cases of infective disease.

To study the relative effect of the temperature and chemical purity of the atmosphere I constructed a small experimental chamber of wood fitted with large glass observation windows and rendered air-tight.

On one side of the chamber were fixed two small electric heaters, and a tin containing water was placed on these in order to saturate the air with water vapour. On another side of the chamber was placed a large radiator through which cold water could be circulated when required, so as to cool the chamber. In the roof were fixed three electric fans, one big and two small, by means of which the air of the chamber could be stirred. The chamber held approximately 3 cm. of air. In one class of experiments we shut within the chamber seven or eight students for about half an hour, and observed the effect of the confined atmosphere upon them. We kept them until the CO_2 reached 3 to 4 per cent., and the oxygen had fallen to 17 to 16 per cent. The wet-bulb temperature rose meanwhile to about 80° to 85° F., and the dry bulb a degree or two higher. The students went in chatting and laughing, but by-and-by, as the temperature rose, they ceased to talk and their faces became flushed and moist. To relieve the monotony of the experiment we have watched them trying to light a cigarette, and, puzzled by their matches going out, borrowing others, only in vain. They had not sensed the diminution of oxygen, which fell below 17 per cent. Their breathing was deepened by the high percentage of CO_2, but no headache occurred in any of them from the short exposure. Their discomfort was relieved to an astonishing extent by putting on the electric fans placed in the roof. Whilst the air was kept stirred the students were not affected by the oppressive atmosphere. They begged for the fans to be put on when they were cut off. The same old stale air containing 3 to 4 per cent. CO_2 and 16 to 17 per cent. O_2 was whirled, but the movement of the air gave relief, because the air was 80° to 85° F. (wet bulb), while the air enmeshed in their clothes in contact with their skin was 98° to 99° F., wet bulb. If we outside breathed through a tube the air in the chamber we felt none of the discomfort which was being experienced by those shut up inside. Similarly, if one of those in the chamber breathed through a tube the pure air outside he was not relieved.

R. A. Rowlands and H. B. Walker carried out a large number of observations in the chamber, each acting as subject in turn.

They recorded the effect on the respiratory ventilation and on the pulse rate both when resting and when working. The work consisted in pulling a 20-kilo. weight about 1 metre high by means of a pulley and rope.

In some of the experiments the exhaled carbonic acid was absorbed, and in others carbonic acid was put into the chamber. The subjects inside could not tell when the gas was introduced, not even if the percentage were suddenly raised by 2. The introduction of this amount of the gas made no sensible difference to them, but increased their pulmonary ventilation.

In every one of the experiments they suffered from the heat, and the putting on of the fans gave great relief, and in particular diminished the pulse rate during and after the working periods. The relief became much greater when cold water was circulated through the radiator and the temperature of the chamber lowered 10° F.

The subjects wore only a vest, pants, and shoes in most of these experiments. When they wore their ordinary clothing the effect on the frequency of the pulse was more marked and the discomfort from heat and moisture much greater.

I have made observations on men dressed in the Fleuss rescue apparatus for use in mines, and exposed in a chamber to 120° F. dry dulb and 95° F. wet bulb. The skin temperature rises to the rectal temperature and the pulse is greatly accelerated—*e.g.*, to 150—and there arises danger of heat stroke. The conditions are greatly relieved by interposing on the inspiratory tube of the apparatus a cooler filled with carbonic-acid snow. The cool inspired air lowers the frequency of the heart and makes it possible for the men to do some work at 95° F. wet bulb, and to endure this temperature for two hours.

The observations made by Pembrey and Collis on the weaving-mill operatives at Darwen show that the skin of the face may be 4° to 13° F. higher in the mill when the wet bulb is 71° F. than at home when the wet-bulb temperature is about 55° F. The tendency of the warm, humid atmosphere of the mill is to establish a more uniform temperature of the body as a whole (surface and deep temperatures) and to throw a tax upon the power of accommodation as indicated by the rapid pulse and low blood-pressure.

The mill-workers are wet with the steam blown into the sheds, their clothes and bodies are moist, and the long hours of exposure to such uncomfortable conditions are most deleterious to physical vigour and happiness. The operatives asked that they might be allowed to work without steam-injectors and with diminished ventilation, so that the mill rooms became saturated with moisture evaporated from the bodies of the operatives. The old regulations, while forbidding more than 6 parts in 10,000 CO_2, put no limit to the wet-bulb temperature, and this often became excessive on hot summer days. The operatives were quite right. Less ventilation and a lower wet bulb is far better than ample ventilation and a high wet bulb. The permissible limit of CO_2 has now been raised to 11 parts in 10,000, and the wet-bulb temperature is to be controlled within reasonable limits.

The efficiency of workers in mills, mines, tunnels, stoke-holes, &c., is vastly increased by the provision of a sufficient draught of cool and relatively dry air, so as to prevent over-taxing of the heat-regulating mechanism. Mr. F. Green informs me that by means of forced draught the stoke-hole of an Orient steamer is rendered the coolest place when the ship is in the tropics.

The electric fan has vastly improved the conditions of the worker in the tropics. I would suggest that each clerk should have a fan just as much as a lamp on his desk. It will pay the employer to supply fans.

In the modern battleship men are confined very largely to places artificially lit and ventilated by air driven in by fans through ventilating-shafts. The heat and moisture derived from the bodies of the men, from the engines, from cooking-ranges, &c., lead to a high degree of relative moisture, and thus all parts of the ironwork inside are coated with granulated cork to hold the condensed moisture and prevent dripping.

The air smells with the manifold smells of oil, cooking, human bodies, &c., and the fresh air driven in by fans through the metal conduits takes up the smell of these, and is spoken of by the officers with disparagement as 'tinned' or 'potted' air. This air is heated when required by being made to pass over radiators. Many of the officers' cabins and offices for clerks, typewriters, &c., in the centre of a battleship, have no portholes, and are only lit and ventilated by artificial means. The steel nature of the structure prevents the diffusion of air which takes place so freely through the brick walls of a house. The men in their sleeping quarters are very closely confined, and as the openings of the air-conduits are placed in the roof between the hammocks, the men next to such openings receive a cold draught and are likely to shut the openings. To sleep in a warm moist 'fugg' would not much matter if the men were actively engaged for many hours of the day on deck and there exposed to the open air and the rigours of sea and weather. In the modern warship most of the crew work for many hours under deck, and some of the men may scarcely come on deck for weeks or even months. Considering the conditions which pertain, it seems to be of the utmost importance that all the men in a battleship should be inspected at short intervals by the medical officers so that cases of tuberculosis may be weeded out in their incipiency. The men of every rating should do deck drill for some part of every day. In the Norwegian navy every man, cooks and all, must do gymnastic drill on deck once a day. In the case of our navy, with voluntary service, the men should welcome this in their own interest.

In a destroyer visited by me twelve men occupied quarters containing about 1,700 cubic feet of air. There was a stove with iron pipe for chimney, from which fumes of combustion must leak when in use, and a fan which would not work. When the men are shut down the moisture is such that boots, &c., go mouldy, and the water dips off the structure. The cooling effect of the sea-water washing over the steel shell of the boat is beneficial in keeping down the temperature in these confined and ill-ventilated quarters. On the manœuvring platform in the engine-room the wet-bulb temperature reaches a very high degree owing to the slight escape of steam round the turbines. Commander Domvile was kind enough to send me the wet and dry bulb temperatures taken there on a number of days. The wet bulb was found to be never below 80° F., sometimes reached 95° and even 98° F. It is impossible for officers to work at these temperatures without straining the heat-regulating mechanism of the body and diminishing their health and working capacity. If such wet-bulb temperatures are unavoidable, means should be provided, such as fans, which would alleviate the discomfort and fatigue caused thereby. A supply of compressed air fitted with a nozzle might be arranged and used occasionally to douche the body with cool air. I have tried this plan and found it very effectual, and can recommend the compressed-air bath as the substitute for a bracing cold wind.

The suitability of the clothing is of the greatest importance, not only to the comfort but to the efficiency of man as a working machine, *e.g.*, power of soldiers to march. On a still day the body is confined by the clothes as if by a chamber of stagnant air, for the air is enclosed in the meshes of the clothes and the layer in contact with the skin becomes heated to body temperature and saturated with moisture.

The observations of Pembrey show that himself and four soldiers, marching in drill order on a moderately warm day, lost more water and retained more water in their clothes than on another similar day when they worked with no jacket on. The average figures were loss of moisture 1,600, against 1,200 grms., and water retained in clothes 254, against 109 grms. With no jacket the pulse was, on the average, increased 28 against 41 in drill order, and rectal temperature 1° against 1°·5 F. The taking off of the jacket or throwing open of the jacket and vest very greatly increase the physiological economy of a march. It is absurd that on a hot summer day Boy Scouts should march with a coloured scarf knotted round their necks. Nothing should be worn for ornament or smartness which increases the difficulty of keeping down the body temperature. The power to march and the efficiency of an army depend on prevention of heart stagnation and avoidance of fatigue of the heart.

I conclude, then, that all the efforts of the heating and ventilating engineer should be directed towards cooling the air in crowded places and cooling the bodies of the people by setting the air in motion by means of fans. In a crowded room the air confined between the bodies and clothes of the people is almost warmed up to body temperature and saturated with moisture, so that cooling of the body by radiation, convection and evaporation becomes reduced to a minimum. The strain on the heat-regulating mechanism tells on the heart. The pulse is accelerated, the blood is sent in increased volume to the skin, and circulates there in far greater volume, while less goes through the viscera and brain. As the surface temperature rises, the cutaneous vessels dilate, the veins become filled, the arteries may become small in volume, and the blood-pressure low, the heart is fatigued by the extra work thrown upon it. The influence of the heat stagnation is shown by the great acceleration of the pulse when work is done and the slower rate at which the pulse returns to its former rate on resting.

The increased percentage of carbonic acid and diminution of oxygen which has been found to exist in badly ventilated churches, schools, theatres, barracks, is such that it can have no effect upon the incidence of respiratory disease and higher death-rate, which statistical evidence has shown to exist among persons living in crowded and unventilated rooms. The conditions of temperature, moisture, and windless atmosphere in such places primarily diminishes the heat loss, and secondarily the heat production, *i.e.*, the activity of the occupants, together with total volume of air breathed, oxygen taken in and food eaten. The whole metabolism of the body is thus run at a lower plane, and the nervous system and tone of the body is unstimulated by the monotonous, warm, and motionless air. If hard work has to be done it is done under conditions of strain. The number of pathogenic organisms is increased in such places, and these two conditions run together—diminished immunity and increased mass influence of infecting bacteria.

The volume of blood passing through, and of water vapour evaporated from, the respiratory mucous membrane must have a great influence on the mechanisms which protect this tract from bacterial infection. While too wet an atmosphere lessens evaporation, a hot dry atmosphere dries up the mucous membrane. As the immunising powers depend on the passage of blood plasma into the tissue spaces, it is clear that a proper degree of moisture is important. The temperature, too, must have a great influence on the scavenger activity of the ciliated epithelium and leucocytes in the mucous membrane of the nose.

In the warm moist atmosphere of a crowded place the infection from spray, sneezed, coughed, or spoken out, is enormous. On passing out from such an atmosphere into cold moist air the respiratory mucous membrane of the nose is suddenly chilled, the blood-vessels constricted, and the defensive mechanism of cilia and leucocyte checked. Hence the prevalence of colds in the winter. In the summer the infection is far less. We are far more exposed to moving air, and the sudden transition from a warm to a cold atmosphere does not occur. We believe that infection is largely determined by (1) the mass influence of the infecting agent; (2) the shallow breathing and diminished evaporation from, and flow of tissue lymph through, the respiratory tract, in warm, moist confined air. Colds are not caught by exposure to cold *per se*, as is shown by the experience of Arctic explorers, sailors, shipwrecked passengers, &c.

We have very great inherent powers of withstanding exposure to cold. The bodily mechanisms become trained and set to maintain the body heat by habitual exposure to open-air life. The risk lies in overheating our dwellings and overclothing our bodies, so that the mechanisms engaged in resisting infection become enfeebled, and no longer able to meet the sudden transition from the warm atmosphere of our rooms to the chill outside air of winter. The dark and gloomy days of winter confine us within doors, and, by reducing our activity and exposure to open air, depress the metabolism; the influence of smoke and fog, gloom of house and streets, cavernous places of business and dark dwellings, intensify the depression. The immunity to a cold after an infection lasts but a short while, and when children return, after the summer holidays, to school and damp chill autumn days, infection runs around. The history of hospital gangrene and its abolition by the aseptic methods of Lister—likewise the history of insect-borne disease—show the great importance of cleanliness in crowded and much occupied rooms. The essentials required of any good system of ventilation are then (1) movement, coolness, proper degree of relative moisture of the air; (2) reduction of the mass influence of pathogenic bacteria. The chemical purity of the air is of very minor importance, and will be adequately insured by attendance to the essentials.

As the prevention of spray (saliva) infection by ventilation is impossible in crowded places, it behoves us to maintain our immunity at a high level. We may seek to diminish the spray output of those infected with colds by teaching them to cough, sneeze, and talk with a handkerchief held in front of the mouth, or to stay at home until the acute stage is past.

In all these matters nurture is of the greatest importance, as well as Nature. A man is born with physical and mental capacities small or great, with inherited characteristics, with more or less immunity to certain diseases, with a tendency to longevity of life or the opposite, but his comfort and happiness in life, the small or full development of his physical and mental capacities, his immunity and his longevity of life, are undoubtedly determined to a vast extent by nurture.

By nurture—use the word in its widest sense to include all the defensive methods of sanitary science—plague, yellow fever, malaria, sleeping-sickness, cholera, hospital gangrene, &c., can be prevented by eliminating the infecting cause: smallpox and typhoid by this means, and also by vaccination; and most of the other ills which flesh is supposed to be heir to can be kept from troubling by approximating to the rules of life which a wild animal has to follow in the matter of a simple, and often spare diet, hard exercise, and exposure to the open air.

There is nothing more fallacious than the supposition commonly held that over-feeding and over-coddling indoors promotes health. The two together derange the natural functions of the body. He who seeks to save his life will lose it.

The body of a new-born babe is a glorious and perfect machine, the heritage of millions of years of evolution.

> 'Not in entire forgetfulness,
> And not in utter nakedness,
> But trailing clouds of glory do we come.
>
> . . . . .
>
> Shades of the prison house begin to close
> Upon the growing Boy.'

The ill-conditioned body, anæmic complexion and undersized muscles, or the fat and gross habit, the decay of the teeth, the disordered digestion, the nervous irritability and unhappiness are the result of 'Nurture'—not Nature.

In institutions children may be disciplined to vigorous health. After leaving school they are set adrift to face monotonous work in confined places, amusement in music-halls and cinema shows in place of manly exercise in the open air, injudicious diet, alcohol, and tobacco—everything which the trainer of an athlete would repel.

> 'And custom lie upon thee with a weight
> Heavy as frost, and deep almost as life.'

British Association for the Advancement of Science.

SECTION K: DUNDEE, 1912.

ADDRESS

TO THE

BOTANICAL SECTION

BY

PROFESSOR FREDERICK KEEBLE, Sc.D.,

PRESIDENT OF THE SECTION.

It is with more than the normal trepidation natural to presidents that I, who have worked on the border-lines of several biological sciences, undertake the task of addressing the members of this Section. As well might a rogue and snapper-up of unconsidered trifles recite his doggerel songs before a bench of learned magistrates.

Therefore, although I have studied from their works the ways of presidents, and although I shall strive to keep to the path which they have mostly trod, yet should I stray I plead with Autolycus that—

> 'When I wander here and there,
> I then do most go right.'

The addresses which I have consulted show me two alternative models.

I may take all knowledge for my province and discourse on the progress of our science as a whole. This is Ercles' vein, a tyrant's vein. Or as a lover of a department of the science and more condoling, I may confine my Address to a special branch of Botany. Each method has its merits and its drawbacks, and the one is corrective of the other.

The departmental method depicts the tree of knowledge in sympodial symmetry. The branch which the president of one year holds out for our inspection is seen arising from an erstwhile dormant lateral bud far back from the growing point of the branch exhibited by his predecessor. Under the magic of the presidential hands the new branch grows as grows the enchanted mango. Like the lean kine it eats up the fat kine, and by the end of the Address it dominates all other branches.

The general method shows the tree in other guise. As an artist is wont to paint a tree, so the historian draws it on monopodial lines, with branches standing in due subservience to the leader and in strict co-ordination with one another. Together these methods tell the truth, which is that the tree of knowledge grows, like many another broad-leaved tree, by a mixed process of monopodial sequences following upon sympodial developments.

What is to the specialist, and indeed for a space is, the luxuriant predominance of his branch appears in historical perspective but as a new lateral for the extension of all the sub-lateral shoots of science.

Such a new basis for the further growth of all the branches of Botany is provided by the lusty shoot of Mendelism, and after weighing the alternatives, and with the reserves announced already, I propose to try to show that this recent outgrowth of the tree of knowledge is destined not to mar its symmetry, but rather to aid the growth of the whole crown. This, my chief task, should have been my first care had not an event occurred since the last meeting of this Association which compels me, in common with all botanists, to divert thought from its preoccupation and to look back along the route which our science has travelled during the last few decades.

That event, I need not say, was the death of Sir Joseph Hooker, a former president of this Association and twice president of this Section. The most venerable and distinguished of British botanists, Sir Joseph Hooker was well-nigh the last survivor of that band of Victorian naturalists who helped to lay the foundations of biology and to disseminate broadcast the knowledge which they made. The story of the labours of that group of naturalists—Lyell, Darwin, the Hookers, Wallace, Huxley, Galton, and others scarcely less distinguished—has been told so often that there is no need to retell it now. Nor need I recount the work of Hooker. His discoveries are known and require no re-enumeration. They are incorporated with the common fund of knowledge. British botanists will determine, doubtless, to consecrate a special occasion to the commemoration of Hooker's services to science and to the perpetuation of his memory. My duty it is to express, on behalf of native botanists and of our guests who honour us with their presence, our sense of loss in the death of Sir Joseph Hooker and our admiring recognition of his achievements.

And with the example of that long life devoted until its latest hour to the pursuit of science I would fain address myself forthwith to my special task; but despite my will I find my thoughts enchained in the contemplation of the life and times of Hooker. Systematist, explorer, critic, writer, administrator, Sir Joseph was first and last a botanist. The versatile Hooker was a specialist.

Thus I find myself turned again to the thoughts which vexed my mind at the outset of this Address, urged now to ask outright whether the specialisation of our times has the quality which distinguished that of Hooker and his contemporaries.

This is the uneasy phantom that has been haunting me and luring me to the ramparts when I should be wooing my chosen theme. It haunts me, refusing to be laid. Reason fails to exorcise that ghost. Its uneasy presence lingers near me even though I conjure it with specious arguments; urging that these days are days of specialisation *à outrance*: that nowadays both in the art and practice part of life we live by the intensive cultivation of small-holdings; that the fields of science are parcelled out in small allotments. Were I—a simple officer—the sole subject of this visitation I should attribute it to fantasy, and with Horatio cry 'Tush!' but beside this poor Bernardo, Marcellus, officer and scholar, has likewise seen it 'in the same figure like the King that's dead,' and who may refuse to entertain a ghost presenting this—the highest of credentials?

Therefore I offer it again my arguments, insisting that at least among our elders we have specialists as versatile as any of the Victorians. The ghost is not impressed. Instead, it rises to a fuller height, and lays its incorporeal finger on the row of volumes which line the shelves above my head. My obsequious eye follows the direction, and beholds Lyell's 'Principles,' Darwin's 'Voyage,' Hooker's 'Journal,' Huxley's 'Essays,' Wallace's 'Island Life,' Galton's 'Natural Inheritance,' and the other classics from his clients' pens. With the dawn of my comprehension the spectre vanishes, and I am alone, but not in peace. The message left with me appears to translate as follows: The present generation has become expert in intensive cultivation of scientific knowledge, but it has forgotten how to market its produce. In the preoccupation of specialisation it neglects the art of expression. It sinks the artist in the artizan. Each specialist exchanges 'separates'—hateful term—with other specialists, but few among us are on speaking terms with the cultured general public curious to know what science is achieving.

The translation into common English of our scientific works is done, like that of foreign classics, too much by hacks and amateurs, and too little by skilled hands. The present generation lets its modesty wrong it; for the science of our day is no less full—nay, many times more full—of interest and wonder than that of fifty years ago.

Still worse: to fail to cultivate the art of expression is to blunt the power of thinking, for the adage 'clear thinking means clear writing' stands though the subject and object be transposed.

Such is the nature of the charge which my visitant left with me; and though as it must have known, my rough translation fails to convey the sober grace of the original, I think that I shall not be alone in pleading guilty to that charge.

Nor perhaps will my fellow-specialists resent an attempt to trace the origin of our lack of literary grace. This defect is in part inevitable and in part remediable. Inevitable because of the increasingly engrossing nature of scientific investigation, because of the relatively small natural gift of expression which nature has vouchsafed to the English race, and because, as science becomes more complex, its followers think more and more in symbols, and those who think in symbols are apt to write in shorthand. The defect is remediable because it is traceable in some measure to the training to which we submit our youth. That training neglects too much the literary side of education.

As it seems to me, there is a fundamental error in our mode of training men of science. The error consists in this: that students who come to English Universities are treated in intellectual matters not as youths but as men of mature minds. The professorial potter takes the clay as he finds it, and, no matter what its state, fires it forthwith, and lo! in course of time it is converted into earthenware. Were the assumption on which he acts well-founded, the method might be justified. If our undergraduates were, as we assume they are, well found in general culture, trained already in scientific method, familiar with the language of our fathers, and apt also to read and speak and write some other tongue, then let us take them straightway and bake them in the oven of specialisation.

But I at all events have never met those students, and, outside the ranks of genius—which training toucheth not—I believe they do not exist. The error, as I conceive it, lies in our failure to apply, in drafting schemes of training, the biological law that as society grows older its young men grow younger. Undergraduates call themselves men, not solely from a sense of pride, but also in obedience to tradition. Centuries ago they went up to the University as men of fifteen or sixteen; now they go up as youths of eighteen or nineteen. With respect to moral discipline we are not unforgetful of their youth, but with respect to intellectual education we treat them as though they were grown up. Even the saving second subject has, I am told, been discarded from the final honours course. Let me give an example in illustration of our methods. It is found that a student in his second or third year knows no German, and we advise him to learn it. But in what a way, with our tacit approval, does he set about the task! So that he may tear the meaning from a scientific text as John Ridd clutched the arm of Carver Doone and tore the muscle out of it as the string comes out of an orange!

This barbarism we permit, because we know that it is no barbarism but expediency for a trained workman to take up any tool he needs and to use it as he wills. In the elegant language of modern literature 'and what he thought he most required he went and took the same as we.'

Yet, unless we hold that mental training is a scholastic fiction and that the teachers' sole business is to supply carefully selected and copious provender for the stuffing of students like Surrey fowls, it must be our care to encourage general as well as special culture in our students.

A further criticism which I have to make upon our University methods will seem to some far-fetched. We are prone to forget that the twin gifts of youth are enthusiasm and idleness. The former we encourage, but the latter, falling within the category of morals, we visit with our displeasure. There is, however an idleness which is not laziness, but a resting period of the organism tired with the trouble of growing up. I could wish that our English Universities understood intellectual liberty as well as German Universities understand it. We are apt to mind our sheep too much, and to overrate the virtue of docility.

I would plead for more breadth and less special knowledge, for more licensed freedom, a lesser uniformity, a wider search for gifts, and a slighter regard for specialist attainments. It is never too late for a well-trained mind to master a new subject, but he who neglects the substance of education for the shadow of mere knowledge robs himself of half the pleasure of his work and of every chance of greatness.

In attempting thus to diagnose a complaint which some may think is nonexistent I have laid myself open to attack at every point; yet I have a flickering hope that I may be dealt with after the manner prayed for by a youthful examinee whose paper, which I read, contained the appeal: 'Mr. Examiner, please temper justice with mercy, for I am so young in mind.' This hope I base upon the facts that modern science has at least taught tolerance, and that I have ever found my botanist colleagues conspicuous for this virtue. They understand that even the most minor among prophets prefers the stake to silence, and their good humour acquiesces in the interchange of *rôles* whereby the martyrdom which should be his is borne by them in listening to his wrathful words.

Anticipation of toleration so undeserved leads me to regret almost that I ever introduced that ghost at all. For now that it has served my purpose I am free to admit that I might have laid it long ago by other and *tu-quoque* arts.

I, too, might have pointed to those shelves, and at the sight of Mendel's work it would have vanished with a blush. For with all their gracious gifts the Victorians whose just praises I have sung failed to discover that Mendel was alive among them, and showing a way to solve the problems over which they themselves were puzzling.

The merit of the discovery of the greatness of Mendel's work belongs to our generation, and those of us who had no share in it have at least the right to applaud the discoverers and to score the discovery to our side.

So I may conclude the contrast of Victorian with modern naturalists with the reflection: theirs, the higher meed of culture; ours, perhaps, the greater perspicacity.

If, as I am prepared to maintain, the greatest gift which an experimental science may receive is that of a new, serviceable, general method, then to no man are biologists more indebted than to Mendel, for such a method he gave to our science. If, further, this claim can be established, I am absolved from the task of answering the critics of Mendelian doctrine.

Who does not recollect the answer which John Hunter gave to someone—Jenner, perhaps—who wrote to that great experimenter expressing doubt of the validity of an hypothesis? 'Don't think—try,' was Hunter's fine response.

If it were my purpose to discourse on Mendelian doctrine it would be my duty to carry on that work—like the early builders of that doctrine—with sword in one hand and trowel in the other, and to try in emulation of the pioneers to take an equal joy in using either implement. But my work concerns the method and facts accomplished by its use, and, as I understand philosophy, the writ of criticism does not run in the domain of accomplished fact. A homely illustration will serve to define my attitude. Here is a new knife, and there an old loaf, the crust of which has turned the edge of other implements. If with this knife I cut that loaf, it is idle to tell me that my knife is blunt. One form of criticism, and one only, is valid in such circumstances, and that is the constructive criticism of offering a better instrument. If I want bread, and Mendel's knife can give it to me, I shall go on cutting, indifferent to the stones of destructive criticism.

My business, therefore, is to meet criticism not by dialectics, but by confronting it with the facts accomplished by this method and by showing that its use opens new pathways on the borders of the unknown.

Now, if we scrutinise the method of Mendelian research, we may see that there can be no criticism of it.

Give a chemist a complex mixture of many compounds to describe: how does he proceed? The chemist sorts out the ingredients, and submits them severally to analysis. Such, also, is the method of the Mendelian analyst. Give him that complex mixture which is called an individual, and he sorts out the ingredients and submits them to analysis. Ask him how two complex mixtures behave when they are bred together, and he re-defines the question in such terms that it ceases to be enigmatical, and becomes susceptible of solution by experiment.

I am not concerned to claim for the Mendelian method the exclusive possession of these virtues. All I claim is that for the work of making a physiological analysis of individuals, and of thereby establishing a physiological classification of plants and animals, the Mendelian method has proved its value. It effects the service by a simultaneous analysis of germ and soma.

Let it be conceded at the outset that this analysis is made not by direct but by indirect methods. For so long as the physical nature of living substance remains unknown we can scarcely hope to resolve an individual into its physical components. All that can be done is to make comparative analyses of individuals and to discover how their several components differ from one another.

For our present purpose we may represent the individual by an equation :—

$$\text{Individual} = x + c\,;$$

where c represents the sum of a long series of characters of the individual and x an imaginary or real individual groundwork left after all the Mendelian characters—the sum of which is c—have been removed by analysis from the individual. The Mendelian method is concerned directly with the resolution of c into its components. Indirectly it is concerned also with x; for by the pursuit of the method the full value of c may be determined, and hence that of x may be inferred. This concession made, it is permissible to concentrate our attention on the term c.

Thus the business of the Mendelian is to resolve the complex of characters which is possessed by an individual into its constituent unit characters. As a consequence of this experimental analysis Mendelism is enabled to restate the problem of the behaviour in inheritance of two individuals in these terms :—

The complex of characteristics which distinguishes an individual is the expression of the sum of a long series of characters. As the individual arises from germ cells so each character arises from a germ within the germ cells. Such germs of characters are called factors. When two germ cells unite to form an incipient individual or zygote they bring together the similar factors of a given character—one factor from the one germ cell and the other from the other. As the zygote forms the mature individual, so the paired factors give rise to a character of the individual.

The body characters are the flowers of the factorial seeds implanted in the germ cells.

Some characters are simple and derive from one pair of factors only; others are of an ascending order of complexity and may be traced to the co-operative agency of two, or more than two, pairs of factors. In the case of a complex character the determining factors may be unlike one another or they may be alike. Thus two pairs of different factors are required to produce the character of colour in certain flowers; on the other hand, it is at least probable that certain characters are the outcome of repeated doses of the same factorial stimulant. Further, the individual is a dual thing—a double-barrelled gun. Each barrel is loaded with the factorial charge supplied by one of the two gametes by whose union its duality is constituted. Conversely and consequently a gamete or germ cell is, in comparison with the individual, of single and not of dual nature. It has one barrel only, and therefore can carry or give effect to one, and only one, of the two factorial charges with which the individual was supplied at the time of its formation.

Our image of the double-barrelled gun serves also to illustrate the several states in which an individual may find itself with respect to its charge of factors of any given simple body character.

Both barrels of the gun may be loaded. An individual in like state possesses two factorial charges and produces gametes all of which are alike in the possession of one of these factors. Therefore, such an individual when self-fertilised, or mated with its like, produces gametes which are all alike in this respect, and these gametes, fusing in pairs, give rise to individuals which all possess the character in question. Such individuals are homozygous, they breed true to the character.

Neither barrel may be loaded; and an individual in like state is also homozygous. It breeds true to the absence of the character. If a gamete of the former individual meet with one from the latter individual, the resulting zygote is in like case with that of a double-barrelled gun of which one chamber only is loaded. The zygote is heterozygous for the character. Unlike the homozygotes, which breed true, the heterozygous individual does not breed true to the character in question.

By the application of the foregoing propositions and a little arithmetic, it may be predicted that the offspring of the heterozygote fall into three groups—

one homozygous for the character, and another heterozygous, and a third homozygous for the absence of the character—and that, further, these types of individuals occur in the proportion of 1 : 2 : 1. Needless to say, the prediction is susceptible of verification by experimental breeding from the heterozygote. These are Mendelian commonplaces with which I should have hesitated to occupy our time were it not for the fact that I desire to emphasise the epoch-making nature of Mendel's method. The magic wrought by genius is potent because it is simple. The rules of Mendelian method are simple. If it be urged that I have broken my promise and strayed from method to doctrine I would ask which of the simple propositions I have stated may be demurred to by any student of biology?

The supreme importance of Mendel's contribution to science consists in this: that instead of mixing anything with anything 'in the gruel thick and slab' of a witches' cauldron, he has taught us to cast the horoscope of Fate by the method of genetical analysis of individual characters. Thus the first part of the Mendelian restatement of the old problem of Heredity reads: Investigate one by one the modes of inheritance of the several characters of an individual. Choose for this purpose organisms which are as far as possible alike in all respects except for the character under investigation. Carry the experiment to its conclusion, even to the third or fourth generation. If uncertain results are obtained, ascertain before discarding the method whether the uncertainty may not be due to the interference of other characters not to be suspected *à priori* of exercising an influence upon the expression of the character under investigation.

Who, for example, would suspect a morphological character like thickness of stem of exercising an influence on the time of flowering of a plant? Yet such is the case with the pea (*Pisum sativum*), and there is evidence that when this disturbing influence is removed inheritance of time of flowering follows Mendelian rules.

The second part of the restatement of the problem of genetics may be expressed as follows: Only by the use of individuals of proved constitution with respect to a given character may the effect of external conditions on organisms be determined. The study of variation must be preceded by Mendelian analysis and synthesis. Let me illustrate this theme by an example.

The species *Primula sinensis*, the Chinese primrose, has given rise to many distinct varieties. Among these varieties are some with white flowers and others with magenta, blue, red, or other coloured flowers. Each of these varieties may be obtained of florists in a pure strain—that is to say, in a strain which breeds true to flower-character. For our immediate purpose we will group these varieties into white and coloured forms.

It has been shown, however, that this apparently natural mode of grouping is inadequate to give a correct idea of the genetic constitutions of these races. It would seem self-evident that the white races differ from the coloured races by the lack of flower-pigment; yet Mendelian analysis demonstrates that there are more subtle differences between the different races. These differences become apparent when true-breeding white and coloured plants are crossed with one another; for it is then discovered that two types of white-flowered plants exist, and it is only by their fruits—their offspring—that ye may know them. Thus if certain white-flowered races are chosen for the experiment, the result of crossing white and colour is a coloured F_1 generation. If certain other white races are used and mated with the coloured form the offspring of the cross all bear white flowers. The different genetical behaviours of these heterozygous first generations give the clue to the difference between the two forms of white used as parents. In the former case—that in which the first (F_1) generation consists of coloured offspring—the second (F_2) generation, raised by self-fertilising F_1 individuals or by crossing them with one another, consists of coloured : white in the proportion of 3 : 1.

Whence we conclude that the white used in this experiment owes its character of whiteness to lack of the pigment-producing factor which is present in the coloured parent race. This conclusion is confirmed by the genetical behaviour of the whites of the F_2 generation. Such extracted whites breed true to flower-character—that is, give rise to white-flowered offspring only. White-flowered races which behave in this manner are termed recessive whites.

In the second case—that in which the F_1 generation consists of white-flowered offspring—the F_2 generation, from selfed or intercrossed F_1 plants, consists of three white : one coloured. The coloured offspring breed true. Of the three whites one breeds true to whiteness and the other two give rise, like the white F_1 generation, to three white : one colour. White races which thus impose their whiteness on the offspring of their union with a coloured race are known as dominant whites. Mendelians account for the genetical behaviour of dominant whites by assuming that they carry the character for colour and also a character for colour-inhibition. This hypothesis is amply justified by genetical results. Nevertheless it is an hypothesis which is novel to biology. It propounds a series of questions to the physiologist and biochemist, and in so doing exemplifies the fruitfulness of Mendelism. We shall see immediately whether the biochemist is able to take up this Mendelian challenge and what answer he can give to it.

At present, however, we are concerned to show by an example the necessity of prefacing the study of variation by Mendelian analysis. It was stated just now that the cross, dominant white by colour, results in a white F_1. That statement requires amplification. Grown under normal conditions the F_1 individuals bear pure white flowers; but if grown in somewhat higher temperatures the flowers develop a distinct though pale flush of colour. It is easy to show that the factor for colour is unaffected by the changed conditions, for the flushed F_1 individuals yield offspring of the same kind and in the same proportions as those produced by white F_1 plants.

It is fairly evident that the flushing is produced by the destructive action of heat on the inhibitor. In pre-Mendelian times this response to temperature would have been added without more ado as yet another ornament to dress the window of that old curiosity shop which is stocked with miscellaneous and heterogeneous articles all ticketed with the label ‘ variation.’

But in the light of Mendelism we may see in this effect of temperature the result of the casting-vote of circumstance on a heterozygous constitution. We may recall instances—as, for example, those provided by the well-known experiments on the effects of high temperatures on insect larvæ—which seem to show that environmental agencies may single out not only characters but also factors for attack. Thus we may begin to cohere in series the hitherto sundered and scattered phenomena of variation.

It is not yet possible to say how much of variation is to be put down to the interplay of characters, or, rather, to the differential effects of external conditions on characters which tend to balance one another; but this at least may be said—that the old and worn controversy on acquired characters was so much waste of words, because the problem purporting to be discussed had never been defined. Like the half of human quarrels, it was a quarrel about words.

It is stated in the books that the formation of peloric (regular) flowers may be induced by uniform illumination. Was the material used in the research homozygous or heterozygous? Does uniform illumination just prevent the unpaired factor from inducing normal growth? If so, what is the effect on the homozygous normal? These are examples of questions which suggest themselves at every turn, and they will abide the answer of experiment. The time is approaching when it will be possible to test the validity of the hypothesis on which the super-hypothesis of natural selection rests apparently secure from verification or disproof.

That hypothesis maintains that everything is in a state of flux; that variation occurs at all times and affects all parts. This may be true of multiple mongrels; of organisms which are heterozygous for many characters. On the other hand, nothing is more surprising than the stability of forms which are pure-bred for a fair number of characters, and it is at all events a suggestion not to be rejected summarily that plants pure bred for a considerable number of characters may exhibit a constancy and stability not usually associated with our ideas of living things.

In any case, it is open to the biologist to provide himself with suitable material wherewith to study the range and scope of variation and to investigate the conditions under which the organism discards old characters and regresses or acquires new ones and progresses. It is open to the Mendelian breeder to standardise creation.

Thus in fulfilling the first part of its task—that of defining the pure-bred—the Mendelian method has provided the material for the fulfilment of the second part—namely, the investigation of the conditions which make for the stability and instability of the organism. I think the time has come when this latter task might be undertaken on a large scale and with good prospects of success.

So far I have played the part of one of those street-corner watchers of the skies who offer a telescope for the inspection of the heavens. I have now to take a turn myself, and by means of the binoculars of Mendelism and Physiology survey not the celestial bodies, but certain new features of a small and narrow terrestrial field which this instrument brings within our ken. My survey has reference to the phenomena of the pigmentation of plants, and is confined to those presented by the anthocyan or sap pigments to which the colours of many flowers are due.

Until recently knowledge of the processes of pigmentation advanced along two main and independent lines. One line of advance—that followed with such brilliant success by Bateson and the Cambridge school, as well as by other students of genetics—has led to a wealth of exact knowledge with respect to the factors and characters which determine coloration. The other line of advance, pursued with no less brilliant results by Chodat and Bach and by Palladin and his associates, has resulted in a great increase of our understanding of the biochemistry of pigmentation.

The merit of being the first to combine the genetical with the biochemical method belongs to Miss Wheldale, to whom, moreover, we owe a good working hypothesis of the nature of the processes involved in pigment-formation. The work of Palladin and of Chodat and Bach is so well known that I need not review it in any detail. To Palladin we owe in large measure the conception that respiration consists in a sequence of enzyme-like actions, the later of which result in oxidations and are ascribed to oxydases. To the same observer we owe also the suggestion that chromogens play a part in the oxidations set up by oxydases and that these colourless chromogens may undergo either alternate oxidation and reduction and so take a continuous part in oxydase action, or undergo permanent oxidation and so constitute the pigments of the plant.

Chodat and Bach have given us a serviceable conception of the nature of oxydases. According to the Chodat-Bach hypothesis oxydases are of dual nature; the complete oxydase consisting of two parts—a peroxydase and an organic peroxide. An oxydase reacts with oxidisable reagents, such as guaiacum, to produce a characteristically coloured product. Hence these reagents may be termed oxydase-reagents. Peroxydases react with oxydase reagents only if there be added, as a substitute for the organic peroxide of the complete oxydase, a source of active oxygen in the form of hydrogen peroxide. Both oxydases and peroxydases occur in the cells of plants, and may be identified in extracts therefrom.

The work of Gortner on the pigments of insects adds confirmation to the view that pigments are the product of the action of oxydase on chromogens. Thus he has shown that the black or brown melanin of the integuments of insects is produced by the action of an oxydase, tyrosinase, on some such product of protein-hydrolysis as tyrosin.

Miss Wheldale's studies have led her to formulate the hypothesis that the anthocyan pigments of plants are the outcome of a series of chemical changes of the following order: Glucosides hydrolysed by emulsin yield chromogens which, acted on by oxydases, give rise to anthocyan pigments. The difficulty in the way of further advance lay in the unsatisfactory nature of the methods for identifying oxydases derived from plant tissues. Hence when we turned our attention to this subject Dr. E. F. Armstrong and I made it our first task to search for means whereby we should be able not only to identify, but also to locate, oxydases and peroxydases in plant-tissues. Clarke had tested already numerous oxydase-reagents and found that certain among them are adapted for microchemical use. As the result of a considerable number of trials of known reagents we have found that α-naphthol and benzidine are each adapted admirably for the purpose of locating oxydases. By means of these reagents we have been able to map out the distribution of oxydase and peroxydase in the flowers and other parts of various plants, and although the work is laborious and the technique as yet imperfect, the results afford strong confirmation of the current hypothesis of the mode of formation of anthocyan pigments. This confirmation, however, was rendered possible only by reason of the fact that we worked with races of plants bred on Mendelian lines, and hence of known genetic constitutions.

Our method of investigation is briefly as follows. The oxydase-reagent is used in weak alcoholic solution, the part of the plant to be tested is incubated in the solution for a suitable time, and if no oxydase action takes place—that is, if no characteristic coloration of the tissues occurs—the material is tested for peroxydase by the addition of hydrogen peroxide. The method may be employed for intact corollas or petals or for sections of plant-tissues.

It is important to mention that the first result of immersing a sap-pigmented tissue in either reagent is the decolorisation of the tissue. For example, a corolla of a coloured-flowered race of *Primula sinensis* loses its colour completely after being immersed for an hour or two in either reagent. The decolorised corolla, which in the case of *P. sinensis* remains colourless, is treated with hydrogen peroxide, with the result that a well-marked peroxydase reaction is obtained. The reaction is confined to the non-chlorophyllous parts of the corolla, and does not occur, except in the epidermal hairs, in the region of the yellow or green eye, the tissues of which contain chlorophyll. Indeed, there is good reason to believe that chlorophyll inhibits oxydase action.

By treating similar flowers with each of our two reagents we find that the actions of α-naphthol and benzidine are, in a considerable measure, supplementary one of the other. Thus the lilac-blue α-naphthol reaction is confined or almost confined to the veins of the corolla, the brown benzidine reaction is exhibited by the superficial (epidermal) cells and also by the veins. In order to emphasise the facts of distribution we speak of the peroxydases of *P. sinensis* as epidermal peroxydase and bundle oxydase. The former occurs in the epidermis and in the epidermal hairs, the latter in the bundle sheath which accompanies the veins.

Similarly, if sections of a stem of *P. sinensis* be investigated they are found to contain a superficial peroxydase and a deep-seated peroxydase. As the result of remarking the peroxydases, not of any unknown variety taken at hazard, but of the several varieties characterised by constant differences of depth and extent of pigmentation, we have been able to show that the distribution of peroxydase in any one race coincides broadly with the distribution of pigment in the most pigmented races. In other words, in *P. sinensis* the peroxydase framework for pigmentation occurs throughout the species, and the building of the several colour varieties is determined by the activity of the factor for chromogen production. If we conceive of this factor as administered in a series of doses we can form a fair picture of the mode of evolution of the series of varieties characterised by increasing or decreasing amount of pigmentation of their vegetative parts.

On turning to investigate the peroxydases in white-flowered races of *P. sinensis*, we shall expect to find from analogy with the peroxydases of the stem that these agents of pigment-formation are not lacking from the corollas of recessive whites. The application of our reagents shows that this expectation is correct, and that those white-flowered races which lack the factor for colour contain epidermal and bundle peroxydase. Hence we conclude that the absence of colour from recessive white flowers is due not to the absence of peroxydase, but to absence of chromogen. This conclusion is in conformity with that arrived at previously by Mendelian methods; for, as we have noted already, these methods demonstrate that anthocyan pigmentation of the flower of *P. sinensis* depends on the presence of one factor only, and that the absence of pigmentation which is characteristic of recessive whites is due to the absence of that single colour-factor.

The result of our investigation of the peroxydases of dominant white flowers is, on the other hand, quite different from that given by recessive whites. When corollas of dominant white races are treated with α-naphthol or benzidine and subsequently with hydrogen peroxide, they show no sign of peroxydase neither in epidermis nor in bundles. Hence such flowers either lack peroxydase or else they contain a substance which inhibits peroxydase from exercising its oxidizing action on our oxydase-indicators.

That oxydases may be inhibited *in vitro* has been demonstrated already by Gortner, who has shown that the addition of certain phenolic compounds—orcin, resorcin, etc., prevents tyrosinase from exercising its characteristic action upon tyrosin.

Assuming that an inhibitor of peroxydase occurs in dominant white flowers, it may be supposed to act either by destroying peroxydase or by setting up conditions under which the activity of peroxydase is arrested. Assuming further that the inhibitor acts in the latter way, it follows that if means of destroying or removing the inhibitor be discovered and employed, the peroxydase released from the inhibitory grip should be free to effect the oxidation of our reagents.

This train of reasoning gave us a point of departure for experiment. Starting from this point Dr. Armstrong and I have found in hydrogen cyanide a means of removing peroxydase-inhibition. Thus if dominant white flowers are immersed in a 0.4 per cent. solution of hydrogen cyanide for twenty-four hours, washed, and treated with either of our reagents together with hydrogen peroxide, pronounced peroxydase reactions are obtained, both in the epidermal and bundle tissues of the corolla. Carbon dioxide in aqueous solution produces a like, albeit a less pronounced effect.

Now, it so happened that we had at our disposal a race of Primulas, the flowers of which lend themselves peculiarly well to the purpose of confirming these observations. The race in question is characterised by blue flowers with fairly symmetrically placed paired white patches on each petal. We have reason to believe from the known ancestry of this race that these white patches are produced by a localised inhibitor.

Corollas of these flowers treated with α-naphthol or benzidine become quite colourless. When, however, hydrogen peroxide is added the natural pattern is restored. The parts originally blue are stained lilac-blue or brown according to the reagent used, and the inhibitory patches stand out as in the intact flower as white areas on the coloured ground.

If instead of submitting the particoloured flowers directly to the oxydase reagent, they are treated first with hydrogen cyanide, and then treated with the reagent and subsequently with hydrogen peroxide, the inhibition located in the white areas is found to have been removed, and the peroxydase reaction is produced over blue and white areas alike.

Hence the Mendelian hypothesis of the inhibitory nature of dominant whites is confirmed by biochemical methods. Moreover, these methods demonstrate that the inhibitor acts not by destroying but by preventing the action of oxydase upon the chromogen.

There are many other aspects presented by the phenomena of oxydase distribution in *P. sinensis* and other plants which we have investigated. Some I may enumerate, but lack of time must be my excuse for not dealing fully with any of them.

The close proximity in the flower of the superficial and deep oxydases suggests that the latter may co-operate with the former in producing flower-pigments. This possibility entails the hypothesis of a translocation of oxydase from the region in which it is secreted to that in which it acts, and there are not a few facts which are in favour of this view; for example, the lines of deep colour which occur along the veins of many flowers, the frequency with which the walls of cells appear to contain oxydase, the occurrence of oxydase in the mesophyll cells which adjoin the bundle sheath, and the evidence provided by the mutual influence of stock and scion in grafted plants and in graft hybrids. Though these and other subjects must be passed over, I cannot resist giving what appears to me to be the most elegant mode of demonstrating the relation between oxydases and pigmentation which we have as yet observed. The plant which has served for this purpose is the Sweet William (*Dianthus barbatus*), and any of the old-fashioned races of this plant common in cottage gardens suffices, provided that it be an ever-sporting race. Such a race is known by the fact that it bears, on one and the same head, flowers of different colours. The race which we have used is very sporting, a single plant bearing in one inflorescence deep magenta, pale magenta, white with limited rose flush, and all but pure white flowers.

If a petal of each of these flowers be treated with the benzidine reagent, it is found that the extent and amount of the oxydase reaction, as measured by the distribution and depth of brown coloration indicative of oxydase, coincide precisely with the extent and amount of pigmentation. The full-coloured petal gives a uniform deep brown reaction, the light magenta a uniform but paler reaction, the petal with a limited rosy flush gives a slight reaction, limited to the pigmented area, and the all-but-white petal gives none but the slightest reaction, and that only in the part of the petal which contained traces of pigment. Thus—unless the results are due to a partial inhibition which has eluded our attempts at demonstration—it would seem established that the ever-sporting habit is due to differences in the amount of oxydase in the diversely coloured flowers.

The Sweet William is also noteworthy in that it contains white races, some of which give an oxydase reaction in their petals and some of which give no oxydase reaction. Breeding experiments now in progress will decide whether or no these white races, like those of Sweet Peas investigated by Bateson and Punnett, mated together yield coloured progeny. If so the factors for colour, long wandering yet not lost, which meet again in reversionary coloured crossbreeds, may prove to be a chromogen factor and an oxydase factor.

Finally a brief reference must be made to our observations on the periodic fluctuation of oxydase in plants. Various observers have noticed that plant tissues give the peroxydase reaction much more generally than the oxydase reaction. The observations now to be described indicate that this is due to the greater stability of peroxydase as compared with the organic peroxide.

Under certain circumstances a tissue which gives only the peroxydase reaction may exhibit the direct oxydase reaction. Moreover, the extent of the peroxydase reaction, as judged by the depth of coloration of the reagent, varies in similar plants at different times.

Enquiry into the meaning of these fluctuations led us to the discovery that the nature and amount of oxydase contained in a plant tissue varies in an orderly manner according to external conditions.

Among the conditions which determine this fluctuation are light and darkness. Plants subjected to normal illumination possess less oxydase than those which are kept in darkness. After one or two days' exposure to darkness plants of *P. sinensis* contain more peroxydase than sister plants kept under normal conditions of illumination. Moreover, after such an exposure to darkness tissues which under normal conditions give only peroxydase-reactions yield distinct oxydase-reactions.

Whether these phenomena are general among plants we are not yet in a position to say; but repeated experiment enables us to vouch for them in the case of *P. sinensis*. Should the results of similar investigations with other plants show that this diurnal variation of the oxydase-content of plant tissues is of general occurrence, we may perhaps discover therein the means whereby many of the phenomena of periodicity exhibited by plants are maintained and regulated. We know that the light and darkness of the day and night set up rhythms in the plant; for example, that the leaves of various plants assume nocturnal and diurnal positions. We know further that the rhythm thus established may be maintained for a certain time under uniform conditions of illumination. This is the case with the Sensitive plant and many another.

Animals also exhibit a like periodicity. Thus some years ago Dr. Gamble and I showed that certain shrimp-like animals, *Hippolyte varians*, roll up their brilliant chromatophores at night and assume a sky-blue colour. When daylight comes they put on their daytime dress by spreading out the pigment of their chromatophores in far-reaching superficial networks. Kept in the dark these animals retain for many days this periodic habit, and when the hour of night arrives, although they have no light to tell it by, they lay aside their daily garb and put on the uniform of night. So also the plant-animal *Convoluta roscoffensis*, which lives on the seashore, orders its behaviour by the sun and moon. It lies on the sand till the waves of the making tide are upon it, and then descends to security and darkness. When the tide recedes it rises to the light. Even the uncongenial surroundings of a tea-cup and a laboratory fail to break this habit; for in these surroundings its uprisings and down-lyings keep time with the tides.

To one who has scrutinised with perplexed mind these mysteries of biology the speculation may be permitted that light and darkness may work these wonders through the control of chemical agents such as oxydases. But though it be legitimate to make a speculation of this kind it is idle to hunt the unknown to the death without the lethal weapon of experiment, and so I leave it for the present unpursued, and with it my Address. We have it on the authority of a poet and philosopher that to the traveller on a lonely road each bush becomes a bear, and I am not oblivious of the fact that oxydases have obtruded themselves with a certain obstinacy in the course of my Address. Nevertheless, obsession has its uses and significance, for it is the after-effect of enthusiasm; and though I have dealt, perhaps at undue length, with special problems and with suggestions, I venture to think that I have made out my case for the opportuneness of an *entente cordiale* between Physiology and Mendelism.

British Association for the Advancement of Science.

SECTION L: DUNDEE, 1912.

ADDRESS

TO THE

EDUCATIONAL SCIENCE SECTION

BY

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An Objective Standard in Education.

Of those who deny to Education a place among the sciences the name is legion, for they are many. The mere classification as a science is not perhaps of much consequence, but it is useful for the student of Education to examine the popular view, and see how far it is justified. The following statement, the words of a former occupant of this chair, will be generally accepted as representing the prevailing opinion :—

'If we take science to mean, as commonly understood, organised knowledge, and if we are to test the claim of any body of facts and principles to be regarded as Science by the ability to predict, which the knowledge of these facts and principles confers, can we say that there exists an organised and orderly arrangement of educational truth, or that we can logically, by any causative sequence, connect training and character either in the individual or in the nation? . . . It is very doubtful whether we can say that educational science is yet sufficiently advanced to satisfy these tests.'

First with regard to organised knowledge, there is certainly a great mass of matter available in the subject of Education. It is true that there is nothing easier than to show that this matter is not at present well organised. It is only too easy to find examples of contradictions among those who make a study of Education and venture to write or speak on the subject. We are told that there is hardly any important statement made by a writer on Education that cannot be met by a direct contradiction in the works of some other educational writer. It has to be admitted that writers on Education in the past have been strangely opinionative and dogmatic in view of the very complex and delicate problems they have had to handle. Too frequently they assumed a simplicity in their subject-matter that was certainly not there. Even the massive common-sense of Dr. Johnson was not able to keep him from regarding Education as a study that had reached its limits long before his time. But between those who regard Education as too simple to need any further examination, and those who treat it as so complex as to defy human analysis, there are those who take the view that Education is a science like any other, though they admit that there may be room for wide difference of opinion regarding the stage of development it has reached.

At the present moment it is becoming increasingly evident that educational theory is consolidating : it can now be claimed that there exists a great body of educational doctrine that is of general acceptation. It goes without saying that there are many and deep differences among the various schools of educational writers. But if we compare any two schools we shall find that the points of agreement far outnumber the points of difference. This was true even in the older times of naïf theory, but is making itself very evident in these latter days. Anyone who has occasion to read all the books on the theory of Education as they appear, is impressed in spite of himself by the large body of doctrine that is common to them all. It is not that the books lack originality : each writer has his new point of view or his new interpretation of certain phenomena; yet each either baldly states or tacitly takes for granted a great body of truth that is held to be generally accepted. This body of recognised truth is gradually increasing as the result of collective thinking and the corrections involved in active criticism. Already critics are beginning to find fault with any writer who produces a book—not avowedly a text-book—that professes to deal with the whole range of Education. He is reminded that what is now wanted is a special development along certain definite lines. The general principles of Education are held to be established and accepted.

In confirmation of what has been said, it may be added that within the past year or two have appeared no fewer than five separate treatises each bearing the same title : 'The Principles of Education.' These books are mainly for the use of students, and contain what are regarded as the accepted results of educational investigation up to the present date. Their authors obviously recognise the existence of a certain body of truths on which all are agreed. In some of the professions it is customary to speak familiarly of 'the books,' meaning the standard works to which appeal is constantly being made. If among teachers we have not yet reached this stage, we are obviously far on the way towards it. The books are there, but the profession needs some time yet before, in its own deliberate way, it recognises their importance. By and by it will realise the fact that it has at its disposal material that will enable it to prophesy and thus fulfil the second condition imposed upon all who lay claim to scientific knowledge. It is true that in the past there was little diffidence about prophesying : it was the fulfilment that gave trouble. Wolfgang Ratke supplies if not the first at any rate the most dramatic application of a control test in the working of educational prophecy. He went to prison because the people of his time did not make allowance for the insufficiency of the body of knowledge on which he based his predictions. There was indeed nothing scientific about the procedure of Ratke. He was at the empirical stage, and could not rise above it. His modern fellows have not quite got beyond the empirical, but they are on their way.

No claim is here made that Education has yet justified her demand to be recognised as a fully developed science; but it may be fairly maintained that she has at least entered upon the stage of scientific method : she is seeking to free herself from mere empiricism. In such a struggle there are at least two possible lines of action.

The first requires some ingenuity, but is natural and pleasant. It consists in superimposing principles upon the facts of the case. The educational theorist invents or assumes certain broad general principles, then proceeds to fit in all the observed facts, and often shows great skill in the process. This method is of very general application. Sometimes it is worked consciously and deliberately, as in the case of Socrates' doctrine of Reminiscence. Here we have the whole scheme of teaching simplified by this superimposed generalisation. Quite frequently, however, the broad underlying principles are not brought to clear consciousness, and are, in fact, sometimes contradictory to each other. Examples may be found in Rousseau. For our present purpose this tendency towards what may be called rational pedagogy is best illustrated in the system of Education elaborated by Herbart. Though the metaphysical basis on which he builds is generally regarded as false, it was deliberately adopted by him, and if it is once granted to him all the rest of his system must be admitted to be built up on strictly scientific principles. It is true that while logically Herbart's pedagogy was built upon his psychology, in point of fact his pedagogical thinking preceded and dominated his psychological theory. While Pestalozzi sought to psychologise Education, Herbart may be said rather to have educationalised psychology. In any case he supplies us with a system that challenges recognition as scientific, whether the claim be admitted or not.

The other method by which a study may seek to escape from mere empiricism is by dealing with observed results so as to reach the underlying principles. In this method, instead of setting up principles and making the facts square with them, we examine the phenomena and seek to discover the underlying principles.

Obviously this at once introduces the experimental method, since no satisfactory progress can be made by mere passive observation. This is the stage we have now reached in educational theory. We are passing from an appeal to experience to an appeal to experiment. Naturally educational method has always had to stand or fall by its results, but in estimating results there has too frequently been a confusion between cause and effect. So soon as a conscientious analysis of educational problems is attempted there comes the need of experiment. Certain questions have arisen demanding a definite answer, and the answers supplied must stand the test of practical application. Education is, in fact, called upon to prophesy, and to stand or fall by the results. Now the method of experiment is really a system of tentative prophecy, under rigidly determined conditions. We acquire skill in prophesying by a process of trial and error. We become prophets by prophesying. From all the knowledge at our disposal we calculate that a certain process will give a certain result. We apply the process, and then if the result is not what we expected we examine all the conditions, seek out the cause of our error, and proceed to another tentative prophecy. By and by we acquire the power of prophesying with confidence within certain recognised limits, and within those limits we may claim to proceed scientifically.

But in the evaluating of results that is necessary in this process of training in prophecy there is need for some recognised standard. Unless this condition be fulfilled there can be no general agreement among investigators. Accordingly the first step in raising a study to the scientific level is the establishment of such a standard. In the study of Education in the past—and it must be admitted that the same is true to a large extent at the present—the standard adopted was in most cases a subjective one. There is a tendency to have everything determined by individual opinion. Certain educational processes are gone through; certain results follow in the lives of the educands. The causal relations involved are arranged by the individual observer to suit his own views. According to some the battle of Waterloo was won on the playing-fields of Eton; according to others the battle of Colenso was lost there. We have need of some standard that is independent of private opinion.

Obviously the whole question of the relativity of knowledge is here involved. The educator is too apt to apply to his own case the Protagorean view, and maintain that 'man is the measure of all things; of things that are, that they are, and of things that are not, that they are not.' Into this antique problem we need not here enter. There is a sense in which the epigram of Protagoras may be justified. Without doubt, for his own practical purposes, the individual is the measure of his universe of experience. But so far as his universe has to do with the universes of others, the individual needs some common standard, something outside of himself, something that others besides himself recognise—in short, an objective standard.

The matter may be illustrated by what took place in the development of certain of the sciences. The secondary qualities involved in the Lockian epistemology—such things as colours, tastes, smells, sounds—lend themselves to a subjective standard; but so long as we confine ourselves to a standard of this kind we cannot be said to treat such matters scientifically. The individual is the sole judge of how a particular sound or colour strikes him, and against his decision there is no appeal. But it seems as if we could not have a science of sounds or of colours based on this individual judgment. Each observer would rely upon his own sensations and would interpret them in his own way. Fortunately in the study of physics it was discovered that certain of the conditions of sensation are constant. When we get a knowledge of wave-lengths, and the laws of refraction and reflection, we have passed from the merely subjective sphere, we have an outside standard, we can compare, abstract, and analyse independently of the individual. 'C natural' has a definite meaning to science, even if there were not a single ear that could hear the sound. It is true that, in the ultimate resort, we cannot eliminate the individual observer. He is too important in ordinary life, and a great deal of the work of science is done after all at his address. How *red* strikes an observer is as important to a scientist as is the exact wave-length that is necessary to produce *red*. The relation between a certain wave-length and a certain sensation is complicated by the individual peculiarities of the sense organs of the individual concerned. In certain respects the science of optics is self-contained, and has a definite objective standard. In certain other respects it depends for its data on individual experiences, and has to content itself with a subjective standard. No doubt it can call in the aid of Physiology, a science that has an objective standard of its own, and in this way eliminate a certain amount of subjectivity. But in the last resort there is a corner of the field in which no objective standard can be obtained.

It is true that in pure mathematics we appear to get into a region where the subjective may be practically excluded altogether, but even here the science of space and time is limited by the fact that it can deal with its data only from the point of view of human limitations. And there are certain borderline studies that are mathematical in their essence, yet have a direct reference to our bodily organs. Linear Perspective, for example, is usually regarded as a science, indeed as an exact science. Yet when we look into the matter we find that Linear Perspective is nothing more than a conventionalised method of treating, in an exact way, the results of individual experience. The whole science is really an objective standard by which the ordinary processes of vision may be compared, analysed, and classified. Perspective tells us what we ought to see. It is not independent of our sense functions, it is only a mode in which the variable subjective is reduced to uniformity by the application of the objective standard. Indeed, in the teaching of art there sometimes arises a curious conflict between the subjective element and the objective. Students who have studied Perspective before they are called upon to draw real objects set before them, are very apt to draw according to the rules they have learned, instead of observing what is actually before them and reproducing that as it appears to their senses. In other words, they set up the objective standard as paramount. So markedly is this the case that sometimes the study of Perspective is forbidden till familiarity with model drawing has been attained. When a teacher urges a pupil to draw what he sees, and not merely what he knows from the rules of Perspective he ought to see, we have an appeal to the subjective standard. The teacher is turning from the science of Perspective to the art of Drawing.

This illustration is of particular advantage to us in our present work, because it not only exhibits the subjective standard working alongside of the objective, but it introduces the idea of an *exact* science in relation to our human organs. Astronomy is an exact science, and yet the problem of the ‘personal equation’ shows that even here the subjective must be taken into account. The ‘personal equation’ is, in fact, nothing but the elimination by quantitative methods of the disturbing subjective elements. It is by similar methods that we must seek to establish an objective standard in Education. The difficulty in this subject is very great. Astronomy and Physics touch the subjective only at what may be called the point of application, the point at which they are brought into contact with human life. Their subject-matter is external, and lends itself to objective treatment. In Education the subject-matter is human nature, which is so complex and involves such volatile elements that it is almost impossible to reduce its working to fixed laws. The same difficulty obviously applies in Psychology. Itself a comparatively new subject, Psychology has great difficulty in getting recognition as a science. For this there are two main reasons. To begin with, Psychology began life as a branch of philosophy, and scientific men regard with suspicion anything that comes from that quarter. Besides, there was the less reason to make room for the new subject since it had already a settled place in the hierarchy of studies. The second reason is that which interests us here—the difficulty of establishing an objective standard. The descriptive generalities of Dugald Stewart and Thomas Brown had to give way to something based upon laws that are generally accepted. The line of least resistance in seeking for an objective standard in Psychology is to fall back upon a physiological basis. It is generally admitted that nerve action can be referred to an objective standard, and by correlating psychic and bodily phenomena psychologists are able to get a series of recognised principles on the physical side that may be easily interpreted in terms of spirit. Psycho-physics has at least a plausible claim to rank among the sciences, and the unbridged gulf between mind and matter is conveniently ignored. As a matter of fact such a generalisation as the Fechner-Weber law ranks parallel with the laws of Linear Perspective—that is, it is a law that states in an unjustifiably exact way what ordinarily takes place in the individual experience. While rejecting the materialistic alliance, Herbart, as a psychologist, deliberately set up a mechanical system of ideas as forces, and in this way established at once an objective standard by means of which all mental process may be understood and manipulated. So scientific is his system that he claims that the interaction of the ideas may be calculated in certain cases by a simple application of the rule of three. With Herbart, Psychology has certainly been raised to the rank of a science; but unfortunately it has to be admitted that his objective standard has been illegitimately assumed.

Just as Psychology utilises Physiology in its effort to gain a standing as a science, so Education is inclined to use Psychology. Frequently we hear Psychology described as a science, while Education is relegated to a place among the arts. It is natural, therefore, for the educator who wishes to claim rank in science to appropriate the scientific status of his auxiliary science. As a matter of fact Education has captured Psychology. This is only one of many cases in which a profession has taken possession of an abstract study, and in this way enabled the abstract study to make real progress. Theology as a study has gained greatly by the fact that it is a compulsory subject for those who are preparing for a great profession. Astronomy owes a great deal to the support it has received from its practical value to navigators. Physiology would not be what it is to-day had it not become an essential subject in the preparation for the practice of medicine. Physiologists sometimes complain that their subject is hampered by its professors having to waste time in teaching mere medical students; it is well to remember, however, that but for the demands of the medical profession Physiology would have been left to the few private investigators who might be able at their own cost to carry on under adverse conditions the work that is now being done in thousands of well-equipped laboratories. In the same way it is greatly to the advantage of Psychology that it has become an essential part of the professional training of teachers. The subject is now receiving an amount of attention that it would never have had but for the support of its connection with the profession of teaching. But after all a teacher is not a mere psychologist: Education is more than applied Psychology. If Education is to rank as a science it cannot be in virtue of its use of another study that itself has an insecure foothold among the sciences. It must establish for itself an objective standard.

Mere quantitative manipulation of the elements of a study, if only carried out on a sufficiently large scale, has a tendency to evolve an objective standard, apart from any deliberate search for such a standard. We may gather something from an examination of a standard of this kind that, unexpected and unsought, evolved itself in the ordinary course of educational administration. What Binet and his colleagues and followers have been trying to do of set purpose was, to some extent at least, accomplished automatically by the working of the system of individual examinations under the English and Scotch Codes of Elementary Education. Binet has drawn up certain tables with the express purpose of testing the intelligence of children at various ages. But we are only at the threshold of investigation work of this kind, and the tests cannot be regarded as satisfactory, either in themselves or in their application. But they have been drawn up with the deliberate purpose of supplying a more or less objective standard of intelligence. Now in the British Elementary School Codes we have the examination requirements from the pupils of different ages set out in a series of tables each corresponding to one of the seven grades known technically as 'standards.' The purpose of these tables of requirements was not primarily to determine the intelligence of the pupils, but rather to indicate certain minimum amounts of information that had to be communicated in consideration of a certain money payment. Yet these tables bear a generic resemblance to those of Binet, and in actual practice the 'standards' did win acceptance as a test of intelligence. The requirements were perhaps less scientifically determined than are those of Binet's tests, but their practical value was very much greater, because of the extremely wide range of their application.

When the Codes had been in working order for a score of years it became evident to thoughtful observers that there had arisen a standard of comparison among pupils in elementary schools that was gradually being recognised all over the country. It was an objective standard, as was shown by the fact that each of the standards began to have a meaning of its own, apart from the individual school in which a particular pupil happened to be found. No doubt there were differences in detail. A Standard III. boy in one school would be found to have greater knowledge and skill than a Standard III. boy in another. But the important point is that the phrase 'a Standard III. boy' came to have a definite meaning apart from any particular school. It began to be used absolutely, and not merely relatively. Further, if a boy were found to be in a standard lower than his years warranted, people had no diffidence in drawing their own conclusions regarding his ability. It will be remembered that Binet tells us, somewhat vaguely, that if a boy is a year behind others of the same age who have had the same opportunities it indicates that he is duller than the others, but not necessarily permanently so. If, however, the pupil is two years behind the normal test for his age there is a presumption in favour of his being inherently and permanently duller than his fellows. All this is very familiar and indeed commonplace to the elementary teachers who were brought up under the Code Examinations by standards. To tell the truth, M. Binet's tests are regarded with much suspicion by such elementary teachers as have been induced to give them attention. They have the feeling that here we have a University Professor working out as something new a belated scheme that has had its day, and in that day done a great deal of damage. They are afraid that the prestige given to the intelligence tests may encourage the re-establishment of the rigid individual examination system from which they have escaped. All the same, experienced elementary teachers do not deny that the old system did at least have the effect of establishing a generally recognised standard. Their belief is that the standard was not worth what it cost.

It is left for Binet's successors to invent a better scheme than he was able to produce, and in this way to establish an objective standard, at least in respect of intelligence. Such a standard is needed in many connections, but there is one special department of educational administration where such a standard is at present urgently required. Nothing better illustrates the groping of Education after a scientific basis than the present demand for some means of determining which children are 'defective' and which merely dull. So imperative is the need for an objective standard here that it must be satisfied at any price, with the result that the decision is being more and more left to the doctors instead of to the teachers. The cause is not difficult to find. Physiology has already an objective standard, and the doctors are evidently expected to get their results by physical examination. No other explanation is admissible, since they are not only not superior to teachers in their knowledge of the mental reactions of the child, but obviously inferior. At present the argument moves backwards and forwards. Some say : Give the teachers a tincture of physiological knowledge, and then they will manifestly be the best persons to determine the defective stage. Others reply : Give the medical men some little experience of school conditions and the working of the immature mind, and they cannot but be the proper authorities on all questions of intelligence. The important point in this competition for power between the two professions is the implied recognition of the need for an objective standard, and the admission that, at present, such a standard does not exist. Much investigation, experimenting, and verification are necessary before the truth on this particular subject can be reached. But the recognition of the existence of the problem is in itself an indication of progress, and the need for scientific method in working it out is being more fully recognised. From our standpoint it is important to note that we are here dealing with a problem that is distinctly educational, and the bringing in of men from another profession does not make it less so. If the doctor acquires the power of dealing with delicate questions of intelligence, it is because he has learnt to be an educationist if not an educator. Medical men who specialised in this matter would no doubt very soon attain to high skill, since their previous training gives them a very suitable preparation to begin the study of Education. Doctors are consulted regarding 'defectives' mainly for two reasons. First, these defective children are naturally classed in the popular mind with the mentally deranged, and these have always been regarded as peculiarly suitable subjects for the doctor. Further, there exists, without doubt, the implicit feeling in the public mind that the doctor has definite standards while the teacher has only general impressions. But it has to be noted that this invasion of the field of Education by men from another realm of study does not in any way affect the claims of Education to rank as a nascent science with needs and methods of its own. If the doctors can supply Education with an objective standard, Education should be very grateful, but need not abdicate in favour of medicine. Education may use the results of both Psychology and Physiology without in any way surrendering its claims to be an independent science. We must not, of course, make too much of the distinctions among the sciences. Nothing but error can result from seeking to make each of them rigidly self-contained. So far as Education is concerned, what we have to seek is that objective standard that we have conceded to be essential to the recognition of a study as a possible science, and this without falling back on the standards of either pure Psychology or pure Physiology.

We may learn something from what we have found out about the results of the individual examination system. The general tendency of quantitative methods is to eliminate the subjective element. Even in the case of marking examination papers experience shows that the use of numerical marks tends to objectify results, and to get rid of some at least of the difficulty involved in the personal equation of the examiners. Marking by general impression of a whole paper is much less free from subjective variation. Every individual number set down as a mark implies a fresh exercise of the critical power, and when there are many questions there is a compensating principle at work, inasmuch as each impression is recorded as it is made and the addition of the marks produces a balancing in which the latest impression has not the determining influence it too frequently has when a paper is marked as a whole. If an examination includes many subjects, many examiners, and a great body of examinees, the subjective element in the marking is, to a large extent, eliminated, and we can deal with the results in accordance with what is practically an objective standard. We must not, of course, neglect the fact that after all the whole basis of the results is the judgment of the individual examiner on the material submitted to him. This corresponds to the application to real life of any of the physical sciences. Here, as in many of the other sciences, we have a surd of subjectivity that can never be got rid of entirely. But its disturbing influence can be minimised by the counteracting influences of other forces in the quantitative manipulation of the data.

Of late the quantitative method of dealing with educational problems has been greatly developed. Karl Pearson's product-moment formula has enabled us to make an accurate arithmetical statement of the amount of correlation that exists between series of quantitative data. By the application of this formula, and the simpler formulæ of Professor Spearman, it is now possible to correlate a great many facts that were formerly treated as having only a problematic connection with each other. If these formulæ produce really reliable results, we have at our command a means of answering definitely and definitively a great number of questions that have hitherto been regarded as the more or less legitimate matter for the professional controversialist. The vexed question of 'formal training,' for example, may be set at rest once and for all by a sufficiently extended series of correlations of the results of pupils' progress in certain subjects. The peculiarity of this method of dealing with correlations is that once we have handed over our facts to the formulæ, the process passes out of our hands altogether. We have only to work out our equations and the results make their appearance. Here we certainly seem to have reached an objective standard.

Such results, however, are not unnaturally regarded with some suspicion. Once the formulæ have been established by mathematical proof they must, of course, be accepted as irrefutable on that side; but their application to educational problems is so mechanical and indeed inhuman that many are unwilling to accept and use them. Some people are doubtful whether, in dealing with human beings, it is desirable, even if it were possible, to have an objective standard that eliminates humanity from all human problems. It has to be pointed out to such critics that all human problems must begin with the individual and end with the individual. All the intermediate process may be carried on in the pure objectivity of quantity, without dehumanising the application of its results. This will be kept in view when we deal with the average.

Apart from this danger of dehumanising our subject, there are two real possibilities of error in the application of the formulæ. First, there is the danger that the investigator may be satisfied with an application to an insufficient number of cases. The second danger is that the subjective element may cause error in the preparation of the data. If the first possible source of error be minimised, the second will be practically removed. Granted a really wide investigation, there is little room for serious error. If a sufficiently large number of cases be examined, and these cases selected under sufficiently varied conditions, the subjective variations will neutralise each other, and a reliable result will be produced. It must never be forgotten that the Pearson and other formulæ are merely means of dealing with material already acquired. It is only to this extent that they supply an objective standard. Many of the recognised sciences are in no better case.

The hope of the evolution of Education as a science lies in the proper manipulation of the method of experiment. Students of Education have always been in the habit of asking questions, but they have not always waited for an answer. Nor have they usually taken sufficient care in making their questions precise. They have not laid down with the necessary detail the conditions implied in the question, and when they have reached some answer they have been too often content either to accept it without any verification at all, or with the support of nothing but a few general considerations that seemed to confirm it. In the newer educational investigations questions are set out in great detail. They are usually limited to one point, and all the relevant conditions are carefully laid down. Various control tests are applied during the progress of the investigation, and every precaution taken against the introduction of interfering forces. Then when a result has been obtained various confirmatory tests are applied. Even when all has gone well so far the result is not regarded as authoritative till the experiment has been repeated with the same results by different experimenters working under different general conditions, though, of course, all the detailed conditions must be precisely the same as in the original experiment.

The questions asked are often of a very practical character. In the current number of *Child-Study* Mr. W. H. Winch gives an example. The question is whether one gets better results in working ‘problems’ in Arithmetic by (*a*) direct teaching for a certain period in how to work such problems; or (*b*) spending the same period in giving the pupils practice in working such problems. Mr. Winch gives a very instructive account of all the conditions under which his experiment was carried out, including all the necessary precautions. The result is that those who had had the teaching scored an average of 11·1 in the final test, while those who had had the practice scored only 9·2: the group that was taught improving on its preliminary record to the extent of 34 per cent., while the group that had been confined to practice improved by only 11 per cent. It is thus demonstrated, at present, that teaching counts for more than practice in the preparation of pupils to do problems in Arithmetic. But the fact cannot be regarded as a part of the permanent possessions of the teacher till it is verified by many more experiments in this country and abroad.

We have seen that even at our present stage of advancement there is quite a respectable collection of recognised facts in connection with Teaching and Education, and that these are in process of organisation. We shall soon have such a volume of well-arranged knowledge as shall meet the first requirement for recognition as a science. But while organisation is imperatively needed and must go on, there is an equally urgent need for new knowledge. There are hundreds of definite practical questions that are being asked by teachers every day, and unfortunately answered according to individual experience, if not indeed according to individual caprice. Some few questions about the memory are now definitely answered, and practical educators have the benefit of the results of experiments; but there are scores of points with regard to memory on which there is still doubt, and yet these are points on which the practical educator must adopt a definite line in his daily work. He cannot postpone his decision: he must do one thing or another, and in the meantime he has no standard. Such investigations as are being undertaken by the committees of this section are helping to increase the total body of knowledge at present available. It is true that hitherto these investigations have been mainly concerned with psychological matters, and certainly our store of psychological knowledge is not so great as to warrant any complaint at the concentration on this aspect. But it is pleasant to note that this year we are having a report on more distinctively pedagogic matters. There could be no more useful subject of inquiry suggested than an investigation into the questions that are most urgently demanding answers at this time among the practical educators of the country. To discover and classify these, and then to correlate them with the various investigations that are being made throughout the world, would be to render a very practical service to the study of Education. The truths thus acquired and recorded could be fitted in to the mass already at our disposal, and the result would be a great strengthening of that objective standard that is so essential to the independent progress of our study.

Education ranks with a group of studies that deal with humanity in its various aspects. Psychology naturally is the science that underlies them all, since it is the abstract study of human nature which is their raw material. But Politics, Economics, Sociology, Eugenics, all claim to be sciences, and if we probe into their standards we find that they are largely statistical. It is quite possible by careful investigation among the subject-matter of these sciences to organise a system of general principles based upon averages obtained from a very wide field of investigation. These principles are of very general application, though they may not enable us to prophesy in individual cases. This, indeed, is at the root of a great deal of the criticism levelled at the claims of Education to rank as a science. A parent or an education authority presents a boy to an educator and calls for a prophecy. The educator must decline, since he cannot honestly prophesy in an individual case, though he may be prepared to venture on a reasoned statement of what is likely to occur in the boy's educational career. The educator is, in fact, in precisely the same position as a medical man called in to a case. He can prophesy, but only in general terms. In both cases it is the application of general principles to a particular case.

This raises the whole question of the value of the average in matters of Education. Psychologists in addressing teachers are beginning to warn them that the average is only an abstraction, and really does not exist. We are told that what the teacher has to concern himself with is 'the living child here and now before him,' and he is accordingly warned against the insubstantiality of the elusive abstract. But this is to confound two distinct things. It is true that the teacher must always deal with a living pupil here and now before him. But in his dealing with that living pupil he has to apply a paid-up capital of knowledge of men and of boys in general. He must seek to understand the living boy by the aid of knowledge previously acquired, and this knowledge is represented by the average. The master may be unable to prophesy with certainty how Jones minor will act under certain specified conditions. But from a knowledge of Third Form boys in general he can make a guess that is very likely to hit the mark. The teacher who applies his knowledge of the average Third Form boy to the minor Jones, without modification to suit Jones's case, acts unintelligently, but the possibility of blunders by a dull master does not reduce the value of the knowledge of the average in the hands of one who is capable. The concept of the average boy as it is developed by experience and study in the mind of the master forms a standard by which other boys may be estimated. This standard is partly subjective, partly objective. In so far as the standard is acquired by the personal experience of the master it is subjective. The unreasoned but very effective knowledge of boy nature that enables an efficient master who is guiltless of any acquaintance with educational theory to know how a boy is likely to act under given circumstances results from the training of experience, and is peculiar to its possessor. On the other hand, the knowledge of boy nature that has been acquired by deliberate study and by experiment is something that has an existence independent of the individual. It is objective, or at any rate has an objective bias.

We must distinguish in practice between the average and the type. The average boy may have no existence in reality, he may be a pure abstraction; but the type is concrete, and may be regarded as the embodiment of all the essentials that go to make up the average, with the addition of certain qualities that must be present in some form or other, though the particular form is immaterial. The average is to the type as the concept is to the generalised image. The type may form a very useful standard for masters whose tendency is strongly towards the concrete, but the average has a special and a different value, and in capable hands is more effectively applied because it is of a wider range. To consider a class as made up of types tends to break up the class feeling, and make the master think of his pupils as a mere group of separate individuals. Undoubtedly the master must in certain connections think of his pupils as individuals, but in other connections he must deal with his class as a whole, as a psychological unit.

This introduces one of the most striking developments of modern educational theory. The older psychologists treated their subject as limited to the study of the mature human individual. The introduction of the idea of development led to the founding of genetic psychology with its consideration of the individual at his various stages. A further advance is marked by the appearance of collective psychology which carries the study of the individual into his relations with other individuals. Naturally both changes were of the greatest advantage to education. The first gave scientific guidance to the popular movement known as Child-Study, the second suggested the scientific study of the class as a collective organism. It is true that this collective psychology is at present in its infancy. But while we owe much to the French psychologists with their dazzling exposition, we are glad to turn to our more solid McDougall for the best scientific basis available for a sound collective psychology. The material he has supplied is waiting to be worked up from the educational side. His statement of the relation between the instincts and the emotions and his manipulation of Mr. Shand's theory of the sentiments provide tempting material for the establishment of an objective standard in connection with the training of the individual character and the interaction of individual characters in groups. Naturally the results must be expressed in averages, and equally naturally there will be a complaint from certain practical educators. What is the use, it will be asked, of information about how classes in general act? What we want to know is how this particular class before which I stand is going to act. But this is to confound the practice of a science with the science itself. There must always be an intelligent intermediary between the principles of a science and their application to the affairs of life. In this respect the nascent science of Education differs in no way from those that are more fully developed. The educator who prides himself on being specially practical is frequently very unreasonable in his demands from educational theory. He is rather apt to complain that it does not supply him with sufficiently detailed instructions. What he wants is a series of recipes which, if scrupulously followed, will inevitably produce certain specified results. But such men take a very humiliating view of their profession. So far from seeking this spoon-feeding, they should rejoice that their work demands the exercise of intelligent initiative. Herein consists, in fact, the dignity of the educator's office. He must be master of the organised knowledge that Education has acquired, and must have the power of making the appropriate application of that knowledge to every case as it arises. To assist him in avoiding error he is entitled to look for an objective standard at the hands of those who make Education their special study, but for the use of that standard he must himself accept the full responsibility.

British Association for the Advancement of Science.

SECTION M: DUNDEE, 1912.

ADDRESS

TO THE

AGRICULTURAL SECTION

BY

T. H. MIDDLETON, M.A.,

PRESIDENT OF THE SECTION.

Early Associations for Promoting Agriculture and Improving the Improver.

THE honour which has been done me by the Council of the British Association in electing me to be President of the new Section, 'Agriculture,' carries with it as its first privilege the opportunity of congratulating those officers of the Sub-sections who have been the means of securing for agriculture the place which its students have long coveted in the meetings of our great Association for the Advancement of Science. Sir Horace Plunkett, than whom no one has shown a better appreciation of the advantages of association for the improvement of agriculture, made his Address as Chairman of the Sub-section a special plea for the recognition of this subject, and a subsequent Chairman, Major Craigie, used, I have been led to understand, all those arts of peaceful persuasion of which he is a master, in order to secure the formation of this Section. Other officers of the Sub-sections, too, have worked hard for this result, and to them those of us who are now assembled to take part in the first meeting of the new Section owe a debt of gratitude.

In view of the subjects which, in recent years, have engaged the attention of the Agricultural Sub-sections, it was suggested to me that for the Dundee meeting a paper dealing with some agricultural question of the past would be appropriate. As from my personal point of view this would have the advantage of causing me to renew acquaintance with treasured, but latterly somewhat neglected, old volumes which fill my study shelves, I readily accepted the suggestion. A subject from the past has for me the additional attraction that it relieves me altogether from a discussion of questions related to my daily work. A President of a Section is, indeed, expected to speak about his own work; but mine belongs to that category which many people seem anxious to learn about before it has been made public, and in which nobody is particularly interested after, in buff, or blue, or white garb, it has been issued by His Majesty's Stationery Office, and is purchasable at the modest figure which represents the cost of paper and printing.

With the view, then, of informing ourselves of certain phases of agricultural progress in the past, and at the same time gaining some inspiration for the future, I propose to invite your attention to the prototypes of Section M. When did the first associations for improving agriculture appear? What were the circumstances which led to their formation? What were their aims? What did they accomplish? These are the questions which I shall try to answer, or, rather, to which I shall attempt to indicate the answers; for to deal with them adequately would occupy much more time than is at our disposal to-day.

It is not inappropriate that the relation of societies and associations to agriculture should occupy our attention in this town of Dundee, for here, in 1796, there was published a work on agriculture by a prominent Forfarshire agriculturist, James Donaldson, in which a vigorous appeal was made for the establishment of societies so that a spirit of improvement might be aroused among farmers. Donaldson, in referring to the Reports of the Board of Agriculture, then being issued, complained that it was quite impossible to reach the farmer by means of such expensive volumes. He urged, therefore, the publication of a cheap journal, and with this the formation of county societies with the object of spreading far and wide the information of which Sir John Sinclair and Arthur Young had collected so large an amount. Three years later, in May 1799, the Board, taking Donaldson's hint, addressed a circular letter to landowners on the subject, which resulted in the formation, between the beginning of the century and 1815, of a number of local associations for improving agriculture.

I began the preparation of this Address with the intention of giving some account of the progress made in the end of the eighteenth and the beginning of the nineteenth centuries, when Sir John Sinclair and Arthur Young were so actively engaged in promoting agriculture. But, as my notes progressed, I found that it would be necessary to limit my remarks to a period ending with the accession of George III. I will therefore ask you to follow me while I endeavour to trace the rise and progress of the Improver of Agriculture and the work of associations which, before the year 1760, prepared the foundation on which, in the second half of the eighteenth century, Sir John Sinclair and Arthur Young reared the superstructure of the first Board of Agriculture. While this subject calls for no references to my official work, I may perhaps be permitted to claim that it is one on which I may appropriately address you, inasmuch as the functions of the old Board closely resembled those of that Division of the present Board of Agriculture and Fisheries with the supervision of which I am charged.

Although Section M meets for the first time to-day, and though Sub-sections for agriculture belong to recent years, agriculture, on many occasions in the past, has occupied a place in the discussions of the British Association. To one of the earliest meetings Justus von Liebig contributed a report on the state of organic chemistry which he subsequently republished under the title 'Chemistry in its Application to Agriculture and Physiology.' In the dedication of this volume to the British Association occur these words: 'One of the most remarkable features of modern times is the combination of large numbers of individuals representing the whole intelligence of nations for the express purpose of advancing Science by their united efforts, of learning its progress, and of communicating new discoveries.'

I think that Liebig's statement, which had reference more especially to the sciences then occupying the attention of the members of the British Association, applies also to movements for promoting the study of agriculture. I can find no evidence that societies for the advancement of agricultural knowledge existed among the ancients. The question is, however, not one which I have investigated fully, and without further study I am not prepared to state definitely that such societies belong exclusively to modern times. The old Scottish writer 'A Lover of his Country' states that 'the Propagation of this useful Science (agriculture) was the Care, as well as the first Rise of many considerable and famous Societies in *Athens*': and so it may have been that centuries before St. Paul visited the Areopagus, the Athenians congregated on Mars Hill discussed questions of husbandry after the manner of Socrates, Critobulos, and Ischomachus in Xenophon's 'Œconomics.' Indeed, if we reflect that Europe was backward among the continents in giving attention to husbandry; that 2,800 years before Christ an Emperor of China is said to have instituted a ceremony for the purpose of impressing on his subjects the importance and the dignity of agriculture; that the Egyptians had developed a Land of Goshen before the time of Joseph; that the trees in the 'paradises' of Persia were planted by those princes skilled in arboriculture who are praised by Socrates, and that the treatises of Mago, the 'Father of Husbandry,' were among the treasured possessions of which Carthage was despoiled by her Roman conqueror, it does not seem improbable that associations for the advancement of agriculture may have existed in ancient times.

But if such associations did exist, they either neglected to appoint recorders or their records were among the many old writings on husbandry which are known to have been lost. It is certain that Columella, who in the first century of our era garnered the wisdom from all known works on agriculture, had never heard or read of associations for promoting agriculture. For in his First Book on Husbandry he laments the absence both of the means of instruction and of the desire for study among his fellow-countrymen, and, writing of agricultural education, he sorrowfully describes how in the case of other arts,[1] 'everyone sends for a person from the society and assembly of the wise to form his mind and instruct him in the precepts of virtue; but Husbandry alone, which, without all doubt, is next to, and as it were near akin to wisdom, is in want of both masters and scholars.' And he proceeds, 'For hitherto, I have not only heard that there are, but I have myself seen, schools of professors of Rhetoric, and as I have already said of Geometry and Music; or, which is more to be wondered at, academies for most contemptible vices, for delicately dressing and seasoning of victuals, for contriving and making up dainty and costly dishes for promoting gluttony and luxury; and I have also seen head-dressers and hair-trimmers; but, of Agriculture, I have never known any that professed themselves either teachers or students.'

These quotations, while they show that associations for the advancement of agriculture were unknown to Columella, also show the Roman writer to have been fired with that zeal for knowledge which possessed our own Improvers of Agriculture in the seventeenth and eighteenth centuries. Nor is it a difficult task to trace back to the ancients the 'Spirit of the Improver,' which appeared in England about the middle of the seventeenth century. The influence of the classical writers may indeed be traced in the second half of the sixteenth century; but at that period English agriculturists were impressed only by the practice of the ancients, as exemplified in the careful rural economy of Roman husbandmen; the knowledge, or science, of agriculture—on the importance of which several of the ancient writers have discoursed at length—did not attract Englishmen before Bacon's time.

Interest in the practice of improved husbandry was first aroused in England by the books of Fitzherbert. The extent to which this author stimulated agriculture may be inferred from the appreciation with which his works were received in his own day, and copied by others for a century. He himself does not appear to have been acquainted with the classical writers. He describes the English practices with which he was familiar; he quotes frequently from the Scriptures and refers to early religious works, but only in writing of animal diseases, when he cites the 'Sayinge of the Frenche man,' is there any indication that he was influenced by foreign authors. Fitzherbert's 'Boke of Husbandry' and 'Surueyenge,' while they are free from the direct influence of Roman writers, show us, nevertheless, that the English agriculture of his day owed much to Roman traditions. The careful business methods and accounting of the farm bailiffs of the Middle Ages, with which Thorold Rogers has acquainted us, were the methods which Fitzherbert learned and counselled, as they were the methods which Columella taught.

It was between 1523, when Fitzherbert's 'Boke of Husbandry' was first printed, and 1557, when Tusser published his 'Points of Good Husbandry,' that the classical writers began to exert a direct influence on English farming. In 1532[2] there appeared Xenophon's 'Treatise of Householde,' 'ryht counnyngly translated out of the Greke tonge into Englyshe by Gentian Hervet,' which at once became popular and ran through a number of editions. At least as early as 1542 editions of the works on agriculture and gardening of Cato, Varro,

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[1] I quote from the edition published in 1745 by A. Millar, London.

[2] The earliest edition in the Cambridge University Library is dated 1537, but Dr. Peter Giles informs me that the earliest copy in the British Museum is dated 1534, and that, according to the old Bodleian Catalogue, Oxford had a copy dated 1532. My own copy is a 1767 reprint which describes Hervet's translation (in 1537 bound in one volume with Fitzherbert's 'Husbandry and Surveying) as having been extremely popular.

Columella, and Palladius[3] were published in England, and they must certainly have been known to Tusser, for in his 'Five Hundred Points of Good Husbandry,' composed some years later, there is clear evidence of the influence of the writings of Xenophon and Columella. From the latter author Tusser adopts the method of a calendar, and he appears now and again to adapt Roman maxims to modern conditions. Thus in his calendar Columella says of March that it 'is the proper time to cleanse meadows, and to defend and secure them from cattle; in warm and dry places indeed that ought to be done even from the month of January,' and Tusser in his calendar for March rhymes :—

'Spare meadow at Gregorie Marshes at Pask
For feare of drie Sommer no longer time ask
Then hedge them and ditch them, bestow thereon pence,
corne, meadow, and pasture aske alway good fence.'

It might be, of course, that in discussing the same subject, a subject moreover which does not admit of much difference of opinion, the similarity of the above-quoted passages is accidental; but many of Tusser's rhymes so closely follow Xenophon's 'Householde' and Columella's Eleventh Book that I am satisfied Tusser was familiar with both these ancient writers. Here, for example, from Tusser, is the charge concerning sick servants which Ischomachus gives to his young wife :—

'To Seruant in Sickness see nothing ye grutch,
a thing of a trifle shell comfort him mutch.'

And here is a maxim for the housewife that Columella enforces :—

'The woman the name of a huswife doth win
by keeping hir house and of dooings therein
And she that with husband will quietly dwell
must thinke on this lesson and follow it well.'

Until the dawn of the twentieth century no mere man would have been found to question the conclusion come to in the above verse; nevertheless, the emphasis on the 'quietly dwell' indicates that in this particular case the inspiration is derived from Columella rather than from Xenophon. For while the woman described by the Greek writer is likened to the queen bee, by the Roman there is much lamentation because of the emergence of the 'butterfly.' Columella refers to the diligent dames of ancient Rome who lived at home and studied to improve their husbands' estates, and contrasts them with their successors in the first century, who had become indolent, refused to make their own clothes, and spent their husbands' incomes on dress. He then remarks, 'Is it a wonder that these same ladies think themselves mightily burdened with the care of rural affairs, and esteem it a most sordid business to stay a few days in their country houses ?'

Personal carefulness on the part of master and mistress was to the Roman the essence and the sign alike of good husbandry; by Tusser's rhymes this lesson was enforced at a time when an increase in the cost of living was attracting attention all over the country; his book went through a number of editions, and his pointed rhymes appear to have exercised a greater influence on the rural economy of the first half of the seventeenth century than the works of any other writer. Thus, for example, in Best's 'Farming Book,' written by a Yorkshire gentleman in 1641 for the guidance of his son, Tusser is frequently cited as an authority. But Best, though a classical scholar himself and probably acquainted with some of the ancient writers on husbandry, makes no reference to them; the Yorkshire squire apparently regarded the writings of Varro and Columella as being of no real use to a farmer.

It was, then, the practice of husbandry that engaged the English agriculturist's attention from the time of Walter de Henley to Thomas Tusser, and the purpose of my digression into domestic subjects is to show that when the ancient writers were rediscovered in the middle of the sixteenth century, it was not the frequent

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[3] A translation of Palladius into English was made about 1420, but it was not discovered and published until within recent times.

references of Xenophon to the science of husbandry but his economic and moral teaching: not Columella's First Book, with its appeal for doctors and disciples who might apply themselves to the study of agriculture, but his Eleventh Book, with its calendar of operations and its directions for the ordering of the bailiff and the bailiff's wife, that attracted Tusser and his readers.

The awakening of interest in husbandry was largely due to the rapid changes in the economic conditions of England which set in about Fitzherbert's time. These changes we cannot now discuss, but their magnitude may be indicated by the rise in price of the single staple, wheat. According to Thorold Rogers, the average price between 1400 and 1540 was 5*s.* 11¾*d.*, the decennial average for the last four decades being 5*s.* 5½*d.*, 6*s.* 8¾*d.*, 7*s.* 6*d.* and 7*s.* 8½*d.* Between 1541 and 1582 the average price was 13*s.* 10½*d.*, and 16*s.* 8*d.* for the last twelve years of this period. Between 1583 and 1642 the average price rose to 36*s.* 9*d.* In particular years high prices were reached, and in 1596 and 1597 Fleetwood chronicles prices of from 80*s.* to 104*s.* per quarter.

The change in the cost of living directed men's attention to the husbandry and housewifery recommended by Fitzherbert and Tusser. The smaller landowners, who could no longer afford to live on their rents, and who saw that yeomen and tenant farmers were prospering, turned their attention to farming, and agriculture became an important occupation of the educated classes.

The yeoman and tenant farmer did not ask for text-books on agriculture, but the new agriculturists required information, and thus there arose at the end of the sixteenth century a great demand for books. The booksellers were not slow to make provision for the demand, writers were secured, books were published, and of the more popular many editions were sold.

Even such a ready writer and successful adapter of other men's books as Gervase Markham got more work than he could do. His book on live-stock was bought up so freely that in 1617 he resolved to write no more on this subject, and the public demand was satisfied by the issue of reprints. His 'Farewell to Husbandry,' too, was reprinted in many forms. My copy, for example, is a fourth revision printed by William Wilson for John Harison in 1649, while the copy figured in MacDonald's 'Agricultural Writers' is also a fourth revision, with an almost identical title-page, but printed by Edward Griffin for John Harison in 1638.

At the end of the sixteenth century the practice of continental farming began to attract attention in England, and a further proof of the demand for information which then existed was the translation in 1600 of 'Maison Rustique,' a French work by two doctors of medicine, Charles Stevens and John Liebault. This volume, in its English form known as the 'Countrie Farme,' contains seven books and nine hundred quarto pages. It is intended to be a complete guide for residents in the country, and deals with everything that the landowner wants to know, from the care of his health to forecasting the weather. The work is interesting in other ways than as an indication of the new appetite for books. As in Heresbach's works, translated by Barnaby Googe in 1577, we find in the 'Countrie Farme' the acknowledged influence of the ancients. Reference is made to the Greek writings of Hesiod, to Mago of Carthage, and to the high esteem in which the Latin works of Columella, Varro, and Cato were held: we are informed that French translations of the works of the three last-named were in existence in 1582.

The English agriculturists of the sixteenth century went abroad for more than books. Gerarde, who like others of his profession deserted medicine to the great advantage of botany, had obtained a number of foreign plants for his collections. From the gardener, too, England learned of the skill of the Flemings, and would gladly have copied their practice. But the Flemings were too busy to write books; so Englishmen went to see for themselves how and why they prospered.

Sir Richard Weston, a Surrey landowner, who succeeded to his estates in 1613 and who had travelled in Brabant and Flanders, was the first English agriculturist to introduce practices approved on the Continent. He grew turnips for feeding cows, a century before the time of Turnip Townshend; nearly three hundred years ago he was experimenting, as we are still doing, with clover seed grown in different countries; he had thirty to forty acres of clover sown with barley, and he was inveighing against the sophistication of 'outlandish' grass seeds and contriving plans for raising pure stocks at home in the approved fashion of to-day.

The importance of such crops as clover, lucerne, sainfoin, and turnips was quickly recognised, and agriculturists wished to hear and read more about the husbandry of the places from which they had come. Information was supplied in the works of the alien writers Plattes and Hartlib; the latter especially, by his 'Legacie' and his 'Reformed Husbandman,' did much to popularise a knowledge of Continental farming and to suggest 'the errors, defects, and inconveniences of our English husbandry.' Hartlib was a widely travelled man, and gave our Improvers many fresh ideas, among them a suggestion for a 'Colledge of Husbandry,' but we cannot claim him as an English agriculturist.

It was not only from Brabant and Flanders that travellers brought to England information about foreign agriculture. As one result of the development of commerce voyagers were introducing from distant countries such important plants as the potato and tobacco, and were exciting interest by their stories of foreign products. A desire to make experiments with these novelties was but natural, and experimental farming received a powerful impetus from the teachings of Francis Bacon, the first exponent of the inductive method. Having, as he wrote, 'taken all knowledge to be my province,' Bacon was himself an amateur farmer, and if he was not a successful one he was at least intent upon introducing methods of 'industrious observation and grounded conclusions.' It is to Bacon, I think, that Arthur Young alludes in a passage in which he describes a Lord Chancellor of England as having procured and read every published work on husbandry so that he might learn how to farm, and who, having met with ill-success, collected the offending books and lighted a bonfire! But let us not think lightly of the efforts of this distinguished amateur farmer. The agricultural writers of the succeeding century, indeed, refer to the influence of Bacon in terms that suggest for Agricultural Science the origin of the phœnix. We may, at least, agree that about the time of Bacon's bonfire this subject first began to attract the notice of scholars.

In spite of the political troubles of the second quarter of the seventeenth century, agriculture continued to secure increased attention, for England had learned that in war or peace the food-supply must be cared for, and the importance of corn-growing increased with the rise in prices. Thus when the Commonwealth was established everything favoured a forward movement. At peace and able to return to country pursuits, the combatants, Cavaliers and Roundheads alike, became active improvers. Engineer agriculturists, like Vermuyden, carried out great drainage-works. Many estates had changed hands, and the new owners, not a few of whom, as Harte remarks, 'had risen from the plough,' were glad to return to it; others were amateur farmers intent on learning. The books of the old and trusted writers, Fitzherbert and Tusser, had been followed by the works of such authors as Norden, Markham, Plattes, and Hartlib. Bacon's teaching emphasised the need for further study and experiment. Behind the political and economic changes were the powerful, moral influences of the Puritan movement; it was at this time and under these conditions that the Spirit of the Improver, which had animated Columella, appeared among English agriculturists.

The first practical farmer to plead the cause of the improvement of agriculture was Walter Blith, one of Cromwell's soldiers, who is supposed to have been a Yorkshire landowner, but who for some years, at least, was stationed in Ireland. To him may be attributed the first improvements in Irish farming. Writing in 1770 Harte says: 'Ireland it must be confessed had a wretched method of husbandry and strong prejudices and beliefs in that method when Blythe alone (who then lived in Ireland) was sufficient to open men's eyes by his incomparable writings.'

Blith was himself an ardent agriculturist, and prefaced his practical book, 'The English Improver Improved,' by seven epistles designed to attract the attention of all classes of his fellow-countrymen to agriculture. These epistles were addressed to 'The Lord Generall Cromwell,' the 'Industrious Reader,' the 'Nobility and Gentry,' the 'Honourable Society of the Houses of Court and Universities,' the 'Souldiery of these nations of England, Scotland, and Ireland,' the 'Husbandman, Farmer, or Tenant,' and to the 'Cottager, Labourer, or Meanest

Commoner.' With the 'Lord Generall' he pleaded for an Agricultural Holdings Act and the other legislative measures required by Improvers. To the 'Industrious Reader' he expounded the reasons for the methods of his book, and commented on the work of previous authors, commending Sir Francis Bacon's 'Naturall History' as 'worthy of high esteeme, it is full of Rarities and true Philosophy.' He exhorted the army to set themselves to the improvement of the land now that they have the 'goodness and welcomeness of a Calme after a storm.' But it is in the epistle to the 'Honourable Society of the Houses of the Court and Universities' that chief interest lies for us, for here we find an appeal for the systematic study of agriculture in words that recall the classical writers. Blith showed that agriculture required the close study of the learned, and that the societies (*i.e.*, the Colleges) of the Universities might if they wished do much for its advancement. He approaches them as a suppliant with no suggestion that they should abandon their 'sublimer Notions,' but with the hope that they may be induced to regard agriculture as a recreation, so that, as he says, 'you may step a little into the field and Country and cast away an hour or two upon this Subject at your leisure.' He adds, 'You that have the Theorick, may easiest discover the Mysteries of the Practick, and from you have I found most encouragement to this work, and seen most experiences of good husbandry than from any, and from you too I expect and waite for more discoveries of some thing I scarce know what to name it, which lies yet in obscurity, but I will call it the Improvement of the Improver.'

Were we not now concerned with the spirit rather than with the form of the improvement, an interesting parallel might be drawn between the topics which Blith considers of greatest importance and those which to-day are engaging attention. In his epistle to the Society, for example, there is an appeal to the learned to give their attention to Applied Science. Discussing the progress of the Dutch, Blith deplores that policy which Englishmen afterwards termed *laisser faire*. He says, 'Our niceness in not nursing the fruits of our own bowells hath given them the opportunity to Improve our native commodities to the advance of their Manufacturidge to our shame, their praise'; then addressing members of the Universities he adds, 'I speak to wise men whom I would have more publique men. . . . Let me entreat you for the Peoples and your own posterity sake . . . put your shoulders to the work, greater things remaine and larger Improvements are yet to be discovered.'

The earnest advocacy of Blith, the Essays of 'my good friend Mr. Samuell Hartleps,' and the energy of landowners like Sir Richard Weston led to a demand for the records of experiments, and in 1658 there was issued the first series of abstracts of agricultural experiments with which I am acquainted, under the title 'Adam out of Eden.' The experiments recorded by the author, Ad. Speed, are of considerable interest; but I mention him for another reason. He appears to have made a living by propounding improvements of an imaginary character. He wrote tracts for noblemen and others, containing estimates of the profits to be gained by adopting new methods. Blith scathingly refers to him as 'Mr. Speed that superlative Improver,' and remarks that so long as his books were private 'I could bear it, and suffer wiser than myself to bee fooled because I was not wise enough as to beware of him, but now that they come to be sold in the Stationers' Shops, and spread abroad the country, to deceive, and beguile the Nation, I cannot forbear.' This was written in 1652; as my edition of 'Adam out of Eden' is dated 1659, it is clear that the nation continued to be 'beguiled' for a considerable period by this particular Adam, the forerunner of a numerous family. Whenever there is a revival of interest in agriculture he flourishes; the new manure, the ravaging insect, the blighting fungus, all serve to bring 'Adam out of Eden,' and so long as an interested and gullible public exists 'that superlative Improver Mr. Speed' will be found among us. The pamphlet and the stationers' shop have become antiquated; the Adam of to-day has other methods, which I will not venture to particularise. After all, it is a healthy sign. It is only when the public thirst is deep that Adam gets his chance, and like Blith we must resign ourselves now and again to 'bee fooled,' for is it not one of the methods by which the Improver is improved?

Walter Blith's appeal for the assistance of the learned did not long remain unanswered. At the time his 'English Improver Improved' was published a society of scientific men had already been formed in London, and ten years later this society first received the name Royal Society, at the suggestion of John Evelyn. On October 15, 1662, Evelyn's 'Discourse on Forest Trees' was presented to the Society. Five years later, when the 'Sylva' was published, the author in the preface tells us that the Royal Society was then doing much for husbandry. Evelyn records his own experience in studying agriculture and forestry, how he had read all the old authors and got but little good from his studies, and he congratulates his countrymen that 'the *World* is now advis'd and (blessed be *God*) redeemed from that base and servile submission of our noblest *Faculties* to their blind *Traditions*.' Again, referring to the absence of a 'compleat *System of Agriculture*, which as yet seems a *desiderata*,' he says: 'It is (I assure you) what is one of the Principal *Designs* of the Royal Society; not in this Particular only, but through all the Liberal and more useful *Arts*; and for which (in the estimation of all equal *Judges*) it will merit the greatest of *Encouragements*; that so, at last, what the learned *Columella* has wittily reproach'd, and complained of, as a defect in that *Age* of *his*, concerning *Agriculture* in general, and is applicable *here*, may attain its desired *Remedy* and *Consummation* in *This* of *Ours*.' He then quotes Columella's remarks about the Schools of Rhetoric, Geometry, and Music, and the absence of agricultural professors and scholars, which I have already read.

John Evelyn was one of the prominent members of the Royal Society, and he seems to have taken a leading part in defending it against the attacks to which, in the first years of its existence, it was subjected. With much satisfaction he points out, in dedicating the second edition of the 'Sylva' to King Charles II, that his essay and the work of the Royal Society have in the past eight years resulted in the planting of over two million timber-trees, and he adds that he has preserved the testimonials he has received with the more care '*because they are* Testimonials *from so many* honourable *Persons, of the* Benefit *they have receiv'd from the* Endeavours *of the* Royal Society, *which now adayes passes through so many* Censures.'

With the exception of the 'Societies of Learning and Gallantry' of the 'Houses of Court and Universities' addressed by Blith, the Royal Society is the earliest to which any influence on agriculture may be traced, and it is certainly the first society which definitely included the improvement of agriculture as coming within its scope. It appears to have depended in no small degree for its early successes on the public interest aroused by the writings of Evelyn and Houghton, and there is evidence that the Society gave much attention to agriculture during the second half of the seventeenth century, and that its patronage was much valued. The immediate influence of the Royal Society may be traced in Worlidge's '*Systema Agriculturæ*.' In my edition (the third, 1681), Worlidge makes a strong plea for improving agriculture; he quotes at length from ancient writers to prove the esteem in which the art was held by them, and then says: '*Also a most evident demonstration and sure Argument of the Utility, Pleasure and Excellency of this Branch of Natural Philosophy, is the principal care the* Royal *and* Most Illustrious Society *take for the advancement thereof, and for the discovery of its choicest and rarest secrets*.' He also refers with special approbation to the work of Evelyn, not only in promoting forestry, but in improving the making of '*that incomparable Liquor* Cider.'

Evelyn's 'Pomona,' in which he discourses of fruit trees and cider, gives an interesting glimpse of some of the early activities of the Royal Society, for the work itself is based chiefly on contributions by members of the Society to its '*well furnish'd Registers, and* Cimelia.' Evelyn is careful to point out that these contributions were original papers and that it was not the design of the Society to 'accumulate repetitions where they can be avoided.' These new observations being in the Society's esteem '*and according to my Lord* Bacon's' preferable even when '*rude and imperfect* draughts' than commonplaces '*adorn'd with more* pomp.' Evelyn himself was not practically acquainted with cider-making, and his own interest in the subject, like that of the majority of his fellow-members, was Baconian—*i.e.*, it consisted in a search for 'grounded conclusions and profitable inventions and discoveries.' Possibly, too, the badness of the French wines of the period had some share in directing the attention of the Fellows of the Royal Society to cider, for as early as January 28, 1662, they listened to a discourse on the Adulteration of Wine, by Dr. Charleton, which so stirred Evelyn that he wrote: '*To sum up all: If* Health *be more precious than* Opinion, *I wish our Admirers of* Wines, *to the prejudice of* Cider, beheld but the Cheat themselves; *the* Sophistications, Transformations, Transmutations, Adulterations, Bastardizings, Brewings, Trickings, *not to say even* Arsenical Compassings, *of this Sophisticated* God *they adore; and that they had as true an* Inspection *into those* Arcana Lucifera, *which the* Priests *of his* Temples (*our* Vintners *in their* Taverns) *do practice; and then let them drink freely that will.* Give me good *Cider.*' And so apparently said some of his fellows, for the Society's curator, the 'ingenious Mr. Hooke,' introduced a new cider-press, and Sir Paul Neile, that 'most worthy member' Dr. Beale of Yeovil, and others, were commanded to discourse on the manufacture of varieties of this pure beverage, and to recommend such brews as Pepin-cider adapted for splenetic gentlemen, and Gennet-moyl suited to the palates of ladies.

In other ways the members of the Royal Society encouraged one another in making improvements; thus when in 1666 Evelyn's 'worthy friend' Mr. Hake went on a journey, he returned carrying with him—for eight hundred miles—some grafts for Evelyn, together with a 'taste of the most superlative perry the world certainly produces.' It was by means such as these, and by a policy which approved 'plainness and usefulness' rather than 'niceness and curiosity' that the newly formed Royal Society commended itself to the country.

It is indeed probable that agricultural questions occupied much more of the attention of the Royal Society in the earlier years of its existence than the printed records suggest; we are told, for example, by the Scottish Improver, 'A Lover of his Country,' that one of its most illustrious members, Sir Robert Boyle, was an enthusiastic agriculturist; he says: 'I had the Honour to be known to that excellent Person and oft in his Company. He was the greatest Lover of Agriculture I ever knew, and I wonder he never wrote of it. I heard him say, it was a Pity there was not Seminaries of that, the most useful, and except Pasturage, the most ancient of all Sciences.'

Not only were agriculturists attracted by the practical investigations of the Royal Society, but impressed by the value of its methods and organisation, and Worlidge suggests that nothing would more conduce to improving agriculture than the constitution of subordinate Provincial Societies '*whose principal care and office might be to collect all such Observations, Experiments, and Improvements they find within their Province . . . which of necessity must abundantly improve* Science *and* Art *and advance* Agriculture *and the* Manufactures.'

The proposal made by Worlidge was unheeded at the time, for not until nearly a century after his suggestion was made did English Agricultural Societies begin to appear. A retrograde movement set in soon after the Restoration, and although the Government sought to foster improvements and passed several Acts with the object of stimulating farming, Harte tells us that a 'total change of things, as well as the very cast and manner of thinking, joined with immoral dissipation, and a false aversion to what had been the object and care of mean despised persons, soon brought the culture of the earth into disrepute with the nobility and gentry.'

An insight into the conditions of the last quarter of the seventeenth century and the first quarter of the eighteenth is given us by Lisle, who wrote the Introduction to his 'Observations on Husbandry' in 1713. He begins by remarking that it is one of the misfortunes of the age that it lacks honourable conceptions of a country life, he draws attention to the fact that in the decadent days of Rome luxury increased and husbandry was neglected. He calls on the landowner to look round him and see how many fine estates are daily mortgaged or sold, 'and how many antient and noble families destroyed by the pernicious and almost epidemic turn to idleness and extravagance.' He discusses at length the advantages of an agricultural career and recommends it as a profession for the eldest sons of gentlemen, who might regard it as 'a school of profit and education; whereas,' he continues, 'it is rather looked on as a purgatory for the disobedient, a scene of punishment, to which a son, who answers not his father's expectations, is to be abandoned; or a condition of life of which none would make choice, but such whom fortune has not in other respects favoured. If the country gentlemen therefore frequently consist of persons who are either rusticated by their parents in anger, or who, making a virtue of necessity, settle on their estates with aversion or indifference, it is no wonder the comedians exhibit them on our stage in so despicable and ridiculous a figure; but this is the fault of the persons and not of the art. Were they properly initiated in the study of Agriculture, and pursued it as they ought, it would be so far from excluding them from useful knowledge, and bringing them into contempt, that I may venture to assert, they would find it the best school of education, and the fittest to prepare them for the service of their country in the two houses of parliament of Great Britain.'

Such were the dispiriting social conditions with which the successors of Evelyn in the Royal Society had to contend. The agricultural experiments of the Society therefore attracted but little attention outside the ranks of the curious. Houghton, a contemporary of Evelyn's, started a periodical publication, 'Houghton's Letters,' but it soon ceased. A generation later, and about the period to which Lisle refers in the above quotation, a work on husbandry was written by a Fellow, John Mortimer. It is dedicated to the Society, 'to whose encouragement, inquiries, and direction it owes its birth.' Special thanks are given to another Fellow, Dr. Sloane, who assisted the author, and 'has greatly contributed to the advancement of useful knowledge.'

Testimony to the activity of the Royal Society at this period is also to be found in a work on 'Curiosities of Nature and Art in Agriculture and Gardening,' a translation from the French of the Abbot de Vallemont by Bishop William Fleetwood, published anonymously in 1707; this work contains the passage 'The Royal Society of *England* who are so zealous for the Perfection of Agriculture and Gardening, have apply'd themselves with great Care to find out the true way to make Salt-petre, which they likewise allow to be the chief Promoter of the Vegetation of Plants.'

About this time botanical questions of much interest to agriculturists were occupying the attention of the Royal Society. Robert Ball and Samuel Moreland were investigating reproduction in plants, and a few years later Richard Bradley, another Fellow, Professor of Botany at Cambridge, but more of an agriculturist than a botanist, was explaining how, by cross-breeding, 'such rare kinds of plants as have not yet been heard of' may be produced. He refers specifically to a cross between a carnation and a sweet-william, but by inference to Burgoyne's Fife and the other things 'not yet heard of,' that are associated with agriculture and botany in the Cambridge of to-day.

Various causes, among which the influence of Fellows of the Royal Society must be given an important place, led the landowners and the educated classes of England again to turn their attention to agriculture about the beginning of George II's reign. The revival was associated with and followed, as it has in recent time, a development in gardening. William and Mary were patrons of horticulture, they greatly improved the Royal Gardens, and the nobility, in imitation, laid out parks and *parterres*. This demand gave opportunity to the professional gardener, and the garden-designer and nurseryman started business. It likewise gave authors their opportunity, and that it was well taken advantage of is proved by the popularity of Miller's Gardeners' Dictionary written by the Gardener of the Botanic Gardens at Chelsea, and dedicated to Sir Hans Sloane, President, and to the Fellows of the Royal Society.

A second writer on gardening of this period, the Reverend John Laurence, of Bishop Weremouth, Durham, may be mentioned, for he was also one of the chief agricultural writers of the first half of the eighteenth century. In 1726 he published a large folio work entitled 'A New System of Agriculture.' This book marks the time when what the author calls the 'Spirit of Gardening' appeared, and it proves that gardening was very popular with the landed and educated classes for some years before the revival in agriculture began. Laurence refers, as Lisle did a few years before, to the lack of interest in husbandry, and remarks: 'If Gentlemen could persuade themselves to cast their Estates into Beauty and Order, they would quickly experience it the noblest Exercise and greatest Delight.' Every age, he remarks, has its own amusements, 'This Age, it is plain, seems to taste and relish everything new on the Subject of vegetable Nature.' He attributes the 'relish' not merely to progress in the art of gardening, fostered by nobles and statesmen, but to the Royal Society—of whom he says that their Philosophical Transactions 'are standing Memoirs of the Zeal and Activity of many Persons of Quality and Learning,' whose 'Discourses and Experiments' have 'advanced much Light in the Art of Husbandry.' Incidentally he refers to the condition of the North of England, and says that the county of Durham may properly be termed the Garden of the North, such are the possibilities of improvement afforded by its soil and situation and the 'hasty Diligence of a wise and polite People.' Laurence's work was intended to place before these northern agriculturists an account of all the experiments that had been made in husbandry, and especially to set forth in plain language the information published by the Royal Society. He appeals to his brethren in the Church to exert themselves in the cultivation of their glebes which would give them both the 'Relaxation and Refreshment they require,' and he says, 'I should think myself extremely happy if I could be instrumental in reviving among Gentlemen whose affairs do not oblige them to spend a great part of their Year in *London*, a Spirit of improving their Estates and imploying their Time in making Experiments, which cannot be expected from the farmer.'

Although for seventy years after its formation, and throughout a period during which agriculture was neglected by the landed classes, the Royal Society did much to keep alive the Spirit of the Improver, the unfortunate apathy of the agriculturist prevented that progress which appeared to be imminent when John Evelyn wrote his 'Pomona.' It was not possible for a learned society in London to investigate agricultural questions in the absence of the scientific agriculturist himself; subjects of agricultural interest were therefore discussed chiefly from a theoretical standpoint, and, neglecting the teachings of Bacon and the example of Evelyn, there arose that use of the deductive method which in the past two centuries has done so much to hinder the progress of agricultural science.

The first to show up the fallacy of the deductive method in studying this subject was Jethro Tull, who, though he himself fell into the errors which he condemned, was, in his understanding of the true relationships of science and practice, far ahead of any of his contemporaries. A lawyer by training, he probably took to agriculture because of his poor health. He worked at it for twenty years before he was induced to set out his views in writing, and it was years after he began farming before he read anything on the subject. Dissatisfied with the practice of his times he set himself to reason out new methods and to make experiments. He got suggestions from foreign travel; he tells us, for example, that the first hint of the value of horse-hoeing husbandry was derived from the ploughed vineyards of France; but he was careful to submit his ideas to the test of experiment before he adopted them in farm practice. He was not a Fellow of the Royal Society. He lived a lonely life, and until the fame of his farming spread abroad and he published his 'Horse-hoeing Husbandry,' in 1730-31, he appears to have devoted himself entirely to experiments in farming. The appearance of his book occasioned much correspondence, and thereafter he made himself acquainted with both the ancient and modern writers on husbandry, and used his knowledge to good effect in his arguments with the writers whom he terms collectively *Equivocus*. His temper, which, if one may judge from his references to his labourers, was far from serene, was much tried by his controversies with *Equivocus*, and his criticisms of the writers and scientific men of the preceding half-century are severe. He remarks, for example, on the superficial knowledge of agriculture shown by 'Mr. Laurence, a divine; Mr. Bradley, an academic; Dr. Woodward, a Physician; Mr. Houghton, an Apothecary; these for want of practice could not have the true theory; and the writers who are acquainted with the common practice, as Mr. Mortimer (whether for want of leisure, or not being qualified, I do not know) have said very little of any theory.'

Tull himself, a thoroughly experienced, practical farmer, whose successful methods had drawn widespread attention to his Berkshire farm, showed no hesitation in setting out his own views on roots, leaves, the pasture of plants, and other scientific subjects then engaging attention. His remarks were based on original observations, and it is clear that he did not merely copy opinions from scientific treatises. He freely criticised the writings of others; even 'Mr. Boyle' and that 'miracle of a man Sir Isaac Newton' are severely handled by this critical farmer, and in a characteristic sentence he remarks: 'From Sir Isaac's transmutation arguments we may learn that a man never ought to depend entirely upon his own for support of his own hypothesis.' An admirable sentiment which I am afraid that Tull himself, and many another agriculturist since his time, failed to lay to heart. When we remember how meagre were Tull's opportunities for study, that he lived a retired life in the country, that he had long abandoned letters for practical farming and only began to read the works of others late in life; if, further, we remember that his health was bad, and that his appliances for scientific study were indifferent—'my microscope, indeed, is but a very ordinary one,' he writes—we must give him a foremost place among the scientific agriculturists, not only of the eighteenth century, but of all time. His wide knowledge and keen reasoning place his 'Horse-hoeing Husbandry' in a class by itself among the works of the early Improvers.

Jethro Tull's great work was published two generations after Walter Blith first endeavoured to awaken the Spirit of the Improver in English farmers. Throughout this period not much progress had been made, but a change was at hand. When in 1730 Turnip Townshend left politics and went down to Norfolk to farm his estates, the tide had turned, and henceforward throughout the eighteenth century there was a rapid improvement in the practice of English agriculture. Of these developments no small share may be attributed to the influence exercised by the Royal Society during the first seventy years of its existence.

The agriculture of Scotland had not shared in the revival due to the work and writings of the English Improvers, and was in a very backward state in the middle of the seventeenth century. Its condition is indicated by John Ray, who in 1661, some months before the Royal Society received its charter, set out from Cambridge to spend the Long Vacation in a Scottish tour. He crossed the Tweed on August 16, and proceeded from Berwick, *viâ* Dunbar, to Edinburgh. His first day's Journal gives us his impressions of what is now, and probably was then, one of the foremost agricultural districts in Scotland. 'The ground in the valleys and plains bears good corn,' he says, but 'the people seem to be very lazy, at least the men.' Scottish women, he writes, 'are not very cleanly in their houses, and but sluttish in dressing their meat.' 'They have neither good bread, cheese, or drink. They cannot make them, nor will they learn. Their butter is indifferent, and one would wonder how they contrive to make it so bad.'

There is evidence in the Journal that this rude fare disagreed with the traveller, who had but recently quitted Trinity 'high table,' and he is unduly severe on the people of the Lothians. He draws attention, for example, to the plainness of the Scottish women and the vanity of the men; he criticises Edinburgh, and having studied its architecture he remarks on the College that 'the building of it is mean and of no great capacity, in both comparable to Caius College.' Surely a superfluous comment in the Journal of a Scholar of St. Catherine's and a Fellow of Trinity!

Ray's speech appears to have been as unguarded as his Journal, for he records that 'the Scots cannot endure to hear their country or their countrymen spoken against,' and it is indeed a fortunate thing that he did not seek to cross the Tay. It would have gone ill with him in 1661 had he ventured to criticise the Highlanders or their food and drink. English Science would, I fear, have suffered an irreparable loss, and there might have been no sufficient reason for that Cambridge Club whose members engage at festive gatherings to prevent the association of the name of Ray with indifferent viands.

Agricultural affairs in Scotland became worse instead of better after Ray's visit, for a series of disastrous seasons set in as the century drew to a close. In August 1694 we are told that 'a cold east wind accompanied by a dense sulphurous fog passed over the country, and the half-filled corn was struck down with mildew.' So bad was the harvest that Hugh Miller tells of the children of a Cromarty farmer who spent the whole winter trying to pick out seed corn from the blighted crop. In the following year summer and winter were alike tempestuous, and thereafter for five seasons there was scarcity, amounting in some districts to famine. By 1701, when the climate began to improve, much land had gone out of cultivation. Landowners could not get tenants to take vacant farms, and the outlook was of the gloomiest. But a change was at hand, and at the time when Lisle and Laurence were deploring the lack of interest which English gentlemen manifested in rural economy, Scotland was beginning those improvements which in a century made Scottish agriculture and gardening the best in Europe. It is indeed a remarkable circumstance that four generations after John Ray recorded his impressions of this lazy, backward nation, we should find another member of the Cambridge Science School, Professor Martyn, taking some pains to prove that Philip Miller of Chelsea never saw Scotland. Miller was so great a gardener, Martyn remarks, that it was generally supposed he must have been a Scotchman; but it was a satisfaction to the Cambridge people of 1800 to know that this distinguished man (whose name and ancestry had then a local interest, for his son was first Curator of the Botanic Garden) had won his pre-eminent place in the gardening world on a purely English ancestry. I am not satisfied that Martyn was right, but in view of the manner in which the Spirit of the Improver was conveyed to Scotland, it would be ungracious to argue the point.

The North Country saying 'the Gordons hae the guidin' o't' applies to agriculture as to much else in Scottish history. In 1706 Lord Huntly married the daughter of the soldier Earl of Peterborough, himself a notable Improver. Lady Huntly (afterwards that Duchess of Gordon who is extolled by 'A Lover of his Country') found the north-east of Scotland in the miserable condition in which 'King William's dark years' had left it. As late as 1709 many farms north of the Grampians were lying waste, the country was depopulated, and the state of the labouring poor was wretched in the extreme. Lady Huntly sent to England for ploughs and ploughmen and taught her neighbours the methods of improved husbandry. Three North Country lairds, stimulated by her example, forsook politics and fighting for draining, planting, and experimenting with French grasses. Among the three was Sir William Gordon of Invergordon, who began those improvements in Easter Ross which have converted the shores of the Cromarty Firth into one of the choicest agricultural tracts of Britain. It was to share in the improving of this district that my own great-grandfather, later in the century, deserted Teesdale for the parish of Cromarty. Thus the work of this first group of Scottish Improvers has for me a very direct interest.

Just as in England a revival of agriculture occurred after the Civil War, so in Scotland the disturbances of 1715 were followed by important developments. After the Union Scotchmen in increasing numbers took the high road to London, and at first with much less profit to themselves than those acquainted with the Scot in modern times might suppose. As a result of social intercourse the upper classes began to copy the manners and customs of their rich English neighbours, and prices and the cost of living rose rapidly. These economic changes, as in England a century before, turned the attention of landowners to the improvement of their estates; but as the Scottish laird of the beginning of the eighteenth century did not take readily to farming, a few of the more enlightened men among them saw that if improvements were to be made special measures were necessary. Impressed by the usefulness of the Royal Society, these reformers conceived the idea of establishing an Agricultural Society in Scotland. This Society, which met for the first time in Edinburgh on June 8, 1723, and adopted the name of 'The Honourable the Society of Improvers in the Knowledge of Agriculture in Scotland,' was the first association to be formed for the express purpose of promoting agriculture. Some account of its work is given in its 'Transactions' published twenty years later, but for a contemporary view of the problems which engaged the Society's attention we must go to a book published in Edinburgh in 1729 under the title of 'An Essay on Ways and Means for Inclosing, Fallowing, Planting, &c., Scotland, and that in Sixteen Years at farthest, by a Lover of his Country.'

Of all old books on agriculture this is, to me, the most interesting. The anonymous writer is believed to have been Brigadier-General Mackintosh of Borlum, one of the Rebel Leaders of 1715, who fell into the hands of the English at Preston, was imprisoned in Newgate, and sentenced to death. But this Highlander was not to be held by English gaolers. With some of his comrades he overpowered the prison guard and made good his escape. The Essay was written, its author informs us, in 'my Hermitage'—supposed to have been a cell in Edinburgh Castle—and the writer remarks that he can give no better reason for his work '*than other* Enthusiasts *do, the Spirit* moves me.'

Assuming 'A Lover of his Country' to have indeed been Mackintosh of Borlum, the prisoner employed his enforced leisure to great advantage. He displays more familiarity with the classical authors than any of his predecessors, or for that matter than any of his successors, except Harte and Adam Dickson, and he had obviously studied all the more important works published in England in the previous century. He argues that since the Union, Scotland had not made progress, and that, while extravagance had spread and necessaries greatly increased in cost, no attempt had been made to learn good rural economy from the English. He points out that until they improve their estates Scottish lairds cannot hope to emulate English landowners. He counsels fallowing and inclosing, and recommends that skilled English labourers should be brought to teach English methods. He indicates where the best workmen might be obtained. Men from Devonshire for denshiring (paring and burning); men from Cambridgeshire for draining; men from Hertfordshire for ploughing; from Hereford for fruit planting; and from Shropshire for hedging. He estimates that six hundred and forty men would be required for Scotland. A 'regimental number,' he facetiously remarks, but a welcome regiment, for they would be armed only with spade and shovel! He would apportion a group of these men to every county in Scotland and place them under the guidance of County Supervisors. 'And if I might have my wish,' he says, 'we should not go on by Halfs, and all *Europe* should be quickly disabused of the Reproach they load us with of *Idleness* and *Poverty*.' In another passage he prophesies that '*Scotland* from one of the poorest, ugliest, and most barren Countries of *Europe*, is, in a very few Years, become one of the richest, most beautiful and fertile Nations of it,' and who would now assert that the old rebel's prophecy has not been fulfilled?

Other objects requiring the attention of the Scottish Improver of 1729 were Land Tenure and Education. Our author urges upon landowners the folly of a system of tenure which demands services from their tenants, and which affords the occupiers no incentive to improve their farms, and he advocates leases not only in the interests of the lairds themselves but in justice to the farmers.

Mackintosh was an enthusiastic educational reformer; he animadverts on the unsuitable instruction offered in rural schools and explains how easily it might be improved. Their Scottish schoolmasters knew Latin; why should they not translate passages from Varro, Columella, and other writers which boys might read? For the use of Charity Schools in remote parts the 'learned Mr. *Ruddiman*' should be employed 'to translate and compendize' the works of Cato, Palladius, and others. Why, he asks, should not one day a week be set aside for the discussion of agricultural subjects, so that country boys might 'dispute on points of Husbandry or breeding or fattening Cattle'? And why should those advanced pupils, who read Greek and Latin authors, have unsuitable text-books placed in their hands? Might not the chaste Hesiod and the useful Varro supplant such works as those of Sappho and Ovid? And why send so many dunces to the university? Let able lads go there by all means, but for the sake of fashion why spend the family income on educating one son at college and neglecting the others? From the school 'A Lover of his Country' turns to the kirk. Much might be done if ministers fallowed or enclosed their glebes, and used their pulpits to enforce lessons of thrift and honest dealing. Then, like Blith, he appeals to the universities. 'And certainly,' he says, 'the learned Masters of our Universities are too well acquainted with the Ancients not to know that Agriculture was the first Care of all Legislatures'; and he continues: 'Natural Philosophy was acquired and attained to by those that laboured the Ground and sowed Seeds, saw their different Ways of Propagation, planted and transplanted Trees, Herbs, Roots, found what ground produced this best, what that, and left their well demonstrated experiments to their sons and posterity; they were the very first Philosophers. . . . To be sure Natural Philosophy was the first, and the sagacious Husbandman the first Philosopher; as the wandering Star-gazing Shepherd or Herdsman was the first Astronomer.'

There is much else in this essay on 'Ways and Means' to which attention might be directed, but my immediate object is to indicate the nature of the questions that stirred the early Scottish Improvers to take action, and having done this I must pass on to notice the work of the Honourable the Society of Improvers in the Knowledge of Agriculture.

As already mentioned, the Society of Improvers was constituted at a meeting held on June 8, 1723. At subsequent meetings in the same summer rules were drafted and the Society began its useful career at once. A Council of twenty-five members was elected, the Council was divided up into sub-committees, each of which was charged with the care of a special branch of agriculture; the rules set out that the members of committees were to 'chuse different subjects in

Agriculture and mark down their thoughts thereon in writing.' They were also to correspond with the most intelligent agriculturists all over the country and to endeavour to get small local societies formed. The chief duty of each sub-committee was, however, to give advice on the means of carrying out improvements. Members were asked to send in an exact statement of their difficulties, and answers were forwarded by the Society. If the suggestions proved useful, the recipient of advice was expected to report the result for the benefit of his fellow members.

The volume of 'Select Transactions,' published in 1743, contains a number of specimens of the questions sent in and the answers supplied. Such subjects as the draining of boggy land, the use of marl and lime, the effects of seaweed as manure, the cultivation of potatoes, hops, sainfoin, and flax; the feeding of cattle and the employment of steeps for corn were dealt with. Most of the correspondence is with Scotchmen, but occasionally letters from others occur, including an interesting communication from Jethro Tull in which he says that 'twenty years ago there was much the same way of tillage in England as is now in Scotland, but it has since been exploded by experience, and the farmers have enriched both the land and themselves by plowing it more than they were wont.' Directions for lime-burning are contributed by Mr. Lummis, 'who came from England and made the Rotheran Plough.' The 'Transactions' have an advertisement of this plough, from which it appears that the Earl of Stair had sent one of his men to be taught by 'the best Plough and Wheel-Carriage wrights in England,' and that Rotheran ploughs of very superior workmanship were being made at Newliston, West Lothian. The Earl of Stair further laid agriculturists under obligation by introducing turnips, cabbages, and carrots as field crops, and he bred very good Galloway cattle. Another notable man among these early improvers was the Earl of Islay, who gave special attention to the cultivation of peaty soils and succeeded in producing good corn and grass on land previously thought to be of little value. He also planted extensively, and, according to Maxwell, introduced the larch, among other trees, to Scottish foresters.

The Society did not confine its attention exclusively to agriculture. It noted a natural connection between the agricultural and fishing industries, and did much to promote the latter, thus establishing an early precedent for the association of agriculture with fisheries for administrative purposes. Manufactures too were encouraged, and in this connection there stands out the name of the Duke of Hamilton, who moved the following 'Overture': 'That all of you and all under your Influence, should, for Examples to others, buy no foreign Linen for Shirting, Bed-linen, or any other Household-furniture; and that you should propagate to the utmost of your power the wearing of home-made stamped Linen.' The consequence, we are informed, was that 'even at Publick Assemblies of Persons of the greatest Distinction, the whole Company appeared dressed in Linen of our own Manufacture.' The Duke's success with linen led him next to propose a resolution against the drinking of foreign spirits, so that the great sum annually sent to France for brandy might be kept at home! The consequences were not so immediately noticeable as in the case of linen, for the local records of the east of Scotland show that the smuggling of French brandy was a very profitable trade throughout the eighteenth century. It is, however, the case that at a later date the Duke's advice was followed, for not only linen but liquor of native manufacture came to be appreciated, 'even at Publick Assemblies of persons of the greatest Distinction'; at assemblies, moreover, on both sides of the Tweed!

As the Fellows of the Royal Society had interested themselves in cider, so the Honourable Society of Improvers selected whisky for their care, and a good deal of attention was given to improvements in distilling. Their correspondence shows that Scottish progress in this art was mainly attributed by them to Mr. Henricus Van Wyngaerden, a Dutchman who settled in Edinburgh about 1730, and 'followed the distilling business with success and a fair character'; but here I question if the Society are altogether just to the enterprise of the father of one of their own most prominent members, Duncan Forbes of Culloden. About 1670 John Forbes, a staunch Presbyterian, who owned an estate in the parish of Ferintosh, withstood Charles and James, and suffered for conscience sake. When William came to the throne, John's son Duncan (father of the Society's member) claimed compensation from the Scottish Parliament. His claim to compensation was proved, but Parliament, having no money wherewith to pay, granted him permission to distil all the grain of Ferintosh at a nominal duty. Thus, says John Hill Burton, Ferintosh became 'illustrious as the head-quarters of the distilling of whisky,' and he adds, 'so short-sighted is man, the efforts of the "merry monarch" to subdue the spirit of the stubborn Presbyterian became the source of conviviality for generations after he and his roystering companions were in their graves.' To Duncan Forbes and Ferintosh rather than to the Duke of Hamilton or Mr. Henricus Van Wyngaerden may be attributed the wide advertisement of native liquor and the gradual displacement of French brandy.

During the twenty-two years of its existence the Honourable Society of Improvers became a powerful and important body. Its influence, it should be noted, was obtained by educational methods, for its funds were small, it had no State subsidy as had the Irish Society, it offered no premiums, but it drew together in the cause of agricultural improvement many of the most prominent Scotchmen of the period, and it undoubtedly laid the foundations of that successful agriculture for which Scotland has ever since been noted. In 1743 the Society had 299 members, and an examination of the list reveals many well-known names representing all sections of the educated classes of Scotland, with the notable exception of the clergy. The great territorial houses were represented by the Dukes of Athole, Hamilton, and Perth, the Earls of Breadalbane, Hopetoun, Stair, and others; politics by Duncan Forbes of Culloden and his restless neighbour Simon of the '45; the College of Justice, the Bar, Writers to the Signet, the University of Edinburgh, and the Medical profession by Bethunes, Campbells, Dalrymples, Erskines, Fergusons, Gordons, and many another well-known Scottish name. A link with the Board of Agriculture and Fisheries of to-day existed in Sir John Anstruther of Anstruther; and of another member, Alexander Brodie of Brodie, Lord Lyon, I have been frequently reminded in preparing this Address, for my copy of 'Columella' bears his bookplate.

But of all the members, those who best deserve our notice are Thomas Hope of Rankeilor, President, and Robert Maxwell of Arkland, editor of the 'Transactions.' Mackintosh refers to Hope as a man who had taught improved agriculture to hundreds of his fellow-countrymen. He studied the subject not only in England, but in France, Flanders, Holland, and other Continental countries, and Maxwell says of him 'that it has been much owing to *Mr. Hope of Rankeilor* your Preses, that this Society was entered into and that the Spirit of it rose so high,' and adds that he 'has been instructing others in the Knowledge of it and been preaching up the publick and private Advantages arising from it for a continued Tract of more than Twenty Years' Time.' Of the spirit which animated Robert Maxwell himself we have ample evidence in the Dedication of the 'Select Transactions.' Reviewing what has been done by the Society and considering that which might still be done, Maxwell writes, 'since the Case stands thus, how much doth it concern the Publick and every Individual that Agriculture be encouraged and that the *Knowledge* of it, the efficient Cause of all those inestimable Benefits, should be taught to all who are willing to learn the Principles of this the most useful of all the Sciences; to all who desire to know the secret Causes why some plants enrich, and others impoverish the Ground in which they grow; why different Methods of Husbandry produce different Effects; and in general to all who incline to study the Reasons for and against, the different Methods practised? They that do not study Agriculture as a Science do right only by chance, and that rarely happens. Why then should Reason be so little exercised, as generally it is, in this Matter of the greatest Importance?' He then refers to the opinions of Virgil, and to the views expressed by Columella on the subject of teaching agriculture, and he urges the Society to take steps to found a professorship, or to secure an inspector of experiments; 'surely a practical farmer should be chosen, who could teach Rules established upon rational Experiments tried in our own country; one who has given Testimonies that he has studied, and does understand the Principles of Agriculture. Your careful Endeavours to get a Fund settled for such a Professor or Inspector would crown your former labours. It will then be said and be certainly true, that you did all you could as *Improvers in the Knowledge of Agriculture.*'

Maxwell proposed that the Society should address a Memorial to the King on the subject of a Professorship. 'You are,' he wrote, 'a great Body of loyal Subjects and generally of great Distinction, and I humbly think upon a proper

Application to his Majesty, you could not fail to have sufficient Influence to get such a Professor or Inspector named or both.'

But alas! Neither professor nor inspector did Maxwell see, for within two years Prince Charles Edward had landed in Scotland, the Marquis of Tullibardine was rallying the Highlanders to the Stuart flag, and the loyalty of the Honourable Society was subjected to a strain which it could not withstand. Most of the members took the advice of Duncan Forbes and held out for the King, but others, like the Duke of Perth and Lords Cromartie, Balmerino, and Lovat, followed Prince Charlie. When peace was restored, the Honourable Society, and not a few of its members, had ceased to exist; but the purpose for which it was founded had been achieved, and the Spirit of the Improver lived on.

One of the objects of the Honourable Society of Improvers was to develop local societies. Two of these may be traced in Scotland before 1745, one in Buchan, the other in East Lothian. The former appears to have been started about 1730 by James Ferguson of Pitfour among his Buchan tenantry. Ferguson was a friend of Thomas Hope's and believed in his methods of 'preaching improvements.' He supplied the members of the Buchan Society with books and he himself attended their meetings. In 1735 this Society published a small volume which had been drawn up by the members at their meetings, entitled 'A True Method of Treating Light Hazely Ground; or, an Exact Relation of the Practice of Farmers in Buchan containing Rules for Infields, Outfields, Haughs, and Laighs.' In many respects this is a remarkable little work. It relates exclusively to local farming, and while the inspiration may have come from Edinburgh, the book itself bears no evidence of outside influence. Their independence is indeed a noteworthy characteristic of the members of this Buchan Society. From certain references which appear in their 'Proceedings' it may be surmised that they were well acquainted with agricultural writers. But instead of recounting the opinions of others, and speculating as to their value for Buchan, this Society of tenant-farmers adopted the true scientific method, they described their practices in detail, discussed them fully, and, being satisfied that they were applicable to local conditions, they reduced their methods to rules. But the Society were most careful to point out that these rules held only for the conditions of Buchan. Their soil 'differs from all others in natural qualities,' it is therefore necessary to give 'uncommon rules in managing it.' They even exclude land lying near the sea in their own immediate neighbourhood; 'in this relation,' they say, 'we make no record of our coast side—neither are our Rules calculated for that part of the country, but are only to be received at two or three miles distance from the sea.'

The Buchan Society's attitude to the agricultural practices recommended by others is well illustrated by the following comment on steeps for seed. Several methods of steeping are mentioned by them, but they add: 'This we doubt not may have some good effect, but frankly own we can give no advice from experience, and so refer the inquisitive to the elaborate works of elder practitioners.' In matters too deep for them, their philosophy rested on a firm basis. Here, for example, is an explanation of the early fruiting of wild oats. This pestilent weed they urge all farmers to destroy by 'cropping the wild oats how soon they come out of the hose, who appear always about eight days before the tame. Thus is Providence so kind as to tack that to their nature which is the means of their own destruction.'

Although improved practices did not reach Scotland for a century after they had been adopted in England, they spread much more rapidly among the northern people. In 1720 there were but a few landowners who made any attempt at improvements in husbandry. In 1723 the Honourable the Society of Improvers was formed, and ten years later we can trace a small society of Aberdeenshire tenants applying the scientific method to the common practice of Scottish farming. The tenant farmers of the North had educated men within their own ranks, and through these men a knowledge of improved practice quickly reached the others. The compiler of the Buchan 'Rules,' James Arbuthnot, tenant of Wester Rora in the parish of Longside, was a type of this class. He had received a classical education and belonged to a branch of a well-known family. The verses of a local poet written on his death show him to have been a man greatly respected in Buchan. A reference to men of the same class—the educated Scottish farmer of the eighteenth century—is made in a lecture given by the Rev. Harry Stuart to the Forfarshire Agricultural Association in 1853. Speaking of his own farming ancestors, Mr. Stuart describes how his grandfather had been made heir to six relatives all in one month by the ambitions of Prince Charlie, and had to begin farming on his own account at seventeen; but before he settled to the plough he had wrestled hard with his books, had committed his Latin grammar to memory, and, as was then the custom in the better schools, had been wont to address the dominie in the Latin tongue. 'There were many such farmers then as he,' says Mr. Stuart, 'reading their Livy to their breakfast and having a tilt at the fencing-foils in the evening with the young fellows. . . . After the fashion of the times he kept open house and I heard all the good and the bad of the new schools just opened discussed a thousand times over by his visitors, many of them retired farming officers, who had seen much in other countries and in a rough enough way, and who did a great deal in spiriting on improvements.' These men, representatives as a rule of a minor branch of some powerful family, fought when the country was at war and farmed when there was peace; they exercised a considerable influence on the development of agriculture, especially in the Highlands.

The second of the local Scottish societies, existing before 1745, was that established by an enlightened landowner, John Cockburn of Ormistown, in East Lothian. Robertson, in his 'Rural Recollections,' gives July 18, 1736, as the date of its formation. With Cockburn were associated Sir John Dalrymple and other country gentlemen. From a reference made to their meetings by Henry Home, it would appear that in this Society we have the origin of the 'farmers' dinner.' Home counsels landlords to 'convene' tenants once a year to a 'hearty meal' at which they were to be instructed in new methods of husbandry. 'It was by such means,' he adds, 'that the late John Cockburn of Ormistown promoted emulation and industry among his people.' But Cockburn did not confine himself to an annual dinner. Monthly meetings were held for the discussion of agricultural improvements, and these were much appreciated not only by Cockburn's tenants but by neighbouring landowners like the Earl of Stair and the Duke of Perth, who attended regularly. Even the '45 did not suppress these monthly meetings, and after Preston Pans the Duke of Perth was mindful enough of Ormistown to send troops to protect the members, so that they might quietly continue their criticisms of Tull and their appreciations of turnips. For which action, had Prince Charlie retained his hold on Scotland, the Duke might have been created first Chairman of the Scottish Board of Agriculture, a Department of State of which, even then, men were beginning to dream!

Maxwell tells us that the Dublin Society (established 1731) was formed in imitation of the Society of Improvers. It is clear when Arthur Young wrote that to the Dublin Society 'belongs the undisputed merit of being the father of all similar societies now existing in Europe' he meant that it was the oldest of existing agricultural societies, and not the first society of its kind. The Dublin Society soon after its formation received a Government grant and could therefore spend much more on its work than its Scottish prototype. Time will not permit of a reference to the work of this Society; but mention may be made of the experimental farm established by the unfortunate John Wynn Baker, under its auspices. The farm was started in 1764 and continued until about 1770. Schemes were drawn up by Baker in consultation with the Society, and an annual grant of 200*l*. was made in support of the experiments; two volumes giving the results were issued. Baker was, as Arthur Young says, 'a very ingenious man' who worked hard for the Society, made experiments in agriculture, recorded meteorological information, manufactured implements, and wrote essays; but he lived in poverty and 'broke his heart' because of the Society's treatment of him. Young and he projected a literary partnership, but Baker's views on authorship could not be reconciled to the demands of the publishers of the time, and it was not until after Baker's death that Young realised in his 'Annals' (1784) the periodical publication which, with Baker, he had tried to start some ten years earlier.

In 1754 the Royal Society of Arts was established, and almost immediately afterwards it began to give attention to agriculture. A record of its valuable work written by Sir Henry Truman Wood has recently been published in the Society's 'Journal.'

The same year that saw the formation of the Royal Society of Arts brought together in Edinburgh a small group of distinguished men who formed themselves into the Select Society. The purposes were the discussion of philosophical questions and practice in public speaking. The idea came from Allan Ramsay, an artist and son of the poet. Alexander Wedderburn was elected Chairman (as Lord Loughborough, the first Scottish Lord Chancellor of England, he affixed the Seal that gave Sir John Sinclair his Board of Agriculture), and among the members were Adam Smith, David Hume, Henry Home (later Lord Kames), and William Robertson (afterwards Principal of Edinburgh University). This Society soon attracted all Edinburgh residents who were in any way distinguished. But in one respect it was a failure; certain members, we are informed, always talked, and the wisdom of others was in danger of being suppressed and unavailing. It is said, for example, that Adam Smith and David Hume never opened their lips! It appears therefore to have been decided that the Society's genius should be turned to practical objects, and within the Select Society a new organisation, the Edinburgh Society, was formed in 1755, 'for the encouragement of Arts, Sciences Manufactures and Agriculture'—*i.e.*, for the same purposes as the Society of Arts had been established in London a few months earlier.

An account of the Edinburgh Society is given by Ramsay in his 'History of the Highland and Agricultural Society of Scotland,' from which it appears that the methods of this society—the offering of premiums for live-stock and implements—were those which have since been everywhere adopted. In 1759, for example, we read that at the show of horses nine stallions were exhibited, 'all very good.' But the goodness of the stallions and of the objects did not bring prosperity to the Edinburgh Society; talent was more abundant than money in Edinburgh in the middle of the eighteenth century, subscriptions remained unpaid, the premium list had to be reduced, and finally the Select and the Edinburgh Societies disappeared together in 1765.

The success of the Royal Society of England and the influence of the Honourable the Society of Improvers of Scotland did not escape notice on the Continent, and after the Peace of Aix-la-Chapelle (1748), when France realised the necessity of developing her agriculture, societies were established in that country. The Marquis of Tourbilli, a well-known writer on agriculture, took the lead in forming a society at Tours; a second important society was formed in Brittany; and so useful did they prove that by 1761, Harte informs us, there were thirteen at work for the improvement of agriculture in France. Each society was assigned a district, and in the larger districts subsidiary societies were formed; of these there were nineteen in 1761.

The movement spread to other countries about the same time. In 1751 George II founded an agricultural society in Hanover, which awarded half-yearly premiums for dissertations on agricultural subjects. In 1759 the Swiss established a society in Berne, which later became the most important agricultural association in Europe. An active society also began work in Tuscany before 1760.

The value of the work of the early associations was also generally recognised in this country, and in the second half of the eighteenth century many others were formed. Among them may be mentioned the Gordon's Mill (Aberdeen) Farming Club, the Highland and Agricultural Society, the British Wool Society, the Norwich, York, and Bath Societies, and the local agricultural societies of Doncaster, Cornwall, Brecon, and Leicester.

Before concluding these notes on the early associations let me ask your attention very briefly to some of the evidences of their influence on the agriculture of a later period.

The chief aims of the early societies were to impress upon landowners in the first place the interest afforded by the study of agriculture and in the second the duty of providing an increased supply of food for the nation. Nothing is more marked in the writings of such Improvers as Blith, Worlidge, Lisle, Laurence, and Mackintosh than their insistence on the importance of agriculture as a subject of study. Until the educated among their fellow-countrymen could be interested in the principles of agriculture, it was clear to these far-seeing men that progress could not be made. Exhortation, persuasion, and satire are employed by turns with the object of securing attention for agricultural questions.

Worlidge, for example, extols a country life and laments the fact that in his day the populace esteemed the country but a place for beasts, while the cities were for men; and Mackintosh of Borlum in upbraiding Scottish gentlemen for despising agriculture, is careful to indicate that the 'pertest Speaker and Despiser of the Farmer' he had met with was 'an upstart estated Spark, Son of a Merchant, who Cato, Cicero, and Varro, all say, don't put together Money so innocently, if the fairest dealing Merchant as the Countryman does.'

The change in the attitude of the educated classes to agriculture that took place within a century of the formation of the Royal Society is indicated in all the works published after 1750. Hirtzel, of Berne, *e.g.*, in 'The Rural Socrates' (second edition, 1764) remarks: 'It is no longer a controvertible point whether the science of Agriculture merits the distinguished attention of philosophical minds, and is the proper study of the most enlightened understanding; since the proof is beyond contradiction, that a judicious rural economy is one of the chief supports of the prosperity of a State.' In Henry Home's dedication of 'The Gentleman Farmer' to the President of the Royal Society (1776), we find this passage: 'Agriculture justly claims to be the chief of arts, it enjoys beside the signal pre-eminence of combining deep philosophy with useful practice'; and in the preface to the same work he says: 'Our gentlemen who live in the country have become active and industrious. They embellish their fields, improve their lands, and give bread to thousands.' He contrasts these pursuits with those which formerly occupied the country gentleman: 'His train of ideas was confined to dogs, horses, hares, foxes; not a rational idea entered the train, not a spark of patriotism, nothing done for the public.'

How unlike the state of affairs described by Home were the conditions in a country resembling Britain, but in which the Spirit of the Improver had not been awakened, may be indicated by a quotation from a report on the farming of Holstein and Mecklenburg sent to Sir John Sinclair in 1794. The writer, M. Voght, states that the agriculture of North Germany was fifty years behind that of England, and explains its depressed state by saying: 'Our noblemen are no farmers, and our farmers no gentlemen; our authors on agriculture possess no cultivated land, and those few who could give to the public the precious results of long experience and labour would starve their printer for want of readers.'

The landowner of North Germany, towards the end of the eighteenth century, was, indeed, in very much the same state as the landowner of Britain in the first quarter; and it is when we compare the conditions described by Lisle, Mackintosh, Home, and Voght, that we begin to appreciate how much British farming owes to such associations as the Royal Society of England and the Honourable the Society of Improvers of Scotland. Had not the interest of landowners, and of the educated classes generally, been secured, there is no reason to suppose that the agriculture of Britain in 1794 would have been markedly in advance of that of Germany.

I have already pointed out that both in England and Scotland the first impetus towards progress was economic in its character, and throughout the seventeenth and eighteenth centuries economic causes were constantly accelerating the improvement of agriculture; but we must not make the mistake of supposing that a rise in prices necessarily brings about improvements in husbandry. A motive for improvement is provided and more labour may be drawn to agriculture, but it does not follow that there will be a real advance, and that there will be more food produced for the use of workers in other industries. Without changes of system, *i.e.*, without improvements based on new discoveries, the effect of a rise of prices in a self-supporting country would merely be to alter the proportion of the population engaged in agriculture, and to form congested districts. This was the danger that threatened England early in the seventeenth and Scotland early in the eighteenth centuries; but fortunately for each country an intellectual revival followed close on the rise in prices, and attention was directed not only to the necessity for more food, but to the need for improvements which would afford a surplus for the support of the industrial classes.

As Adam Dickson, writing at the time, and Thorold Rogers, reviewing economic history a century later, both point out, there is no question that the rise of the mercantile class led to a development of the commercial spirit among the landowners of the eighteenth century; but this is not the particular aspect of the economic question that is suggested by a study of the records of these societies and of the works of the early Improvers. Their first contention was that agriculture was a subject worthy of study for its own sake, their second that it was worthy of study for the sake of the nation. The appeal to self-interest occurs, it is true, but it is not insisted on as being the all-important consideration.

At the present time, when so much of our food comes from other countries, we do not, perhaps, sufficiently realise the extent to which the commercial prosperity of Britain was built up by the aid of, or depended upon an improved agriculture; but the relationship of industrial progress to agriculture was never lost sight of by the early Improvers, and not only the food-supply of the industrial classes, but the system of agriculture best suited for rearing the type of worker required for developing new industries was carefully considered by them.

Within recent years the Improvers of the eighteenth and early nineteenth centuries have been much criticised for their land policy, their enclosures, and their treatment of labourers; but one thing at least the agriculturists of 1760-1815 saw more clearly than their modern critics, they recognised that if their country was to become a great manufacturing nation, more food must be grown; and to this task they applied themselves so successfully that, as Porter points out, the land of Great Britain, which, in 1760, supported about eight million inhabitants, in 1831 supported sixteen millions. When we reflect that the implements of husbandry were rude, that thorough drainage had not been introduced, that artificial manures (except crushed bones) were hardly known, that oilcakes were scarce, that grain was too valuable to be given freely to cattle, that in bad seasons live-stock had to be starved so that men might be fed, that in good seasons prices fell rapidly, and with them farming profits, and that credit was difficult to obtain and interest high, those of us who know something about the ordinary work of the farmer can realise the strenuous efforts that must have been necessary to wring from land a sufficiency to feed this rapidly growing nation and to maintain it in health and comparative comfort. Even as late as 1836 Porter shows that it would have been impossible to feed any considerable part of the people on imported food. 'To supply the United Kingdom with the single article of wheat,' he says, 'would call for the employment of more than twice the amount of shipping which now annually enters our ports.'

Nor was it for man only that an increased food-supply was required; between 1760 and 1831 there was a great addition to the horse population, owing to the improvement of roads and the substitution of horses for oxen on farms. To the increase in the number of horses contemporary writers attributed in some degree the high prices ruling at the end of the eighteenth century.

Part of the additional food-supply was obtained by enclosing about seven million acres of land between 1760 and 1834; but as more than three times this area must already have been enclosed, as much of the land enclosed after 1760 was of poor quality, and as all of it had formerly contributed in some degree to the food-supply of the country, it is obvious that between 1760 and 1834 the rate of production per acre must have been largely increased.

Improvements in the art of agriculture cannot be rapidly introduced; there is first of all an experimental stage, and when improved methods have been learned they pass but slowly from district to district. Before any marked advance in the art can take place, there must therefore occur a period during which a foundation is being laid. It was about 1760 that our population began to increase rapidly, and it was then that agriculturists were called upon to produce more food. As we have seen, they were able to double the food-supply in seventy years. It cannot be doubted that this marvellous feat was rendered possible by the pioneer societies of the preceding century, or that it was the spirit of the Improver, which the early associations had fostered, that animated the men from whom Arthur Young and Sir John Sinclair learned. If, in place of those enterprising agriculturists whose improvements are described in the Reports of the first Board of Agriculture, our shires had been occupied by the dull-witted country gentlemen referred to by Lisle, or the 'upstart sparks' condemned by Mackintosh, the history of this country must have been very different. Behind the military and naval victories which made Britain a great Power, was a commissariat supported by the agricultural classes. For the great industrial army which the genius of Arkwright, Watt, and other inventors provided with employment there was raised an ever-increasing food-supply. Political and industrial development alike depended on the rate of increase of the population, and this again on the rate at which the means of subsistence could be raised from British soil.

Although the economic position has undergone a revolution there is still work for the Improver; no longer indeed do our industrial classes depend for subsistence on the surplus products of the British farmer, but after a long period of forgetfulness, once again it has been recognised that a progressive agriculture is essential to the well-being of the nation. This is not the time to discuss the nature of the questions which press upon us to-day; but let us not forget that they are our questions. To this newly formed Section of the British Association has descended the task of the early associations; it is the privilege of its members to preserve, and to hand down to their successors, that Spirit of the Improver which animated alike the ancient writers of Greece and Rome and the British societies of the seventeenth and eighteenth centuries; and to-day we may take to ourselves the exhortation of Walter Blith, for his words apply to Section M as they did to its predecessors, 'from you, too, I expect and waite for more discoveries of some thing, I scarce know what to name it, which lies yet in obscurity, but I will call it the Improvement of the Improver.'

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