THE FUTURE OF PHYSIOLOGICAL RESEARCH.

ADDRESS

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BY

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THE FUTURE OF PHYSIOLOGICAL RESEARCH.

Bearing in mind the fact that one of the objects of the British Association is to interest the public in the advancement of scientific truth, it has been the practice of the presidents of the various sections to make some remarks of a general character, or to give a *résumé* of the recent progress of science in their particular department. I shall follow so far the example of my predecessors. I shall not attempt to enumerate, far less to describe, the contributions made to anatomical and physiological science during the past year, because that would entail a long and wearisome report regarding investigations with which most of us are already acquainted by the perusal of those excellent summaries that appear from time to time in our scientific and medical periodicals. With the view of limiting the scope of this address, I propose to offer a few observations bearing generally upon some of the scientific and social relations of anatomy and physiology, with the view of interesting the public in what we have been doing, and what we hope yet to do.

These sciences present different views of the same great system of truth. Each can be conceived as existing independently, while at the same time the one science is the complement of the other. Anatomy is the science of organic form, while physiology is that of organic function. The anatomist investigates structure, its form, general arrangements, and laws, and he may include in his survey the purposes or functions which the structure fulfils. Recently an opinion has been prevalent, and has cropped up in various quarters, that anatomy is but a preparatory science for physiology. This opinion has probably arisen in consequence of the rapid growth of physiological science during the last twenty or thirty years. But there can be no doubt that anatomy has a rôle of her own by no means inferior to that of physiology. She has to educe the formal laws which determine the structure of organised bodies and their parts, and thus she establishes the basis for scientific classification and arrangement. Anatomy is the beginning, of course, of all medical education, and the ground-work on which the practical arts of medicine and surgery are reared; but in a broader sense, the science has to do with the structure of every animal, from the simplest to the most complex, and from the facts obtained in the investigation of the structure of any animal, we are able to recognise the relationships it has with other animals, or in other words, its position in the Zoological scale.

Methods of Anatomy.

The methods of anatomical science are dissection, description, and comparison. These methods have been followed by anatomists from the birth of the science, but in recent times they have been largely supplemented by the use of the microscope, and by the employment of various modes of preparing tissues for microscopical inquiry. Now-a-days the anatomist not only describes naked-eye appearances displayed by the art of dissection, but he scrutinises every tissue and organ with the aid of the microscope. Hence it is, the historian of the progress of anatomical knowledge in this century will have to relate, as one of its chief features, the development of microscopical anatomy or histology. In no department of scientific work is greater activity manifested at present than in this. Scarcely a month passes without adding materially to our stores of knowledge, so as to make it almost impossible for a man to keep abreast of modern histology, and at the same time devote due attention to other departments of anatomy or physiology. In Germany and France, men devote their energies to histology as the business of their lives, and occupy chairs in many universities distinct from those of anatomy or physiology. In this country, from social and other considerations, such a division of labour is not generally made, but the time will assuredly come when it must be done.

Histology.

It may be supposed from these remarks that I regard histology as lying entirely within the province of anatomy. By no means. Histology is neutral territory between both. It is that department of knowledge where the two sciences overlap. The physiologist must investigate minute structure, in which the beginnings of physiological processes take place, because without knowledge of it all his ideas as to functions of organs or tissues would be superficial and unsatisfactory. When a physiologist examines a tissue, or a section of an organ, however, the morphological aspect is not what is prominently before his mind, but its mode of function. To him the form, size, position, and relations of the cell are not the special subjects of interest, but its probable mode of action in the economy. He therefore wishes it could be seen working, or at all events in conditions as nearly normal as possible. This desire has already led to the invention of various new methods of research, such as those of the hot stage, or plans for the observation of changes in cells or fibres in parts accessible to the microscope, methods which have already been fruitful of good results. I have a firm belief that this line of work has by no means been followed to the end, and that along it the physiologist will still be conducted to rich harvests in the fields of histological research.

Methods of Physiology.

The kindred science of physiology has for its object the elucidation of function, and it has, in addition to the methods of anatomy, namely, dissection, description, and comparison, those of pathological observation and experimentation. It is confessedly the science most difficult of all to prosecute. The subjects of investigation are intricate in structure, and are formed of complex chemical materials, which are in constant interaction with the surrounding world. Each animal is a machine, the intricacies of which are infinitely more involved than those of any human manufacture. To stop this machine, in the attempt to discover the action of one of its parts, is a proceeding, in many instances, which interferes with the very part the action of which we wish to find out. As we descend in the scale of animal life, and the machine becomes less complex, this difficulty is not so obtrusive, inasmuch as in many animals of simple organization there is not the same dependence of organ upon organ, and of tissue upon tissue, as we find in the more complex. But in most experimental researches in other sciences, the conditions are also manifold, and the acumen of the philosopher in all is tested in distinguishing the essential from the non-essential conditions.

In the further prosecution of physiology as a physical science, which it really is, experimental inquiry, with the aid of precise instruments, and the facts derived from the observation of the course and effects of disease, seem to me to be the two lines of evidence which will in future weigh with us in coming to just conclusions. No doubt it is quite true that much of the minute anatomy of the human body, and more so of the minute anatomy of the bodies of the lower animals, is still unknown, and that there are probably many details, visible only to the microscope, not yet discovered, which may influence our opinions as to the exact functions of parts. This is especially true of the structure of the nerve centres. We have at present only very general conceptions of the arrangements of the cells and fibres in these parts, and it is highly probable that future discoveries in this difficult field of investigation may change our views, not only of nervous action in general, but of the functions of particular centres. Accordingly there can be little doubt that as naked eye dissection has revealed structural arrangements which have hitherto guided the physiologist to correct notions of function, so in the future a similar service will be done to physiology by the histologist. Still physiology will have to depend less on aid of this nature, and more on the facts obtained by the methods of pathological observation and experiment. These methods are essentially of the same order. They vary the circumstances of the phenomenon we wish to investigate, and by the application of well known logical rules, we succeed in eliminating the cause of a phenomenon from its indifferent accompaniments. Diseased conditions, as has been well said, are experiments ready at hand, and every physician and surgeon of scientific spirit is from day to day engaged in investigating these conditions, not only with the view of curing his patient, but with the hope of throwing light on complex physiological processes. But direct experiment has the advantage over the observation of pathological effects, that it enables us to vary the conditions of the phenomenon as we desire. Thus the functions of the nerves were ascertained by the experiment of dividing each in turn, and watching the effect. When a function is arrested immediately on the division of a nerve, it is held that that function requires the nerve in order to its performance.

The Vivisection Question.

I make these remarks regarding the value of the experimental method in physiology, because we cannot forget the attempt which has recently been made to restrict us in the use of this important aid in prosecuting our science. I shall not enter again upon the controversy which has raged in this country regarding experiments upon animals, because by the passing of the Bill a practical solution of the question has been arrived at in the meantime, and it now becomes us, as good citizens, to do all in our power to carry out the provisions of the act, and give it a fair trial. I may be permitted to say, however, that I always recognised the right of the public to agitate on this question if they considered that cruelty was being perpetrated. I hope the day will never come when tales of suffering inflicted either on man or beast will be heard by us with calm indifference. The complaint I have against a section of the public is, that they believed apparently all they were told, and condemned us without waiting for explanation or defence. At the same time, it was not wise to meet this agitation with contempt and scorn for the ignorance of those who carried it on ; and it seems to me that the appointment of a Royal Commission to investigate the facts of the case was the best thing that could have been done by the Government. That commission was composed of three eminent statesmen—Lord Cardwell, Lord Winnmarleigh, and Mr Forster; of a great lawyer, skilled in the art of obtaining and weighing evidence, Sir John Karslake ; of one of the leading biologists in this country, Professor Huxley ; of a surgeon who knew the relation of physiology to the practical art cf treating disease, Mr Erichsen ; and of a leading journalist and most able thinker, Mr Hutton, the editor of the *Spectator*. Thus composed of men likely by character and previous training to ascertain the truth, and to suggest wise procedures, it held numerous meetings, examined witnesses partial and impartial, collected a body of evidence of a most interesting and diverse character, and gave in a report which, while it recommended legislation, is generally in favour of physiologists. No one can read the evidence in the blue book, and the report founded thereon, without coming to the conclusion that the case of those who raised the outcry against physiologists in this country completely broke down. On considering this report, the Government brought in a Bill, certain of the provisions of which seemed not only oppressive to physiologists, but were calculated, if carried into law, to impede the progress of science. The members of the medical profession who knew the value of the experimental method in physiological research, and who were painfully conscious of the many imperfections of the art due to want of knowledge, were now aroused, and, by a use of the machinery of the *British Medical Association*, they aided the few physiologists of the country in making representations to the Government, which were favourably received, and which led to important modifications in the Bill. That Bill has now passed into law, and I appeal to our opponents to desist from further agitation. The case has been tried and the verdict has been given. For my own part I was all along opposed to legislation as being quite unnecessary in the circumstances, but I had, at the same time, that confidence in the common sense and good feeling of our legislators, as to expect a Bill favourable to physiologists when the facts were put before them. Some of our opponents, led away by their feelings, have put in print many erroneous statements. Hosts of pamphlets have been circulated, many of them well meant, but utterly wrong both in form and matter. For a season these pamphlets produced effects, and many people of good intentions were led astray. But a reaction began, and when the leading members of the medical profession came forward boldly and stated their opinions, it was soon completed.

The only preventive for such casual excitements is the diffusion of knowledge. I have no belief whatever in the theory that most people are fools on questions of this kind. The great majority of our people of both sexes are perfectly capable of reasoning and of forming sound opinions. What they require is knowledge, evidence, and representations strong enough to overcome the bias of prejudice. I therefore warn our opponents that if the agitation be continued, we will appeal to the bar of public opinion. We will instruct the public through the press, on the platform, and by the pamphlet, and I have no fear of what the issue will be. The fact that the members of the medical profession who, by knowledge and habits of thought, are best competent to judge in this matter, acted as they did, indicates at once the result.

Importance of teaching Biology.

This leads me to say a word as to the diffusion of biological knowledge among the people. I regard this as one of the healthiest signs of our day. A general knowledge of the structure and functions of the human body, of its necessities, of those agencies which act prejudicially upon it, and of those conditions which favour long life, the relief of pain, the prevention of sickness, and the transmission of healthy offspring, cannot fail in being of high practical importance. Furthermore, the acquisition of knowledge of the general laws of life as seen in the various living things about us, in addition to being an intellectual training of great value, will probably engender a feeling of kindness for every living thing, and thus even animals will share in the benefit. At one time knowledge of this kind was almost wholly reserved for the medical profession; but now it is taught in every village school. The instruction of ladies in a knowledge of the general structure and functions of the human body has recently been successfully carried on in various parts of the country, more especially in Edinburgh and Cambridge, and I can state from my own experience of this matter, that there is no difficulty whatever in so treating the subject as to make it interesting and instructive without giving it too much of a professional character. The effect of education of this kind will be that, within one or two generations, many social questions will be viewed more from the physiological standpoint than at present; doctors will be able to give an intelligible explanation to their patients of their condition, when it is deemed judicious to do so—a feat not easy of performance at present; the management of the sick will be better attended to on more rational principles; quackery will waste away by degrees, because it will have no ignorance and credulity on which to feed; and legislation will be prompted in many instances not by emotional agitations, but by enlightened views of the physical nature of man.

I cannot help mentioning the name of Professor Huxley in connection with the introduction of this great subject among our educational appliances, both as to what should be taught, and how to teach it; and it may not be considered presumptuous in me to predict that this alone will entitle him to a place in the thoughts of posterity.

Practical Aspects of Anatomy and Physiology.

There is an impression in the minds of many regarding our scientific work which I would like to remove, and here I direct my remarks, not to purely scientific men, but to the public. Many still think that anatomy and physiology have no practical side, and consequently they do not take that interest in their prosecution which they otherwise would do. The results of the triumphs of physics, chemistry, and engineering, are so patent to all as to excite universal interest, so that you will often find a man of average intelligence readily engrossed in any new discovery of physics or of chemistry, while he is indifferent to new facts in the domain of biological science. This state of mind, of course, is due to a want of appreciation of the practical aspect of our work; and I hold that till the man be better informed, he is quite entitled to take this view of the matter. But I wish to point out that, although our sciences occupy their own place as abstract systems of truth, bearing no apparent relation to the wants either of humanity or of the lower animals, still they have also a practical aspect of the highest importance. My belief is that every advance in science, by adding to the sum of human knowledge, and thus enabling man to have a correct idea of his true position in the universe, and of his relations to it, will ultimately promote both his own material well-being and that of the other living things about him. I do not see how it can be otherwise; and the history of the past supports this view. Knowledge promotes civilisation; and the progress of civilisation, on the whole, lessens suffering, and increases the physical sources of happiness both to man and beast. The thought must therefore be urged, that every research, however far removed it may appear to be at first from having any relation to the welfare of living things, occupies its place in leading to this grand consummation—life, liberty, and happiness to all. From many illustrations which occur to the mind, I shall take only one. M. Pasteur proved that in the atmosphere there exist germs or particles of matter, call them what you will, which excited fermentation and putrefaction in certain fluids. Of this, I think there cannot be any reasonable doubt. Whether fermentation be always the result of the presence of germs is another question, upon which I shall not enter, nor shall I engage on a discussion of the question of so-called spontaneous generation, which, though highly probable, has never in my opinion been proved. These investigations of Pasteur, relating to which a great controversy has taken place, referred to animal and vegetable organisms of the very humblest type, organisms so small that to prove their very existence in the air, indirect and complicated methods of procedure had to be adopted. But Mr Lister, who once occupied the Chair of Surgery in this University, and who now adorns the Chair of Clinical Surgery in Edinburgh, was attracted, whilst he was in Glasgow, by the doctrines of the eminent French chemist; he repeated experiments to satisfy himself of their truth, and he came to the conclusion that these particles in the air are the sources of disturbance in wounds, leading to suppuration, putrefaction, and many grave constitutional symptoms. To remove the influence of these germs, he devised the antiseptic system of treating wounds, a system first put into operation in this city, and which is attended with great success in the hands of those who practice it carefully. Slowly but surely this system—the greatest advance in surgerysince the days of John Hunter—is winning its way in this country, on the continent, and in America. The surgical mind is eminently conservative and not easily convinced; but it gives way after a struggle, and the benefit both of the preliminary caution, and of the subsequent vigorous adoption, is to humanity. What does the practice of this system of treating wounds mean? It means, speaking generally, the banishment of pyæmia and surgical fever from hospitals, the possibility of performing many serious operations with comparative safety to the patient, the relief of pain in the dressing of wounds, and the saving of human lives. I need scarcely add that Professor Lister did much in his earlier years to give him a high place among British physiologists, but, in addition, he has shewed the successful application of purely scientific knowledge to the advancement of the art of surgery, and in suggesting a method by which life may be saved and suffering mitigated, he has earned the gratitude of humanity.

Importance of Investigations on the Physiological Action of Active Substances.

There is another field of physiological research which promises to confer great practical benefit on the human race. I refer to the investigation of the physiological action of active substances, which may lead us not only to the discovery of important therapeutic agents, but to a knowledge of the relation which exists between the chemical constitution of a substance and its physiological effects. Already a considerable amount of work of this kind has been accomplished. The physiological action of the various anæsthetics, such as chloroform, chloral, alcohol, &c., of narcotics, such as morphia, narceine, narcotine, codeine, and many others, and of alkaloids, such as strychnine, brucine, nicotine, atropine, hyoscyamine, physostigmine, muscarine, veratrine, aconitine, digitaline, santonine, ergotine, and quinine, has been carefully studied. The celebrated research of Professor Crum Brown and Dr Thomas R. Fraser, upon the physiological action of the methyl, amyl, and ethyl substitution compounds of certain alkaloids, in which they shewed that a change in chemical composition was attended by a change in physiological action, opened up a new field of discovery. The investigations of Dr B. W. Richardson on the action of homologous series of alcohols and ethers, and the observations made by Professor Dewar and myself on the action of the chinoline and pyridine series of bases, and their substitution compounds, all tended to illustrate the same general truth. Nor must I forget to mention an interesting series of investigations made by Professor Gamgee, of Manchester, and his pupils, communicated to our section at the present meeting, on the action of various compounds of the rare metal vanadium, on the action of chromium salts, and on the differences between the physiological actions of ortho-, meta-, and pyro-phosphoric acids. Here again we had a further illustration of the important facts that the physiological action of any active substance is affected (1) by the number of atoms in the molecule and its complexity of structure; and (2) by the degree of stability of the molecule. That is to say, the more complex the molecule, the more intense and prolonged will its action probably be; and, on the other hand, if the molecule of a substance tend readily to break down or split up while circulating in the blood, it will act more intensely than if it held firmly together for a considerable time. These generalizations are merely tentative. We have not yet sufficient data to entitle us to term them general laws.

Now, no one can glance over any work on organic chemistry without seeing on every page the names of substances regarding the physiological action of which we know nothing. I would not have these investigated in a promiscuous manner, with the vague hope of coming upon something new. Here, as elsewhere in science, we must be guided so far by the light cast upon the unknown by former discoveries, and by those general laws which have been formulated by previous investigators. Nor is the mere discovery of new poisons anything but a "sorry task," unless the research lead us to an agent likely to be of therapeutic value, or to the enunciation of an important general principle. But former experience warrants us in hoping, nay in expecting, that new useful agents will yet be discovered. I need not refer to the practical applications of chloroform and ether, as these are too well-known to need any eulogy from me; but I may be allowed to direct attention to chloral, first discovered by Liebig in 1832, and known for many years merely as the ultimate product of chlorine upon alcohol. It was only a few years ago that Liebreich of Berlin pointed out its important physiological action, and it is now recognised as a therapeutic agent of the highest value. Its use, no doubt, has often been sadly abused, and people have often trifled with a powerful physiological agent even to the loss of their lives; but when we think of the hours of pain which many a weary sufferer has escaped by its use, we cannot but regard it as a boon to humanity.

Here the physiologist must go hand in hand with the chemist. The chemist in his laboratory prepares the substances, and builds up new compounds by those wonderful synthetic processes which are now the glory of his science; it is then the duty of the physiologist to investigate the actions of these. By united work, who can foretell what may be accomplished? For example, may we not hope to see the day when such a substance as quinine, or a substance having similar therapeutic properties, may be produced artificially; or, may we not obtain an anæsthetic as potent and even less dangerous than those at present employed?

Nor have we yet investigated the physiological action of the active principles of thousands of plants, many of which may prove to be of great value. Let us remember the well-known words of Shakespeare, as Romeo—the love-stricken Romeo—repairs to Friar Laurence's cell, "when grey-ey'd morn smiles on the frowning night." The old friar thus soliloquises:—

> "I must up-fill this osier cage of ours
> With baleful weeds and precious-juiced flowers.
>
> . . . . .
>
> Many for many virtues excellent
> None but for some, and yet all different.
> O, mickle is the powerful grace that lies
> In herbs, plants, stones, and their true qualities:
>
> . . . . .
>
> Within the infant rind of this weak flower
> Poison hath residence, and medicine power;
> For this, being smelt, with that part cheers each part;
> Being tasted, slays all senses with the heart."
>
> —*Romeo and Juliet, Act II., Scene III.*

I cannot help noticing here in passing that Shakespeare appears to have conceived the notion of the physiological antagonisms of drugs, which is generally regarded as quite modern, although the practice of using antidotes has been followed from the earliest times. Thus, in the interview between the Queen and Cornelius, the physician, in Cymbeline, she says:—

> "Having thus far proceeded,
> (Unless thou think'st me devilish) is't not meet
> That I did amplify my judgment in
> Other conclusions? I will try the forces
> Of these thy compounds on such creatures as
> We count not worth the hanging (but none human),
> To try the vigour of them, and apply
> Allayments to their act, and by them gather
> Their several virtues, and effects."
>
> —*Cymbeline, Act I., Scene VI.*

Relation of Physiology to Medicine.

I may now be permitted to say a few words regarding the present position or attitude of physiological science. I am in the habit of thinking of physiology, not only as a physical science in itself, but as having a direct relation to two other sciences—medicine and psychology. Carrying out this idea, were a sculptor to form a group, he might represent physiology, on the one hand, dispensing gifts and affording assistance to medicine, and on the other, pointing upwards to psychology as the greater sister of the three. Abandoning metaphor, there can be no doubt physiology is most intimately connected with these sciences. First of all, with regard to medicine, and by this term of course I mean the whole art of detecting and curing disease, there are many problems which physiology alone can solve. The origin of disease, the steps of the changes by which organs and tissues become so altered as to produce what is called a diseased state, the effects of one diseased organ upon others which are healthy, the actions of remedial substances, both in the healthy and in the diseased condition, are all physiological processes, many of which cannot, in the present condition of society, be thoroughly investigated by a practitioner, who is often too busy a man to engage in this kind of work. Such labour must be handed over, to a large extent, to a special class of men. They must investigate, experiment, and work up the subject in the laboratory, either the physiological laboratory of the university or school of medicine, or of the hospital or infirmary, as the business of their lives, and from time to time announce the results. These results must be checked by past experience, or by a knowledge of cases apposite to the point, by the men who come into daily contact with patients, and their verdict, so far as any practical benefit is concerned, must usually be regarded as final.

Importance of Systematic Investigation of Diseases.

In the present state of science, we have not reached that subdivision of labour, nor need it be ever absolutely complete. Many of the best contributions to physiological and pathological science, during the past twenty years, have been from men busy in practice. Such busy men will, no doubt, always be found in the ranks of the medical profession, and they will contribute so far to the advancement of medicine; but in the future, much scientific work, as a basis of the practical treatment of disease, must be done by men specially devoted to the laboratory, the pathological theatre, and the clinical ward. The origin and progress of those diseased processes which cause cancer, tubercle, rheumatism, and gout, with all their attendant evils, the discovery of the poisons which produce fever in its manifold forms, the modes of counteracting these poisons so as to arrest the progress of fever at an early stage, and the investigation of those diseases which destroy thousands of our domestic animals, are all subjects which must be investigated more systematically and on a larger scale than has yet been done. Such stupendous work can scarcely be left to individual effort. To carry it on requires men, time, and money, and these can only be supplied by the aid of governments, or municipalities, or by private munificence. Already excellent work has been done by Professor Burdon-Sanderson and his coadjutors, by Dr Klein, and by Dr Thudicum, for the Medical Officer of the Privy Council, and by Professor Rutherford, Dr Braidwood, and others, at the instance of the *British Medical Association;* but still the amount of aid given is small alongside of what is lavished, for example, in warlike experiments. Compared with what is needed for the manufacture, testing, and equipment of an eighty-ton gun, designed to destroy human life and property (no doubt on the theory that it is for the ultimate welfare of the State to do so), a small sum would be necessary, but authorities do not yet see the vast importance of inquiries of this kind, and consequently consider two or three thousand pounds per annum sufficient. We accept gratefully what help is given, but we look for more. I hope to see the day when Government will equip and thoroughly furnish a body of men for the investigation on a large scale of the genesis of such diseases as tubercle or of typhus fever, both of which kill in Great Britain alone thousands of people annually, just as they have sent out a *Challenger* expedition to explore the depths of the sea, or have at present a number of brave men engaged in the attempt to discover the North Pole. To strike at the root of one of those great maladies that afflict the human race, such as cancer, tubercle, or fever, would confer an inestimable blessing on humanity, and honour on the Government that proposed and carried out the undertaking.

Relation of Physiology to Psychology.

As I have said, physiology is intimately connected with psychology, or the science of mind, and as this department of physiological work has lately been my chief study, I may be allowed to refer to it a little more in detail.

Psychology may be divided into two parts: first, all those phenomena which we may include under the term mind properly so-called, such as feeling, volition, and intellectual processes; and second, the phenomena which are associated with, and which indicate the alliance between, mind and matter. Every mental act may be regarded in the present state of knowledge as having a double aspect,—on the one side it is known to our consciousness, and on the other side it is the result of a number of physical processes occurring in the brain.

The Methods of Psychology.

In the investigation of mental phenomena, two modes of inquiry have been hitherto followed: First, that of introspection and reflection, in which the investigator looks within himself for the facts of his experience; and second, that of the examination of physiological processes which coincide with sensorial or mental changes. It is evident that the first of these methods, usually called the subjective, is open to the objection that by it a mind attempts to observe its own operations, and that the proceeding is somewhat analogous to asking a machine to investigate its own mechanism. This objection urged in other words by Comte, Maudsley, and others, may be answered by replying that the subjective method does not attempt to explain the physiological phenomena concomitant with mental states, but the laws which regulate these mental states themselves. Suppose a complicated machine possessed consciousness, I can readily understand that by the exercise of this consciousness it might be unable to discover the relation and mechanism of its own parts, because in attempting to do so the machinery would be so interfered with as to prevent normal action; but it might still be able to study the products of its operations. I do not, therefore, decry this old method of psychological research as it is so much the fashion to do in these days. Apart altogether from the philosophical speculations and systems of philosophy founded upon them, I think many data accumulated by such men as Locke, Berkeley, David Hume, Thomas Reid, Dugald Stewart, Thomas Brown, Sir William Hamilton, and James Mill, have as good a right to be considered correct as some of the quasi-metaphysical conceptions of modern physical science. Subjective inquiry carried on by such men cannot be given up as a mode of psychological research. It may not carry us much further than it has done, but it has rendered good service already, and may possibly do more.

But, on the other hand, the objective method appears to me to be the one which, in future, will be principally cultivated, and it is for this reason that, as a physiologist, I wish especially to refer to it.

It is the business of physiology to supply psychology with information regarding physical processes occurring in the nervous system; and it is one of the special features of the physiology of the present day to direct attention to the physical side of mental phenomena. No doubt Aristotle, Hobbes, and Hartley incorporated into their psychological theories much that was purely physiological; but in their days the physiology of the nervous system was in a crude state, and, consequently, did not lead to great results. In comparatively recent times, a new inductive and experimental department of science has arisen, the nature of which is indicated by the term physiological psychology, and which is being diligently cultivated by numerous workers, both at home and abroad. In our own country the writings and researches of Herbert Spencer, Alexander Bain, Dr Laycock, George Henry Lewes, Dr Maudsley, Dr Carpenter, Alfred Barratt, and James Sully, and on the continent those of Fechner, Helmholtz, Wundt, Hermann Lotze, Taine, Donders, Plateau, and Dalboef, have excited much interest, and have led to the formation of a new school of thought.

I think it right to mention here specially the name of Professor Laycock, who has done more, in my opinion, in this field of inquiry than any other member of the medical profession of this country in our time. His teaching has largely contributed to our present humane methods of treating the insane; he has attracted year by year some of the best students of the University of Edinburgh to this important department of medical practice; and his earlier writings incontestibly shew that, many years ago, and prior to most of the writings of those great men whose names I have just enumerated, he not only recognised the value of physiological research with regard to mental phenomena, but made important contributions himself.

Physiology has thus encroached on psychology, and is attempting to supply from the objective side an explanation of at least the simpler mental phenomena. As a proof of awakened interest in this department, one of the features of the past year has been the appearance of *Mind*, a quarterly journal of Psychology, edited by my able friend Professor Croom Robertson of University College. In the prospectus of this journal, it is stated that "psychology, while drawing its fundamental data from subjective consciousness, will be understood in the widest sense, as covering all related lines of objective inquiry. Due prominence will be given to the physiological investigation of nerve structure." This quotation indicates the view which the editor takes of the relation of the two sciences, and already valuable papers have appeared on subjects connected with physiological psychology, from the pens of Sully, Lewes, Wundt, and others.

Now a certain class of thinkers are alarmed by work of this kind. They are afraid of the tendency "to represent the mental fact as a physical fact," and they are inclined to shut their eyes to the physical facts connected, undoubtedly, with psychological processes, and to be contented with the study of subjective phenomena. But as most admit that there are two aspects in which mental phenomena may be viewed, why should not both be looked at carefully? If it be also admitted, that it is impossible to connect any physical process (supposing we knew it) occurring in brain cells with an act of consciousness, what is the use of taking a one-sided view of the phenomena in question? Why not study both sides of the problem, and give up the attempt at reconciliation which is entirely beyond the pale of our faculties? This mystery of mind and matter has puzzled thoughtful men from the earliest times. Some have attempted a reconciliation. They have reasoned in a circle, so that most people, after perusing their works, are no nearer an ultimate solution than they were at the beginning. We always come back to this view of the case, namely, that every fact of mind has two aspects, a physiological and a psychological. That is one way of looking at the problem, and it is the one which, in the present state of knowledge, personally I prefer. But there is another. Thus, as has been well argued by Mr George Henry Lewes in his recent work, *Problems of Life and Mind,* two very different descriptions may be given of one and the same mental activity. The one may be expressed in the language of psychology, which is the language we commonly use to describe our feelings; the other may be stated in the language of physiology, a language intelligible only to those acquainted with the present state of physiological research. He says: "All that we have to guard against, is the tendency to mistake difference of aspect for difference of process, and to suppose that changes in feeling can exist independently of changes in the organism, or that any change in the organism can be effected otherwise than by some previous change." This way of stating the question may be more satisfactory to some minds. At all events, it is a fair attempt to solve the puzzle of our present state of existence, in which we are constantly brought face to face with the antithesis of object and subject.

Abandoning these speculations which are fruitless in practical effects, let me now endeavour very briefly to indicate the lines of inquiry in the domain of physiology along which progress has been and may be made in the attempt to solve psychological phenomena; and I wish it to be understood that I do not take these in any logical order, but merely adduce them by way of illustration. It will also be my aim not so much to describe what has been done in the past, as to indicate what remains to be done in the future.

Research in Physiological Psychology.

First of all, then, it is quite evident that all researches on the general physiology of the great nerve centres are of paramount importance. Such researches as those of Hitzig, Fritsch, and Ferrier on the excitability of the cerebral hemispheres, supplying new ideas regarding the mechanism of the brain as a compound organ; of Wundt on central innervation and consciousness, in which he discusses in a manner never before attempted, the phenomena of reflex excitation; of William Stirling on the summation of excitations in reflex mechanisms; of various French physiologists on the mode of action of ganglia in insectæ; and of many others, are all recent important contributions to this department of science. Here, however, we have to confess that we have little accurate information regarding the minute structure of the parts involved, and consequently no anatomical basis on which to found our views. We have a general idea of strands of nerve-fibres and groups of nerve-cells of various forms, but we have no precise knowledge of the relative quantity of these, or of the relations of one group of nerve-cells to another group. We are unacquainted with any peculiarity in structure, for example, by which even an accomplished histologist could identify three microscopical sections as respectively portions of the brain of a man, of a monkey, and of a sheep. All this has still to be worked out. Every little area of brain matter has to be surveyed and carefully described. Supposing this were done in the case of the human brain, and of the brains of the higher animals, the same must be attempted with the brains of animals lower in the scale. I can then conceive a grand collection of facts which may throw light on the intricate working of different kinds of brains, and, perhaps, afford a rational explanation of certain simple psychological characters.

Suggested Investigation.

What I mean may perhaps be better understood by a research, which I would suggest by way of experiment. No one who has kept an aviary of small birds—say a collection of our native and foreign finches—can have failed to observe marked differences of character and habits among different members of the same genus, and even among different members of the same species. One manifests cunning, another combativeness, a third kindness to smaller brethren, a fourth bullies all about him, a fifth may usually be quiet and peaceable, but occasionally gives way to uncontrollable rage, and so on. The question arises, then, Have these psychological peculiarities any organic basis, any explanation in the structure of the brain? or, Are we to rest satisfied by asserting that these peculiarities are due to the action of some kind of psychical principle regarding which we know nothing? I have little doubt most will agree that these psychical characteristics of birds depend on peculiarities of brain structure the result of hereditary transmission through many generations. If so, here we have an opportunity of examining the microscopical structure of small brains, relatively simple, and easy of manipulation, with the view of ascertaining whether or not there are any structural differences which may account for these differences in psychical character. This is a line of inquiry likely, in my opinion, to establish an organic basis for a comparative psychology.

Recent Researches on the Chemistry of the Brain.

But in studying the physiology of the brain as an organ of mind (and the same holds good with regard to the other great nerve centres), we must not forget that in addition to mere structure, two other factors have to be taken into notice. These are, first, the chemical constitution of the brain itself; and, second, the amount of chemical interchange that goes on between it and the blood. There are so many exceptions to the general rule, that size of brain and number of convolutions are proportional to the degree of mental power, as to render it highly probable that to account for these exceptions, we must assume differences of minute structure, differences of chemical constitution, and differences of chemical interchange between blood and brain. That is to say, we may have two brains equal in size and in number of convolutions belonging to two individuals very unequal in mental capacity. This may be accounted for either by supposing that the minute structure of a convolution of the one may be more intricate than that of the other, or the one brain may be richer in certain complicated chemical compounds, the splitting up of which into simpler bodies are necessary in processes of thought; or, finally, the activity of chemical interchanges between the blood and the brain may be much more rapid in the one than in the other. All this, however, must remain a matter of conjecture until we know more of the chemistry of the brain than we at present do. I have, therefore, hailed with satisfaction the appearance of an elaborate paper by Dr Thudicum, entitled, "Researches on the Chemical Constitution of the Brain," in a recent volume of "Reports of the Medical Officer of the Privy Council and Local Government Board." It is impossible to give here a detailed account of this laborious inquiry in which Dr Thudicum and his assistant, Mr C. T. Kingzett, have analysed the brains of oxen, requiring no fewer than a thousand of these in the undertaking. The result, generally speaking, has been the discovery of seventeen compounds, for the first time detected as ingredients in brain matter; and in an appendix, Dr Thudicum gives a list of no fewer than eighty-two substances which have been detected by himself and other chemists in the brain. Even admitting what is highly probable, that many of these are products of the decomposition of a few more complex substances, still we obtain from a mere inspection of this list some idea of the intricate chemical nature of this part of our bodies.

Various striking thoughts are put forth by Dr Thudicum at the end of his paper, a few of which I may be allowed to refer to with the view of shewing how chemical considerations may assist us in our conceptions of the working of the nervous system. He says: "During these proceedings the first striking fact which meets the inquirer is, that nerve-matter contains abundance of water. This, in conjunction with the peculiar manner in which the water is contained, engenders a mobility of ultimate particles within certain limits of movement. It also gives penetrability by liquid diffusion, while excluding porosity and its capillary effects, by which means a ready nutrition by diffusion in one direction, and ready cleansing from the effete crystallisable products of life in another, are insured. Consequently the brain as a whole is essentially made up of colloid matter, and may be compared to a colloid septum, on the one side of which is arterial blood and cerebrospinal fluid of the ventricles; on the other side, however, is cerebrospinal fluid of the arachnoideal space and venous blood. It follows from this that the large amount of water present in the brain is not there, so to say mechanically only, like water in a sponge, and capable of being pressed out mechanically, but is chemically combined as colloid hydration water, or better, water of colloidation."

Dr Thudicum divides a large amount of the matter occurring in the brain into three groups, viz. phosphorised bodies, consisting of carbon, oxygen, hydrogen, nitrogen, and rich in phosphorus; nitrogenised bodies, containing only carbon, oxygen, hydrogen, nitrogen, and no phosphorus; and, third, oxygenised bodies, formed of carbon, hydrogen, and oxygen alone. The phosphorised bodies he divides into three sub-groups, termed kephalines, myelines, and lecithines. Each of these has certain definite chemical characteristics, which he summarises as follows:—"The kephalines possess the tendency to be oxydised, oxydisability; the myelines are not easily changed by any agent or influence, and possess therefore stability; the lecithines easily fall to pieces, they are afflicted with lability."

He then points out the remarkable tendency of the phosphorised bodies to combine with other substances, shewing a diversity of affinities "not possessed by any other class of chemical compounds in nature at present known." He shews that these affinities are influenced by the amount of water present, and by the mass of the substance or re-agent presented to the brain matter, so that the interchange "of affinities may produce a perfectly incalculable number of states of the phosphorised, and consequently of brain matter. This power of answering to any qualitative and quantitative chemical influence by reciprocal quality or quantity, we may term the state of *labile equilibrium*; it foreshadows on the chemical outside the remarkable properties which nerve matter exhibits in regard of its vital functions."

All of these remarks by Dr Thudicum point to a field of research which will not be explored for many a year to come. But there can be little doubt that when the chemical statics of the brain have been accurately ascertained, we will be in a position to study the chemical interchanges between the blood and the nervous tissue. Should the skill of our physiological chemists succeed in unravelling these, then we will be in a position to understand at least two different sets of phenomena. These are—(1) The chemical changes which undoubtedly take place during the occurrence of mental phenomena; and (2) The exact nature of the action of such substances as alcohol, narcotics, and the various alkaloids which are known to act on the nervous system. I need scarcely add that accurate knowledge regarding the physiological action of these substances would probably be of great service in the treatment of disease.

Researches on Sensory Impressions.

In the second place, researches into the physiology of the senses afford another series of data for the psychologist. These researches may be said to be of three kinds—(1) inquiries into the anatomical and physiological mechanism of the sense organ itself, such as, in the case of vision, the general structure of the eye as an optical instrument, and its movements by the action of muscles, so as to secure the conditions of monocular or binocular vision; (2) inquiries into the nature of the specific action of the external stimulus upon the terminal organ of sense, and the transmission of the effect to the brain; as, for example, the action of light on the retina, and transmission along the optic nerve; and (3) experiments in which various stimuli are permitted to act under certain conditions on the terminal apparatus, and the result is observed and recorded by the consciousness of the experimentalist himself, as in researches on colour, duration of impressions on the retina, positive and negative after images, &c. By these three modes of inquiry a large number of facts relating chiefly to the senses of hearing and vision have been collected; and most of these facts, inasmuch as they assist him in understanding the conditions of sensory impressions and sensational effects, are of importance to the psychologist.

Measurement of Time in Sensory Impressions.

The next step of importance made by physiology into the domains of psychology, is the measurement of time or duration in sensational effects.* This has been carefully measured by objective methods. Speaking generally, the time occupied from the commencement of the action of the stimulus to the termination of a sensation, may be divided into four portions, each of which has a certain psychological interest:—First, an interval of time is occupied by the primary physical change produced by the stimulus. During this interval, called the period of latent stimulation, no effect is observed. Thus, when a motor nerve distributed to a muscle is stimulated by a short electrical shock, about 1-60th of a second passes before the muscle contracts. Second, when the change in the nerve or terminal organ has begun, a second interval of time is occupied in the transmission of the impression to the nerve centre, which is succeeded by a third interval, during which changes occur in the nerve centre, and the result of which is a sensation. The time occupied in transmission, or the rate of conductivity in nerve, is tolerably well known, being at the rate of about 200 feet per second in the nerves of man; but the time occupied in the production of the sensation in the centre has not yet been clearly ascertained, owing to the difficulty of supposing such a sensory nerve centre to be, previous to the stimulus, in a state of absolute inaction. Lastly, it has been found that when a nervous action of any kind has been initiated by a stimulus, it goes on for some time after the stimulus has ceased to act. This prolongation of the sensation may be well studied in the case of impressions on the eye, where the time of the duration of the impression has been measured by Helmholtz, Plateau, and others. These distinguished observers also found that the length of time

* In the following observations, I am much indebted to the essays of Mr James Sully, contained in his volume, *Sensation and Intuition.* London. 1874.

occupied by the after effect varied according to the intensity of the light. Thus, after a weak light, the unchanged impression lasts longer than with a strong light. A strong illumination is followed by an after impression fading sooner than with a feeble stimulus; the result being that so far as the retina is concerned, it comes to the same thing whether an intense light acts for a brief time, or a faint light for a longer time.

Exhaustion of Nerve or Sensory Organ.

This line of research has also made it possible to measure the time required for exhausting a nerve or sensory organ. When, for instance, a limited area of the retina has been stimulated for a certain time, and the stimulus has been removed, the after positive effect, due to increased excitation of the parts, disappears, and is followed by a negative effect, due to temporary diminution of the sensibility of the parts, in the form of what is called the negative after-image. Suppose, for example, an area of the retina be acted upon for a period of from five to ten seconds, and the stimulus be then removed, the so-called positive after-image vanishes quickly, and the negative after-image, frequently of a complementary colour to that of the exciting cause, appears, and lasts for a short time, gradually fading away as the nervous parts recover from the effects of the stimulus. Similar phenomena may be observed in studying the durations of sensations of tone, which I have frequently perceived in experiments made by myself; but it is more difficult to identify, by description and designation, the after effects in the case of audition than in the case of vision. Probably it may be found still more difficult to notice these after sensations in the other senses, although in all there is often the experience of a lingering feeling after the cause has been removed, which no doubt has its place in those transient sensations which assist in filling up the spaces, as it were, in our conscious life.

In experiments upon a sensory organ, such as the retina, a little consideration will shew that it is almost impossible to ascertain the effect of a stimulus upon a retina which has never before been affected. This difficulty has been felt by all experimenters. Molecular action in such a structure has been in operation from the very beginning, and such action, if of sufficient intensity, must produce a certain effect on the conducting tract, and on the recipient centre. This effect, although of too weak intensity to produce those changes which result in consciousness, must be taken into account in the measurement of the intensity and duration of sensory impressions. Thus the eye has a light of its own due to changes in the retina, although this may never be conscious to us as a luminous impression. This conception of the state of matters in a terminal organ such as the retina, when applied to actions going on in the brain, at once indicates that similar actions, or rather that similar states of unrest, of change, variation, and modification, are going on in these deeper parts which may never result in consciousness, *per se*, but which altogether may have an influence on our mental existence comparable to that of the feeble impressions constantly transmitted to the cerebrum from the viscera, sometimes termed the internal senses.

Relation between Strenth of Sensation and Magnitude of Stimulus.

Having shewn that sensory impressions are distinctly related to time, the next advance made by physiologists was to prove that there was a relation between the strength of the sensation and the magnitude of the stimulus. Here there are difficulties in explaining what is meant, because language fails. We have no words to discriminate ideas which hitherto have related to two distinct fields of knowledge—the objective and the subjective. To speak of the strength or magnitude of a sensation seems to be using terms applicable only in another region, and quite inapplicable to psychological phenomena, although no one has any doubt in distinguishing the intensity or magnitude of one pain from that of another. There is no difficulty in understanding the phrase-magnitude of the stimulus. A weight of ten pounds is greater than that of one pound, light from ten candles of equal size is more than that given out by one, and the tones of a violin of equal pitch and quality, may vary in intensity according to the pressure of the bow on the string. It is difficult, however, to obtain an absolute measurement of variations in sensation, which is, of course, a subjective phenomenon. This can only be done by varying the objective cause, by observing a large number of instances, and by expressing variations in the subjective phenomenon in terms applied to variations in the objective cause. If the average result obtained from a large number of instances indicate any ratio between the magnitude of the stimulus and the subjective phenomenon, then we may conclude that there is a relation between the two.

This mode of inquiry, first originated by Professor E. H. Weber, in his celebrated experiments on tactile impressions, (and which were first introduced to notice in this country by Professor Allen Thomson), was afterwards carried out by his colleague Professor Fechner, and has been subsequently elaborated by Professor Wundt. It has led to various remarkable results, the chief of which are—(1) That in the case of each sense there is an upper and a lower limit, beyond which the amount of stimulus produces no appreciable difference of effect; and (2) that within this range there is a definite ratio between the stimulus and the amount of the sensation. The upper limit beyond which an increase of external stimulation is not followed by any observable increase in sensational effect, was first observed by Professor Wundt. The lower limit has been noted by many observers, and it is indicated in almost every physiological text-book. Now it does not matter much to us, in taking a general view of things, what the limits are, provided we are sure that such limits exist, inasmuch as it indicates another element of proof that psychological phenomena, so far as sensation is concerned, occur within certain physical limits.

Fechner's Investigations.

The next step naturally was to establish the ratio between the magnitude of the stimulus and the magnitude of the sensation. To do this directly is impossible, as any estimation of the amount of sensational effect following a given stimulus would probably be erroneous, because our perceptions are usually qualitative and only rarely, and never absolutely, quantitative. Fechner recognised this fact, and he employed for the solution of the problem various methods by which he measured not sensations themselves, but the amount of discriminative sensibility between two sensations produced by stimuli of unequal magnitudes, and he studied the ratio between the difference of weight and the absolute quantity of the stimulation. By varying the amount of the stimulus in every possible way, he eliminated the chances of error, and arrived at definite results. These results he formulated into a general "psychophysical law," which may be expressed in various ways. Mathematically it may be put, that "sensation increases in proportion to the logarithm of the stimulus." Now "logarithms increase in equal degrees when the numbers so increase that the increment has always the same ratio to the magnitude of the number." It may be put in another way by saying that "the more intense a sensation the greater must be the added or diminished force of stimulation in order that this sensation undergo an appreciable change of intensity." The mode of arriving at some of Fechner's results may be better understood by an experiment which any one can repeat. In the case of muscular sensation, suppose two weights, A and B: we wish to ascertain the least difference between these perceptible by the muscular sense, say when we lift them in the hand. Let it be so arranged that both weights are composed of different pieces, so that the one may be made less or more than the other at pleasure. If A and B be nearly equal in absolute weight, the person on whom the experiment is made will judge them to be of equal weight. Let weights be now added to B until the difference between A and B becomes perceptible, and as a test, let the weights be again removed from B until, in sensational effect, A becomes again equal to B; let the same experiment be repeated with weights of different absolute amount, and it will be found that there is a distinct ratio between the absolute weight and the weight that had to be added to it or taken from it to produce the least perceptible difference of impression, whatever these weights may be, up to the limit, of course, which I have already noticed. It will always be found that the additional or substracted weight is one-third that of the absolute weight,—a fraction which indicates the degree of intensity of the stimulus required to produce the least perceptible feeling of difference of sensation, and which may be termed the "*constant proportional*" of that kind of sensation. This fraction, in the case of sensibility to temperature, Fechner found to be one-third; Renz, Wolf, and Volkmann arrived at the same fraction with regard to auditory impressions; and various observers have found that in visual impressions it is one-hundredth.

Now, the intensity of sensation depends on two conditions: (1) the intensity of the excitation; and (2) the degree of excitability of the sensory organ at the moment of excitation. But suppose the excitability of the organ equal on two occasions, the intensity of the sensation does not increase proportionately to the increase of the excitation. That is to say, suppose we bring into a dark chamber a luminous body such as a candle—it produces a certain luminous sensation; then introduce a second, third, and fourth—the excitation is double, triple, or quadruple; but experiment shews that the increase in the amount of the sensation is much less; in other words, let the stimulus increase from 10 to 100 times, and from 100 to 1000 times, the sensation will be only one, two, and three times stronger. The importance of the discovery of this remarkable law is, that it shews a distinct mathematical relationship between stimulation and sensation. Possibly it may be found to have applications to other psychological phenomena. May it not vary in different animals, and even in different individuals?

Criticism of Fechner's Method.

It is quite noticeable, however, that in the case of each sense, the law did not hold good throughout the whole range of variations in intensity of stimulus; and it is not surprising, when we consider the complexity of the conditions, that such should be the case. All of these experiments were made in the case of visual impressions, for example, on the living eye, connected by the optic nerve with the brain; and it is manifestly impossible, as has been remarked by Hermann, "to localise this relationship between sensational effect and variation in amount of stimulus, which has been called the psycho-physical law of Fechner." Between the sensational effect and the first contact of the stimulus, there are a series of complicated processes occurring in retina, nerve, and brain, processes undergoing incessant modification by the interchanges between these tissues and the warm circulating blood. In which of these does this relation between stimulus and conscious state occur—in retina, in optic nerve, or in brain? The only method of answering this question, so far as I know, is to examine the effects of stimulation upon these parts separately. It is manifestly next to impossible to do this in the case of the optic nerve and the brain; but by the method pursued by Holmgren, in Sweden, and by Professor Dewar and myself in this country, it can be done, so far as the retina is concerned. In carrying out this method, Professor Dewar and I found that light produced a change in the electrical condition of the retina in an eye removed from the head or kept in normal conditions, and we ascertained that the general phenomena of this change corresponded with our sensational experiences of luminous impressions. We were therefore entitled to assume that the change in the electrical conditions of the retina, produced by the action of light, might be regarded as a phenomenon intimately related to those changes in the brain which result in consciousness of a luminous impression. Consequently we had an opportunity of ascertaining whether or not Fechner's law agreed with the effects of a stimulus of light in altering the electrical condition of the retina, and we found that it did so. The inference, therefore, is that the relation between degree or variation in stimulus and the corresponding sensation of a luminous impression, is a function of the sense organ or retina.

Mode of investigating Sensory Organ itself.

I may here remark that this mode of inquiring into sensory impressions has by no means been exhausted. The subjective method of observing sensational effect under the stimulus of light from revolving disks, by the contrasting of colours, by comparison of auditory sensations produced by tones of different intensity, pitch, and quality, is always open to the charge that the results may not be due to specific histological structure of the sense organ, as is almost invariably assumed, but to structure of the recipient of impressions from the sense organ, namely, the brain. The only way of proving that the effects are due to structural peculiarities of the sense organ is to examine the effects of stimuli applied to the sense organ separated from the brain by some method the same or analogous to ours. If in these circumstances the sense organ give results similar to those observed in the phenomena of consciousness, then we may assume that these results are due to specific peculiarities of the sense organ, and not to the brain. If, on the other hand, the results do not agree, then we must look in the brain for the mechanism by which these different results are produced. Thus I have always held, that as there is little or no histological evidence of complexity of structure in the retina capable of accounting for the theory of Thomas Young regarding the perception of colours, or of the facts of colour-blindness, or of the sensibility of different zones of the retina to lights of different colours, we may have to look to the complex structure of the corpora quadrigemina, cerebellum, or some portion of the cerebral hemispheres for an explanation of these facts. It may be objected that such scepticism simply removes the difficulty a little further back, but I think it is better to search for facts than to be contented with an hypothesis.

Conclusion.

Time will not permit me to discuss other researches in this field of inquiry, nor the interesting speculations which have sprung from them, but I think I have said enough to shew the line of advance in this direction.

True it is that apparently the physiological causation of many mental phenomena may be, in its precise nature, inaccessible to direct proof, but it is our duty as physiologists to push legitimate research as far as it will go. I would remark also that such researches are not incompatible with those spiritual ideas, matters of faith and not of science, which are the basis of our most cherished hopes. They demand, however, caution in the scrutiny of facts, and judgment in drawing conclusions from them. More than in any other kind of scientific labour, perhaps, it is of the utmost importance here to keep the mind unbiassed, a task by no means easy. To maintain a calm unprejudiced attitude to inquiries which seem to demand a change of opinion regarding what was supposed to be final, requires an effort which varies in different persons. Some find it comparatively easy to do so, while others succeed only after a severe struggle. Still it is the state of mind which a man true to science ought to aspire to, so that while he will not be blown about by every wind of doctrine, he may be ready to accept what is apparently true when he has had it clearly put before him.

In conclusion, let me observe that it would save not a little heart-burning, and might possibly remove acrimony from various scientific and social controversies, could we only remember that it is not very probable that we, in this nineteenth century, have arrived at the final solution of many problems which have puzzled wise men from the earliest times. Probably we have got nearer the truth, but it is presumptuous to suppose that we have reached the ultimate truth. Many hypotheses much in favour at present may turn out to be inadequate. Still if they serve as stepping-stones to something better, and to more rational conceptions of the mysterious phenomena about us, they will have done good service. In the meantime it is our duty vigorously to prosecute research, in all departments, pushing ahead fearlessly, and with that enthusiasm which is the prime mover in all great deeds, so that we may be able to transmit our department of knowledge to posterity not only less burdened with error, but with many additions of truth.