days before and after the eclipse and several hours before sunrise and after sunset. If observations show that the brightness of the zodiacal light is materially diminished during totality, in any part of the region where the moon's shadow darkens the atmosphere, this will go far to show that the zodiacal light originates in the earth's atmosphere; but if, as seen through the shaded air, the zodiacal light appears brighter than ever, it would follow that its location is far from us, and that it is an appendage of the sun.

(4) The observers of the zodiacal light should not fail to record the phenomena sometimes seen on the opposite side of the horizon, and called *Gegenschein*, or the anti-zodiacal light. Similarly, observers of the twilight phenomena should record the appearances in the horizon at the opposite side of the sun, or the so called anti-twilight arc, or band.

(5) Observers to whom the sun is beyond the horizon, and for whom the atmosphere between them and the sun is not illumined owing to the presence of the moon's shadow, will have a good opportunity, for a few minutes, to see any faint comet that may have been hidden to astronomers by the glare of the sunlight, and, if such should be seen, they should record the apparent altitude and azimuth of the nucleus.

The diagrams I. and III. trace the shadow-cone westward to South California and eastward to India, but this should not prevent observers still further west on the Pacific, or east over India and Japan, from recording and reporting such phenomena as they may observe.

Washington, August. CLEVELAND ABBE.

## THE BRITISH ASSOCIATION.

### SECTION D.

#### BIOLOGY.

OPENING ADDRESS BY PROF. J. S. BURDON SANDERSON, M.A., M.D., LL.D., F.R.SS. L. & E., PRESIDENT OF THE SECTION.

IT has long ceased to be possible in the course of an annual address in Section D to give an account even of the most important advances which have been made during the preceding twelve months in the various branches of knowledge which are now included under the term Biology. One reason is that each of the biological subjects has acquired such vast dimensions; the other, that the two main branches—Morphology, which strives to explain why plants and animals have assumed the forms and structure which they possess, and Physiology, which seeks to understand how the living organism works—have now diverged from each other so widely as regards subject and method, that there seems to be danger of complete separation of the one from the other.

From this sundering of sciences which a generation ago were intimately united, however inevitable it may be, Physiology chiefly suffers, as being even to the naturalist less attractive and interesting. The study of form and structure has the great advantage that it brings the observer into direct relation with objects which excite his curiosity without requiring too great an effort to understand them. This was the case even when Anatomy was mainly descriptive, and Zoology and Botany occupied themselves chiefly with classification and with definition of species. How much more is it the case now that Anatomy, Zoology, and Botany have become built into one system, of which the Doctrine of Evolution is the corner stone! Morphology, the name now given to this system, has, if I am not mistaken, this advantage over all other subjects of scientific study—that while attractive to the beginner, it is perfectly satisfactory to the mature student. It derives its perfectness from its subject—the *order* of the plant and animal world. For inasmuch as its fundamental conception is the development of all organisms, however complicated, from elementary forms, and as the theoretical development of the plant and animal world (in other words the science of morphology), claims to be nothing more than a synthesis of the observed facts of its actual development, the science is co-ordinate and conterminous with living nature, and strives after a perfection which is that of nature itself.

Physiology is without this source of attractiveness. Its first lessons present difficulties to the beginner which, unless he is contented (as, indeed, ordinary students are) to accept as true what he does not understand, are, to say the least, discouraging; while to the more mature student, who has mastered more or less some part of the subject, it fails to present a system of knowledge of which all the parts are interdependent and can be referred to one fundamental principle, comparable to that of development or evolution.

It is easy to understand that this must be so if we consider the present position of the subject, and the nature of the work which the physiologist has to do. That work is of two kinds. He has first to determine what are the chemical and physical endowments of living matter in general, and of each of the varieties of living matter which constitute the animal and plant organism in particular. Then, these having been investigated, he has to determine how these processes are localized so as to constitute the special function of each structure, and the relation between structure and process in each case. The order I have indicated is the logical order, but in the actual progress of physiology this order has not been followed, *i.e.* there has not been a correlation of structure with previously investigated process, for in former days physiologists spoke of assimilation, secretion, contraction, and the like, as functions of muscles, glands, or other parts, without recognizing their ignorance of their real nature. But now, no one who is awake to the tendencies of thought and work in physiology, can fail to have observed that the best minds are directed with more concentration than ever before to those questions which relate to the elementary endowments of living matter, and that if they are still held in the background it is rather because of the extreme difficulty of approaching them than from any want of appreciation of their importance.

It is to some of these questions that I am anxious to draw the attention of the Section to-day. I feel that I have set myself a difficult task, but think that, even should I succeed very partially, the attempt may be a useful one. And I am encouraged by the consideration that the interest they possess is one which is common to plant and animal physiology, and that if we really understood them, they would furnish a key, not only to the phenomena of nutrition and growth, but even to those of reproduction and development, and by the belief that it is in the direction of elementary physiology, which means nothing more than the study of the endowments of living material, that the advance of the next twenty years will be made.

Nearly fifty years ago, J. R. Mayer's[1] treatise on the relation between organic motion and the exchange of material in living organisms was published in Germany. Although its value was more appreciated by physicists than by biologists, it was in its purpose, as well as in its subject-matter, physiological. In it Mayer showed for the first time that certain functions of the animal body, which up to that time had been considered most vital, are strictly within reach of measurement, *i.e.* referable to physical standards of quantity. He was even able to demonstrate that those quantitative relations between different kinds of energy which physicists were then only beginning to recognize, held good as regards the processes peculiar to the living organism.

Almost immediately after the appearance of this now celebrated work, a series of discoveries were made in physiology, which constituted the period we are now considering an epoch. Mayer himself had proved that muscles in doing work and producing heat do not do so at the expense of their own substance. But this fact could not be understood until Bernard showed that sugar is one of the most important constituents of the blood, and its storage and production a chief function of the liver. Helmholtz next succeeded in proving what Johannes Müller[2] had declared to be nearly impossible—namely, that the time occupied by the propagation of a motor impulse from the brain to a muscle could be measured, and showed it to be proportional to the distance traversed. Next, du Bois-Reymond investigated the electrical phenomena of living beings, and marshalled them under a physical theory which stood its ground against the severest criticism for more than a generation. And finally, the hydrodynamic principles relating to the circulation, set forth by Dr. Thomas Young in his Croonian Lecture forty

[1] J. R. Mayer, "Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel" (Heilbronn, 1845).
[2] Müller's "Physiology," translation of second edition, p. 2;

years before, were demonstrated experimentally by Ludwig, at the very time when Helmholtz was giving definite form to the great natural philosopher's theory of colour perceptions.

The effect of these discoveries was to produce a complete revolution in the ways of thinking and speaking about the phenomena of life. The error of the past had been to believe that, although the heart resembled a pump, although digestion could be imitated in the laboratory, and comparisons of vital with physical processes could be used for illustration, it was always wrong to identify them. But, inasmuch as it had been learned that sensation is propagated along a nerve just as sound is propagated through the air, only with something like a tenth of the velocity, that the relations between the work done, the heat produced, and the fuel used, can be investigated in the living body just as they are in the steam-engine, it now came to be felt that, in other similar cases, what had been before regarded as peculiarly vital might be understood on physical principles, and that for, the future the word "vital" as distinctive of physiological processes might be abandoned altogether. In looking back, we have no difficulty in seeing that the lines of investigation which were then initiated by such men as Helmholtz, Ludwig, Brücke, du Bois-Reymond, Donders, Bernard, are those along which, during the succeeding generation, the science of physiology advanced; nor can anyone who is acquainted with the literature of that time doubt that these leaders of physiological thought knew that they were the beginners of a new epoch. But such an epoch cannot occur again. We have adopted once for all the right, *i.e.* the scientific method, and there is not the least possibility of our recurring to the wrong. We have no new departure, no change of front in prospect; but even times which are not epochal have their tendencies, and I venture to submit to you, that in physiology the tendency of the present time is characterized by the concentration of the best efforts of the best minds on what I have already referred to as elementary questions. The work of investigating the special functions of organs, which during the last two decades has yielded such splendid results, is still proceeding, and every year new ground is being broken and new and fruitful lines of experimental inquiry are being opened up; but the further the physiologist advances in this work of analysis and differentiation, the more frequently does he find his attention arrested by deeper questions relating to the essential endowments of living matter, of which even the most highly differentiated functions of the animal or plant organism are the outcome. In our science the order of progress has been hitherto and will continue to be the reverse of the order of Nature. Nature begins with the elementary and ends with the complex (first the amœba, then the man). Our mode of investigation has to begin at the end. And this not merely for the historical reason that the first stimulus to physiological inquiry was man's reasonable desire to know himself, but because differentiation actually involves simplification. For just as in manufactures it is the effect of division of labour that less is required of each workman, so in an organism which is made up of many organs, the function of each is simpler.

Physiology, therefore, first studies man and the higher animals and proceeds to the higher plants, then to invertebrates and cryptogams, ending where development begins. From the beginning her aim has been to correlate function with structure, at first roughly, afterwards, when, as I have explained, her methods of observation became scientific, more and more accurately—the principle being *that every appreciable difference of structure corresponds to a difference of function*; and conversely that each endowment of a living organ must be explained, if explained at all, as springing from its structure.

It is not difficult to see whither this method must eventually lead us. For inasmuch as function is more complicated than structure, the result of proceeding, as Physiology normally does, from structure to function, must inevitably be to bring us face to face with functional differences which have no structural difference to explain them. Thus, for example, if the physiologist undertakes to explain the function of a highly differentiated organ like the eye, he finds that up to a certain point, provided that he has the requisite knowledge of dioptrics, the method of correlation guides him straight to his point. He can mentally or actually construct an eye which will perform the functions of the real eye, in so far as the formation of a real image of the field of vision on the retina is concerned, and will be able thereby to understand how the retinal picture is transferred to the organ of consciousness. Having arrived at this point he begins to correlate the known structure of the retina with what is required of it, and finds that the number of objects which he can discriminate in the field of vision is as numerous as, but not more numerous than, the parts of the retina, *i.e.* the cones which are concerned in discriminating them. So far he has no difficulty; but the method of correlation fails him from the moment that he considers that each object point in the field of vision is coloured, and that he is able to discriminate not merely the number and the relations of all the object points to each other, but the colour of each separately. He then sees at once that each cone must possess a plurality of endowments for which its structure affords no explanation. In other words, in the minute structure of the human retina, we have a mechanism which would completely explain the picture of which I am conscious, were the objects composing it possessed of one objective quality only, being colourless, but it leaves us without explanation of the differentiation of colour.

Similarly, if he is called upon to explain the function of a secreting gland, such, *e.g.*, as the liver, there is no difficulty in understanding that, inasmuch as the whole gland consists of lobules which resemble each other exactly, and each lobule is similarly made up of cells which are all alike, each individual cell must be capable of performing all the functions of the whole organ. But when by exact experiment we learn that the liver possesses not one function but many—when we know that it is a storehouse for animal starch, that each cell possesses the power of separating waste colouring-matter from the blood, and of manufacturing several kinds of crystallizable products, some of which it sends in one direction and others in the opposite—we find again that the correlation method fails us, and that all that our knowledge of the minute structure has done for us is to set before us a question which, though elementary, we are quite unable to answer.

By multiplying examples of the same kind, we should in each case come to the same issue, namely, *plurality of function with unity of structure*, the unity being represented by a simple structural element—be it retinal cone or cell—possessed of numerous endowments. Whenever this point is arrived at in any investigation, structure must for the moment cease to be our guide, and in general two courses or alternatives are open to us. One is to fall back on that worn-out *Deus ex machinâ*, protoplasm, as if it afforded a sufficient explanation of everything which cannot be explained otherwise, and accordingly to defer the consideration of the functions which have no demonstrable connection with structure as for the present beyond the scope of investigation; the other is, retaining our hold of the fundamental principle of correlation, to take the problem in reverse, *i.e.* to use analysis of function as a guide to the ultra-microscopical analysis of structure.

I need scarcely say that of these two courses the *first* is wrong, the *second* right, for in following it we still hold to the fundamental principle *that living material acts by virtue of its structure*, provided that we allow the term structure to be used in a sense which carries it beyond the limits of anatomical investigation, *i.e.* beyond the knowledge which can be attained either by the scalpel or the microscope. We thus (as I have said) proceed from function to structure, instead of the other way.

The departure from the traditions of our science which this change of direction seems to imply is indeed more apparent than real. In tracing the history of some of the greatest advances, we find that the recognition of function has preceded the knowledge of structure. Haller's discovery of irritability was known and bore fruit, long before anything was known of the structure of muscle. So also, at a later period, Bichat was led by his recognition of the physiological differences between what he termed the functions of organic and animal life, to those anatomical researches which were the basis of the modern science of Histology. Again, in much more recent times, the investigation of the function of gland cells, which has been carried on with such remarkable results by Prof. Heidenhain in Germany, and with equal success by Mr. Langley in this country, has led to the discovery of the structural changes which they undergo in passing from the state of repose to that of activity: nor could I mention a better example than that afforded (among many others relating to the physiology of the nervous system) by Dr. Gaskell's recent and very important discovery of the anatomical difference between cerebro-spinal nerves of different functions. We may therefore anticipate that the future of physiology will differ from the past chiefly in this respect—that whereas hitherto the greater part of the work has consisted in the interpretation of facts arrived at in the first instance by anatomical methods of research, Histology, once the guide of Physiology, has now become her handmaid.

During the last ten or fifteen years Histology has carried her methods of research to such a degree of perfection that further improvement scarcely seems possible. As compared with these subtle refinements, the "minute anatomy" of thirty years ago appears coarse—the skill for which we once took credit seems but clumsiness. Notwithstanding, the problems of the future from their very nature lie as completely out of reach of the one as of the other. It is by different methods of investigation that our better equipped successors must gain insight of those vital processes of which even the ultimate results of microscopical analysis will ever be, as they are now, only the outward and visible sign.

In what has preceded, I have endeavoured to show that at present the fundamental questions in physiology, the problems which most urgently demand solution, are those which relate to the endowments of apparently structureless living matter, and that the most important part of the work of the immediate future will be the analysis of these endowments. With this view, what we have to do is, first, to select those cases in which the vital process offers itself in its simplest form, and is consequently best understood; and, secondly, to inquire how far in these particular instances we may, taking as our guide the principle I have so often mentioned as fundamental, viz. the correlation of structure with function, of mechanism with action, proceed in drawing inferences as to the mechanism by which these vital processes are in these simplest cases actually carried out.

The most distinctive peculiarity of living matter as compared with non-living is that it is ever changing while ever the same, *i.e.* that life is a state of ceaseless change. For our present purpose I must ask you, first, to distinguish between two kinds of change which are equally characteristic of living organisms—namely, those of growth and decay on the one hand, and those of nutrition on the other. Growth the biologist calls evolution. Growth means the unfolding, *i.e.* development, of the latent potentialities of form and structure which exist in the germ, and which it has derived by inheritance. A growing organism is not the same to-day as it was yesterday, and consequently not quite the same now as it was a minute ago, and never again will be. This kind of change I am going to ask you to exclude from consideration altogether at this moment, for in truth it does not belong to Physiology, but rather to Morphology, and to limit your attention to the other kind which includes all other vital phenomena. I designated it just now as nutrition, but this word expresses my meaning very inadequately. The term which has been used for half a century to designate the sum or complex of the non-developmental activities of an organism is "exchange of material," for which Prof. Foster has given the very acceptable substitute Metabolism. Metabolism is only another word for "change," but in using it we understand it to mean that, although an organism in respect of its development may never be what it has been, the phases of alternate activity and repose which mark the flow of its life-stream are recurrent. Life is a Cyclosis in which the organism returns after every cycle to the same point of departure, ever changing yet ever the same.

It is this antithesis which constitutes the essential distinction between the two great branches of biology, the two opposite aspects in which the world of life presents itself to the inquiring mind of man. Seen from the morphological side, the whole plant and animal kingdom constitutes the unfolding of a structural plan which was once latent in a form of living material of great apparent simplicity. From the physiological side this apparently simple material is seen to be capable of the discharge of functions of great complexity, and therefore must possess corresponding complexity of mechanism. It is the nature of this invisible mechanism that physiology thirsts to know. Although little progress has as yet been made, and little may as yet be possible, in satisfying this desire, yet, as I shall endeavour to show you, the existing knowledge of the subject has so far taken consistent form in the minds of the leaders of physiological thought that it is now possible to distinguish the direction in which the soberest speculation is tending.

The *non-developmental* vital functions of protoplasm are the absorption of oxygen, the discharge of carbon dioxide and water and ammonia, the doing of mechanical work, the production of heat, light, and electricity. All these, excepting the last, are known to have chemical actions as their inseparable concomitants. As regards electricity, we have no proof of the dependence of the electrical properties of plants and animals on chemical action. But all the other activities which have been mentioned are fundamentally chemical.

Let us first consider the relation of oxygen to living matter and vital process. For three-quarters of a century after the fundamental discoveries of Lavoisier and Priestley (1772–76), the accepted doctrine was that the effete matter of the body was brought to the lungs by the circulation and burnt there, of which fact the carbon dioxide expired seemed an obvious proof. Then came the discovery that arterial blood contained more oxygen than venous blood, and consequently that oxygen must be conveyed as such by the blood-stream to do its purifying work in all parts of the body, this advance in the understanding of the process being crowned a few years later by the discovery of the oxygen-carrying properties of the colouring-matter of the blood, in which the present President of the Royal Society took so prominent a part. Finally, between 1872 and 1876, as the result of an elaborate series of investigations of the respiratory process, the proof was given by Pflüger[1] that the function of oxygen in the living organism is not to destroy effete matter either here or there, but rather to serve as a food for protoplasm, which, so long as it lives, is capable of charging itself with this gas, absorbing it with such avidity, that, although its own substance retains its integrity, no free oxygen can exist in its neighbourhood. This discovery, of which the importance is comparable with that of Lavoisier, can best be judged of by considering its influence on other fundamental conceptions of the vital process. The generally accepted notion of effete matter waiting to be oxidized was associated with a more general one, viz. that the elaborate structure of the body was not permanent, but constantly undergoing decay and renewal. What we have now learnt is, that the material to be oxidized comes as much from the outside as the oxygen which burns it, though the reaction between them, *i.e.* the oxidation, is intrinsic, *i.e.* takes place within the living molecular framework.

Protoplasm, therefore, understanding by the term the visible and tangible presentation to our senses of living material, comes to consist of two things—namely, of framework and of content—of channel and of stream—of acting part which lives and is stable, and of acted-on part which has never lived and is labile, that is, in a state of metabolism, or chemical transformation.

If such be the relation between the living framework and the stream which bathes it, we must attribute to this living, stable, acting part, a property which is characteristic of the bodies called in physiological language ferments, or enzymes, the property which, following Berzelius, we have for the last half-century expressed by the word *catalytic;* and use, without thereby claiming to understand it, to indicate a mode of action in which the agent which produces the change does not itself take part in the decompositions which it produces.

I have brought you to this point as the outcome of what we know as to the essential nature of the all-important relation between oxygen and life. In botanical physiology the general notion of a stable catalyzing framework, and of an interstitial labile material, which might be called catalyte, has been arrived at on quite other grounds. This notion is represented in plant physiology by two words, both of which correspond in meaning—Micellæ, the word devised by Nägeli, and the better word Tagmata, substituted for it by Pfeffer. Nägeli's word has been adopted by Prof. Sachs as the expression of his own thought in relation to the ultra-microscopical structure of the protoplasm of the plant cell. His view is that certain well-known properties of organized bodies require for their explanation the admission that the simplest *visible* structure is itself made up of an arrangement of units of a far inferior order of minuteness. It is these hypothetical units that Nägeli has called micellæ.

Now, Nägeli in the first instance confounded the micellæ with molecules, conceiving that the molecule of living matter must be of enormous size.[2] But, inasmuch as we have no reason for believing that any form of living material is chemically homogeneous, it was soon recognized, perhaps first by Pfeffer, but eventually also by Nägeli himself, that a micella, the ultimate

[1] *Pflüger's Archiv*, vol. vi., 1872, p. 43. and vol. x., 1875, p. 251, "Ueber die physiologische Verbrennung in den lebendigen Organismen."

[2] Nägeli, "Theorie der Gährung; Beitrag zur Moleculär Physiologie," p. 121 (1879).

element of living material, is not equivalent to a molecule, however big or complex, but must rather be an arrangement or phalanx of molecules of different kinds. Hence the word tagma, first used by Pfeffer,[1] has come to be accepted as best expressing the notion. And here it must be noted that each of the physiologists to whom reference has been made regards the micellæ, not as a mere aggregate of separate particles, but as connected together so as to form a system, a conception which is in harmony with the view I gave you just now from the side of animal physiology, of catalyzing framework and interstitial catalyzable material.

To Prof. Sachs, this porous constitution of protoplasm serves to explain the property of vital turgescence—that is, its power of charging itself with aqueous liquid—a power which Sachs estimates to be so enormous that living protoplasm may, he believes, be able to condense water which it takes into its interstices to less than its normal volume. For our present purpose it is sufficient for us to understand that to the greatest botanical thinkers, as well as to the greatest animal physiologists, the ultimate mechanism by which life is carried on is not, as Prof. Sachs[2] puts it, "slime," but "a very distensible and exceedingly fine network."

And now let us try to get a step further by crossing back in thought from plants to animals. At first sight, the elementary vital processes of life seem more complicated in the animal than in the plant, but they are, on the contrary, simpler; for plant protoplasm, though it may be structurally homogeneous, is dynamically polyergic—it has many endowments—whereas in the animal organism there are cases in which a structure has only one function assigned to it. Of this the best examples are to be found among so-called excitable tissues, viz. those which are differentiated for the purpose of producing (along with heat) mechanical work, light, or electricity. In the life of the plant these endowments, if enjoyed at all, are enjoyed in common with others.

By the study, therefore, of muscle, of light organ, and of electrical organ, the vital mechanism is more accessible than by any other portal. About light organs we as yet know little, but the little we know is of value; of electrical organs rather more; about muscle a great deal.

To the case of muscle, Engelmann, one of the best observers and thinkers on the elementary questions which we have now before us, has transferred the terminology of Nägeli and Pfeffer as descriptive of the mechanism of its contraction. Muscular protoplasm differs from those kinds of living matter to which I have applied the term "polyergic," in possessing a molecular structure comparable with that of a crystal in the respect, that each portion of the apparently homogeneous and transparent material of which it consists resembles every other.

With this ultra-microscopical structure, its structure as investigated by the microscope may be correlated, the central fact being that, just as a muscular fibre can be divided into cylinders by cross-sections, so each such cylinder is made up of an indefinite number of inconceivably minute cylindrical parts, each of which is an epitome of the whole. These, Engelmann, following Pfeffer, calls ino-tagmata. So long as life lasts each minute phalanx has the power of keeping its axis parallel with those of its neighbours, and of so acting within its own sphere as to produce, whenever it is awakened from the state of rest to that of activity, a fluxion from poles to equator. In other words, muscle, like plant protoplasm, consists of a stable framework of living catalyzing substance, which governs the mechanical and chemical changes which occur in the interstitial catalyzable material, with this difference, that here the ultra-microscopical structure resembles that of a uniaxial crystal,[3] whereas in plant protoplasm there may be no evidence of such arrangement.

According to this scheme of muscular structure, the contraction, *i.e.* the change of form which, if allowed, a muscle undergoes when stimulated, has its seat not in the system of tagmata but in the interstitial material which surrounds it, and consists in the migration of that labile material from pole to equator, this being synchronous with explosive oxidation, sudden disengagement of heat and change in the electrical state of the living substance. Let us now see how far the scheme will help us to an understanding of this marvellous concomitance of chemical, electrical, and mechanical change.

It is not necessary to prove to you that the discharge of carbon dioxide and the production of heat which we know to be associated with that awakening of a muscle to activity which we call stimulation, are indices of oxidation. If we take this fact in connection with the view that has just been given of the mechanism of contraction, it is obvious that there must be in the sphere of each tagma an accumulation of oxygen and oxidizable material, and that concomitantly with or antecedently to the migration of liquid from pole to equator, these must come into encounter. Let us for a moment suppose that a soluble carbohydrate is the catalyzable material, that this is accumulated equatorially, and oxygen at the poles, and consequently that between equator and poles water and carbon dioxide, the only products of the explosion, are set free. That the process is really of this nature is the conclusion to which an elaborate study of the electrical phenomena which accompany it has led one of the most eminent physiologists of the present time, Prof. Bernstein.[1] To this I wish for a moment to ask your attention.

Prof. Bernstein's view of the molecular structure of muscular protoplasm is in entire accordance with the theory of Pflüger and with the scheme of Engelmann, with this addition, that each ino-tagma is electrically polarized when in a state of rest, depolarized at the moment of excitation or stimulation, and that the axes of the tagmata are so directed that they are always parallel to the surface of the fibre, and consequently have their positive sides exposed. In this amended form the theory admits of being harmonized with the fundamental facts of muscle-electricity—namely, that cut surfaces are negative to sound surfaces, and excited parts to inactive—provided that the direction of the hypothetical polarization is from equator to pole, *i.e.* that in the resting state the poles of each tagma are charged with negative ions, the equators with positive; and consequently that the direction of the discharge in the catalyte at the moment that the polarization disappears is from pole to equator.

Time forbids me even to attempt to explain how this theory enables us to express more consistently the accepted explanations of many collateral phenomena, particularly those of electrotonus. I am content to show you that it is not impossible to regard the three phenomena—viz. chemical explosion, sudden electrical change, and change of form—as all manifestations of one and the same process—as products of the same mechanism.

In plants, in certain organs or parts in which movement takes place, as in muscles in response to stimulation, the physiological conditions are the same or similar, but the structural very different; for the effect is produced not by a change of form, but by a diminution of volume of the excited part, and this consists not of fibres, but of cells. The way in which the diminution of volume of the whole organ is brought about is by diminution of the volume of each cell, an effect which can obviously be produced by flow of liquid out of the cell. At first sight therefore the differences are much more striking than the resemblances.

But it is not so in reality, for the more closely we fix our attention on the elementary process rather than on the external form, the stronger appears the analogy—the more complete the correspondence. The state of turgor, as it has been long called by botanical physiologists, by virtue of which the framework of the protoplasm of the plant retains its content with a tenacity to which I have already referred, is the analogue of the state of polarization of Bernstein. As regards its state of aggregation, it can scarcely be doubted that, inasmuch as the electrical concomitants of excitation of the plant cell so closely correspond with those of muscle, here also the tagmata are cylindrical, and have their axes parallel to each other. Beyond this we ought perhaps not to allow speculation to carry us, but it is scarcely possible to refrain from connecting this inference with the streaming motion of protoplasm which in living plant cells is one of the indices of vitality. If, as must I think be supposed, this movement is interstitial, *i.e.* due to the mechanical action of the moving protoplasm on itself, we can most readily understand its mechanism as consisting in rhythmically recurring phases of close and open order in the direction of the tagmatic axes.

In submitting this hypothesis I do not for a moment forget that the facts relating to the contractility of plant cells have as yet been insufficiently investigated. No one has as yet shown that when the leaf of the sensitive plant falls, or that of the fly-trap closes on its prey, heat is developed or oxidation takes

[1] Pfeffer, "Pflanzenphysiologie," p. 12 (Leipzig, 1881).
[2] Sachs, "Experimental-Physiologie," p. 443 (1865).
[3] Brücke, "Vorlesungen," second edition, vol. ii. p. 497.

[1] Bernstein, "Neue Theorie der Erregungsvorgänge und electrischen Erscheinungen an den Nerven- und Muskelfasern," *Untersuchungen aus dem Physiologischen Institut* (Halle, 1888).

place, but it does not seem to me very rash to anticipate that if it were possible to make the experiment to-morrow it would be found to be so.

I have thus endeavoured (building on two principles in physiology, firstly that of the constant correlation of mechanism and action, of structure and function, and secondly the identity of plant and animal life both as regards mechanism and structure; and on two experimentally ascertained elementary relations, viz. the relation of living matter or protoplasm to water on the one hand, and to oxygen and food on the other) to present to you in part the outline or sketch of what might, if I had time to complete it, be an adequate conception of the mechanism and process of life as it presents itself under the simplest conditions. To complete this outline, so far as I can to-day, I have but one other consideration to bring before you, one which is connected with the last of my four points of departure —that of the relation of oxygen to protoplasm, a relation which springs out of the avidity with which, without being oxidized or even sensibly altered in chemical constitution, it seizes upon oxygen and stores it for its own purposes. The consideration which this suggests is that if the oxygen and oxidizable material are constantly stored, they must either constantly or at intervals be discharged, and inasmuch as we know that in every instance without exception in which heat is produced or work is done, these processes have discharge of water and of carbon dioxide for their concomitants, we are justified in regarding these discharges as the sign of expenditure, the charging with oxygen as the sign of restitution. In other words, a new characteristic of living process springs out of those we have already had before us—namely, that it is a constantly recurring alternation of opposite and complementary states, that of activity or discharge, that of rest or restitution.

Is it so, or is it not? In the minds of most physiologists the distinction between the phenomena of discharge and the phenomena of restitution (*Erholung*) is fundamental, but beyond this, unanimity ceases. Two distinguished men, one in Germany and one in England—I refer to Prof. Hering and Dr. Gaskell—have taken, on independent grounds, a different view to the one above suggested, according to which, life consists, not of alternations between rest and activity, charge and discharge, loading and exploding, but between two kinds of activity, two kinds of explosion, which differ only in the direction in which they act, in the circumstance that they are antagonistic to each other.

Now when we compare the two processes of rest, which as regards living matter means restitution, and discharge, which means action, with each other, they may further be distinguished in this respect, that, whereas restitution is autonomic, *i.e.* goes on continuously like the administrative functions of a well-ordered community, the other is occasional, *i.e.* takes place only at the suggestion of external influences; that, in other words, the contrast between action and rest is (in relation to protoplasm) essentially the same as between waking and sleeping.

It is in accordance with this analogy between the alternation of waking and sleeping of the whole organism, and the corresponding alternation of restitution and discharge, of every kind of living substance, that physiologists by common consent use the term Stimulus (*Reiz, Prikkeling*), meaning thereby nothing more than that it is by external disturbing or interfering influence of some kind that energies stored in living material are (for the most part suddenly) discharged. Now, if I were to maintain that restitution is not autonomic, but determined, as waking is, by an external stimulus—that it differs from waking only in the direction in which the stimulation acts, *i.e.* in the tendency towards construction on the one hand, towards destruction on the other—I should fairly and as clearly as possible express the doctrine which, as I have said, the two distinguished teachers I have mentioned, viz. Dr. Gaskell[1] and Prof. Hering, have embodied in words which have now become familiar to every student. The words in question, "anabolism," which being interpreted means winding up, and "catabolism," running down, are the creation of Dr. Gaskell. Prof. Hering's equivalents for these are "assimilation," which, of course, means storage of oxygen and oxidizable material, and "disassimilation," discharge of these in the altered form of carbon dioxide and water. But the point of the theory which attaches to them lies in this, that that wonderful power which living material enjoys of continually building itself up out of its environment, is, as I have already suggested, not autonomic, but just as dependent on occasional and external influences or stimuli, as we know the disintegrating processes to be; and accordingly Hering finds it necessary to include under the term stimuli not only those which determine action, but to create a new class of stimuli which he calls *Assimilations-Reize*, those which, instead of waking living mechanism to action, provoke it to rest.

It is unfortunately impossible within the compass of an address like the present to place before you the wide range of experimental facts which have led two of the strongest intellects of our time to adopt a theory which, when looked at *a priori*, seems so contradictory. I must content myself with mentioning that Hering was led to it chiefly by the study of one of the examples to which I referred in my introduction—namely, the colour-discriminating functions of the retina; Dr. Gaskell by the study of that very instructive class of phenomena which reveal to us that among the channels by which the brain maintains its sovereign power as supreme regulator of all the complicated processes which go on in the different parts of the animal organism, there are some which convey only commands to action, others commands to rest, the former being called by Gaskell catabolic, the latter anabolic. To go further than this would not only wear out your patience but would carry me beyond the limits I proposed to myself, viz. the mechanism of life in its simplest aspects. I therefore leave the subject here, adding one word only. The distinction which has suggested to their authors the words on which I have been commenting is a real one, but it implies rather the interference with each other of the simultaneous operation of two regulating mechanisms, than an antagonism between two processes of opposite tendencies carried on by the same mechanism; or, putting it otherwise, that the observed antagonism is between one nervous mechanism and another, and not between two antagonistic functions of the same living material.

Without attempting to recapitulate, I have a word to say by way of conclusion on a question which may probably have suggested itself to some of my audience.

I have indicated to you that although scientific thought does not, like speculative, oscillate from side to side, but marches forward with a continued and uninterrupted progress, the stages of that progress may be marked by characteristic tendencies; and I have endeavoured to show that in physiology the questions which concentrate to themselves the most lively interest are those which lie at the basis of the elementary mechanism of life.

The word Life is used in physiology in what, if you like, may be called a technical sense, and denotes only that state of *change with permanence* which I have endeavoured to set forth to you. In this restricted sense of the word, therefore, the question "What is Life?" is one to which the answer is approachable; but I need not say that in a higher sense—higher because it appeals to higher faculties in our nature—the word suggests something outside of mechanism, which may perchance be its cause rather than its effect.

The tendency to recognize such a relation as this is what we mean by vitalism. At the beginning of this discourse I referred to the anti-vitalistic tendency which accompanied the great advance of knowledge that took place at the middle of the century. But even at the height of this movement there was a reaction towards vitalism, of which Virchow,[1] the founder of modern pathology, was the greatest exponent. Now, a generation later, a tendency in the same direction is manifesting itself in various quarters. What does this tendency mean? It has to my mind the same significance now that it had then. Thirty years ago the discovery of the cell as the basis of vital function was new, and the mystery which before belonged to the organism was transferred to the unit, which while it served to explain everything was itself unexplained. The discovery of the cell seemed to be a very close approach to the mechanism of life, but now we are striving to get even closer, and with the same result. Our measurements are more exact, our methods finer; but these very methods bring us to close quarters with phenomena which, although within reach of exact investigation, are as regards their essence involved in a mystery which is the more profound the more it is brought into contrast with the exact knowledge we possess of surrounding conditions.

If what I have said is true, there is little ground for the apprehension that exists in the minds of some that the habit of

[1] See Gaskell in *Ludwig's Festschrift*, and Hering, "Zur Theorie der Vorgänge in der lebendigen Substanz," pp. 1–22 (Prag, 1888).

[1] Virchow, "Alter und Neuer Vitalismus," *Archiv für pathol. Anat* 1856, vol. ix. p. 1. See also Rindfleisch, "Ärztliche Philosophie," pp. 10–1 Wurzburg, 1888).

scrutinizing the mechanism of life tends to make men regard what can be so learned as the only kind of knowledge. The tendency is now certainly rather in the other direction. What we have to guard against is the mixing of two methods, and so far as we are concerned the intrusion into our subject of philosophical speculation. Let us willingly and with our hearts do homage to "divine Philosophy," but let that homage be rendered outside the limits of our science. Let those who are so inclined, cross the frontier and philosophize; but to me it appears to be more conducive to progress that we should do our best to furnish professed philosophers with such facts relating to structure and function as may serve them as aids in the investigation of those deeper problems which concern man's relations to the past, the present, and the unknown future.

## SECTION H.

### ANTHROPOLOGY.

OPENING ADDRESS BY PROF. SIR WILLIAM TURNER, M.B., LL.D., F.R.SS.L. & E., PRESIDENT OF THE SECTION.

TWENTY-SIX years have passed by since the British Association for the Advancement of Science last assembled in this city. Many of the incidents of that meeting are still fresh in my memory, the more vividly, perhaps, because it was the first meeting of the Association that I had attended. The weather, so important a factor in most of our functions, was dry and bright. The visitor, instead of being enshrouded in that canopy of mist and smoke which so often meets the traveller as he approaches your city, was greeted with light and sunshine. The cordial welcome and reception so freely granted by the community, and more especially the princely yet gracious hospitality exercised by the President, your eminent townsman, now Lord Armstrong, are all deeply imprinted on my memory. But, apart from these attractions, which added so much to the amenities of the occasion, the meeting was one of deep interest to all those Members and Associates who were engaged in biological study.

Lyell's famous book on the "Antiquity of Man" had been published shortly before. The essays on the "Origin of Species" by natural selection, by Charles Darwin and Alfred Russel Wallace, had appeared only five years earlier in the Journal of the Linnean Society, and in 1859 Darwin's treatise on the "Origin of Species," in which its illustrious author summarized the facts he had collected and the conclusions at which he had arrived, had been published. Although no President of the British Association had up to that time given his adhesion to the new theory, yet it was clear that men were beginning to see, in many instances perhaps only dimly, how the theory of evolution by natural selection was destined to work a remarkable change, amounting almost to a revolution, in our conceptions of biological questions generally, and their applicability to the study of man.

At that time Anthropology had not assumed so definite a position in the work of the Association as it now possesses. Neither a Department nor a Section was devoted to it, and the subjects which it embraces were scattered abroad, either in the Department of Anatomy and Physiology, in the Section of Geography and Ethnology, in that of Geology, or in that of Statistics. It is true that a vigorous attempt was made about that time to give it a more independent position, but it was not until the Association met in Nottingham, in 1866, that it was assigned a definite Department, and at the Montreal meeting, in 1884, Anthropology assumed the dignity of a Section.

But although the youngest Section of the Association, the Science of Man is not the youngest of the sciences. Long before the British Association came into existence, Man, in his physical, racial, geological, and psychological aspects, had been studied by hosts of able and industrious inquirers. All that the Association has done in establishing a special Section of Anthropological Science has been to bring together, as it were, into a single focus all those workers who apply themselves to the study of man in his various aspects.

As presiding over the proceedings of the Section on this occasion, it is a part of my duty to open its public business with an address. For me, as doubtless for many of those who have preceded me in this honourable office, one's mind has been somewhat exercised in the choice of a subject. In a branch of biological science so vast as Anthropology, in which the room for selection is so ample, the difficulty of making a choice is perhaps still further increased. As a professional anatomist, whose life's work it has been to study the structure of the human body in its normal aspects, to inquire into the variations which it exhibits in different individuals, and to compare its structure with that of various forms of animal life, it at first occurred to me that an address on the physical characteristics of some of the races of men would be appropriate. But further consideration led me to think that such a subject would be too technical for a general audience, and that it might perhaps be productive of greater interest on the part of my auditors if I selected a topic which, whilst strictly scientific in all its bearings, yet appeals more distinctly to the popular mind, and is now attracting attention. Hence I have chosen the subject of Heredity, by which I mean that special property through which the peculiarities of an organism are transmitted to its descendants throughout successive generations, so that the offspring, in their main features, resemble their parents.

The subject of Heredity, if I may say so, is in the air at the present time. The journals and magazines, both scientific and literary, are continually discussing it, and valuable treatises on the subject are appearing at frequent intervals. But though so important a topic of existing scientific thought and speculation, it is by no means a new subject, and certain of its aspects were under discussion so far back as the time of Aristotle. The prominence which it has assumed of late years is in connection with its bearing on the Darwinian Theory of Natural Selection, and, consequently, biologists generally have had their attention directed to it. But in its relations to Man, his structure, functions, and diseases, it has long occupied a prominent position in the minds of anatomists, physiologists, and physicians. That certain diseases, for example, are hereditary was recognized by Hippocrates, who stated generally that hereditary diseases are difficult to remove, and the influence which the hereditary transmission of disease exercises upon the duration of life is the subject of a chapter in numerous works on practical medicine, and forms an important element in the valuation of lives for life insurance.

The first aspect of the question which has to be determined is whether any physical basis can be found for Heredity. Is there any evidence that the two parents contribute each a portion of its substance to the production of the offspring so that a physical continuity is established between successive generations? The careful study, especially during the last few years, of the development of a number of species of animals mostly but not exclusively among the Invertebrata, by various observers, of whom I may especially name Bütschli, Fol, E. Van Beneden, and Hertwig, has established the important fact that the young animal arises by the fusion within the egg or germ-cell of an extremely minute particle derived from the male parent with an almost equally minute particle derived from the germ-cell produced by the female parent. These particles are technically termed in the former case the *male pronucleus*, in the latter the *female pronucleus*, and the body formed by their fusion is called the *segmentation nucleus*. These nuclei are so small that it seems almost a contradiction in terms to speak of their magnitude; rather one might say their minimitude, for it requires the higher powers of the best microscopes to see them and follow out the process of conjugation. But notwithstanding their extreme minuteness, the pronuclei and the segmentation nucleus are complex both in chemical and molecular structure. From the segmentation nucleus produced by the fusion of the pronuclei with each other, and from corresponding changes which occur in the protoplasm of the egg which surrounds it, other cells arise by a process of division, and these in their turn also multiply by division. These cells arrange themselves in course of time into layers which are termed the germinal or embryonic layers. From these layers arise all the tissues and organs of the body, both in its embryonic and adult stages of life. The starting-point of each individual organism—*i.e.* of each new generation—is therefore the segmentation nucleus. Every cell in the adult body is derived by descent from that nucleus through repeated division. As the segmentation nucleus is formed by the fusion of material derived from both parents, a physical continuity is established between parents and offspring. But this physical continuity carries with it certain properties which cause the offspring to reproduce, not only the bodily configuration of the parent, but other characters. In the case of Man we find along with the family likeness in form and features a correspondence in temperament and disposition, in the habits and mode of life, and sometimes in the tendency to particular diseases. This transmission of characters from parent to offspring is summarized in the well-known expression that "like begets like," and it rests upon a physical basis.

The size of the particles which are derived from the parents, called the male and female pronuclei, the potentiality of which is so utterly out of proportion to their bulk, is almost inconceivably small when compared with the magnitude of the adult body. Further, by the continual process of division of the cells, the substance of the segmentation nucleus is diffused throughout the body of the new individual produced through its influence, so that each cell contains but an infinitesimal particle of it. The parental dilution, if I may so say, is so attenuated as to surpass the imagination of even the most credulous believer in the attenuation of drugs by dilution. And yet these particles are sufficient to stamp the characters of the parents, of the grandparents, and of still more remote ancestors on the offspring, and to preserve them throughout life, notwithstanding the constant changes to which the cells forming the tissues and organs of the body are subjected in connection with their use and nutrition. So marvellous, indeed, is the whole process, that even the exact contributions to recent knowledge on the fusion of the two pronuclei, instead of diminishing our wonder, have intensified the force of the expression "*magnum hereditatis mysterium.*"

In considering the question of how new individuals are produced, one must keep in mind that it is not every cell in the body which can act as a centre of reproduction for a new generation, but that certain cells, which we name germ-cells and sperm-cells, are set aside for that purpose. These cells, destined for the production of the next generation, form but a small proportion of the body of the animal in which they are situated. They are, as a rule, marked off from the rest of the cells of its body at an early period of development. The exact stage at which they become specially differentiated for reproductive purposes varies, however, in different organisms. In some organisms, as is said by Balbiani to be the case in *Chironomus*, they apparently become isolated before the formation of the germinal layers is completed; but, as a rule, their appearance is later, and in the higher organisms not until the development of the body is relatively much more advanced.

The germ-cells after their isolation take no part in the growth of the organism in which they arise, and their chief association with the other cells of its body is that certain of the latter are of service in their nutrition. The problem, therefore, for consideration is the mode in which these germ or reproductive cells become influenced, so that after being isolated from the cells which make up the bulk of the body of the parent they can transmit to the offspring the characters of the parent organism. Various speculations and theories have been advanced by way of explanation. The well-known theory of Pangenesis, which Charles Darwin with characteristic moderation put forward as merely a provisional hypothesis, assumes that *gemmules* are thrown off from each different cell or unit throughout the body which retain the characters of the cells from which they spring; that the gemmules aggregate themselves either to form or to become included within the reproductive cells; and that in this manner they and the characters which they convey are capable of being transmitted in a dormant state to successive generations, and to reproduce in them the likeness of their parents, grandparents, and still older ancestors.

In 1872, and four years afterwards, in 1876, Mr. Francis Galton published most suggestive papers on Kinship and Heredity (Proc. Roy. Soc. Lond., 1872, and Journ. Anthrop. Inst., vol. v., 1876). In the latter of these papers he developed the idea that "the sum-total of the germs, gemmules, or whatever they may be called," which are to be found in the newly fertilized ovum, constitute a *stirp*, or root; that the germs which make up the stirp consist of two groups—the one which develops into the bodily structure of the individual, and which constitutes, therefore, the personal structure; the other, which remains latent in the individual, and forms, as it were, an undeveloped residuum; that it is from these latent or residual germs that the sexual elements intended for producing the next generation are derived, and that these germs exercise a predominance in matters of heredity; further, that the cells which make up the personal structure of the body of the individual exercise only in a very faint degree any influence on the reproductive cells, so that any modifications acquired by the individual are barely, if at all, inherited by the offspring.

Subsequent to the publication of Mr. Galton's essays, valuable contributions to the subject of Heredity have been made by Profs. Brooks, Jaeger, Naegeli, Nussbaum, Weismann, and others. Prof. Weismann's theory of Heredity embodies the same fundamental idea as that propounded by Mr. Galton; but as he has employed in its elucidation a phraseology which is more in harmony with that generally used by biologists, it has had more immediate attention given to it. As Weismann's essays have, during the present year, been translated for and published by the Clarendon Press (Oxford, 1889), under the editorial superintendence of Messrs. Poulton, Schönland, and Shipley, they are now readily accessible to all English readers.

Weismann asks the fundamental question, "How is it that a single cell of the body can contain within itself all the hereditary tendencies of the whole organism?" He at once discards the theory of pangenesis, and states that in his belief the germ-cell, so far as its essential and characteristic substance is concerned, is not derived at all from the body of the individual in which it is produced, but directly from the parent germ-cell from which the individual has also arisen. He calls his theory the *continuity of the germ-plasm*, and he bases it upon the supposition that in each individual a portion of the specific germ-plasm derived from the germ-cell of the parent is not used up in the construction of the body of that individual, but is reserved unchanged for the formation of the germ-cells of the succeeding generation. Thus, like Mr. Galton, he recognizes that in the stirp or germ there are two classes of cells destined for entirely distinct purposes: the one for the development of the *soma* or body of the individual, which class he calls the *somatic* cells; the other for the perpetuation of the species, *i.e.* for reproduction.

In further exposition of his theory Weismann goes on to say, as the process of fertilization is attended by a conjugation of the nuclei of the reproductive cells—the pronuclei referred to in an earlier part of this address—that the nuclear substance must be the sole bearer of hereditary tendencies. The two uniting nuclei would contain the germ-plasms of the parents, and this germ-plasm also would contain that of the grandparents as well as that of all previous generations.

To make these somewhat abstract propositions a little more clear, I have devised the following graphic mode of representation:—


Let the capital letters A, B, C, D, &c., express a series of successive generations. Suppose A to be the starting-point, and to represent the somatic or personal structure of an individual; then *a* may stand for the reproductive cells, or germ-plasm, from which the offspring of A, viz. B, is produced. B, like A, has both a personal structure and reproductive cells or germ-plasm, the latter of which is represented by the letters *ab*, which are intended to show that whilst belonging to B they have a line of continuity with A. C stands for an individual of the third generation, in which the reproductive plasm is indicated by *abc*, to express that, though within the body of C, the germ-plasm is continuous with that of both *b* and *a*. D also contains the reproductive cells, *abcd*, which are continuous with the germ-plasm of the three preceding generations, and so on.

It follows, therefore, from this theory that the germ-plasm possesses throughout the same complex chemical and molecular structure, and that it would pass through the same stages when the conditions of development are the same, so that the same final product would arise. Each successive generation would have therefore an identical starting-point, so that an identical product would arise from all of them.

Weismann does not absolutely assert that an organism cannot exercise a modifying influence upon the germ-cells within it; yet he limits this influence to such slight effect as that which would arise from the nutrition and growth of the individual, and the reaction of the germ-cell upon changes of nutrition caused by alteration in growth at the periphery leading to some change in the size, number, and arrangements of its molecular units. But he throws great doubt upon the existence of such a reaction, and he, more emphatically than Mr. Galton, argues against the idea that the cells which make up the somatic or personal structure of the individual exercise any influence on the reproductive cells. From his point of view the structural or other properties which characterize a family, a race, or a species are derived solely from the reproductive cells through continuity of their germ-plasm, and are not liable to modification by the action on them of the organs or tissues of the body of the individual organism in which they are situated. To return for one moment to my graphic illustration in elucidation of this part of the theory. The cells which make up the personal structure of A or B would exercise no effect upon the character of the reproductive cells *a* or *ab* contained within them. These latter would not be modified or changed in their properties by the action of the individual organism A or B. The individual B would be in hereditary descent, not from A + *a*, but only from *a*, with which its germ-plasma *ab* would be continuous, and through which the properties of the family, race, or species would be transmitted to C, and so on to other successive generations.

The central idea of Heredity is permanency; that like begets like, or, as Mr. Galton more fitly puts it, that "like *tends* to produce like." But though the offspring conform with their parents in all their main characteristics, yet, as everyone knows, the child is not absolutely like its parents, but possesses its own character, its own individuality. It is easy for anyone to recognize that differences exist amongst men when he compares one individual with another; but it is equally easy for those who make a special study of animals to recognize individual differences in them also. Thus a pigeon or canary fancier distinguishes without fail the various birds in his flock, and a shepherd knows every sheep under his charge. But the anatomist tells us that these differences are more than superficial—that they also pervade the internal structure of the body. In a paper which I read to the meeting of this Association in Birmingham so long ago as 1865,[1] after relating a series of instances of variation in structure observed in the dissections of a number of human bodies, I summarized my conclusion as follows: "Hence, in the development of each individual, a morphological specialization occurs both in internal structure and external form by which distinctive characters are conferred, so that each man's structural individuality is an expression of the sum of the individual variations of all the constituent parts of his frame."

As in that paper I was discussing the subject only in its morphological relations, I limited myself to that aspect of the question; but I might with equal propriety have also extended my conclusion to other aspects of man's nature.

Intimately associated, therefore, with the conception of Heredity—that is, the transmission of characters common to both parent and offspring—is that of Variability—that is, the appearance in an organism of certain characters which are unlike those possessed by its parents. Heredity, therefore, may be defined as the perpetuation of the like; Variability, as the production of the unlike.

And now we may ask, Is it possible to offer any feasible explanation of the mode in which variations in organic structure take their rise in the course of development of an individual organism? Anything that one may say on this head is of course a matter of speculation, but certain facts may be adduced as offering a basis for the construction of an hypothesis, and on this matter Prof. Weismann makes a number of ingenious suggestions.

Prior to the conjugation of the male and female pronuclei to form the segmentation nucleus a portion of the germ-plasm is extruded from the egg to form what are called the *polar bodies*. Various theories have been advanced to account for the significance of this curious phenomenon. Weismann explains it on the hypothesis that a reduction of the number of ancestral germ-plasms in the nucleus of the egg is a necessary preparation for fertilization and for the development of the young animal. He supposes that by the expulsion of the polar bodies one-half the number of ancestral germ-plasms is removed, and that the original bulk is restored by the addition of the male pronucleus to that which remains. As precisely corresponding molecules of this plasm need not be expelled from each ovum, similar ancestral plasms are not retained in each case; so that diversities would arise even in the same generation and between the offspring of the same parents.

Minute though the segmentation nucleus is, yet microscopic research has shown that it is not a homogeneous structureless body, but is built up of different parts. Most noteworthy are the presence of extremely delicate threads or fibrils, called the *chromatin filaments*, which are either coiled on each other, or intersect to form a network-like arrangement. In the meshes of this network a viscous—and, so far as we yet know, structureless—substance is situated. Before the process of division begins in the segmentation nucleus these filaments swell up and then proceed to arrange themselves at first into one and then into two star-like figures before the actual division of the nucleus takes place.[1] It is obvious, therefore, that the molecules which enter into the formation of the segmentation nucleus can move within its substance, and can undergo a readjustment in size and form and position. But this readjustment of material is, without doubt, not limited to those relatively coarse particles which can be seen and examined under the microscope, but applies to the entire molecular structure of the segmentation nucleus. Now it must be remembered that the cells of the embryo from which all the tissues and organs of the adult body are derived are themselves descendants of the segmentation nucleus, and they will doubtless inherit from it both the power of transmitting definite characters and a certain capacity for readjustment both of their constituent materials and the relative positions which they may assume towards each other. One might conceive, therefore, that if in a succession of organisms derived from common ancestors the molecular particles were to be of the same composition and to arrange themselves in the segmentation nucleus and in the cells derived from it on the same lines, these successive generations would be alike; but if the lines of adjustment and the molecular constitution were to vary in the different generations, then the products would not be quite the same. Variations in structure, and to some extent also in the construction of parts, would arise, and the unlike would be produced.

In this connection it is also to be kept in mind that in the higher organisms, and, indeed, in multicellular organisms generally, an individual is derived, not from one parent only, but from two parents. Weismann emphasizes this combination as the cause of the production of variations and the transmission of hereditary individual characters. If the proportion of the particles derived from each parent and the forces which they exercise were precisely the same in any individual case, then one could conceive that the product would be a mean of the components provided by the two parents. But if one parent were to contribute a larger proportion than the other to the formation of a particular organism, then the balance would be disturbed, the offspring in its character would incline more to one parent than to the other, according to the proportion contributed by each, and a greater scope for the production of variations would be provided. These differences would be increased in number in the course of generations, owing to new combinations of individual characters arising in each generation.

As long as the variations which are produced in an organism are collectively within a certain limitation, they are merely individual variations, and express the range within which such an organism, though exhibiting differences from its neighbours, may yet be classed along with them in the same species. It is in this sense that I have discussed the term Variability up to the present stage of this address. Thus all those varieties of mankind which, on account of differences in the colour of the skin, we speak of as the white, black, yellow races and red-skins are men, and they all belong to that species which the zoologists term *Homo sapiens*.

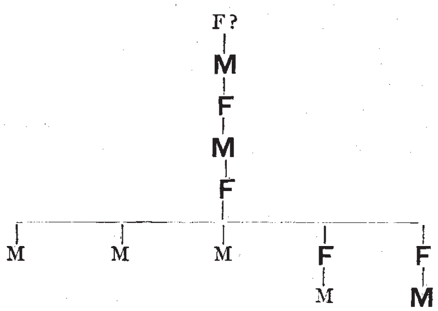
But the subject of Variability cannot, in the present state of science, be confined in its discussion to the production of individual variations within the limitations of a common species. Since Charles Darwin enunciated the proposition that favourable variations would tend to be preserved, and unfavourable ones to be destroyed, and that the result of this double action, by the accumulation of minute existing differences, would be the formation of new species by a process of natural selection, this subject has attained a much wider scope, has acquired increased importance, and has formed the basis of many ingenious speculations and hypotheses. As variations, when once they have arisen, may be hereditarily transmitted, the Darwinian theory might be defined as Heredity modified and influenced by Variability.

This is not the place to enter on a general discussion of the Darwinian theory, and even if it were, the time at our disposal

[1] Transactions of Sections, p. 111, 1865, and Trans. Roy Soc. Edinburgh, vol. xxiv., 1865.

[1] The observations more especially of Flemming, E. Van Beneden, Strasburger, and Carnoy may be referred to in connection with the changes which take place in nuclei prior to and in connection with their division.

would not admit of it. But there are some aspects of the theory which would need to be referred to in connection with the subject now before us. It may be admitted that many variations which may arise in the development of an individual, and which are of service to that individual, would tend to be preserved and perpetuated in its offspring by hereditary transmission. But it is also without question that variations which are of no service, and, indeed, are detrimental, to the individual in which they occur, are also capable of being hereditarily transmitted. This statement is amply borne out in the study of those important defects in bodily structure which pathologists group together under the name of Congenital Malformations. I do not require to go into much detail on this head, or to cite cases in which the congenital defect can only be exposed by dissection, but may refer, by way of illustration, to one or two examples in which the defect is visible on the surface of the body. The commonest form of malformation the hereditary transmission of which has been proved is where an increase in the number of digits on the hands or feet, or on both, occurs in certain families, numerous instances of which have now been put on record. But in other families there is an hereditary tendency to a diminution in the number of digits or to a defect in the development of those existing. I may give an illustration which occurred in the family of one of my pupils, the deformity in which consisted in a shortening or imperfect growth of the metacarpal bone of the ring finger of the left hand, so that the length of that finger was much below the normal. This family defect was traceable throughout six generations, and perhaps even in a seventh, and was, as a rule, transmitted alternately from the males to the females of the family (*Journ. Anat. and Phys.*, vol. xviii. p. 463)—

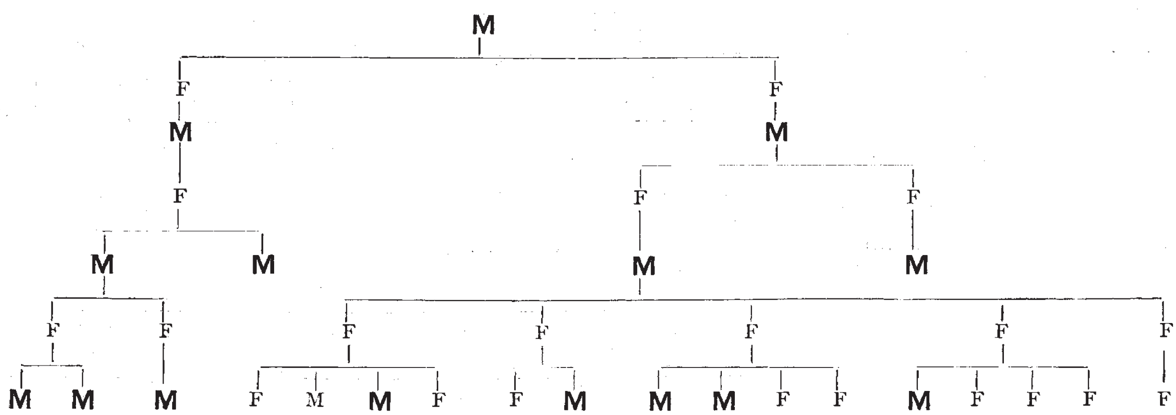



In this and the following diagrams M stands for male, F for female, whilst the block type (**M** or **F**) marks the individual or generation in which the variation occurred.

Another noticeable deformity which is known to be hereditary in some families, and which may be familiar to some of my auditors, is that of imperfect development of the upper lip and roof of the mouth, technically known as hare-lip and cleft palate.

These examples illustrate what may be called the coarser kinds of hereditary deformity, where the redundancies or defects in parts of the body are so gross as at once to attract attention. But modifications or variations in structure that can be transmitted from parent to offspring are by no means limited to changes which can be detected by the naked eye. They are sometimes so minute as to be determined rather by the modifications which they occasion in the function of the organ than by the ready recognition of structural variations. One of the most interesting of these is the affection known as Daltonism, or colour-blindness, which has distinctly been shown to be hereditary, and which is due, apparently in the majority of cases, to a defect in the development of the retina, or of the nerve of sight which ends in it, though in some instances they may be occasioned by defective development of the brain itself. Dr. Horner has related a most interesting family history (cited in "Die Allgemeine Pathologie," by Dr. Edwin Klebs, Jena, 1887), in which the colour-blindness was traced through seven generations. In this family the males were the persons affected, though the peculiarity was transmitted through the females, who themselves remained unaffected. The family tree showed that in the sixth generation seven mothers had children. Their sons, collectively nine in number, were all colour-blind with the exception of one son, while none of their nine daughters showed the hereditary defect. (See diagram below.)

The eye is not the only organ of sense which exhibits a tendency to the production of hereditary congenital defects. The ear is similarly affected, and intimately associated with congenital deafness is an inability to speak articulately, which occasions the condition termed Deaf-mutism. Statisticians have given some attention to this subject, both as regards its relative frequency and its hereditary character. The writer of the article "Vital Statistics," in the Report of the Irish Census Commissioners during the decades ending 1851, 1861, 1871, has discussed at some length the subject of congenital deaf-mutism, and has produced a mass of evidence which proves that it is often hereditarily transmitted. In the Census Report for 1871 (vol. lxxii. Part II., "Report on the Status of Disease," p. 1, 1873), 3297 persons were returned as belonging to this class, and in 393 cases the previous or collateral branches of the family were also mute. In 211 of these the condition was transmitted through the father; in 182 through the mother. In 2579 cases there was one deaf-mute in a family; in 379 instances, two; in 191 families, three; in 53, four; in 21, five; in 5, six; and in each of two families no fewer than seven deaf-mutes were born of the same parents. In one of these two families neither hereditary predisposition nor any other probable physiological or pathological reason was assigned to account for the peculiarity, but in the other family the parents were first cousins. Mr. David Buxton,




who has paid great attention to this subject (*Liverpool Medico-Chirurg. Journ.*, July 1857; January 1859), states that the probability of congenital deafness in the offspring is nearly seven times greater when both parents are deaf than when only one is so; in the latter case the chance of a child being born deaf is less than three-quarters per cent.; in the former, the chances are that 5 per cent. of the children will be deaf-mutes. Mr. Buxton refers to several families where the deaf-mutism has been transmitted through three successive generations, though in some instances the affection passes over one generation to reappear in the next. He also relates a case of a family of sixteen persons, eight of whom were born deaf and dumb, and one at least of the members of which transmitted the affection to his descendants as far as the third generation. There can be little doubt that congenital deaf-mutism, in the great majority of instances, is associated with a defective development, and therefore a structural variation of the organ of hearing, though in some cases, perhaps, the defect may be in the development of the brain itself.

Although a sufficient number of cases has now been put on record to prove that in some families one or other kind of congenital deformity may be hereditarily transmitted, yet I do not wish it to be supposed that congenital malformations may not arise in individuals in whom no hereditary tendency can be traced. It is undoubtedly true that family histories are in many cases very defective, and frequently cannot be followed back for more than one, or, at the most, two generations; so that it is not unlikely that an hereditary predisposition may exist in many instances where it cannot be proved. Still, allowing even for a considerable proportion of such cases, a sufficient number will remain to warrant the statement that malformations or variations in structure which have not been displayed by their ancestors may arise in individuals belonging to a particular generation.

The variations which I have spoken of as congenital malformations arise, as a rule, before the time of birth, during the early development of the individual; but there is an important class of cases, in which the evidence for hereditary transmission is more or less strong, which may not exhibit their peculiarities until months, or even years, after the birth of the individual. This class is spoken of as hereditary diseases, and the structural and functional changes which they produce exercise most momentous influences. Sometimes these diseases may occasion changes in the tissues and organs of the body of considerable magnitude, but at other times the alteration is much more subtle, is molecular in its character, requires the microscope for its determination, or is even incapable of being recognized by that instrument.

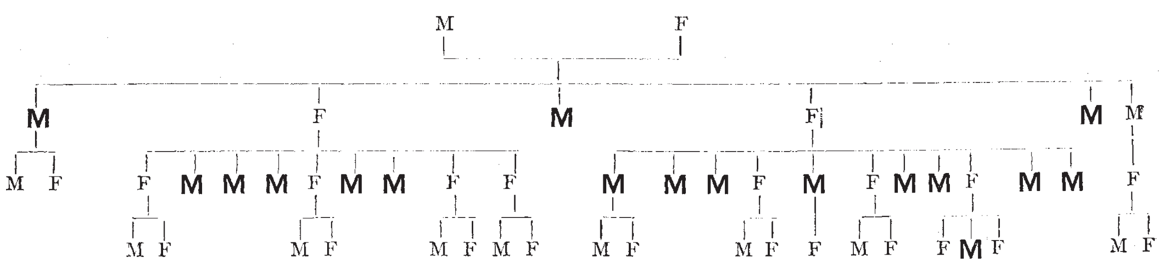
Had one been discussing the subject of hereditary disease twenty years ago, the first example probably that would have been adduced would have been tuberculosis, but the additions to our knowledge of late years throw some doubt upon its hereditary character. There can, of course, be no question that tubercular disease propagates itself in numerous families from generation to generation, and that such families show a special susceptibility or tendency to this disease in one or other of its forms. But whilst fully admitting the predisposition to it which exists in certain families, there is reason to think that the structural disease itself is not hereditarily transmitted, but that it is directly excited in each individual in whom it appears by a process of external infection due to the action of the tubercle bacillus. Still, if the disease itself be not inherited, a particular temperament which renders the constitution liable to be attacked by it is capable of hereditary transmission.

Sir James Paget,[1] when writing on the subject of cancer, gives statistics to show that about a quarter of the persons affected were aware of the existence of the same disease in other members of their family, and he cites particular instances in which cancer was present in two and even four generations. He had no doubt that the disease can be inherited—not, he says, that, strictly speaking, cancer or cancerous material is transmitted, but a tendency to the production of those conditions which will finally manifest themselves in a cancerous growth. The germ from the cancerous parent must be so far different from the normal as after the lapse of years to engender the cancerous condition.

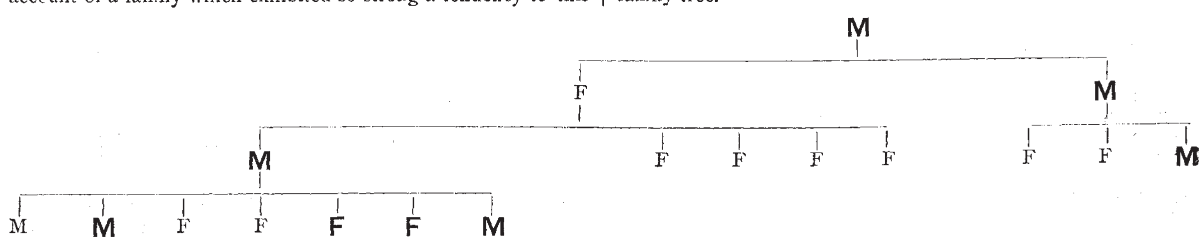
Heredity is also one of the most powerful factors in the production of those affections which we call gout and rheumatism. Sir Dyce Duckworth, the latest systematic writer on gout, states that in those families whose histories are the most complete and trustworthy the influence is strongly shown, and occurs in from 50 to 75 per cent. of the cases; further, that the children of gouty parents show signs of articular gout at an age when they have not assumed those habits of life and peculiarities of diet which are regarded as the exciting causes of the disease.

Some interesting and instructive family histories, in which the hereditary transmission of a particular disease through several generations has been worked out, are recorded by Prof. Klebs in his "Allgemeine Pathologie." I may draw from these one or two additional illustrations. Some families exhibit a remarkable tendency to bleed when the surface of the body is injured or bruised, and the bleeding is stopped with difficulty. The hæmorrhagic tendency is not due to the state of the blood, but to a softening or degeneration of the walls of the blood-vessels, so that they are easily torn. In one family, the tree of which is here subjoined, this peculiarity showed itself in one generation in three out of four males; in the next generation, in thirteen out of fourteen males; whilst in the immediately succeeding generation only one out of nine males was affected; so that it would seem as if the tendency was fading away in it. It is remarkable that throughout the series, though the transmission of the affection went through the female members, they themselves remained free from it.

*The bleeding family Mampel, recorded by Dr. Lossen.*




Another illustration may be taken from the well-known disease of the eyeball called cataract. Dr. Appenzeller has given an account of a family which exhibited so strong a tendency to this affection that the males were affected in four generations, though the females did not entirely escape, as is shown in the subjoined family tree.




In neither of these families can it be said that the structural lesion itself is transmitted, but that the tendency or predisposition to produce it is inherited. The germ-plasm, therefore, in these individuals must have been so modified from the normal as to carry with it certain peculiarities, and to induce the particular form of disease which showed itself in each family.

[1] "Lectures on Surgical Pathology," third edition, revised and edited by the author and W. Turner (London, 1870).

In connection with the tendency to the transmissibility of either congenital malformations or diseases, consanguinity in the parents, although by no means a constant occurrence, is a factor which in many cases must be taken into consideration.[1] If we could conceive both parents to be physiologically perfect, then it may be presumed that the offspring would be so also; but if there be a departure in one parent from the plane of physiological perfection, then it may safely be assumed that either the immediate offspring or a succeeding generation will display a corresponding departure in a greater or less degree. Should both parents be physiologically imperfect, we may expect the imperfections if they are of a like nature to be intensified in the children. It is in this respect, therefore, that the risk of consanguineous marriages arises, for no family can lay claim to physiological perfection.

When we speak of tendencies, susceptibilities, proclivities, or predisposition to the transmission of characters, whether they be normal or pathological, we employ terms which undoubtedly have a certain vagueness. We are as yet quite unable to recognize, by observation alone, in the germ-plasm any structural change which would enable us to say that a particular tendency or susceptibility will be manifested in an organism derived from it. We can only determine this by following out the life-history of the individual. Still it is not the less true that these terms express a something of the importance of which we are all conscious. So far as Man is concerned, the evidence in favour of a tendency to the transmission of both structural and functional modifications which are either of dis-service, or positively injurious, or both, is quite as capable of proof as that for the transmission of characters which are likely to be of service. Hence useless as well as useful characters may be selected and transmitted hereditarily.

I have dwelt somewhat at length on the transmissibility of useless characters, for it is an aspect of the subject which more especially presents itself to the notice of the pathologist and physician; and little, if at all, to that of those naturalists whose studies are almost exclusively directed to the examination of organisms in their normal condition But when we look at Man, his diseases form so large a factor in his life that they and the effects which they produce cannot be ignored in the study of his nature.

Much has been said and written during the last few years of the transmission from parents to offspring of characters which have been "acquired" by the parent, so that I cannot altogether omit some reference to this subject. It will conduce to one's clearness of perception of this much-discussed question if one defines at the outset in what sense the term "acquired characters" is employed; and it is the more advisable that this should be done, as the expression has not always been used with the same signification. This term may be used in a wide or in a more restricted sense. In its wider meaning it may cover all the characters which make their first appearance in an individual, and which are not found in its parents, in whatever way they have arisen—

(1) Whether their origin be due to such molecular changes in the germ-plasm as may be called spontaneous, leading to such an alteration in its character as may produce a new variation; or,

(2) Whether their origin be accidental, or due to habits, or to the nature of the surroundings, such as climate, food, &c.

Prof. Weismann has pointed out with great force the necessity of distinguishing between these two kinds of "acquired characters," and he has suggested two terms the employment of which may keep before us how important it is that these different modes of origin should be recognized. Characters which are produced in the germ-plasm itself by natural selection, and all other characters which result from this latter cause, he names *blastogenic*. He further maintains that all blastogenic characters can be transmitted; and in this conclusion, doubtless, most persons will agree with him. On the other hand, he uses the term *somatogenic* to express those characters which first appear in the body itself, and which follow from the reaction of the *soma* under direct external influences. He includes under this head the effects of mutilation, the changes which follow from increased or diminished performance of function, those directly due to nutrition, and any of the other direct external influences which act upon the body. He further maintains that the somatogenic characters are not capable of transmission from parent to offspring, and he suggests that in future discussions on this subject the term "acquired characters" should be restricted to those which are somatogenic.

Thus one might say that blastogenic characters arising in the germ would be acquired in the individual by the action of the germ upon the soma; so that if we return again to the graphic illustration previously employed, the germ-plasm represented by the small italic letters *abcd* would act upon the soma represented by the capital letters A, B, C, D. Somatogenic characters, again, arising in the soma, would be acquired by the action of the soma A, B, C, D, upon the contained germ-plasm *abcd*. But whether those acquired characters expressed by the term somatogenic can or can not be transmitted has been fruitful of discussion.

That the transmission of characters so acquired can take place is the foundation of the theory of Lamarck, who imagined that the gradual transformation of species was due to a change in the structure of a part of an organism under the influence of new conditions of life, and that such modifications could be transmitted to the offspring. It was also regarded as of importance by Charles Darwin, who stated[1] that all the changes of corporeal structure and mental power cannot be exclusively attributed to the natural selection of such variations as are often called spontaneous, but that great value must be given to the inherited effects of use and disuse, some also to the modification in the direct and prolonged action of changed conditions of life, also to occasional reversions of structure. Herbert Spencer believes[2] that the natural selection of favourable varieties is not in itself sufficient to account for the whole of organic evolution. He attaches a greater importance than Darwin did to the share of use and disuse in the transmission of variations. He believes that the inheritance of functionally produced modifications of structure takes place universally, and that as the modification of structure by function is a *vera causa* as regards the individual, it is unreasonable to suppose that it leaves no traces in posterity.

On the other hand, there are very eminent authorities who contend that the somatogenic acquired characters are not transmissible from parent to offspring. Mr. Francis Galton, for example, gives a very qualified assent to this proposition. Prof. His, of Leipzig, doubts its validity. Prof. Weismann says that there is no proof of it. Mr. Alfred Russel Wallace, in his most recent work,[3] considers that the direct action of the environment, even if we admit that its effects on the individual are transmitted by inheritance, are so small in comparison with the amount of spontaneous variation of every part of the organism that they must be quite over-shadowed by the latter. Whatever other causes, he says, have been at work, natural selection is supreme to an extent which even Darwin himself hesitated to claim for it.

There is thus a conflict of opinion amongst the authorities who have given probably the most thought to the consideration of this question. It may appear, therefore, to be both rash and presumptuous on my part to offer an opinion on this subject. I should, indeed, have been slow to do so had I not thought that there were some aspects of the question which seemed not to have been sufficiently considered in its discussion.

In the first place, I would, however, express my agreement with much that has been said by Prof. Weismann on the want of sufficient evidence to justify the statement that a mutilation which has affected a parent can be transmitted to the offspring. It is, I suppose, within the range of knowledge of most of us that children born of parents who have lost an eye, an arm, or a leg, come into the world with the full complement of eyes and limbs. The mutilation of the parent has not affected the offspring; and one would, indeed, scarcely expect to find that such gross visible losses of parts as take place when a limb is removed by an accident or a surgical operation should be repeated in the offspring. But a similar remark is also applicable to such minor mutilations as scars, the transmission of which to the offspring, though it has been stoutly contended for by some, yet seems not to be supported by sufficiently definite instances.

I should search for illustrations of the transmission of somatogenic characters in the more subtle processes which affect living organisms, rather than those which are produced by violence

[1] I may especially refer for a discussion of this subject to an admirable essay, by Sir Arthur Mitchell, K.C.B., "On Blood Relationship in Marriage considered in its Influence upon the Offspring."

[1] Preface to second edition of "Descent of Man," 1885; also "Origin of Species," first edition.
[2] "Factors of Organic Evolution," *Nineteenth Century*, 1886.
[3] "Darwinism," p. 443 (London, 1889).

and accident. I shall take as my example certain facts which are well known to those engaged in the breeding of farm-stock or of other animals that are of utility to or are specially cultivated by man.

I do not refer to the influence on the offspring of impressions made on the senses and nervous system of the mother, the first statement of the effects of which we find in the book of Genesis, where Jacob set peeled rods before the flocks in order to influence the colour and markings of their young; though I may state that I have heard agriculturists relate instances from their own experience which they regarded as bearing out the view that impressions acting through the mother do influence her offspring. But I refer to what is an axiom with those who breed any particular kind of stock, that to keep the strain pure, there must be no admixture with stock of another blood. For example, if a shorthorned cow has a calf by a Highland sire, that calf, of course, exhibits characters which are those of both its parents. But future calves which the same cow may have when their sires have been of the shorthorned blood, may, in addition to shorthorn characters, have others which are not shorthorned but Highland. The most noteworthy instance of this transmission of characters acquired from one sire through the same mother to her offspring by other sires is that given in the often-quoted experiment by a former Lord Morton.[1] An Arabian mare in his possession produced a hybrid the sire of which was a quagga, and the young one was marked by zebra-like stripes. But the same Arabian had subsequently two foals, the sire of which was an Arab horse, and these also showed some zebra-like markings. How, then, did these markings characteristic of a very different animal arise in these foals, both parents of which were Arabians? I can imagine it being said that this was a case of reversion to a very remote striped ancestor, common alike to the horse and the quagga. But, to my mind, no such far-fetched and hypothetical explanation is necessary. The cause of the appearance of the stripes seems to me to be much nearer and more obvious. I believe that the mother had acquired, during her prolonged gestation with the hybrid, the power of transmitting quagga-like characters from it, owing to the interchange of material which had taken place between them in connection with the nutrition of the young one. For it must be kept in mind that in placental mammals an interchange of material takes place in opposite directions, from the young to the mother as well as from the mother to the young.[2] In this way the germ-plasm of the mother, belonging to ova which had not yet matured, had become modified whilst still lodged in the ovary. This acquired modification had influenced her future offspring, derived from that germ-plasm, so that they in their turn, though in a more diluted form, exhibited zebra-like markings. If this explanation be correct, then we have an illustration of the germ-plasm having been directly influenced by the soma, and of somatogenic acquired characters having been transmitted.

But there are other facts to show that the isolation of the germ-cells or germ-plasm from the soma cells is not so universal as might at the first glance be supposed. Weismann himself admits that in the Hydroids the germ-plasm is present in a very finely divided, and therefore invisible, state, in certain somatic cells in the beginning of embryonic development, and that it is then transmitted through innumerable cell generations to those remote individuals of the colony in which sexual products are formed. The eminent botanist Prof. Sachs states that in the true mosses almost any of the cells of the roots, leaves, and shoot axes may form new shoots and give rise to independent living plants. Plants which produce flowers and fruit may also be raised from the leaves of the *Begonia*. I may also refer to what is more or less familiar to everybody, that the tuber of the potato can give rise to a plant which bears flowers and fruit. Now in all these cases the germ-plasm is not collected in a definite receptacle isolated from the soma, but is diffused through the cells of the leaves of the *Begonia* or amidst those of the tuber of the potato, and the propagation of the potato may take place through the tuber for several generations without the necessity of having to recur to the fruit for seed. It seems difficult, therefore, to understand *why, in such cases, the nutritive processes which affect and* modify the soma cells should not also react upon the germ-plasm, which, as Weismann admits, is so intimately associated with them.

[1] Philosophical Transactions, 1881; also Darwin's "Animals and Plants under Domestication," first edition, vol. i. p. 403, 1868.

[2] See, for example, Essays by Profs. Harvey and Gusserow and Mr. Savory; also my "Lectures on the Comparative Anatomy of the Placenta" (Edinburgh, 1876).

Those who uphold the view that characters acquired by the soma cannot be transmitted from parents to offspring undoubtedly draw so large a cheque on the bank of hypothesis that one finds it difficult, if not impossible, to honour it. Let us consider for one moment all that is involved in the acceptance of this theory, and apply it in the first instance to Man. On the supposition that all mankind have been derived from common ancestors through the continuity of the germ-plasm, and that this plasm has undergone no modification from the *persona* or *soma* of the succession of individuals through whom it has been transmitted, it would follow that the primordial human germ-plasm must have contained within itself an extraordinary potentiality of development—a potentiality so varied that all those multiform variations in physical structure, tendency to disease, temperament, and other characters and dispositions which have been exhibited by all the races and varieties of men who either now inhabit or at any period in the world's history have inhabited the earth, must have been included in it. But if we are to accept the theory of Natural Selection, as giving a valid explanation of the origin of new species, then the non-transmissibility of somatogenic acquired characters has a much more far-reaching significance. For if all the organisms, whether vegetable, animal, or human, which have lived upon the earth have arisen by a more or less continuous process of evolution from one or even several simple cellular organisms, it will follow, as a logical necessity of the theory, that these simple organisms must have contained in their molecular constitution a potentiality of evolution into higher and more complex forms of life, through the production of variations, without the intermediation of any external force or influence acting directly upon the soma. Further, this must have endured throughout a succession of countless individual forms and species, extending over we know not how many thousands of years, and through the various geological and climatic changes which have affected the globe.

The power of producing these variations would therefore, on this theory, have been from the beginning innate to the germ-plasm, and uninfluenced in any way by its surroundings. Variations would have arisen spontaneously in it, and, for anything that we know, as it were by accident, and without a definite purport or object. But whether such variations would be of service or dis-service could not be ascertained until after their appearance in the soma had subjected them to the test of the conditions of life and the environment.

Let us now glance at the other side of the question. All biologists will, I suppose, accept the proposition that the individual soma is influenced or modified by its environment or surroundings. Now, if on the basis of this proposition the theory be grafted that modifications or variations thus produced are capable of so affecting the germ-plasm of the individual in whom the variation arises as to be transmitted to its offspring—and I have already given cases in point—then such variations might be perpetuated. If the modification is of service, then presumably it will add to the vitability of the individual, and through the interaction between the soma and the germ-plasm, in connection with their respective nutritive changes, will so affect the latter as to lead to its being transmitted to the offspring. From this point of view the environment would, as it were, determine and regulate the nature of those variations which are to become hereditary, and the possibility of variations arising which are likely to prove useful becomes greater than on the theory that the soma exercises no influence on the germ-plasm. Hence I am unable to accept the proposition that somatogenic characters are not transmitted, and I cannot but think that they form an important factor in the production of hereditary characters.

To reject the influence which the use and disuse of parts may exercise both on the individual and on his offspring is like looking at an object with only a single eye. The morphological aspect of organic structure is undoubtedly of fundamental importance. But it should not be forgotten that tissues and organs, in addition to their subjection to the principles of development and descent, have to discharge certain *specific purposes and* functions, and that structural modifications arise in them in correlation with the uses to which they are put, so as to adapt them to perform modified duties. It may be difficult to assign the exact value which physiological adaptation can exercise in the perpetuation of variations. If the habit or external condition which has produced a variation continues to be practised, then, in all probability, the variation would be intensified in successive generations. But should the habit cease or the external condition be changed, then, although the variation might continue to be for a time perpetuated by descent, it would probably become less strongly marked and perhaps ultimately disappear. One could also conceive that the introduction of a new habit or external condition the effect of which would be to produce a variation in a direction different from that which had originally been acquired, would tend to neutralize the influence of descent in the transmission of the older character.

By accepting the theory that somatogenic characters are transmitted we obtain a more ready explanation how men belonging to a race living in one climate or part of the globe can adapt themselves to a climate of a different kind. On the theory of the non-transmissibility of these acquired characters, long periods of years would have to elapse before the process of adaptation could be effected. The weaker examples, on this theory, would have had to have died out, and the racial variety would require to have been produced by the selection of variations arising slowly, and requiring one knows not how many hundreds or thousands of years to produce a race which could adapt itself to its new environment. We know, however, that this process of the dying out of the weakest and the selection of the strongest is not necessary to produce a race which possesses well-recognizable physical characters. For most of us can, I think, distinguish the nationality of a citizen of the United States by his personal appearance, without being under the necessity of waiting to hear his speech and intonation.

It may perhaps be thought that, in selecting the subject of Heredity for my address, and in treating it, as I have to a large extent done, in its general biological aspects, I have infringed upon the province of Section D. But I am not prepared to admit that any such encroachment has been made. Man is a living organism, with a physical structure which discharges a variety of functions, and both structure and functions correspond in many respects, though with characteristic differences, with those which are found in animals. The study of his physical frame cannot therefore be separated from that of other living organisms, and the processes which take place in the one must also be investigated in the other. Hence we require, in the special consideration of the physical framework of Man, to give due weight to those general features of structure and functions which he shares in common with other living organisms. But whatever may have been the origin of his frame, whether by evolution from some animal form or otherwise, we can scarcely expect it ever to attain any greater perfection than it at present possesses.

The physical aspect of the question, although of vast importance and interest, yet by no means covers the whole ground of Man's nature, for in him we recognize the presence of an element beyond and above his animal framework.

Man is also endowed with a spiritual nature. He possesses a conscious responsibility which enables him to control his animal nature, to exercise a discriminating power over his actions, and which places him on a far higher and altogether different platform than that occupied by the beasts which perish. The kind of evolution which we are to hope and strive for in him is the perfecting of this spiritual nature, so that the standard of the whole human race may be elevated and brought into more harmonious relation with that which is holy and divine.

## REPORTS.

*Report (Second) of the Committee appointed for the purpose of Collecting Information as to the Disappearance of Native Plants from their Local Habitats. Prof. Hillhouse, Secretary.*

As intimated at the close of the Report for 1887,[1] the Committee has given its attention in the first instance to Scotland, and appends hereto such portion of the materials placed at its disposal as, for any reason, it considers desirable to publish. It has excluded a considerable number of plants of little interest, and especially such as the records show to be recent introductions, casuals, escapes, &c., the loss of which is only a return, therefore, to an earlier, but still recent, state. There is little doubt that the list, even thus restricted, will be considerably amplified hereafter.

The plants recorded are numbered in accordance with the "London Catalogue," eighth edition, in which the distribution census of each plant will be found. Nearly all of the records are on the authority of some competent botanist resident in the locality, and whose initials, or some distinguishing initials, are appended. As has been pointed out by more than one correspondent, scarce plants occasionally well-nigh disappear in particular seasons, and hence the records of other than frequent visitors are not fully reliable.

The attention of botanists is particularly drawn to the records under the numbers 52, 264, 374, 406, 570, 575, 687, 910, 932, 993, 1018, 1020, 1478, 1695, and 1772, as giving examples of divers ways, often very curious and interesting, in which plants can become extinct.

The attention of the Committee's correspondents has been, in the main, confined to complete or threatened extinction; but in addition to this there is a general consensus of opinion that the rarer and more conspicuous Alpine plants are less abundant than they used to be. Amongst the localities specially mentioned are Clova and Ben Lawers; such plants (in addition to those given in the list) as *Saxifraga cernua*, *Alsine rubella*, *Gentiana nivalis*, &c., are notably less frequent than twenty years ago. Strange rumours have been communicated to the Committee as to the disappearance of plants from accessible habitats within the range of some of the deer "forests," but it is unable to verify these statements. Most of the correspondents agree, however, that the injudicious action of botanists themselves, and of botanical exchange clubs, has been a potent factor in the changes which have taken place. It is too often forgotten that the very rarity of a plant is the sign, and in great degree also the measure, of the acuteness of its struggle for existence, and that when a plant is in a state of unstable equilibrium with its environment, a small disturbance may have disproportionately great effects.

It will be observed that the "dealer" and "collector" figure largely, especially in connection with the disappearance of ferns. Thus one of the correspondents indicates (and offers to name) a dealer who has extirpated, or well-nigh extirpated, a considerable number of species in the district of Dumfries, and whose conduct he had brought under the notice of the local Natural History Society, of which the correspondent is Secretary. "He had also removed and sold almost all of the plants of *Nymphæa alba* from the lochs of this district before discovery; but now, I am happy to say, he is forbidden access to any estate in this district under penalty of prosecution for trespass." The attention of Natural History Societies may well be drawn to this case, as it happily illustrates at the same time one phase of the disease and a cure.

"Summer visitors" do not appear to be directly responsible for much damage, as their wanderings are probably over too restricted an area to produce much effect. There is no doubt, however, that they provide the larger portion of the customers of the "collector," and so are indirectly answerable for his ravages. The temptation to bring home some rare and beautiful fern, like *Aspidium (Polystichum) Lonchitis*, as a relic of a northern trip, is too great to be resisted, though something may possibly be done by persuading tourists that equally good plants, taken up with all proper care, and at a season when transplanting is not dangerous, can be obtained from any great fern nursery, for a price which is practically lower, often much lower, than that charged upon some Highland railway platform or roadside.

The Committee feels, however, that neither local dealers nor their customers are as a rule amenable to any ordinary appeal or to sentimental considerations, and would suggest therefore that the local Natural History Societies or Field Clubs should keep careful guard over any rare plants to be found within their respective spheres of action, and by appeal to the owner, or in other preferable way, should endeavour to effect their preservation. At the same time, many correspondents draw attention to the insertion by gardening periodicals of the advertisements of collecting dealers, and express the hope that the amount of revenue derived from these advertisements is not so great as to negative the possibility that the gardening journals may be induced, by discontinuing their insertion, to strike a heavy blow at a process which is depriving many districts of our land of one of their chief natural beauties.

39 *Trollius europæus*, L. Extinct in Mid-Aberdeen, &c. (W. W. and J. M.).

52. *Nymphæa alba*, L. Almost extirpated from lochs in the district round Dumfries by a dealer (J. W.). Has disappeared from the district of Birnie, near Elgin, by drainage (G. and T. A.).

[1] The Committee was unable to report in 1888, having lapsed by accident.