SCIENCE

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THE ADDRESS OF THE PRESIDENT OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE[1]—I.

Before entering on the subject of my address, I may be allowed to refer to the loss which the British Association has sustained in the death of Lord Kelvin. He joined the association in 1847, and has been for more than fifty years a familiar figure at our meetings. This is not the occasion to speak of his work in the world or of what he was to his friends, but rather of his influence on those who were personally unknown to him. It seems to me characteristic of him that something of his vigor and of his personal charm was felt far beyond the circles of his intimate associates, and many men and women who never exchanged a word with Lord Kelvin, and are in outer darkness as to his researches, will miss his genial presence and feel themselves the poorer to-day. By the death of Sir John Evans the association is deprived of another faithful friend. He presided at Toronto in 1897, and since he joined the association in 1861 had been a regular attendant at our meetings. The absence of his cheerful personality and the loss of his wise counsels will be widely felt.

May I be permitted one other digression before I come to my subject? There has not been a botanical president of the British Association since the Norwich meeting forty years ago, when Sir Joseph Hooker was in the chair, and in "eloquent and felicitous words" (to quote my fath-

[1] Dublin, 1908.

er's letter) spoke in defense of the doctrine of evolution. I am sure that every member of this association will be glad to be reminded that Sir Joseph Hooker is, happily, still working at the subject that his lifelong labors have so greatly advanced, and of which he has long been recognized as the honored chief and leader.

You will perhaps expect me to give a retrospect of the progress of evolution during the fifty years that have elapsed since July 1, 1858, when the doctrine of the origin of species by means of natural selection was made known to the world in the words of Mr. Darwin and Mr. Wallace. This would be a gigantic task, for which I am quite unfitted. It seems to me, moreover, that the first duty of your president is to speak on matters to which his own researches have contributed. My work—such as it is—deals with the movements of plants, and it is with this subject that I shall begin. I want to give you a general idea of how the changes going on in the environment act as stimuli and compel plants to execute certain movements. Then I shall show that what is true of those temporary changes of shape we describe as movements is also true of the permanent alterations known as morphological.

I shall insist that, if the study of movement includes the problem of stimulus and reaction, morphological change must be investigated from the same point of view. In fact, that these two departments of inquiry must be classed together, and this, as we shall see, has some important results —namely, that the dim beginnings of habit or unconscious memory that we find in the movements of plants and animals must find a place in morphology; and inasmuch as a striking instance of correlated morphological changes is to be found in the development of the adult from the ovum, I shall take this ontogenetic series and attempt to show you that here also something equivalent to memory or habit reigns.

Many attempts have been made to connect in this way the phenomena of memory and inheritance, and I shall ask you to listen to one more such attempt, even though I am forced to appear as a champion of what some of you consider a lost cause—the doctrine of the inheritance of acquired characters.

MOVEMENT

In his book on "The Power of Movement in Plants" (1880)[2] my father wrote that "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." In the previous year Sachs[3] had in like manner called attention to the essential resemblance between the irritability of plants and animals. I give these statements first because of their simplicity and directness; but it must not be forgotten that before this Pfeffer[4] had begun to lay down the principles of what is now known as *Reizphysiologie,* or the physiology of stimulus, for which he and his pupils have done so much.

The words of Darwin which I have quoted afford an example of the way in which science returns to the obvious. Here we find revived, in a rational form, the point of view of the child or of the writer of fairy stories. We do not go so far as the child; we know that flowers do not talk or walk; but the fact that plants must be classed with animals as regards their manner of reaction to stimuli has now become almost a commonplace of physiology. And inasmuch as we ourselves are animals, this conception gives us

[2] P. 571.

[3] *Arbeiten,* II., 1879, p. 282.

[4] "Osmotische Untersuchungen," 1877, p. 202.

a certain insight into the reactions of plants which we should not otherwise possess. This is, I allow, a very dangerous tendency, leading to anthropomorphism, one of the seven deadly sins of science. Nevertheless, it is one that must be used unless the great mass of knowledge accumulated by psychologists is to be forbidden ground to the physiologist.

Jennings[5] has admirably expressed the point of view from which we ought to deal with the behavior of the simpler organisms. He points out that we must study their movements in a strictly objective manner: that the same point of view must be applied to man, and that any resemblances between the two sets of phenomena are not only an allowable but a necessary aid to research.

What, then, are the essential characters of stimuli and of the reactions which they call forth in living organisms? Pfeffer has stated this in the most objective way. An organism is a machine which can be set going by touching a spring or trigger of some kind; a machine in which energy can be set free by some kind of releasing mechanism. Here we have a model of at least some of the features of reaction to stimulation.

The energy of the cause is generally out of all proportion to the effect, *i. e.*, a small stimulus produces a big reaction. The specific character of the result depends on the structure of the machine rather than on the character of the stimulus. The trigger of a gun may be pulled in a variety of different ways without affecting the character of the explosion. Just in the same way a plant may be made to curve by altering its angle to the vertical, by lateral illumination, by chemical agency, and so forth; the curvature is of the same nature in all cases, the release-action differs. One of those chains of wooden bricks in which each knocks over the next may be set in action by a touch, by throwing a ball, by an erring dog, in short by anything that upsets the equilibrium of brick No. 1; but the really important part of the game, the way in which the wave of falling bricks passes like a prairie fire round a group of Noah's Ark animals, or by a bridge over its own dead body and returns to the starting-point, etc.—these are the result of the magnificent structure of the thing as a whole, and the upset of brick No. 1 seems a small thing in comparison.

For myself I see 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 is well to respect, *stimulation* is 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, is a striking feature in the phenomena of reaction. That this should be so need not surprise us. We can, as a rule, only know the stimulus and the response, while the intermediate processes of the mechanism are hidden in the secret life of protoplasm. We might, however, have guessed that big changes would result from small stimuli, since it is 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 is the adaptive side of the fundamental fact that living protoplasm is a highly unstable body. Here I may say one word about the adaptation as treated in the "Origin of Species." It is the present fashion to minimize or deny altogether the importance of natural selection. I do not propose to enter into this subject; I am convinced that the inherent

[5] "The Behavior of the Lower Organisms," 1904, p. 124.

strength of the doctrine will insure its final victory over the present anti-Darwinian stream of criticism. From the Darwinian point of view it would be a remarkable fact if the reactions of organisms to natural stimuli were not adaptive. That they should be so, as they undoubtedly are, is not surprising. But just now I only call attention to the adaptive character of reactions from a descriptive point of view.

Hitherto I have implied the existence of a general character in stimulation without actually naming it; I mean the indirectness of the result. This is the point of view of Dutrochet, who in 1824 said that the environment suggests but does not directly cause the reaction. It is not easy to make clear in a few words the conception of indirectness. Pfeffer[6] employs the word *induction*, and holds that external stimuli act by producing internal change, such changes being the link between stimulus and reaction. It may seem, at first sight, that we do not gain much by this supposition; but since these changes may be more or less enduring, we gain at least the conception of *after effect* as a quality of stimulation. What are known as *spontaneous* actions must be considered as due to internal changes of unknown origin.

It may be said that in speaking of the "indirectness" of the response to stimuli we are merely expressing in other words the conception of release-action; that the explosion of a machine is an indirect reply to the touch on the trigger. This is doubtless true, but we possibly lose something if we attempt to compress the whole problem into the truism that the organism behaves as it does because it has a certain structure. The quality of indirectness is far more characteristic of an organism than of a machine, and to keep it in mind is more illuminating than a slavish adherence to the analogy of a machine. The reaction of an organism depends on its past history; but, it may be answered, this is also true of a machine the action of which depends on how it was made, and in a less degree on the treatment it has received during use. But in living things this last feature in behavior is far more striking, and in the higher organisms past experience is all-important in deciding the response to stimulus. The organism is 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 depends) must have the fullest value allowed it as a characteristic of living creatures.

For the zoological side of biology a view similar to that of Pfeffer has been clearly stated by Jennings[7] in his admirable studies on the behavior of infusoria, rotifers, etc. He advances strong arguments against the theories of Loeb and others, according to which the stimulus acts directly on the organs of movement; a point of view which was formerly held by botanists, but has since given place to the conception of the stimulation acting on the organism as a whole. Unfortunately for botanists these movements are by the zoologists called *tropisms*, and are thus liable to be confused with the geotropism, heliotropism, etc., of plants: to these movements, which are not considered by botanists to be due to direct action of stimuli, Loeb's assumptions do not seem to be applicable.

Jennings's position is that we must take into consideration what he calls "physiological state, *i. e.*, 'the varying internal physiological conditions of the organism,

[6] "Physiology," English edition, I., p. 11.

[7] H. S. Jennings, "Contributions to the Study of the Behavior of the Lower Organisms," Carnegie Institution, 1904, p. 111.

as distinguished from permanent anatomical conditions.' " Though he does not claim novelty for his view, I am not aware that it has ever been so well stated. External stimuli are supposed to act by altering this physiological state; that is, the organism is temporarily transformed into what, judged by its reactions, is practically a different creature.

This may be illustrated by the behavior of *Stentor,* one of the fixed infusoria.[8] If a fine jet of water is directed against the disc of the creature, it contracts "like a flash" into its tube. In about half a minute it expands again and the cilia resume their activity. Now we cause the current to act again upon the disc. This time the *Stentor* does not contract, which proves that the animal has been in some way changed by the first stimulus. This is a simple example of "physiological state." When the *Stentor* was at rest, before it received the first current of water, it was in state 1, the stimulus changed state 1 into state 2, to which contraction is the reaction. When again stimulated it passed into state 3, which does not produce contraction.

We can not prove that the contraction which occurred when the *Stentor* was first stimulated was due to a change of state. But it is a fair deduction from the result of the whole experiment, for after the original reaction the creature is undoubtedly in a changed state, since it no longer reacts in the same way to a repetition of the original stimulus.

Jennings points out that, as in the case of plants, spontaneous acts are brought about when the physiological state is changed by unknown causes, whereas in other cases we can point to an external agency by which the same result is effected.

[8] Jennings, "Behavior of the Lower Organisms," 1906, p. 170.

MORPHOLOGICAL CHANGES

Let us pass on to the consideration of the permanent or morphological changes and the stimuli by which they are produced, a subject to which, in recent years, many workers have devoted themselves. I need only mention the names of Vöchting, Goebel and Klebs, among botanists, and those of Loeb, Herbst and Driesch among zoologists, to remind you of the type of research to which I refer.

These morphological alterations produced by changes in environment have been brought under the rubric of reaction to stimulation, and must be considered as essentially similar to the class of temporary movements of which I have spoken.

The very first stage in development may be determined by a purely external stimulus. Thus the position of the first cell-wall in the developing spore of *Equisetum* is determined by the direction of incident light.[9] In the same way the direction of light settles the plane of symmetry of *Marchantia* as it develops from the gemma.[10] But the more interesting cases are those where the presence or absence of a stimulus makes an elaborate structural difference in the organism. Thus, as Stahl[11] has shown, beech leaves developed in the deep shade of the middle of the tree are so different in structure from leaves grown in full sunlight that they would unhesitatingly be described as belonging to different species. Another well-known case is the development of the scale-leaves on the rhizome of *Circæa* into the foliage leaves under the action of light.[12]

The power which the experimenter has over the lower plants is shown by Klebs, who kept *Saprolegnia mixta,* a fungus found on dead flies, in uninterrupted veg-

[9] Stahl, *Ber. d. Bot. Ges.*, 1885, p. 334.

[10] Pfeffer, in Sachs's *Arbeiten*, I., p. 92.

[11] *Jenaische Zeitschr.*, 1883, p. 162.

[12] Goebel, in *Bot. Zeitung*, 1880.

etative growth for six years; while by removing a fragment of the plant and cultivating it in other conditions the reproductive organs could at any time be made to appear.[13]

Chlamydomonas media, a unicellular green alga, when grown in a 0.4 per cent. nutrient solution, continues to increase by simple division, but conjugating gametes are formed in a few days if the plant is placed in pure water and kept in bright light.[14] Numberless other cases could be given of the regulation of form in the lower organisms. Thus *Sporodinia* grown on peptone-gelatine produces sporangiferous hypha, but on sugar zygotes are formed. Again, *Protosiphon botryoides,* if grown on damp clay, can most readily be made to produce spores by transference to water either in light or in darkness. But for the same plant cultivated in Knop's solution the end can best be obtained by placing the culture in the dark.[15] Still these instances of the regulation of reproduction are not so interesting from our point of view as some of Klebs's later results.[16] Thus he has shown that the color of the flower of *Campanula trachelium* can be changed from blue to white and back again to blue by varying the conditions under which the plant is cultivated. Again, with *Sempervivum*[17] he has been able to produce striking results —*e. g.*, the formation of apetalous flowers with one instead of two rows of stamens. Diminution in the number of stamens is a common occurrence in his experimental plants, and absolute loss of these organs also occurs. Many other abnormalities were induced, both in the stamens and in other parts of the flowers.

[13] *Willkürliche Entwick.*, p. 27.

[14] Klebs, *Bedingungen*, 1896, p. 430.

[15] *Biol. Centralbl.*, 1904, pp. 451–3.

[16] *Jahrb. f. wiss. Bot.*, XLII., 1906, p. 162.

[17] *Abhandl. Naturforsch. Ges. zu Halle*, XXV., 1906, pp. 31, 34, etc.

There is nothing new in the character of these facts;[18] what has been brought to light (principally by the work of Klebs) is the *degree* to which ontogeny is controllable. We are so much in the habit of thinking of the stable element in ontogeny that the work of Klebs strikes us with something of a shock. Most people would allow that change of form is ultimately referable to changed conditions, but many of us were not prepared to learn the great importance of external stimuli in ontogeny.

Klebs begins by assuming that every species has a definite *specific structure,* which he compares to chemical character. Just as a substance such as sulphur may assume different forms under different treatment, so he assumes that the specific structure of a plant has certain potentialities which may be brought to light by appropriate stimuli. He divides the agencies affecting the structure into external and internal conditions, the external being supposed to act by causing alterations in the internal conditions.

It will be seen that the scheme is broadly the same as that of Pfeffer for the case of the movement and other temporary reactions. The internal conditions of Klebs correspond also to the "physiological state" of Jennings.

From what has gone before, it will be seen that the current conception of stimulus[19] is practically identical, whether we

[18] See the great collection of facts illustrating the "direct and definite action of the external conditions of life" in "Variation of Animals and Plants," II., p. 271.

[19] With regard to the terminology of stimulation, I believe that it would greatly simplify matters if our classification of causal conditions could be based on the relation of the nucleus to the rest of the cell. But our knowledge does not at present allow of more than a tentative statement of such a scheme. It is now widely believed that the nucleus is the bearer of the qualities transmitted from generation to generation, and the regulator of ontogeny. May we not, therefore,

look at the phenomena of movement or those of structure. If this is allowable—and the weight of evidence is strongly in its favor—a conclusion of some interest follows.

If we reconsider what I have called the indirectness of stimulation, we shall see that it has a wider bearing than is at first obvious. The "internal condition" or "physiological state" is a factor in the regulation of the organism's action, and it is a factor which owes its character to external agencies which may no longer exist.

The fact that stimuli are not momentary in effect but leave a trace of themselves on the organism is in fact the physical basis of the phenomena grouped under memory in its widest sense as indicating that action is regulated by past experience. Jennings[20] remarks: "In the higher animals, and especially in man, the essential features in behavior depend very largely on the history of the individual; in other words, upon the present physiological condition of the individual, as determined by the stimuli it has received and the reactions it has performed. But in this respect the higher animals do not differ in principle, but only in degree, from the lower organisms. . . ." I venture to believe that this is true of plants as well as of animals, and that it is further broadly true not only of physiological behavior, but of the changes that are classed as morphological.

Semon in his interesting book, "Die Mneme,"[21] has used the word *Engram* for the trace or record of a stimulus left on the organism. In this sense we may say that the internal conditions of Pfeffer, the physiological states of Jennings and the internal conditions of Klebs are, broadly speaking, *Engrams*. The authors of these theories may perhaps object to this sweeping statement, but I venture to think it is broadly true.

The fact that in some cases we recognize the chemical or physical character of the internal conditions does not by any means prevent our ascribing a *mnemic* memory-like character to them, since they remain causal agencies built up by external conditions which have, or may have, ceased to exist. Memory will be none the less memory when we know something of the chemistry and physics of its neural concomitant.

consider it probable that the nucleus plays in the cell the part of a central nervous system? In plants there is evidence that the ectoplasm is the sensitive region, and, in fact, plays the part of the cell's sense-organ. The change that occurs in the growth of a cell, as a response to stimulus, would on this scheme be a reflex action dependent for its character on the structure of the nucleus. The "indirectness" of stimulation would then depend on the reception by the nucleus of the excitation set up in the ectoplasm, and the secondary excitation reflected from the nucleus, leading to certain changes in the growth of the cell.

If the nucleus be the bearer of the past history of the individual, the scheme here sketched would accord with the adaptive character of normal reactions and would fall into line with what we know of the regulation of actions in the higher organisms. Pfeffer ("Physiology of Plants," Eng. trans., III., p. 10) has briefly discussed the possibility of thus considering the nucleus as a reflex center, and has pointed out difficulties in the way of accepting such a view as universally holding good. Delage ("L'Hérédité," 2d edition, 1903, p. 88) gives a good summary of the evidence which induces him to deny the mastery of the cell by the nucleus. Driesch, however ("Analytische Theorie der organischen Entwicklung," 1894, p. 81), gives reasons for believing that the cytoplasm is the receptive region, while the nucleus is responsible for the reaction, and it is on this that he bases his earlier theory of ontogeny.

[20] P. 124 (1904).

[21] "Die Mneme, als erhaltendes Prinzip im Wechsel des organischen Geschehens," von Richard Semon, 1te Auflage, Leipzig, 1904; 2te Auflage, 1908. It is a pleasure to express my indebtedness to this work, as well as for the suggestions and criticisms which I owe to Professor Simon personally.

HABIT ILLUSTRATED BY MOVEMENT

In order to make my meaning plain as to the existence of a *mnemic* factor in the life of plants, I shall for the moment leave the morphological side of life and give an instance of habitual movement.

Sleeping plants are those in which the leaves assume at night a position markedly different from that shown by day. Thus the leaflets of the scarlet-runner (*Phaseolus*) are more or less horizontal by day and sink down at night. This change of position is known to be produced by the alternation of day and night. But this statement by no means exhausts the interest of the phenomenon. A sensitive photographic plate behaves differently in light and darkness; and so does a radiometer, which spins by day and rests at night.

If a sleeping-plant is placed in a dark room after it has gone to sleep at night, it will be found next morning in the light-position, and will again assume the nocturnal position as evening comes on. We have, in fact, what seems to be a habit built by the alternation of day and night. The plant normally drops its leaves at the stimulus of darkness and raises them at the stimulus of light. But here we see the leaves rising and falling in the absence of the accustomed stimulation. Since this change of position is not due to external conditions it must be the result of the internal conditions which habitually accompany the movement. This is the characteristic *par excellence* of habit—namely, a capacity, acquired by repetition, of reacting to a fraction of the original environment. We may express it in simpler language. When a series of actions is compelled to follow each other by applying a series of stimuli they become organically tied together, or *associated,* and follow each other automatically, even when the whole series of stimuli are not acting. Thus in the formation of habit *post hoc* comes to be equivalent to *propter hoc.* Action B automatically follows action A, because it has repeatedly been compelled to follow it.

This may be compared with Herbert Spencer's[22] description of an imaginary case, that of a simple aquatic animal which contracts its tentacles on their being touched by a fish or a bit of seaweed washed against it. If such a creature is also sensitive to light the circumstances under which contraction takes place will be made up of two stimuli—those of light and of contact—following each other in rapid succession. And, according to the above statement of the essential character of associative habit, it will result that the light-stimulus alone may suffice, and the animal will contract without being touched.

Jennings[23] has shown that the basis of memory by association exists in so low an organism as the infusorian *Stentor.* When the animal is stimulated by a jet of water containing carmine in suspension, a physiological state A is produced, which, however, does not immediately lead to a visible reaction. As the carmine stimulus is continued or repeated, state B is produced, to which the *Stentor* reacts by bending to one side. After several repetitions of the stimulus, state C is produced, to which the animal responds by reversing its ciliary movement, and C finally passes into D, which results in the *Stentor* contracting into its tube. The important thing is that after many repetitions of the above treatment the organism "contracts at once as soon as the carmine comes in contact with it." In other words, states B and C are apparently omitted, and A passes directly into D, *i. e.*, into the state which gives contraction as a reaction. Thus we have in

[22] "Psychology," 2d edition, 1870, Vol. I., p. 435.

[23] "Behavior of the Lower Organisms," 1906, p. 289.

an infusorian a case of short-circuiting precisely like the case which has been quoted from Herbert Spencer as illustrating association. But Jennings's case has the advantage of being based on actual observation. He generalizes the result as the "law of the resolution of physiological states" in the following words: "The resolution of one physiological state into another becomes easier and more rapid after it has taken place a number of times." He goes on to point out that the operation of this law is seen in the higher organisms, "in the phenomena which we commonly call memory, association, habit-formation, and learning."

In spite of this evidence of mnemic power in the simplest of organisms, objections will no doubt be made to the statement that association of engrams can occur in plants.

Pfeffer, whose authority none can question, accounts for the behavior of sleeping plants principally on the more general ground that when any movement occurs in a plant there is a tendency for it to be followed by a reversal—a swing of the physiological pendulum in the other direction. Pfeffer[24] compares it to a released spring which makes several alternate movements before it settles down to equilibrium. But the fact that the return movements occur at the same time-intervals as the stimuli is obviously the striking feature of the case. If the pendulum-like swing always tended to occur naturally in a twelve-hours' rhythm it would be a different matter. But Pfeffer has shown that a rhythm of six hours can equally well be built up. And the experiments of Miss Pertz and myself[25] show that a half-hourly or quarter-hourly rhythm can be produced by alternate geotropic stimulation.

We are indebted to Keeble[26] for an interesting case of apparent habit among the lower animals. *Convoluta roscoffensis,* a minute wormlike creature found on the coast of Brittany, leads a life dependent on the ebb and flow of the sea. When the tide is out the *Convoluta* come to the surface, showing themselves in large green patches. As the rising tide begins to cover them they sink down into safer quarters. The remarkable fact is that when kept in an aquarium, and therefore removed from tidal action, they continue for a short time to perform rhythmic movements in time with the tide.

Let us take a human habit, for instance that of a man who goes a walk every day and turns back at a given mile-post. This becomes habitual, so that he reverses his walk automatically when the limit is reached. It is no explanation of the fact that the stimulus which makes him start from home includes his return—that he has a mental return-ticket. Such explanation does not account for the point at which he turns, which as a matter of fact is the result of association. In the same way a man who goes to sleep will ultimately wake; but the fact that he wakes at four in the morning depends on a habit built up by his being compelled to rise daily at that time. Even those who will deny that anything like association can occur in plants can not deny that in the continuance of the nyctitropic rhythm in constant conditions we have, in plants, something which has general character of habit, *i. e.,* a rhythmic action depending on a rhythmic stimulus that has ceased to exist.

On the other hand, many will object that even the simplest form of association implies a nervous system. With regard to

[24] See Pfeffer, *Abhandl. K. Sächs. Ges.,* Bd. XXX., 1907. It is impossible to do justice to Pfeffer's point of view in the above brief statement.

[25] *Annals of Botany,* 1892 and 1903.

[26] Gamble and Keeble, *Quar. Jour. Micros. Sci.,* XLVII., p. 401.

this objection it must be remembered that plants have 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 the other of the plant body. It is true that there is no central nervous system, nothing but a complex system of nuclei; but these have some of the qualities of nerve cells, while intercommunicating protoplasmic threads may play the part of nerves. Spencer[27] bases the power of association on the fact that every discharge conveyed by a nerve "leaves it in a state for conveying a subsequent like discharge with less resistance." Is it not possible that the same thing may be as true of plants as it apparently is of infusoria? We have seen reasons to suppose that the "internal conditions" or "physiological states" in plants are of the nature of engrams, or residual effects of external stimuli, and such engrams may become associated in the same way.

There is likely to be another objection to my assumption that a simple form of associated action occurs in plants—namely, that association implies consciousness. It is impossible to know whether or not plants are conscious; but it is consistent with the doctrine of continuity that in all living things there is something psychic, and if we accept this point of view we must believe that in plants there exists a faint copy of what we know as consciousness in ourselves.[28]

I am told by psychologists that I must define my point of view. I am accused of occupying that unscientific position known as "sitting on the fence." It is said that, like other biologists, I try to pick out what suits my purpose from two opposite schools of thought—the psychological and the physiological.

[27] "Psychology," 2d edition, Vol. I., p. 615.

[28] See James Ward, "Naturalism and Agnosticism," Vol. I., Lecture X.

What I claim is that, as regards reaction to environment, a plant and a man must be placed in the same great class, in spite of the obvious fact that as regards complexity of behavior the difference between them is enormous. I am not a psychologist, and I am not bound to give an opinion as to how far the occurrence of definite actions in response to stimulus is a physiological and how far a psychological problem. I am told that I have no right to assume the neural series of changes to be the cause of the psychological series, though I am allowed to say that neural changes are the universal concomitants of psychological change. This seems to me, in my ignorance, an unsatisfactory position. I find myself obliged to believe that the mnemic quality in all living things (which is proved to exist by direct experiment) must depend on the physical changes in protoplasm, and that it is, therefore, permissible to use these changes as a notation in which the phenomena of habit may be expressed.

(To be concluded)

Francis Darwin

DOCTORATES CONFERRED BY AMERICAN UNIVERSITIES

The accompanying table gives the number of doctorates of philosophy and science conferred this year and during the preceding ten years by forty-two institutions. In the issue of Science for August 30, 1907, will be found the details for the earlier years. The numbers for the eleven years have been as follows: 236, 224, 239, 255, 224, 270, 289, 325, 326, 327, 366. There has thus been a considerable though irregular increase. Unless the number this year is a chance fluctuation, it represents a gain of 12 per cent. above last year and of 50 per cent. above the figures for six or eight years ago.

Columbia and Chicago gave more degrees, 55 and 54, respectively, than have ever before been granted by any institu-