managed to poison more than one dog with morphine in his life, and he had tried with a large number. It required a very large dose to be fatal. A dog was extraordinarily insusceptible to the toxic effects of morphine. Asked by Dr. Gaskell how he knew when his animal had had sufficient anaesthetic not to feel any pain, the witness replied, By its irresponsiveness to a severe stimulus—either by a cry, by an expression of pain in that way, or by shrinking away from the stimulus. Asked if movement might not take place, he said when an animal was under anaesthesia movement was always taking place; the mere fact of the respiratory muscles working drew the legs backwards and forwards, and frequently the legs were quivering a little. He very frequently found in animals that there were movements of the legs more or less synchronous with respiratory movements. That was common under anaesthetics if the animals were not strapped down tightly, but it was no sign of pain. Asked if he had a dog chloroformed sufficiently to remove pain, he could tell how far that dose was removed from a dose which would kill, he said when a dog had been under proper chloroform anaesthesia for some few minutes it was often very difficult with the apparatus which they used to give it a lethal dose. When once the system had become tolerant to chloroform, and it was still given in a rational manner with the proper percentage of 1.9 to 2 per cent., it was extremely difficult. It often took half an hour to finish it off. The danger was at the beginning of the administration. If the anaesthetic were given in an excessive dose at the beginning, the dog went under quickly, literally like the snuff of a candle, but when once he got the animal tolerant to it he could let his kennelman or groom give the chloroform, and he could attend to the operation. He thought the danger at the beginning was due to an overdose. He considered that morphine was a narcotic to a dog. He used it largely in his practice for stopping pain. He found no difficulty in the dosage. He had not used chloral with dogs sufficiently to form an opinion. He had not used urethane at all. He sometimes used morphine before administering chloroform in an operation. It was not his general practice. Asked by Dr. Wilson if he would conclude that if the A.C.E. mixture was used for keeping up artificial respiration, through a tracheal tube, for example, in preference to chloroform, there would be more risk of the animal becoming conscious to pain during a prolonged operation, the witness said, Not at all if the anaesthesia was kept up. It required more pumping of the apparatus to keep the animal deeply anaesthetized with A.C.E. than it did with chloroform alone; and in the same way with ether, it required more pumping than with chloroform alone. The stage of anaesthesia with chloroform was longer—that was to say, one could cease pumping chloroform for a longer interval than one could with either of the other two. Asked if his dogs were so completely muzzled that they could not cry out during the operation, the witness said if they were under chloroform they could not cry out. They would have no muzzle on at all. When he was operating there was nothing to prevent their crying. If they could not cry, they could moan. They just had their heads in the mask, and there was nothing to stop the jaws from opening, or they might have a piece of tape round the nose. They always put a piece of tape round the nose when handling a dog as a matter of precaution, because, however fond a dog was of one, if one did something to it that it did not quite understand it might take hold of one. He would use morphine alone for certain minor operations—for example, the operation of removing a very small tumour or removing a small elliptical piece of skin from the eyes, which was very common in certain breeds of dogs with certain diseases. For serious operations he would give either the two or chloroform itself. In reply to Sir William Collins, he said he did not think it was an easy matter to give a lethal dose of morphine to a dog. Some dogs would become perfectly comatose with a grain, with a small amount. Frequently half a grain would produce a stupor in one animal, when it would take a full grain or a grain and a half to produce it in another animal of the same size. He should say he had administered morphine to dogs as an anaesthetic pretty nearly a hundred times, but not for major operations. Chloroform was a better anaesthetic for major operations. Asked by the chairman if he were going to cut an animal's throat for the purpose of putting in a tube, he would for that preliminary operation, in the case of an ordinary animal patient, use anaesthetics, the witness said, Yes, unless the patient was at the point of death. If he were doing tracheotomy on a hunter that was a roarer, and must be made useful, he would give cocaine. He would not give chloroform, because cocaine answered the purpose.

(To be continued.)

BRITISH ASSOCIATION.

THIS year the British Association for the Advancement of Science held its annual meeting at Dublin. This is the fourth time in the seventy-seven years of its existence that the Association has met in that city. We propose as usual to give a brief account of such part of the proceedings as relates directly or indirectly to medical science. It is interesting to note that the President of the Association is Mr. Francis Darwin, whom, as a Bachelor of Medicine of the University of Cambridge, we may claim as a member of our profession, although he has retired from its practice. Mr. Darwin, who is a son of Charles Darwin, comes of a medical stock, his grandfather having been a practitioner at Shrewsbury, while his great-grandfather, Erasmus Darwin, author of the *Loves of the Plants* and *Zoonomia*, a work in which he anticipated the views of Lamarck on evolution, was a physician at Lichfield. Other members of the family have been members of the medical profession, and Charles Darwin himself studied medicine for a short time at Edinburgh, but gave it up owing to the disgust excited in him by the study of human anatomy, and his horror at the scenes of the operating theatre in pre-anaesthetic days.

PRESIDENT'S ADDRESS.

After some introductory remarks, Mr. DARWIN said he thought it was the first duty of a President to speak on matters to which his own researches had contributed. He began by giving a general idea how the changes going on in the environment acted as stimuli and compelled plants to execute certain movements. In his book *The Power of Movement in Plants*, 1880, p. 571, Charles Darwin wrote:

> It is impossible not to be struck with the resemblance between the foregoing movements of plants and many of the actions performed unconsciously by the lower animals.

The words which he had quoted afforded an example of the way in which science returned to the obvious. There they found revived, in a rational form, the point of view of the child or of the writer of fairy stories. One knew that flowers did not talk or walk; but the fact that plants must be classed with animals as regards their manner of reaction to stimuli had now become almost a commonplace of physiology. And, inasmuch as they themselves were animals, this conception gave them a certain insight into the reaction of plants which they would not otherwise possess. This, he allowed, was a very dangerous tendency, leading to anthropomorphism, one of the seven deadly sins of science. What, he asked, were the essential characters of stimuli and of the reactions which they called forth in living organisms? Pfeffer had stated this in the most objective way. An organism was a machine which could be set going by touching a spring or trigger of some kind; a machine in which energy could be set free by some kind of releasing mechanism. There they had a model of at least some of the features of reaction to stimulation. The energy of the cause was generally out of all proportion to the effect, that was to say, a small stimulus produced a big reaction. The specific character of the result depended on the structure of the machine rather than on the character of the stimulus. The trigger of a gun might be pulled in a variety of different ways without affecting the character of the explosion. Just in the same way a plant might be made to curve by altering its angle to the vertical, by lateral illumination, by chemical agency, and so forth; the curvature was of the same nature in all cases, the release-action differed. For himself he saw no reason why the term *stimulus* should not be used in relation to the action of mechanisms in general; but, by a convention which it was well to respect, *stimulation* was confined to the protoplasmic machinery of living organisms. The want of proportion between the stimulus and the reply, or, as it has been expressed, the unexpectedness of the result of a given stimulus, was a striking feature in the phenomena of reaction. That this should be so need not surprise them. They could, as a rule, only know the stimulus and the response, while the intermediate processes of the mechanism were hidden in the secret life of protoplasm. They might, however, have guessed that big changes would result from small stimuli, since it was clear that the success of an organism in the world must depend partly, at least, on its being highly sensitive to changes in its surroundings. This was the adaptive side of the fundamental fact that living protoplasm was a highly unstable body. It was the present fashion to minimize or deny altogether the importance of natural selection. He did not propose to enter into this subject; he was convinced that the inherent strength of the doctrine would ensure its final victory over the present anti-Darwinian stream of criticism. Hitherto he had implied the existence of a general character in stimulation without actually naming it; he meant the indirectness of the result. Pfeffer (*Physiology*, Engl. edit., p. 11) employed the word *induction*, and held that external stimuli acted by producing internal change, such changes being the link between stimulus and reaction. It might seem, at first sight, that they did not gain much by this supposition; but since these changes might be more or less enduring, they gained at least the conception of *after effect* as a quality of stimulation. What were known as *spontaneous* actions must be considered as due to internal changes of unknown origin. The quality of indirectness was far more characteristic of an organism than of a machine. The reaction of an organism depended on its past history. The organism was a plastic machine profoundly affected in structure by its own action, and the unknown process intervening between stimulus and reaction (on which the indirectness of the response depended) must have the fullest value allowed it as a characteristic of living creatures. H. S. Jennings (*Contributions to the Study of the Behaviour of the Lower Organisms*, Carnegie Institution, 1904, p. 111) held that there must be taken into consideration what he called "physiological state—that was to say, 'the varying internal physiological conditions of the organism, as distinguished from permanent anatomical conditions.'" External stimuli were supposed to act by altering this physiological state—that was to say, the organism was temporarily transformed into what, judged by its reactions, was practically a different creature. Passing on to the consideration of the permanent or morphological changes and the stimuli by which they were produced, Mr. Darwin said these alterations produced by changes in environment had been brought under the rubric of reaction to stimulation, and must be considered as essentially similar to the class of temporary movements of which he had spoken. The very first stage in development might be determined by a purely external stimulus. If they reconsidered what he had called the indirectness of stimulation, they would see that it had a wider bearing than was at first obvious. The "internal condition," or physiological state," was a factor in the regulation of the organism's action, and it was a factor which owed its character to external agencies which might no longer exist. The fact that stimuli were not momentary in effect, but left a trace of themselves on the organism, was in fact the physical basis of the phenomena grouped under memory in its widest sense as indicating that action was regulated by past experience. Semon, in his book *Die Mneme*, had used the word *engram* for the trace or record of a stimulus left on the organism. The fact that in some cases they recognized the chemical or physical character of the internal conditions did not prevent their ascribing a mnemic memory-like character to them, since they remained causal agencies built up by external conditions which had, or might have, ceased to exist. Memory would be none the less memory when they knew something of the chemistry and physics of its neural concomitant. To make his meaning plain as to the existence of a mnemic factor in the life of plants, Mr. Darwin for the moment left the morphological side of life and gave an instance of habitual movement. Sleeping plants were those in which the leaves assumed at night a position markedly different from that shown by day. Thus the leaflets of the scarlet runner (*Phaseolus*) were more or less horizontal by day and sank down at night. This change of position was known to be produced by the alternation of day and night. A sensitive photographic plate behaved differently in light and darkness; and so did a radiometer, which span by day and rested at night. If a sleeping plant were placed in a dark room after it had gone to sleep at night it would be found next day in the light position and would again assume the nocturnal position as evening came on. There was, in fact, what seemed to be a habit built by the alternation of day and night. The plant normally dropped its leaves at the stimulus of darkness and raised them at the stimulus of light. But here one saw the leaves rising and falling in the absence of the accustomed stimulation. Since this change of position was not due to external conditions it must be the result of the internal conditions which habitually accompanied the movement. This was the characteristic *par excellence* of habit—namely, a capacity, acquired by repetition, of reacting to a fraction of the original environment. It might be expressed in simpler language. When a series of actions were compelled to follow each other by applying a series of stimuli, they became organically tied together, or *associated*, and followed each other automatically even when the whole series of stimuli were not acting. Thus in the formation of habit *post hoc* came to be equivalent to *propter hoc*. Action B automatically followed action A because it had repeatedly been compelled to follow it. Jennings (*Behaviour of the Lower Organisms*, 1906, p. 289) had shown that the basis of memory by association existed in so low an organism as the infusorian *Stentor*. When the animal was stimulated by a jet of water containing carmine in suspension, a physiological state A was produced, which, however, did not immediately lead to a visible reaction. As the carmine stimulus was continued or repeated, state B was produced, to which the *Stentor* reacted by bending to one side. After several repetitions of the stimulus, state C was produced, to which the animal responded by reversing its ciliary movement, and C finally passed into D, which resulted in the *Stentor* contracting into its tube. The important thing was that after many repetitions of this treatment the organism "contracts at once as soon as the carmine comes in contact with it." In other words, states B and C were apparently omitted, and A passes directly into D—that was to say, into the state which gave contraction as a reaction. Jennings went on to point out that the operation of this law was seen in the higher organisms, "in the phenomena commonly called memory, association, habit-formation, and learning." In illustration of habit Mr. Darwin cited the instance of a man who went a walk every day and turned back at a given mile-post. This became habitual, so that he reversed his walk automatically when the limit was reached. It was no explanation of the fact that the stimulus which made him start from home included his return. Such explanation did not account for the point at which he turned, which was the result of association. In the same way a man who went to sleep would ultimately wake; but the fact that he woke at four in the morning depended on a habit built up by his being compelled to rise daily at that time. Even those who would deny that anything like association could occur in plants could not deny that in the continuance of the nyctitropic rhythm in constant conditions we had, in plants, something which had the general character of habit—that was to say, a rhythmic action depending on a rhythmic stimulus that had ceased to exist. On the other hand, many would object that even the simplest form of association implied a nervous system. With regard to this it must be remembered that plants had two at least of the qualities characteristic of animals—namely, extreme sensitiveness to certain agencies and the power of transmitting stimuli from one part to another of the plant body. It was true there was no central nervous system, nothing but a complex system of nuclei; but these had some of the qualities of nerve cells, while intercommunicating protoplasmic threads might play the part of nerves. Another objection to his assumption that a simple form of associated action occurred in plants might be that association implied consciousness. It was impossible to know whether or not plants were conscious; but it was consistent with the doctrine of continuity that in all living things there was something psychic, and if one accepted this point of view one must believe that in plants there existed a faint copy of what they knew as consciousness in themselves. It was said that, like other biologists, he tried to pick out what suited his purpose from two opposite schools of thought—the psychological and the physiological. What he claimed was that, as regarded reaction to environment, a plant and a man must be placed in the same great class, in spite of the fact that, as regards complexity of behaviour, the difference between them was enormous. He was told that he had no right to assume the neural series of changes to be the cause of the psychological series, though he was allowed to say that neural changes were the universal concomitants of psychological change. This seemed to him an unsatisfactory position. He found himself obliged to believe the mnemic quality in all living things must depend on the physical changes in protoplasm, and that it was therefore permissible to use these changes as a notation in which the phenomena of habit might be expressed. They had hitherto been considering the mnemic quality of movements; but, as he had attempted to show, morphological changes were reactions to stimulation of the same kind as these temporary changes. It was, indeed, from the morphological reactions of living things that the most striking cases of habit were, in his opinion, to be found. The development of the individual from the germ cell took place by a series of stages of cell division and growth, each stage apparently serving as a stimulus to the next, each unit following its predecessor like the movements linked together in an habitual action performed by an animal. His view was that the rhythm of ontogeny was actually and literally a habit. It undoubtedly had the feature which he had described as pre-eminently characteristic of habit—namely, an automatic quality which was seen in the performance of a series of actions in the absence of the complete series of stimuli to which they (the stages of ontogeny) were originally due.

The resemblance between ontogeny and habit was not merely superficial, but deeply seated. It was with this conclusion in view that he dwelt on the fact that memory had its place in the morphological as well as in the temporary reactions of living things. It could not be denied that the ontogenetic rhythm had the two qualities observable in habit—namely, a certain degree of fixity or automaticity, and also a certain variability. A habit was not irrevocably fixed, but might be altered in various ways. Parts of it might be forgotten, or new links might be added. In ontogeny the fixity was especially observable in the earlier, the variability in the later, stages. Take the case of a man who, from his youth up, had daily repeated a certain form of words. If in middle life an addition was made to the formula, he would find the recently acquired part more liable to vary than the rest. Again, there was the wonderful fact that, as the ovum developed into the perfect organism, it passed through a series of changes which were believed to represent the successive forms through which its ancestors passed in the process of evolution. This was precisely paralleled by their own experience of memory, for it often happened that they could not reproduce the last learned verse of a poem without repeating the earlier part; each verse was suggested by the previous one and acted as a stimulus for the next. The blurred and imperfect character of the ontogenetic version of the phylogenetic series might at least remind them of the tendency to abbreviate by omission what they had learned by heart. In all bisexual organisms the ontogenetic rhythm of the offspring was a combination of the rhythms of its parents. This might or might not be visible in the offspring; thus in the crossing of two varieties the mongrel assumed the character of the prepotent parent. Or the offspring might show a blend of both parental characters. In the case of memory, the introduction of a link from one mental rhythm into another could only occur when the two series were closely similar, and this might remind them of the difficulty of making a cross between distantly related forms. Evolution, in its modern sense, depended on a change in the ontogenetic rhythm. This was obvious, since if the rhythm was absolutely fixed, a species could never give rise to varieties. That being so, they had to ask *in what ways* the ontogenetic rhythm could be altered. An habitual action, for instance, a trick learned by a dog, might be altered by adding new accomplishments; at first the animal would persist in finishing his performance at the old place, but at last the extended trick would be bonded into a rhythm of actions as fixed as was the original simpler performance. Might they not believe that this was what had occurred in evolution? According to the view upheld by Galton and Weismann, onotogeny could only be changed by a fundamental upset of the whole system—namely, by an alteration occurring in its first stage, the germ cell, and this view was now very generally accepted. The same type of change might conceivably occur in memory or habit—that was, the rhythm as a whole might be altered by some cause acting on the nerve centres connected with the earlier links of the series. If they were as ignorant of the growth of human actions as they were of variation, they might have a school of naturalists asserting that all changes in habit originated in the earliest link of the series. But they knew that this was not the case. On the other hand, he fully admitted that the structure of an ovum might in this way be altered, and give rise to a variation which might be the starting-point of a new species. But how could a new species originate according to an epigenetic theory? How could a change in the latter stages of ontogeny produce a permanent alteration in the germ cells? Their answer to this question would depend on their views of the structure of the germ cells. According to the mnemic theory, they had the quality which was found in the highest perfection in nerve cells, but was at the same time a character of all living matter—namely, the power of retaining the residual effects of former stimuli and of giving forth or reproducing under certain conditions an echo of the original stimulus. Germ cells must, like nerve cells, contain engrams, and these engrams must be (like nerve engrams) bonded together by association, so that they come into action one after another in a certain order automatically—that was to say, in the absence of the original stimuli. This seemed to him the strength of the mnemic theory—namely, that it accounted for the preformed character of germ cells by the building up in them of an organized series of engrams. But if this view had its strength, it had also its weakness. Routine could only be built up by repetition, but each stage in ontogeny occurred only once in a lifetime. Therefore, if ontogeny was a routine, each generation must be mnemically connected with the next. This could only be possible if the germ cells were, as it were, in telegraphic communication with the whole body of the organism, so that, as ontogeny was changed by the addition of new characters, new engrams were added to the germ cell. Thus, in fact, the mnemic theory of development depended on the possibility of what was known as somatic inheritance or the inheritance of acquired characters. Somatic inheritance was popularly interesting in relation to the possible inherited effects of education or of mutilations, or of the effects of use and disuse. It was forgotten that it might be, as he had tried to show, an integral part of all evolutionary development. Every one must allow that if Weismann's theory of inheritance was accepted they would not admit the possibility of somatic inheritance. The racial or phyletic life of all organisms was conceived by Weismann as a series of germ cells whose activity was limited to varying, and whose survival in any generation depended on the production of a successful soma, or body capable of housing, protecting, and feeding the germ cell. It was clear that there must be war to the knife between the theory of Weismann and that of the somatists—to coin a name for those who believed in the inheritance of acquired characters. Mr. Darwin gave a few illustrations of the strength of Weismann's position. Some trick or trivial habit appeared in two successive generations, and the son was said to inherit it from his father. But this was not necessarily a case of somatic inheritance, since, according to Weismann, the germ plasm of both father and son contained the potentiality of the habit in question. If they kept constantly in view Weismann's theory of continuity, the facts which were supposed to prove somatic inheritance ceased to be decisive. It did not seem worth while to go in detail into the evidence by which somatists strove to prove their point, because he did not know of any facts which were really decisive. But it was not necessary to look for special facts and experiments, since, if the mnemic theory of ontogeny was accepted, the development of every organism in the world depended on somatic inheritance. He fully acknowledged the strength of Weismann's position. Nevertheless, it must be remembered that, as Romanes (*An Examination of Weismann*, 1893, pp. 169-70) pointed out, Weismann had greatly strengthened his theory of heredity by giving up the absolute stability and perpetual continuity of germ plasm. Germ plasm was no longer that mysterious entity, immortal and self-contained, which used to suggest a physical soul. It was no longer the aristocrat it was when its only activity was dependent on its protozoan ancestors, when it reigned absolutely aloof from its contemporary subjects. The germ-plasm theory of to-day was liberalized, though it was not so democratic as its brother sovereign pangenesis, who reigned, or used to reign, by an elaborate system of proportional representation. But in spite of the skill and energy devoted to its improvement by its distinguished author, Weismannism failed, in his opinion, to be a satisfactory theory of evolution.

All such theories must account for two things which were parts of a single process, but might logically be considered separately: (1) The fact of ontogeny—namely, that the ovum had the capacity of developing into a certain more or less predetermined form; (2) the fact of heredity—the circumstance that this form was approximately the same as that of the parent. The doctrine of pangenesis accounted for heredity, since the germ cells were imagined as made up of gemmules representing all parts of the adult; but it did not account for ontogeny, because there seemed to him no sufficient reason why the gemmules should become active in a predetermined order unless, indeed, they allowed that they did so by habit, and then the doctrine of pangenesis became a variant of the mnemic theory. The strength of Weismann's theory lay in its explanation of heredity. According to the doctrine of continuity, a fragment of the germ plasm was, as it were, put on one side and saved up to make the germ cell of the new generation, so that the germ cells of two successive generations were made of the same material. This again depended on Weismann's belief that when the ovum divided, the two daughter cells were not identical; that, in fact, the fundamental difference between soma and germ cells began at this point. But that was precisely where many naturalists whose observations were worthy of all respect differed from him. Weismann's theory was therefore threatened at the very foundation. Even if they allowed Weismann's method of providing for the identity between the germ cells of two successive generations, there remained a greater problem—namely, that of ontogeny. They no longer looked at the potentiality of a germ cell as Caliban looked on Setebos, as something essentially incomprehensible ruling the future in an unknown way—"just choosing so." If the modern germ cell was to have a poetic analogue, it must be compared to a Pandora's box of architectonic sprites which were let loose in definite order, each serving as a master builder for a prescribed stage of ontogeny. Weismann's view of the mechanism by which his determinants—the architectonic sprites—come into action in due order was to him difficult to grasp. The orderly distribution of determinants depended primarily on their arrangement in the ids, where they were held together by vital "affinities." They were guided to the cells on which they were to act by differential divisions, in each of which the determinants were sorted into two unequal lots. They then became active, that was to say, they broke up into biophores, partly under the influence of liberating stimuli, and partly by an automatic process. Finally the biophores communicated a "definite vital force" to the appropriate cells (*The Evolution Theory*, English translation, i, 373 et seq.). This might be a description of what happened; but inasmuch as it failed to connect the process of ontogeny with physiological processes of which they had definite knowledge, it did not seem a convincing explanation. For himself, he could only say he was not satisfied with Weismann's theory of heredity or of ontogeny. As regards the first, he was inclined to deny the distinction between germ and soma, to insist on the plain facts that the soma was continuous with the germ cell, and that the somatic cells might have the same reproductive qualities as the germ cells (as was proved by the facts of regeneration); that, in fact, the germ cell was merely a specialized somatic cell, and had the essential qualities of the soma. With regard to ontogeny, he had already pointed out that Weismann did not seem to explain its automatic character.

If the mnemic theory were compared with Weismann's views it was clear that it was strong precisely where these were weakest—namely, in giving a coherent theory of the rhythm of development. It also bore comparison with all theories in which the conception of determinants occurred. Why should they make elaborate theories of hypothetical determinants to account for the potentialities lying hidden in the germ cell, and neglect the only determinants of whose existence they had positive knowledge (though they did not know their precise nature)? They knew positively that by making a dog sit up and then giving him a biscuit they built up something in his brain in consequence of which a biscuit becomes the stimulus to the act of sitting. The mnemic theory assumed that the determinants of morphological change were of the same type as the structural alteration wrought in the dog's brain. The mnemic theory agreed with the current view—namely, that the nucleus was the centre of development, or, in Semon's phraseology, that the nucleus contained the engrams in which lay the secret of the ontogenetic rhythm. But the mode of action of the mnemic nucleus was completely different from that of Weismann. He assumed that the nucleus was disintegrated in the course of development by the dropping from it of the determinants which regulated the manner of growth of successive groups of cells. But if the potentiality of the germ nucleus depended on the presence of engrams, if, in fact, its function was comparable to that of a nerve centre, its capacity was not diminished by action; it did not cast out engrams from its substance as Weismann's nucleus was assumed to drop armies of determitants. The engrams were but cut deeper into the records, and more closely bonded one with the next. The nucleus, considered as a machine, did not lose its component parts in the course of use. The fact that the mnemic theory allowed the nucleus to retain its repeating or reproductive or mnemic quality supplied the element of continuity. The germ cell divided and its daughter cells formed the tissues of the embryo, and in this process the original nucleus had given rise to a group of nuclei; these, however, had not lost their engrams, but retained the potentiality of the parent nucleus. They need not, therefore, postulate the special form of continuity which was characteristic of Weismann's theory. They might say, therefore, that the mnemic hypothesis harmonized with the facts of heredity and ontogeny. But the real difficulties remained to be considered, and these, he confessed, were of a terrifying magnitude. The first difficulty was the question how the changes arising in the soma were, so to speak, telegraphed to the germ cells. Weismann held it to be impossible that somatic changes should be telegraphed to the germ cell and be reproduced ontogenetically, a process which he compared to a telegram dispatched in German and arriving in Chinese. According to Semon, what radiates, from the point of stimulation in the soma was the primary excitation set up in the somatic cells; if this was so, the radiating influence would produce the same effect on all the nuclei of the organism. Mr. Darwin said his own point of view was the following: In a plant the ectoplasm might be compared to the sense organ of the cell, and the primary excitation of the cell would be a change in the ectoplasm; but since cells were connected by ectoplasmic threads the primary excitation would spread and produce in other cells a faint copy of the engram impressed on the somatic cells originally stimulated. In all these assumptions they were met by the question to which Weismann had called attention—namely, whether nervous impulses could differ from one another *in quality*? The general opinion of physiologists was undoubtedly that all nervous impulses were identical in quality. There were notable exceptions—for instance, Hering, who strongly supported what might be called the qualitative theory. Mr. Darwin said that he was not competent to form an opinion on the subject, but he was impressed by Hering's argument that the nerve cell and nerve fibre, as parts of one individual (the neuron), must have a common irritability. On the other hand, there was striking evidence, in Langley's experiments on the cross-grafting of efferent nerves, that here at least nerve impulses were interchangeable, and therefore identical in quality. The state of knowledge as regards afferent nerves was, however, more favourable to the speaker's point of view. For the difficulties that met the physiologist—especially as regards the nerves of smell and hearing—were so great that it had been found simpler to assume differences in impulse quality, rather than attempt an explanation of the facts on the other hypothesis. On the whole it might be said that, although the trend of physiological opinion was against the general existence of qualitative differences in nerve impulses, yet the question could not be said to be settled either one way or the other.

Another obvious difficulty was to imagine how within a single cell the engrams or potentialities of a number of actions could be locked up. They could only answer that the nucleus was admittedly very complex in structure. It need not be more complex than Weismann's germ plasm. One conceivable simplification seemed to be in the direction of the pangenes of De Vries. He imagined that these heritage units were relatively small in number, and that they produced complex results by combination, not by each being responsible for a minute fraction of the total result. They might be compared to the letters of the alphabet, which by combination make an infinity of words. Nägeli (*Abstammungslehre*, 1884, p. 73) held a similar view. "To understand heredity," he wrote, "we do not need a special independent symbol for every difference conditioned by space, time, or quality, but a substance which can represent every possible combination of differences by the fitting together of a limited number of elements, and which can be transformed by permutations into other combinations." He applied the idea of a combination of symbols to the telegraphic quality of his idioplasm. He suggested that as the nerves convey the most varied perceptions of external objects to the central nervous system, and there created a coherent picture, so it was not impossible that the idioplasm might convey a combination of its local alterations to other parts of the organism. Another theory of simplified telegraphy between soma and germ cell was given by Rignano. It resembled the theories of Hering, Butler, and Semon in postulating a quality of living things which was the basis both of memory and inheritance. But it differed from them in seeking for a physical explanation or model of what was common to the two. He compared the nucleus to an electric accumulator which in its discharge gave out the same sort of energy that it had received. How far this was an allowable parallel Mr. Darwin was not prepared to say. What interested him was the conclusion that the impulse conveyed to the nucleus of the germ cell was, as far as results were concerned, the external stimulus. Thus, if a somatic cell (A) was induced, by an external stimulus (S) acting on the nucleus, to assume a new manner of development, a disturbance spread through the organism, so that finally the nuclei of the germ cells were altered in a similar manner. When the cellular descendants of the germ cells reached the same stage of ontogeny as that in which the original stimulation occurred, a stimulus came into action equivalent to S as regarded the results it was capable of producing. So that the change originally wrought in cell A by the actual stimulus S was now reproduced by what might be called an inherited stimulus. But when A was originally affected other cells, B, C, D, might have reacted to S by various forms of growth. And, therefore, when during the development of the altered germ cell something equivalent to S came into play, there would be induced, not merely the original change in the development of A, but also the changes originally induced in the growth of B, C, D. Thus, according to Rignano, the germ nucleus releases a number of developmental processes, each of which would, according to Weismann, require a separate determinant. Mr. Darwin went on to say that still another code of communication seemed to him at least conceivable. One of the most obvious characteristics of animal life was the guidance of the organism by certain groups of stimuli, producing either a movement of seeking (positive reaction) or one of avoidance (negative reaction). Taking the latter as being the simplest, it was found that in the lowest, as in the highest, organisms a given reaction followed each one of a number of diverse conditions which had nothing in common, save that they were broadly harmful in character. They withdrew their hands from a heated body, a prick, a corrosive substance, or an electric shock. The interesting point was that it was left to the organism to discover by the method of trial and error the best means of dealing with a sub injurious stimulus. Might they not, therefore, say that the existence of pleasure and pain simplified inheritance? It certainly rendered unnecessary a great deal of detailed inheritance. The innumerable appropriate movements performed by animals were broadly the same as those of their parents, but they were not necessarily inherited in every detail; they were rather the unavoidable outcome of hereditary but unspecialized sensitiveness. It was as though heredity were arranged on a code system instead of by separate signals for every movement of the organism. It might be said that in individual life the penalty of failure was pain, but that the penalty for failure in ontogenetic morphology was death. But it was only because pain was the shadow cast by Death as he approached that it was of value to the organism. Death would be still the penalty of creatures that had not acquired this sensitiveness to the edge of danger. Was it not possible that the sensitiveness to external agencies by which structural ontogeny was undoubtedly guided might have a similar quality, and that morphological variations might also be reactions to the edge of danger? It might be objected that the inheritance of anything so complex as an instinct was difficult to conceive on the mnemic theory. Yet it was impossible to avoid suspecting that at least some instincts originated in individual acquirements, since they were continuous with habits gained in the lifetime of the organism. Thus, the tendency to peck at any small object was undoubtedly inherited; the power of distinguishing suitable from unsuitable objects was gained by experience. It might be said that the engrams concerned in the pecking instinct could not conceivably be transferred from the central nervous system to the nucleus of the germ cells. To that he might answer that that was not more inconceivable than Weismann's assumption that the germ cell chanced to be so altered that the young chicken pecked instinctively. Mr. Darwin asked his hearers to consider another case of what appeared to be an hereditary movement—for instance, the case of a young dog who in fighting bit his own lips. The pain thus produced would induce him to tuck up his lips out of harm's way. This protective movement would become firmly associated with not only the act of fighting but with the remembrance of it, and would show itself in the familiar snarl of the angry dog. This movement was now, he presumed, hereditary in dogs, and was so strongly inherited by themselves—from simian ancestors—that a lifting of the corner of the upper lip was a recognized signal of adverse feeling. Was it really conceivable that the original snarl was due to that unspecialized stimulus they called pain, whereas the inherited snarl was due to fortuitous upsets of the determinants in the germ cell? He was well aware that many other objections might be advanced against the views he advocated. To take a single instance: there were many cases where they would expect somatic inheritance, but where they looked in vain for it. This difficulty, and others equally important, must for the present be passed over. Nor would he say anything more as to the possible means of communication between the soma and the germ cells. To him it seemed conceivable that some such telegraphy was possible. But he would hardly wonder if a majority of his hearers decided that the available evidence in its favour was both weak and fantastic. Nor could he wonder that, apart from the problem of mechanism, the existence of somatic inheritance was denied for want of evidence. But he must once more insist that, according to the mnemic hypothesis, somatic inheritance lay at the root of all evolution. Life was a gigantic experiment which the opposing schools interpreted in opposite ways. His own conviction in favour of somatic inheritance rested primarily on the automatic element in ontogeny. It seemed to him certain that in development they had an actual instance of habit. If this were so, somatic inheritance must be a *vera causa*. Nor did it seem impossible that memory should rule the plasmic link which connected successive generations, since, as he had tried to show, the reactions of living things to their surroundings exhibited in the plainest way the universal presence of a mnemic factor. They might fix their eyes on phylogeny, and regard the living world as a great chain of forms, each of which had learned something of which its predecessors were ignorant; or they might attend rather to ontogeny, where the lessons learned became in part automatic. But they must remember that the distinction between phylogeny and ontogeny was artificial, and that routine and acquisition were blended in life. The great engine of natural selection was taunted nowadays, as it was fifty years ago, with being merely a negative power. He thought the mnemic hypothesis of evolution made the positive value of natural selection more obvious. If evolution was a process of drilling organisms into habits, the elimination of those that could not learn was an integral part of the process, and was no less real because it was carried out by a self-acting system. It was surely a positive gain to the harmony of the universe that the discordant strings should break. But natural selection did more than this; and just as a trainer insisted on his performing dogs accommodating themselves to conditions of increasing complexity, so did natural selection pass on its pupils from one set of conditions to other and more elaborate tests, insisting that they should endlessly repeat what they had learned, and forcing them to learn something new. Natural selection attained in a blind, mechanical way the ends gained by a human breeder; and by an extension of the same metaphor, it might be said to have the power of a trainer—of an automatic master, with endless patience and all time at his disposal.

SECTION OF PHYSIOLOGY.

The address of the President of this Section, Dr. J. S. Haldane, is published in full at page 693.

Proprio-Receptive Reflexes of the Limb.

Professor C. S. Sherrington read a paper on this subject, in which he said there were in muscles a number of receptive organs which regulated the actions of the muscles in which they lay, causing them to contract when the muscles shortened, and to relax when the muscles lengthened. Thus, passive extension of the knee caused reflex contraction of the extensor muscle of the knee, and passive flexion of the knee caused reflex relaxation of the extensor of the knee. These reactions could be shown to be traceable to the afferent nerve of the muscle itself and not of the joints to which the muscle was attached.

"Metabolic Balance Sheet" of Individual Tissues.

The final report of the committee, consisting of Professor Gotch (chairman), Mr. J. Barcroft (secretary), Professor E. H. Starling, and Professor T. G. Brodie, was presented. The work of the committee in connexion with tissue metabolism ranged over an extremely large field, and, bearing this in mind, the committee determined that its first duty was to lay the foundation for future work by the development of sound and fruitful methods, utilizing for the purpose a limited number of tissues. This object had been now more or less obtained, and the committee did not therefore propose to ask for reappointment. It recognized that there was need for further special research as to the metabolism of special tissues, but it considered that this might now be left to individual effort, aided by grants for specific purposes. The following organs had been investigated: The heart, the kidneys, the salivary glands, the pancreas, the intestines, and, to some extent, muscle. In addition to these, a few preliminary experiments had been undertaken on the liver.

Heart.—The amount of oxygen taken up by the heart varied with the activity of the organ. Adrenalin, atropine, and barium chloride increased the oxygen intake of the heart; stimulation of the vagus or administration of pilocarpine, chloroform, or potassium chloride, reduced the quantity of oxygen which the heart required.

Salivary Glands.—The oxygen exchange of the submaxillary gland of the dog, the cat, and the rabbit had been the subject of numerous experiments. The metabolism of these glands was increased by stimulation of the chorda tympani. This increase was three or four fold when the stimulation was accompanied by a flow of saliva; when the gland was atropinized the increased metabolism was much smaller, and amounted in many cases to about 30 per cent. of the total metabolism.

Pancreas.—In the dog intravenous injection of secretion caused a threefold increase in the metabolism of the pancreas, coupled with the flow of pancreatic juice.

Intestines.—A study of the gaseous metabolism of the intestines had furnished positive results. The metabolism was much increased during absorption, whether of water or of peptone (dog).

Kidney.—The kidney had been investigated in the dog and in the frog, the former by the blood gas method, and the latter by perfusion with Nager's solution. The following results had been obtained: Increase in the flow of urine, induced by the injection of salts, caused a threefold increase in the oxygen consumption of the dog's kidney, whether perfused through the renal portal vein or through the renal artery. The metabolism of the kidney was about twice as great when perfused through the renal artery as it was when perfused through the renal portal vein. By whichever path the perfusion fluid was led into the kidney, the metabolism greatly increased when the organ was secreting urine under the influence of diuretics. There was no case of diuresis in either of the animals under observation without greatly increased metabolism.

Skeletal muscle was the only organ the gaseous exchange of which had been seriously studied by previous workers. The committee had verified the coefficient of oxidation as determined by Chauveau and Kaufmann. The production of lactic acid in muscle as the result of incomplete oxidation had been investigated. The production of lactic acid under anaërobic conditions varied with the time and was increased by heat, stimulation, etc.

Comparison of Results obtained from Different Organs.

Some results of general application had been gleaned from the study of the metabolism of individual organs:

1. Increased vascularity does not of itself cause any change in the oxygen consumption. This has been shown in the heart, the pancreas, the salivary glands, and the kidney.
2. Increased vascularity is associated with increased production of carbonic acid in such a way as to suggest the conclusion that some product of the metabolic activity of the organ—probably not CO_2 itself—causes vascular dilatation during increased function activity. This has been shown in the heart and the salivary glands.
3. A comparison of the "coefficients of oxidation" (the quantity of oxygen used up per gram of tissue per minute) varies for different organs when at rest. It is much higher for the glandular organs than for skeletal muscle. The following figures are known:

Name of Tissue.	Animal.	Coefficient of Oxidation.
		C.cm.
Skeletal muscle (Chauveau and Kaufmann)	Horse	0.004
Skeletal muscle (Chauveau and Kaufmann)	Dog	0.0045
Cardiac muscle	Dog	0.01
Cardiac muscle	Cat	0.01
Submaxillary gland	Dog	0.03
Submaxillary gland	Cat	0.02
Submaxillary gland	Rabbit	0.02
Pancreas	Dog	0.03
Intestine	Dog	0.02
Kidney	Dog	0.03

The following calculation would give some approximation to the oxidation taking place in the organs of a dog of 10 kilograms (1) when all the organs were (except the heart) resting; (2) when all were active. The liver is calculated on the basis of other glandular organs—an exterpolation which seemed to be justified in the light of a couple of preliminary experiments:

Organ.	Weight.	Rest.		Activity.	
		Co-efficient.	Quantity of oxygen absorbed	Co-efficient.	Quantity of oxygen absorbed
	Grams.		C.cm.		C.cm.
Skeletal muscle ...	3.235	0.004	12.9	0.08	259
Heart	68	0.03	2.0	0.08	5.4
Salivary glands ...	14	0.03	0.4	0.08	1.1
Pancreas	18	0.04			
Alimentary canal ...	279	0.025	7.0	} 0.08	43.5
(Liver	264	0.03)	7.9		
Kidneys	44	0.035	1.5	0.95	4.2
			31.7		313.2

From the results given above it would appear that the rôle of muscle as the great regulator of heat production is played as much in virtue of the relatively small amount of oxidation which takes place in it when at rest as the relatively great amount during activity.

Climate and Health.

Sir LAUDER BRUNTON, Chairman, presented the third report of the committee appointed to investigate the effect of climate upon health and disease. He pointed out the relation between climate and disease, and contended that the relation between the weather and St. Vitus's dance and infantile palsy had been clearly established, while it was recognized that the rainfall diminished the number of deaths from diarrhoea.

High Altitudes in Relation to Disease.

Dr. MICHAEL C. GRABHAM read a paper entitled, Arrieiro: the Physics of High Altitudes in Relation to Climate and Health. The paper, which was founded on observations from a small plateau 5,300 ft. above the sea, first dealt with the layers or zones of atmospheric strata encountered in the ascent of the mountain Arrieiro, and the unexpected irregularities met with in the dry and moist air composing the 6,000 ft. investigated. The formation of the normal or collar cloud was described as commencing 2,000 ft. above the sea and occupying the next 500 to 800 ft. in altitude. The climate of the lower margin was depressing and very moist, giving a sense of chill out of proportion to the fall of 7° or 8° of temperature involved in the ascent, and attributed by the author to the exhaustion of the ascending breeze, the approach of the humidity to the saturation point, and to the low difference of potentials in the measured electric condition. The collar cloud wasted from the upper surface as constantly as it was supplied from beneath, and a totally different climate was encountered as one emerged from it into the final 2,000 ft. of ascent through overlying dry and often warm breezes, with an accompaniment of a greatly increased electrical tension. The collar cloud was then seen from above, either as a dense white mist or a thin, transparent haze, forming the effective sun screen which was so potent a factor in equalizing the temperature of the underlying districts. The dryness of the upper atmospheric strata was shown to be due to the abstraction of moisture from the N.E. perennial current by the thickly-wooded mountain slopes below on the windward side, and to the presence of dry cosmical currents floating from the east. The author illustrated the steadiness of the prevailing breeze by the foundation and overtone notes of an Aeolian harp which when once developing on exposure were maintained hour after hour without starting into further harmonics, and by the almost unvarying pull upon the strings of his kites which were kept flying night and day for purposes of electrical measurement. The atmospheric moisture at 5,800 ft. normally showed at the temperature of 60° a difference of 5° or 6° between the dry and wet bulbs, and during the prevalence of easterly currents a difference of 18° to 20°, indicating at the temperature of 69° the presence of only 30 per cent. of the total moisture required for saturation. The dry currents, therefore, must be taken into account in applying the general rule that aërial moisture is governed by temperature and pressure. The author then briefly dwelt on the value of relative humidity in relation to evaporation, skin action, and clothing, and emphasized the vital importance of a low absolute humidity in connexion with the absorption of moisture from the air passages and lung cells in certain circumstances, stating that quite four-fifths of the total moisture of the air was to be found below 6,000 metres, and insisting on the study of local conditions as well as general principles in defining the climate of any district. The atmospheric electricity was found to show a difference of potentials—always positive—varying from 80 to 120 volts, except at exposed points, when the tension suddenly increased. The stony outcrops contained stones of magnetite which were found to be strongly polarized. The author's methods of observing by kites and otherwise were described, and he recorded that (St. Elmo) flickerings were seen at night to emanate from rocky peaks as strongly electrified bodies of air passed over them. Dr. Grabham believed aqueous vapour to be essential for such a charge. The paper then touched upon the physiological influence of electrical tension in association with tissue change, secretion, and cerebral and nerve stimulation, and associates the lassitude experienced in cloudy humidity and also in the excessive dryness of the easterly currents mentioned with the low difference of potentials in the atmospheric electric charge which prevails in each of these conditions. The author lastly considered the well-known increase of the red corpuscles of the blood in mountain altitudes, and correlated the experiments of Dr. Frankland and Professor Tyndall on combustion and luminosity with his own elimination of the influence of aqueous vapour in these experiments to show that the union of O and C was not lessened in rarefied air, and that the more perfect combustion obtained was due to greater molecular freedom and a lessened pressure, promptness of action compensating for sparseness of particles. Such an influence could not be overlooked as an exciting stimulus within the human body, for there could be no reason to assign a lesser power of combination to the unrestrained oxygen within the air cells than it had shown in penetrating to the central unburnt gases of the candle flame.

THE WINTER SESSION IN THE MEDICAL SCHOOLS.

THE arrangements in view for the opening of the winter work at the various medical schools will be found in the following paragraphs. It will be seen that the practice introduced a few years ago of commencing work forthwith and without any such ceremonial as an opening address now prevails very widely.

ST. BARTHOLOMEW'S HOSPITAL.—The opening ceremony will be the old students' annual dinner, which, as usual, will be held in the Great Hall of the hospital, on October 1st. The chair will be taken by Mr. Gilbert Barling, of Birmingham.

GUY'S HOSPITAL.—Term will commence without any formal ceremony on October 1st, and a week later—on October 8th—the opening meeting of the Physical Society will be held. On this occasion there will be a dinner in the college at 6.30 p.m., Mr. Cosmo Bonsor, Treasurer of the hospital, in the chair. Later in the evening Sir R. Douglas Powell will deliver an address entitled, Just Procedure of Medicine, Dr. J. F. Goodhart being in the chair.

ST. THOMAS'S HOSPITAL.—There will be no official ceremony, but, as already announced, an old students' dinner will take place at the Savoy Hotel on October 2nd, at 7 for 7.30. The chair will be taken by Mr. T. Wakley.

ST. GEORGE'S HOSPITAL.—Term will commence on October 1st, an address being delivered at 4 p.m. by Dr. Charles Slater, who has chosen as his subject The Laboratory in Medical Education and Practice. The annual dinner will take place at the Whitehall Rooms the same evening, Dr. T. T. Whipham being in the chair. At 2.30 p.m. the same day the St. George's Hospital Club will hold its annual general meeting in the Club Rooms.

THE MIDDLESEX HOSPITAL.—The winter session will commence on October 1st, Mr. A. M. Kellas, B.Sc., Ph.D., delivering an introductory address at 3 p.m. The prizes awarded during the year will subsequently be distributed by Mr. Rudyard Kipling. In the evening the annual dinner of past and present students will take place at the Trocadero, Dr. A. F. Voelcker being in the chair.

THE LONDON HOSPITAL.—There will be an afternoon reception by the staff on October 1st in the library of the medical college from 3 to 5 p.m. The old students' dinner will be held the same evening at the Savoy Hotel, Dr. Rice Oxley being in the chair.

KING'S COLLEGE HOSPITAL.—The commencement of the winter session on October 1st will be marked by the prize distribution for the year. The awards will be handed to the successful competitors by Professor Alexander MacAlister, who will also deliver an address. The old students' dinner will take place the same evening at 7 p.m., at the Waldorf Hotel, Professor M. M. MacHardy presiding.

UNIVERSITY COLLEGE.—The winter session will be inaugurated on October 2nd by an address from Sir Edward Fry. On the same occasion the Dean's report will be read and the prizes and other awards for the past year be distributed. The ceremony will take place in the library of the medical school at 3 p.m. In the same place, at 7.30 p.m., there will be an old students' dinner, at which Dr. A. H. Carter, of Birmingham, will preside.

WESTMINSTER HOSPITAL.—Term will commence on October 1st without prelude. On the following day a dinner of old and present students of the hospital will be held at the Hotel Great Central at 7.30 p.m., Dr. N. W. Bourns in the chair.

CHARING CROSS.—The opening of the winter session on October 1st will be inaugurated by the delivery of the Seventh Biennial Huxley Lecture. The orator is Sir Patrick Manson, who will deal with Recent Advances in Science and their Bearing on Medicine and Surgery.

ST. MARY'S HOSPITAL.—An introductory address will be delivered on October 1st by Sir John Broadbent, in the library