from individual peculiarities or accidental admixture can be obviated, and the prevailing characteristics of a race or group truly ascertained. It is only in an institution commanding the resources of the nation that such a collection can be formed, and it may therefore be confidently hoped that the trustees of the British Museum will appropriate some portion of the magnificent new building, which has been provided for the accommodation of their natural history collections, to this hitherto neglected branch of the subject.

I have mentioned two of the needs of anthropology in this country—more workers and better collections: there is still a third—that of a society or institution in which anthropologists can meet and discuss their respective views, with a journal in which the results of their investigations can be laid before the public, and a library in which they can find the books and periodicals necessary for their study. All this ought to be provided by the Anthropological Institute of Great Britain and Ireland, which originated in the amalgamation of the old Ethnological and Anthropological Societies. But, as I intimated some time ago, the Institute does not at the present time flourish as it should; its meetings are not so well attended as they might be; the journal is restricted in its powers of illustration and printing by want of funds; the library is quite insufficient for the needs of the student.

This certainly does not arise from any want of good management in the Society itself. Its afairs have been presided over and administered by some of the most eminent and able men the country has produced. Huxley, Lubbock, Busk, Evans, Tylor, and Pitt-Rivers have in succession given their energies to its service, and yet the number of its members is falling away, its usefulness is crippled, and its very existence seems precarious. Some decline to join the Institute, others leave it upon the plea that, being unable from distance or other causes to attend the meetings, they cannot obtain the full return for their subscriptions; others on the ground that the journal does not contain the exact information which they require.

There surely is to be found a sufficient number of persons who are influenced by different considerations, who feel that anthropological science is worth cultivating, and that those who are laboriously and patiently tracing out the complex problems of man's diversity and man's early history are doing a good work, and ought to be encouraged by having the means afforded them of carrying on their investigations and of placing the results of their researches before the world—who feel, moreover, that there ought to be some central body, representing the subject, which may, on occasion, influence opinion or speak authoritatively on matters often of great practical importance to the nation.

There must be many in this great and wealthy country who feel that they are helping a good cause in joining such a society, even if they are not individually receiving what they consider a full equivalent for their small subscription—many who feel satisfaction in helping the cause of knowledge, in helping to remove the opprobrium that the British Anthropological Society alone of those of the world is lacking in vitality, and in helping to prevent this country from falling behind all the nations in the cultivation of a science in which, for the strongest reasons, it might be expected to hold the foremost place. It is a far more grateful task to maintain, extend, and if need be improve, an existing organisation, than to construct a new one. I feel, therefore, no hesitation in urging upon all who take interest in the promotion of the study of anthropology to rally round the Institute, and to support the endeavours of the present excellent president to increase its usefulness.

Department of Anatomy and Physiology

OPENING ADDRESS BY J. BURDON-SANDERSON, M.D., LL.D., F.R.S., PROFESSOR OF PHYSIOLOGY IN UNIVERSITY COLLEGE, LONDON, VICE-PRESIDENT OF THE SECTION

On the Discoveries of the Past Half-Century relating to Animal Motion

THE two great branches of Biology with which we concern ourselves in this section, Animal Morphology and Physiology, are most intimately related to each other. This arises from their having one subject of study—the living animal organism. The difference between them lies in this, that whereas the studies of the anatomist lead him to fix his attention on the organism itself, to us physiologists it, and the organs of which it is made up, serve only as *vestigia*, by means of which we investigate the vital processes of which they are alike the causes and consequences.

To illustrate this I will first ask you to imagine for a moment that you have before you one of those melancholy remainders of what was once an animal—to wit, a rabbit—which one sees exposed in the shops of poulterers. We have no hesitation in recognising that remainder as being in a certain sense a rabbit; but it is a very miserable vestige of what was a few days ago enjoying life in some wood or warren, or more likely on the sand-hills near Ostend. We may call it a rabbit if we like, but it is only a remainder—not the thing itself.

The anatomical preparation which I have in imagination placed before you, although it has lost its inside and its outside, its integument and its viscera, still retains the parts for which the rest existed. The final cause of an animal, whether human or other, is muscular action, because it is by means of its muscles that it maintains its external relations. It is by our muscles exclusively that we act on each other. The articulate sounds by which I am addressing you are but the results of complicated combinations of muscular contractions—and so are the scarcely appreciable changes in your countenances by which I am able to judge how much, or how little, what I am saying interests you.

Consequently the main problems of physiology relate to muscular action, or, as I have called it, animal motion. They may be divided into two—namely (1) in what does muscular action consist—that is, what is the process of which it is the effect or outcome? and (2) how are the motions of our bodies co-ordinated or regulated? It is unnecessary to occupy time in showing that, excluding those higher intellectual processes which, as they leave no traceable marks behind them, are beyond the reach of our methods of investigation, these two questions comprise all others concerning animal motion. I will therefore proceed at once to the first of them—that of the process of muscular contraction.

The years which immediately followed the origin of the British Association exceeded any earlier period of equal length in the number and importance of the new facts in morphology and in physiology which were brought to light; for it was during that period that Johannes Müller, Schwann, Henle, and, in this country, Sharpey, Bowman, and Marshall Hall, accomplished their productive labours. But it was introductory to a much greater epoch. It would give you a true idea of the nature of the great advance which took place about the middle of this century if I were to define it as the epoch of the death of "vitalism." Before that time, even the greatest biologist—*e.g.* J. Müller—recognised that the knowledge they possessed both of vital and physical phenomena was insufficient to refer both to a common measure. The method, therefore, was to study the processes of life in relation to each other only. Since that time it has become fundamental in our science not to regard any vital process as understood at all, unless it can be brought into relation with physical standards, and the methods of physiology have been based exclusively on this principle. Let us inquire for a moment what causes have conduced to the change.

The most efficient cause was the progress which had been made in physics and chemistry, and particularly those investigations which led to the establishment of the doctrine of the Conservation of Energy. In the application of this great principle to physiology, the men to whom we are indebted are, first and foremost, J. R. Mayer, of whom I shall say more immediately; and secondly, to the great physiologists still living and working among us, who were the pupils of J. Müller—viz. Helmholtz, Ludwig, Du Bois-Reymond, and Brücke.

As regards the subject which is first to occupy our attention, that of the *process* of muscular contraction, J. R. Mayer occupies so leading a position that a large proportion of the researches which have been done since the new era, which he had so important a share in establishing, may be rightly considered as the working out of principles enunciated in his treatise[1] on the relation between organic motion and exchange of material. The most important of these were, as expressed in his own words: (1) "That the chemical force contained in the ingested food and in the inhaled oxygen is the source of the motion and heat which are the two products of animal life; and (2) that these products vary in amount with the chemical process which produces them." Whatever may be the claims of Mayer to be regarded as a great discover in physics, there can be no doubt

[1] J. R. Mayer, "Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel: ein Beitrag zur Naturkunde," Heilbronn, 1845.

that as a physiologist he deserves the highest place that we can give him, for at a time when the notion of the correlation of different modes of motion was as yet very unfamiliar to the physicist, he boldly applied it to the phenomena of animal life, and thus re-united physiology with natural philosophy, from which it had been rightly, because unavoidably, severed by the vitalists of an earlier period.

Let me first endeavour shortly to explain how Mayer himself applied the principle just enunciated, and then how it has been developed experimentally since his time.

The fundamental notion is this: the animal body resembles, as regards the work it does and the heat it produces, a steam-engine, in which fuel is continually being used on the one hand, and work is being done and heat produced on the other. The using of fuel is the chemical process, which in the animal body, as in the steam-engine, is a process of oxidation. Heat and work are the useful products, for as, in the higher animals, the body can only work at a constant temperature of about 100° F., heat may be so regarded.

Having previously determined the heat and work severally producible by the combustion of a given weight of carbon, from his own experiments and from those of earlier physicists, Mayer calculated that if the oxidation of carbon is assumed to represent approximately the oxidation process of the body, the quantity of carbon actually burnt in a day is far more than sufficient to account for the day's work, and that of the material expended in the body not more than one-fifth was used in the doing of work, the remaining four-fifths being partly used, partly wasted in heat production.

Having thus shown that the principles of the correlation of process and product hold good, so far as its truth could then be tested, as regards the whole organism, Mayer proceeded to inquire into its applicability to the particular organ whose function it is "to transform chemical difference into mechanical effect"—namely, muscle. Although, he said, a muscle acts under the direction of the will, it does not derive its power of acting from the will any more than a steamboat derives its power of motion from the helmsman. Again (and this was of more importance, as being more directly opposed to the prevalent vitalism), a muscle, like the steamboats use in the doing of work, not the material of its own structure, or mechanism, but the fuel—*i.e.* the nutriment—which it derives directly from the blood which flows through its capillaries. "The muscle is the instrument by which the transformation of force is accomplished, not the material which is itself transformed." This principle he exemplified in several ways, showing that if the muscle of our bodies worked, as was formerly supposed, at the expense of their own substance, their whole material would be used up in a few weeks, and that in the case of the heart, a muscle which works at a much greater rate than any other, it would be expended in as many days—a result which necessarily involved the absurd hypothesis that the muscular fibres of our hearts are so frequently disintegrated and reintegrated that we get new hearts once a week.

On such considerations Mayer founded the prevision, that, as soon as experimental methods should become sufficiently perfect to render it possible to determine with precision the limits of the chemical process either in the whole animal body or in a single muscle during a given period, and to measure the production of heat and the work done during the same period, the result would show a quantitative correlation between them.

If the time at our disposal permitted, I should like to give a short account of the succession of laborious investigations by which these previsions have been verified. Begun by Bidder and Schmidt in 1851,[1] continued by Pettenkofer and Voit,[2] and by the agricultural physiologists[3] with reference to herbivora, they are not yet by any means completed. I must content myself with saying that by these experiments the first and second parts of this great subject—namely, the limits of the chemical process of animal life and its relation to animal motion under different conditions—have been satisfactorily worked out, but that the quantitative relations of heat-production are as yet only insufficiently determined.

Let me sum up in as few words as possible how far what we have now learnt by experiment justifies Mayer's anticipations, and how it falls short of or exceeds them. First of all, we are as certain as of any physical fact that the animal body in doing work does not use its own material—that, as Mayer says, the oil to his lamp of life is food; but in addition to this we know what he was unaware of, that what is used is not only not the living protoplasm itself, but is a kind of material which widely differs from it in chemical properties. In what may be called commercial physiology—*i.e.*, in the literature of trade puffs—one still meets with the assumption that the material basis of muscular motion is nitrogenous; but by many methods of proof it has been shown that the true "Oel in der Flamme des Lebens" is not proteid substance, but sugar, or sugar-producing material. The discovery of this fundamental truth we owe first to Bernard (1850-56), who brought to light the fact that such material plays an important part in the nutrition of every living tissue; secondly, to Voit (1866), who in elaborate experiments on carnivorous animals, during periods of rest and exertion, showed that, in comparing those conditions, no relation whatever shows itself between the quantity of proteid material (flesh) consumed, and the amount of work done; and finally to Frankland, Fick, and his associate Wislicenus, as to the work-yielding value of different constituents of food, and as to the actual expenditure of material in man during severe exertion. The subjects of experiment used by the two last-mentioned physiologists were themselves; the work done was the mountain ascent from Interlaken to the summit of the Faulhorn; the result was to prove that the quantity of material used was proportional to the work done, and that that material was such as to yield water and carbonic acid exclusively.

The investigators to whom I have just referred aimed at proving the correlation of process and product for the whole animal organism. The other mode of inquiry proposed by Mayer, the verification of his principle in respect of the work-doing mechanism—that is to say, in respect of muscle taken separately—has been pursued with equal perseverance during the last twenty years, and with greater success; for in experimenting on a separate organ, which has no other functions excepting those which are in question, it is possible to eliminate uncertainties which are unavoidable when the conditions of the problem are more complicated. Before I attempt to sketch the results of these experiments, I must ask your attention for a moment to the discoveries made since Mayer's epoch, concerning a closely related subject, that of the Process of Respiration.

I wish that I had time to go back to the great discovery of Priestley (1776), that the essential facts in the process of respiration are the giving off of fixed air, as he called it, and the taking in of dephlogisticated air, and to relate to you the beautiful experiments by which he proved it; and then to pass on to Lavoisier (1777), who, on the other side of the Channel, made independently what was substantially the same discovery a little after Priestley, and added others of even greater moment. According to Lavoisier, the chemical process of respiration is a slow combustion which has its seat in the lungs. At the time that Mayer wrote, this doctrine still maintained its ascendency, although the investigations of Magnus (1838) had already proved its fallacy. Mayer himself knew that the blood possessed the property of conveying oxygen from the lungs to the capillaries, and of conveying carbonic acid gas from the capillaries to the lungs, which was sufficient to exclude the doctrine of Lavoisier. Our present knowledge of the subject was attained by two methods—viz., first, the investigation of the properties of the colouring matter of the blood, since called "hæmoglobin," the initial step in which was made by Prof. Stokes in 1862; and secondly, the application of the mercurial air-pump as a means of determining the relations of oxygen and carbonic acid gas to the living blood and tissues. The last is a matter of such importance in relation to our subject that I shall ask your special attention to it. Suppose that I have a barometer of which the tube, instead of being of the ordinary form, is expanded at the top into a large bulb of one or two litres capacity, and that, by means of some suitable contrivance, I am able to introduce, in such a way as to lose no time and to preclude the possibility of contact with air, a fluid ounce of blood from the artery of a living animal into the vacuous space—what would happen? Instantly the quantity of blood would be converted into froth, which would occupy the whole of the large bulb. The colour of the froth would at first be scarlet, but would speedily change to crimson. It would soon subside, and we should then have the cavity which was before vacuous occupied by the blood and its gases—namely, the oxygen, carbonic acid gas, and nitrogen previously contained in it. And if we had the means (which

[1] Bidder and Schmidt, "Die Verdauungssäfte und der Stoffwechsel," Leipzig, 1852.
[2] Pettenkofer and Voit, *Zeitschr. f. Biologie*, passim, 1866–80.
[3] Henneberg and Stohmann, "Beiträge zur Begründung einer rationellen Futterung der Wiederkäuer," Brunswick and Göttingen, 1860–70.

actually exist in the gas-pump) of separating the gaseous mixture from the liquid, and of renewing the vacuum, we should be able to determine (1) the total quantity of gases which the blood yields, and (2), by analysis, the proportion of each gas.

Now, with reference to the blood, by the application of the "blood-pump," as it is called, we have learnt a great many facts relating to the nature of respiration, particularly that the difference of venous from arterial blood depends not on the presence of "effete matter," as used to be thought, but on the less amount of oxygen held by its colouring matter, and that the blood which flows back to the heart from different organs, and at different times, differs in the amount of oxygen and of carbonic acid gas it yields, according to the activity of the chemical processes which have their seat in the living tissues from which it flows.[1] But this is not all that the blood-pump has done for us. By applying it not merely to the blood, but to the tissues, we have learnt that the doctrine of Lavoisier was wrong, not merely as regards the place, but as regards the nature of the essential process in respiration. The fundamental fact which is thus brought to light is this, that although living tissues are constantly and freely supplied with oxygen, and are in fact constantly tearing it from the hæmoglobin which holds it, yet they themselves yield no oxygen to the vacuum. In other words, the oxygen which living protoplasm seizes upon with such energy that the blood which flows by it is compelled to yield it up, becomes so entirely part of the living material itself that it cannot be separated even by the vacuum. It is in this way only that we can understand the seeming paradox that the oxygen, which is conveyed in abundance to every recess of our bodies by the blood-stream, is nowhere to be found. Notwithstanding that no oxidation-product is formed, it becomes latent in every bit of living protoplasm; stored up in quantity proportional to its potential activity—*i.e.* to the work, internal or external, it has to do.

Thus you see that the process of tissue respiration—in other words, the relation of living protoplasm to oxygen—is very different from what Mayer, who localised oxidation in the capillaries, believed it to be. And this difference has a good deal to do with the relation of Process to Product in muscle. Let us now revert to the experiments on this subject which we are to take as exemplification of the truth of Mayer's forecasts.

The living muscle of a frog is placed in a closed chamber, which is vacuous—*i.e.* contains only aqueous vapour. The chamber is so arranged that the muscle can be made to contract as often as necessary. At the end of a certain period it is found that the chamber now contains carbonic acid gas in quantity corresponding to the number of contractions the muscle has performed. The water which it has also given off cannot of course be estimated. Where do these two products come from? The answer is plain. The muscle has been living all the time, for it has been doing work, and (as we shall see immediately) producing heat. What has it been living on? Evidently on stored material. If so, of what nature? If we look for the answer to the muscle, we shall find that it contains both proteid and sugar-producing material, but which is expended in contraction we are not informed. There is, however, a way out of the difficulty. We have seen that the only chemical products which are given off during contraction are carbonic acid gas and water. It is clear, therefore, that the material on which it feeds must be something which yields, when oxidised, these products, and these only. The materials which are stored in muscle are oxygen and sugar, or something resembling it in chemical composition.

And now we come to the last point I have to bring before you in connection with this part of my subject. I have assumed up to this moment that heat is always produced when a muscle does work. Most people will be ready to admit as evidence of this, the familiar fact that we warm ourselves by exertion. This is in reality no proof at all.

The proof is obtained when, a muscle being set to contract, it is observed that at each contraction it becomes warmer. In such an experiment, if the heat capacity of muscle is known, the weight of the particular muscle, and the increase of temperature, we have the quantity of heat produced.

If you determine these data in respect of a series of contractions, arranging the experiments so that the work done in each contraction is measured, and immediately thereupon reconverted into heat, the result gives you the total product of the oxidation process in heat.

[1] Ludwig's first important research on this subject was published in 1862.

If you repeat the same experiment in such a way that the work done in each contraction is not so reconverted, the result is *less* by the quantity of heat corresponding to the work done. The results of these two experiments have been found by Prof. Fick to cover each other very exactly. I have stated them in a table[1] in which we have the realisation as regards a single muscle of the following forecast of Mayer's as regards the whole animal organism. "Convert into heat," he said, "by friction or otherwise, the mechanical product yielded by an animal in a given time, add thereto the heat produced in the body directly during the same period, and you will have the total quantity of heat which corresponds to the chemical processes." We have seen that this is realisable as regards muscle, but it is not even yet within reach of experimental verification as regards the whole animal.

I now proceed abruptly (for the time at our disposal does not admit of our spending it on transitions) to the consideration of the other great question concerning vital motion, namely the question how the actions of the muscles of an animal are so regulated and co-ordinated as to determine the combined movements, whether rhythmical or voluntary, of the whole body.

As every one knows who has read the "Lay Sermons," the nature and meaning of these often unintentional but always adapted motions, which constitute so large a part of our bodily activity, was understood by Descartes early in the seventeenth century. Without saying anything as to his direct influence on his contemporaries and successors, there can be no doubt that the appearance of Descartes was coincident with a great epoch—an epoch of great men and great achievements in the acquirement of man's intellectual mastery over nature. When he interpreted the unconscious closing of the eyelids on the approach of external objects, the acts of coughing, sneezing, and the like as mechanical and reflected processes, he neither knew in what part of the nervous system the mechanisms concerned were situated, nor how they acted.[2] It was not until a hundred years after that Whytt and Hales made the fundamental experiments on beheaded frogs, by which they showed that the involuntary motions which such preparations execute cease when the whole of the spinal cord is destroyed—that if the back part of the cord is destroyed, the motions of the hind limbs, if the fore part, those of the fore limbs cease. It was in 1751 that Dr. Whytt published in Edinburgh his work on the involuntary motions of animals. After this the next great step was made within the recollection of living physiologists: a period to which, as it coincided with the event which we are now commemorating—the origin of the British Association—I will now ask your special attention.

Exactly forty-nine years ago Dr. Marshall Hall communicated to the Zoological Society of London the first account of his experiments on the reflex function of the spinal cord. The facts which he had observed, and the conclusions he drew from them, were entirely new to him, and entirely new to the physiologists to whom his communication was addressed. Nor can there be any reason why the anticipation of his fundamental discovery by Dr. Whytt should be held to diminish his merit as an original

[1] RELATION OF PRODUCT AND PROCESS IN MUSCLE
(Result of one of Fick's experiments)

Mechanical product	6670	grammemillimeters.
Its heat-value	15·6	milligrammeunits.
Heat produced	39·0	,,
Total product reckoned as heat ...	54·6	,,

[2] Descartes' scheme of the central nervous mechanism comprised all the parts which we now regard as essential to "reflex-action." Sensory nerves were represented by threads (filets) which connected all parts of the body to the brain ("Œuvres," par V. Cousin, vol. iv. p. 359); motor nerves by tubes which extended from the brain to the muscles; "motor centres" by "pores" which were arranged on the internal surface of the ventricular cavity of the brain and guarded the entrances to the motor tubes. This cavity was supposed to be kept constantly charged with "animal spirits" furnished to it from the heart by arteries specially destined for the purpose. Any "incitation" of the surface of the body by an external object which affects the organs of sense does so, according to Descartes, by producing a *motion* at the incited part. This is communicated to the pore by the thread and causes it to open, the consequence of which is that the "animal spirit" contained in the ventricular cavity enters the tube and is conveyed by it to the various muscles with which it is connected, so as to produce the appropriate motions. The whole system, although it was placed under the supervision of the "*âme raisonable*" which had its office in the pineal gland, was capable of working independently. As instances of this mechanism Descartes gives the withdrawal of the foot on the approach of hot objects, the actions of swallowing, yawning, coughing, &c. As it is necessary that, in the performance of these complicated motions, the muscles concerned should contract in succession, provision is made for this in the construction of the systems of tubes which represent the motor nerves. The weakness of the scheme lies in the absence of fact basis. Neither threads nor pores nor tubes have any existence.

investigator. In the face of historical fact it is impossible to regard him as the discoverer of the "reflex function of the spinal cord," but we do not the less owe him gratitude for the application he made of the knowledge he had gained by experiments on animals to the study of disease. For no one who is acquainted with the development of the branch of practical medicine which relates to the diseases of the central nervous system will hesitate in attributing the rapid progress which has been made in the diagnosis and treatment of these diseases, to the impulse given by Dr. Marshall Hall to the study of nervous pathology.

In the mind of Dr. Marshall Hall the word reflex had a very restricted meaning. The term "excito-motary function," which he also used, stood in his mind for a group of phenomena of which it was the sole characteristic that a sensory impression produced a motor response. During the thirty years which have elapsed since his death, the development of meaning of the word reflex has been comparable to that of a plant from a seed. The original conception of reflex action has undergone, not only expansion, but also modification, so that in its wider sense it may be regarded as the empirical development of the philosophical views of the animal mechanism promulgated by Descartes. Not that the work of the past thirty years by which the physiology of the nervous system has been constituted can be attributed for a moment to the direct influence of Descartes. The real epoch-maker here was Johannes Müller. There can be no doubt that Descartes' physiological speculations were well known to him, and that his large acquaintance with the thought and work of his predecessors conduced, with his own powers of observation, to make him the great man that he was; but to imagine that his ideas of the mechanism of the nervous system were inspired, or the investigations by which, contemporaneously with Dr. Marshall Hall, he demonstrated the fundamental facts of reflex action, were suggested by the animal automatism of Descartes, seems to me wholly improbable.

I propose, by way of conclusion, to attempt to illustrate the nature of reflex action in the larger sense, or, as I should prefer to call it, the Automatic Action of Centres, by a single example —that of the nervous mechanism by which the circulation is regulated.

The same year that J. R. Mayer published his memorable essay, it was discovered by E. H. Weber that, in the vagus nerve, which springs from the medulla oblongata and proceeds therefrom to the heart, there exist channels of influence by which the medulla acts on that wonderful muscular mechanism. Almost at the same time with this, a series of discoveries [1] were made relating to the circulation, which, taken together, must be regarded as of equal importance with the original discovery of Harvey. First, it was found by Henle that the arterial blood-vessels by which blood is distributed to brain, nerve, muscle, gland, and other organs, are provided with muscular walls like those of the heart itself, by the contraction or dilatation of which the supply is increased or diminished according to the requirements of the particular organ. Secondly, it was discovered simultaneously, but independently, by Brown-Séquard and Augustus Waller, that these arteries are connected by nervous channels of influence with the brain and spinal cord, just as the heart is. Thirdly, it was demonstrated by Bernard that what may be called the heart-managing channels spring from a small spot of grey substance in the medulla oblongata, which we now call the "heart-centre"; and a little later by Schiff, that the artery-regulating channels spring from a similar head-central office, also situated in the medulla oblongata, but higher up, and from subordinate centres in the spinal cord.

If I had the whole day at my disposal and your patience were inexhaustible, I might attempt to give an outline of the issues to which these five discoveries have led. As it is, I must limit myself to a brief discussion of their relations to each other, in order that we may learn something from them as to the nature of automatic action.

Sir Isaac Newton, who, although he knew nothing about the structure of nerves, made some shrewd forecasts about their action, attributed to those which are connected with muscles an alternative function. He thought that by means of motor nerves the brain could determine either relaxation or contraction of muscles. Now as regards ordinary muscles, we know that this is not the case. We can will only the shortening of a muscle, not its lengthening. When Brown-Séquard discovered the function of the motor nerves of the blood-vessels, he assumed that the same limitation was applicable to it as to that of muscular nerves in general. It was soon found, however, that this assumption was not true in all cases—that there were certain instances in which, when the vascular nerves were interfered with, dilatation of the blood-vessels, consequent on relaxation of their muscles, took place; and that, in fact, the nervous mechanism by which the circulation is regulated is a highly-complicated one, of which the best that we can say is that it is perfectly adapted to its purpose. For while every organ is supplied with muscular arteries, and every artery with vascular nerves, the influence which these transmit is here relaxing, there constricting, according (1) to the function which the organ is called upon to discharge; and (2) the degree of its activity at the time. At the same time the whole mechanism is controlled by one and the same central office, the locality of which we can determine with exactitude by experiment on the living animal, notwithstanding that its structure affords no indication whatever of its fitness for the function it is destined to fulfil. To judge of the complicated nature of this function we need only consider that in no single organ of the body is the supply of blood required always the same. The brain is during one hour hard at work, during the next hour asleep; the muscles are at one moment in severe exercise, the next in complete repose; the liver, which before a meal is inactive, during the process of digestion is turgid with blood, and busily engaged in the chemical work which belongs to it. For all these vicissitudes the tract of grey substance which we call the *vascular centre* has to provide. Like a skilful steward of the animal household, it has, so to speak, to exercise perfect and unfailing foresight, in order that the nutritive material which serves as the oil of life for the maintenance of each vital process, may not be wanting. The fact that this wonderful function is localised in a particular bit of grey substance is what is meant by the expression "automatic action of a centre."

But up to this point we have looked at the subject from one side only.

No state ever existed of which the administration was exclusively executive—no government which was, if I may be excused the expression, absolutely absolute. If in the animal organism we impose on a centre the responsibility of governing a particular mechanism or process, independently of direction from above, we must give that centre the means of being itself influenced by what is going on in all parts of its area of government. In other words, it is as essential that there should be channels of information passing inwards, as that there should be channels of influence passing outwards. Now what is the nature of these channels of information? Experiment has taught us not merely with reference to the regulation of the circulation, but with reference to all other automatic mechanisms, that they are as various in their adaptation as the outgoing channels of influence. Thus the vascular centre in the medulla oblongata is so cognisant of the chemical condition of the blood which flows through it, that if too much carbonic acid gas is contained in it, the centre acts on information of the fact, so as to increase the velocity of the blood-stream, and so promote the arterialisation of the blood. Still more strikingly is this adaptation seen in the arrangement by which the balance of pressure and resistance in the blood-vessels is regulated. The heart, that wonderful muscular machine by which the circulation is maintained, is connected with the centre, as if by two telegraph wires—one of which is a channel of influence, the other of information. By the latter the engineer who has charge of that machine sends information to headquarters whenever the strain on his machine is excessive, the certain response to which is relaxation of the arteries and diminution of pressure. By the former he is enabled to adapt its rate of working to the work it has to do.

If Dr. Whytt, instead of cutting off the head of his frog, had removed only its brain—*i.e.*, the organ of thought and consciousness—he would have been more astonished than he actually was at the result; for a frog so conditioned exhibits, as regards its bodily movements, as perfect adaptiveness as a normal frog. But very little careful observation is sufficient to show the difference. Being incapable of the simplest mental acts, this true animal automaton has no notion of requiring food or of seeking it, has no motive for moving from the place it happens to

[1] The dates of the discoveries relating to this subject here referred to are as follows:—Muscular Structure of Arteries, Henle, 1841; Function of Cardiac Vagus, E. H. Weber, 1845; Constricting Nerves of Arteries, B. Séquard, 1852, Aug. Waller, 1853; Cardiac Centre, Bernard, 1858; Vascular Centre, Schiff, 1858; Dilating Nerves, Schiff, 1854; Eckhard, 1864; Lovén, 1866. Of the more recent researches by which the further elucidation of the mechanism by which the distribution of blood is adapted to the requirements of each organ, the most important are those of Ludwig and his pupils and of Heidenhain.

occupy, emits no utterance of pleasure or distress. Its life processes continue so long as material remains, and are regulated mechanically.

To understand this all that is necessary is to extend the considerations which have been suggested to us in our very cursory study of the nervous mechanism by which the working of the heart and of arteries is governed, to those of locomotion and voice. Both of these we know, on experimental evidence similar to that which enables us to localise the vascular centre, to be regulated by a centre of the same kind. If the behaviour of the brainless frog is so natural that even the careful and intelligent observer finds it difficult to attribute it to anything less than intelligence, let us ask ourselves whether the chief reason of the difficulty does not lie in this, that the motions in question are habitually performed intelligently and consciously. Regarded as mere mechanisms, those of locomotion are no doubt more complicated than those of respiration or circulation, but the difference is one of degree, not of kind. And if the respiratory movements are so controlled and regulated by the automatic centre which governs them, that they adapt themselves perfectly to the varying requirements of the organism, there is no reason why we should hesitate in attributing to the centres which preside over locomotion powers which are somewhat more extended.

But perhaps the question has already presented itself to your minds, What does all this come to? Admitting that we are able to prove (1) that in the animal body, Product is always proportional to Process, and (2), as I have endeavoured to show you in the second part of my discourse, that Descartes' dream of animal automatism has been realised, what have we learnt thereby? Is it true that the work of the last generation is worth more than that of preceding ones?

If I only desired to convince you that during the last half-century there has been a greater accession of knowledge about the function of the living organism than during the previous one, I might arrange here in a small heap at one end of the table the physiological works of the Hunters, Spallanzani, Fontana, Thomas Young, Benjamin Brodie, Charles Bell, and others, and then proceed to cover the rest of it with the records of original research on physiological subjects since 1831, I should find that, even if I included only genuine work, I should have to heap my table up to the ceiling. But I apprehend this would not give us a true answer to our question. Although, etymologically, Science and Knowledge mean the same thing, their real meaning is different. By science we mean, first of all, that knowledge which enables us to sort the things known according to their true relations. On this ground we call Haller the father of physiology, because, regardless of existing theories, he brought together into a system all that was then known by observation or experiment as to the processes of the living body. But in the "Elementa Physiologiæ" we have rather that out of which science springs than science itself. Science can hardly be said to begin until we have by experiment acquired such a knowledge of the relation between events and their antecedents, between processes and their products, that in our own sphere we are able to forecast the operations of nature, even when they lie beyond the reach of direct observation. I would accordingly claim for physiology a place in the sisterhood of the sciences, not because so large a number of new facts have been brought to light, but because she has in her measure acquired that gift of prevision which has been long enjoyed by the higher branches of natural philosophy. In illustration of this I have endeavoured to show you that every step of the laborious investigations undertaken during the last thirty years as to the process of nutrition, has been inspired by the previsions of J. R. Mayer, and that what we have learnt with so much labour by experiments on animals is but the realisation of conceptions which existed two hundred years ago in the mind of Descartes as to the mechanism of the nervous system. If I wanted another example I might find it in the previsions of Dr. Thomas Young as to the mechanism of the circulation, which for thirty years were utterly disregarded, until, at the epoch to which I have so often adverted, they received their full justification from the experimental investigations of Ludwig.

But perhaps it will occur to some one that if physiology founds her claim to be regarded as a science on her power of anticipating the results of her own experiments, it is unnecessary to make experiments at all. Although this objection has been frequently heard lately from certain persons who call themselves philosophers, it is not very likely to be made seriously here. The answer is, that it is contrary to experience. Although we work in the certainty that every experimental result will come out in accordance with great principles (such as the principle that every plant or animal is both, as regards form and function, the outcome of its past and present conditions, and that in every vital process the same relations obtain between expenditure and product as hold outside of the organism), these principles do little more for us than indicate the direction in which we are to proceed. The history of science teaches us that a general principle is like a ripe seed, which may remain useless and inactive for an indefinite period, until the conditions favourable to its germination come into existence. Thus the conditions for which the theory of animal automatism of Descartes had to wait two centuries, were (1) the acquirement of an adequate knowledge of the structure of the animal organism, and (2) the development of the sciences of physics and chemistry; for at no earlier moment were these sciences competent to furnish either the knowledge or the methods necessary for its experimental realisation; and for a reason precisely similar Young's theory of the circulation was disregarded for thirty years.

I trust that the examples I have placed before you to-day may have been sufficient to show that the investigators who are now working with such earnestness in all parts of the world for the advance of physiology, have before them a definite and well-understood purpose, that purpose being to acquire an exact knowledge of the chemical and physical processes of animal life, and of the self-acting machinery by which they are regulated for the general good of the organism. The more singly and straightforwardly we direct our efforts to these ends, the sooner we shall attain to the still higher purpose—the effectual application of our knowledge for the increase of human happiness.

The Science of Physiology has already afforded her aid to the Art of Medicine in furnishing her with a vast store of knowledge obtained by the experimental investigation of the action of remedies and of the causes of disease. These investigations are now being carried on in all parts of the world with great diligence, so that we may confidently anticipate that during the next generation the progress of pathology will be as rapid as that of physiology has been in the past, and that as time goes on the practice of medicine will gradually come more and more under the influence of scientific knowledge. That this change is already in progress we have abundant evidence. We need make no effort to hasten the process, for we may be quite sure that, as soon as science is competent to dictate, art will be ready to obey.

SECTION F

GEOGRAPHY

OPENING ADDRESS BY SIR JOSEPH D. HOOKER, C.B., K.C.S.I., F.R.S., &c., PRESIDENT OF THE SECTION

On Geographical Distribution

IT has been suggested that a leading feature of the sectional addresses to be delivered on the occasion of this, the fiftieth anniversary of the meetings of the British Association, should be a review of the progress made during the last half century in the branches of knowledge which the sections respectively represent.

It has further been arranged that, at so auspicious an epoch, the sections should, when possible, be presided over by past Presidents of the Association. This has resulted in almost every sectional chair being occupied by a President eminent as a cultivator of the science with which his section will be engaged, though not the one I have the honour of filling, which, from the fact of there being no professed geographer amongst the surviving past Presidents, has been confided to an amateur.

Under these circumstances I should be untrue to myself and to you, if I presumed to address you as one conversant with geography in any extended signification of the word, or if I attempted to deal with that important and attractive branch of it, topographical discovery, which claims more or less exclusively the time and attention of the geographers of this country. It is more fitting for me, and more in keeping with the objects of this Association, that I be allowed to discourse before you on one of the many branches of science the pursuit of which is involved in the higher aims of geographers, and which, as we are informed by an accomplished cultivator of the science, are