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CONTENTS

<i>The Address of the President of the British Association for the Advancement of Science:</i> DR. FRANCIS DARWIN	385
<i>The Analyst, the Chemist and the Chemical Engineer:</i> DR. W. D. RICHARDSON	396
<i>Presentation to Professor Goldschmidt</i>	402
<i>Scientific Notes and News</i>	403
<i>University and Educational News</i>	406
<i>Discussion and Correspondence:—</i>	
<i>The Teaching of Mathematics to Students of Engineering:</i> DR. CHARLES S. PALMER. <i>Hummingbird and Hornet:</i> CHARLES W. MEAD	407
<i>Quotations:—</i>	
<i>The New British Patents Act</i>	409
<i>Scientific Books:—</i>	
<i>Lloyd on the Physiology of the Stomata:</i> DR. CARLTON C. CURTIS. <i>The Principles of Direct-current Electrical Engineering:</i> GEO. C. SHAAD	409
<i>Scientific Journals and Articles</i>	411
<i>Lithium in Radioactive Minerals</i>	412
<i>Special Articles:—</i>	
<i>On the Orbitosphenoid in Some Fishes:</i> DR. EDWIN CHAPIN STARKS. <i>An Explanation of the Cause of the Eastward Circulation of Our Atmosphere:</i> DR. J. M. SCHAE- BERLE	413

THE ADDRESS OF THE PRESIDENT OF THE BRITISH ASSOCIATION FOR THE AD- VANCEMENT OF SCIENCE—II.

HABIT ILLUSTRATED BY MORPHOLOGY

WE have hitherto been considering the mnemonic quality of movements; but, as I have attempted to show, morphological changes are reactions to stimulation of the same kind as these temporary changes. It is indeed from the morphological reactions of living things that the most striking cases of habit are, in my opinion, to be found.

The development of the individual from the germ-cell takes 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.

My view is that the rhythm of ontogeny is actually and literally a habit. It undoubtedly has the feature which I have described as preeminently characteristic of habit, viz., an automatic quality which is 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. This is the chief point on which I wish to insist—I mean that the resemblance between ontogeny and habit is not merely superficial, but deeply seated. It was with this conclusion in view that I dwelt, at the risk of being tedious, on the fact that memory has its place in the morphological as well as in the temporary reac-

tions of living things. It can not be denied that the ontogenetic rhythm has the two qualities observable in habit—namely, a certain degree of fixity or automaticity, and also a certain variability. A habit is not irrevocably fixed, but may be altered in various ways. Parts of it may be forgotten or new links may be added to it. In ontogeny the fixity is especially observable in the earlier, the variability in the later, stages. Mr. Darwin has pointed out that “on the view that species are only strongly marked and fixed varieties, we might expect often to find them still continuing to vary in those parts of their structure which have varied within a moderately recent period.” These remarks are in explanation of the “notorious” fact that specific are more variable than generic characters—a fact for which it is “almost superfluous to adduce evidence.”¹ This, again, is what we find in habit: take the case of a man who, from his youth up, has daily repeated a certain form of words. If in middle life an addition is made to the formula, he will find the recently acquired part more liable to vary than the rest.

Again, there is the wonderful fact that, as the ovum develops into the perfect organism, it passes through a series of changes which are believed to represent the successive forms through which its ancestors passed in the process of evolution. This is precisely paralleled by our own experience of memory, for it often happens that we can not reproduce the last learned verse of a poem without repeating the earlier part; each verse is suggested by the previous one and acts as a stimulus for the next. The blurred and imperfect character of the ontogenetic version of the phylogenetic series may at least remind us of the tendency to abbreviate by omission what we have learned by heart.

¹ “Origin of Species,” 6th edition, p. 122.

In all bi-sexual organisms the ontogenetic rhythm of the offspring is a combination of the rhythms of its parents. This may or may not be visible in the offspring; thus in the crossing of two varieties the mongrel assumes the character of the prepotent parent. Or the offspring may show a blend of both parental characters. Semon² uses as a model the two versions of Goethe’s poem—

Ueber allen Gipfeln, ist Ruh, in allen Wäldern,
hörest du, keinen Hauch.

Ueber allen Gipfeln, ist Ruh, in allen Wipfeln,
spürest du, kaum einen Hauch.

One of these terminations will generally be prepotent, probably the one that was heard first or heard most often. But the cause of such prepotency may be as obscure as the corresponding occurrence in the formation of mongrels. We can only say that in some persons the word “allen” releases the word “Wäldern,” while in others it leads up to “Wipfeln.” Again, a mixture of the terminations may occur leading to such a mongrel form as: “in allen Wäldern hörest du kaum einen Hauch.” The same thing is true of music; a man with an imperfect memory easily interpolates in a melody a bar that belongs elsewhere. In the case of memory the introduction of a link from one mental rhythm into another can only occur when the two series are closely similar, and this may remind us of the difficulty of making a cross between distantly related forms.

Enough has been said to show that there is a resemblance between the two rhythms of development and of memory; and that there is at least a *prima facie* case for believing them to be essentially similar. It will be seen that my view is the same as that of Hering, which is generally described as the identification of memory

² “Die Mneme,” 2d edition, pp. 147, 221, 303, 345.

and inheritance.³ Hering says that "between the *me* of to-day and the *me* of yesterday lie night and sleep, abysses of unconsciousness; nor is there any bridge but memory with which to span them." And in the same way he claims that the abyss between two generations is bridged by the unconscious memory that resides in the germ cells. It is also the same as that of Semon and to a great extent as that of Rignano.⁴ I, however, prefer at the moment to limit myself to asserting the identity of ontogeny and habit, or, more generally, to the assertion in Semon's phraseology, that ontogeny is a mnemonic phenomenon.

Evolution, in its modern sense, depends on a change in the ontogenetic rhythm. This is obvious, since if this rhythm is absolutely fixed, a species can never give rise to varieties. This being so, we have to ask *in what ways* the ontogenetic rhythm can be altered. A habitual action, for instance, a trick learned by a dog, may be altered by adding new accomplishments; at first the animal will persist in finishing his performance at the old place, but at last the extended trick will be bonded into a rhythm of actions as fixed as was the original simpler performance. May we not believe that this is what has occurred in evolution?

We know from experiment that a plant may be altered in form by causes acting on it during the progress of development.

³ Everyone who deals with this subject must take his stand on the foundation laid by Hering in his celebrated address given at Vienna in 1870 and reprinted in No. 148 of Ostwald's "Exakt Klassiker." The passage quoted (p. 14) is from Samuel Butler's translation of Hering in "Unconscious Memory," 1880, p. 110. Butler had previously elaborated the view that "we are one person with our ancestors" in his entertaining book "Life and Habit," 1878, and this was written in ignorance of Hering's views.

⁴ "Sur la transmissibilité des caractères acquis," Paris, 1906.

Thus a beech tree may be made to develop different forms of leaves by exposing it to sunshine or to shade. The ontogeny is different in the two cases, and what is of special interest is that there exist shade-loving plants in which a structure similar to that of the shaded beech-leaf is apparently typical of the species, but on this point it is necessary to speak with caution. In the same way Goebel points out that in some orchids the assimilating roots take on a flattened form when exposed to sunlight, but in others this morphological change has become automatic, and occurs even in darkness.⁵

Such cases suggest at least the possibility of varieties arising as changes in or additions to the later stages of ontogeny. This is, briefly given, the epigenetic point of view.

But there is another way of looking at the matter—namely, that upheld by Galton and Weismann. According to this view ontogeny can 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 is now very generally accepted.

The same type of change may conceivably occur in memory or habit, that is, the rhythm as a whole may be altered by some cause acting on the nerve-centers connected with the earlier links of the series. The analogy is not exact, but such an imaginary case is at least of a different type from a change in habit consisting in the addition of a new link or the alteration of one of the latest formed links. If we were as ignorant of the growth of human actions as we are of variation, we might have a school of naturalists asserting that all changes of habit originate in the earliest link of the series. But we know that this is not the case. On the other hand, I

⁵ Goebel's "Organography of Plants," part II, p. 285.

fully admit that the structure of an ovum may in this way be altered, and give rise to a variation which may be the starting-point of a new species.

But how can a new species originate according to an epigenetic theory? How can a change in the later stages of ontogeny produce a permanent alteration in the germ-cells? Our answer to this question will depend on our views of the structure of the germ-cells. According to the mnemic theory they have the quality which is found in the highest perfection in nerve-cells, but is 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. In Semon's phraseology 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, *i. e.*, in the absence of the original stimuli.

This seems to me the strength of the mnemic theory—namely, that it accounts for the preformed character of germ-cells by the building up in them of an organized series of engrams. But if this view has its strength, it has also its weakness. Routine can only be built up by repetition, but each stage in ontogeny occurs only once in a lifetime. Therefore if ontogeny is a routine each generation must be chemically connected with the next. This can only be possible if the germ-cells are, as it were, in telegraphic communication with the whole body of the organism; so that as ontogeny is changed by the addition of new characters, new engrams are added to the germ-cell.

Thus in fact the mnemic theory of development depends on the possibility of what is known as somatic inheritance or

the inheritance of acquired characters. This is obvious to all those familiar with the subject, but to others it may not be so clear. Somatic inheritance is popularly interesting in relation to the possible inherited effects of education, or of mutilations, or of the effects of use and disuse. It is forgotten that it may be, as I have tried to show, an integral part of all evolutionary development.

WEISMANN'S THEORY

Every one must allow that if Weismann's theory of inheritance is accepted we can not admit the possibility of somatic inheritance. This may be made clear to those unfamiliar with the subject by an illustration taken from the economy of an ant's nest or beehive. The queen,⁶ on whom depends the future of the race, is cut off from all active experience of life: she is a mere reproducing machine, housed, fed and protected by the workers. But these, on whom falls the burden of the struggle for life and the experience of the world generally, are sterile, and take no direct share in the reproduction of the species. The queen represents Weismann's germ-plasm, the workers are the body or soma. Now imagine the colony exposed to some injurious change in environment; the salvation of the species will depend on whether or no an improved pattern of worker can be produced. This depends on the occurrence of appropriate variations, so that the queen bee and the drones, on whom this depends, are of central importance. On the other hand any change occurring in the workers, for instance, increased skill due to practise in doing their work or changes in their structure due to external conditions, can not possibly be inherited, since workers are absolutely cut off from the reproduction

⁶ Nor do the drones share the activity of the workers.

of the race. According to Weismann, there is precisely the same bar to the inheritance of somatic change.

The racial or phyletic life of all organisms is conceived by him as a series of germ-cells whose activity is limited to varying, and whose survival in any generation depends on the production of a successful soma or body capable of housing, protecting, and feeding the germ-cell. Most people would *a priori* declare that a community where experience and action are separated must fail. But the bee's nest, which must be allowed to be something more than an illustration of Weismann's theory, proves the contrary.

It is 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 believe in the inheritance of acquired characters. A few illustrations may be given of the strength of Weismann's position. Some trick or trivial habit appears in two successive generations, and the son is said to inherit it from his father. But this is 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 we keep constantly in view Weismann's theory of continuity, the facts which are supposed to prove somatic inheritance cease to be decisive.

Weismann has also shown by means of his hypothesis of "simultaneous stimulation" the unconvincingness of a certain type of experiment. Thus Fischer showed that when chrysalids of *Arctia caja* are subjected to low temperature a certain number of them produce dark-colored insects; and further that these moths mated together yield dark-colored offspring.

¹I borrow this convenient expression from Plate's excellent book, "Ueber die Bedeutung des Darwin'schen Selectionsprincips," 1903, p. 81.

This has been held to prove somatic inheritance, but Weismann points out that it is explicable by the low temperature having an identical effect on the color-determinants existing in the wing-rudiments of the pupa, and on the same determinants occurring in the germ-cells.

It does not seem to me worth while to go in detail into the evidence by which somatists strive to prove their point, because I do not know of any facts which are really decisive. That is to say, that though they are explicable as due to somatic inheritance, they never seem to me absolutely inexplicable on Weismann's hypothesis. But, as already pointed out, it is not necessary to look for special facts and experiments, since if the mnemonic theory of ontogeny is accepted the development of every organism in the world depends on somatic inheritance.

I fully acknowledge the strength of Weismann's position; I acknowledge also most fully that it requires a stronger man than myself to meet that trained and well-tried fighter. Nevertheless, I shall venture on a few remarks. It must be remembered that, as Romanes² pointed out, Weismann has greatly strengthened his theory of heredity by giving up the absolute stability and perpetual continuity of germ-plasm. Germ-plasm is no longer that mysterious entity, immortal and self-contained, which used to suggest a physical soul. It is 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 today is liberalized, though it is not so democratic as its brother sovereign pangenesis, who reigns, or used to reign, by an elaborate system of proportional representation. But in spite of the skill and

²"An Examination of Weismann," 1893, pp. 169, 170.

energy devoted to its improvement by its distinguished author, Weismannism fails, in my opinion, to be a satisfactory theory of evolution.

All such theories must account for two things which are parts of a single process but may logically be considered separately: (1) The fact of ontogeny, namely, that the ovum has the capacity of developing into a certain more or less predetermined form; (2) the fact of heredity—the circumstance that this form is approximately the same as that of the parent.

The doctrine of pangenesis accounts for heredity, since the germ-cells are imagined as made up of gemmules representing all parts of the adult; but it does not account for ontogeny, because there seems to me no sufficient reason why the gemmules should become active in a predetermined order unless, indeed, we allow that they do so by habit, and then the doctrine of pangenesis becomes a variant of the mnemonic theory.

The strength of Weismann's theory lies in its explanation of heredity. According to the doctrine of continuity, a fragment of the germ-plasm is, 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 are made of the same material. This again depends on Weismann's belief that when the ovum divides, the two daughter cells are not identical; that in fact the fundamental difference between soma and germ-cells begins at this point. But this is precisely where many naturalists whose observations are worthy of all respect differ from him. Weismann's theory is therefore threatened at the very foundation.

Even if we allow Weismann's method of providing for the identity between the germ-cell of two successive generations, there remains, as above indicated, a greater problem—namely, that of ontog-

eny. We no longer look 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 is to have a poetic analogue it must be compared to a Pandora's box of architectonic sprites which are 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 is, I assume, satisfactory to many, but I confess that I find it difficult to grasp. The orderly distribution of determinants depends primarily on their arrangement in the *ids*, where they are held together by "vital affinities." They are guided to the cells on which they are to act by differential divisions, in each of which the determinants are sorted into two unequal lots. They then become active, *i. e.*, break up into biophores, partly under the influence of liberating stimuli and partly by an automatic process. Finally the biophores communicate a "definite vital force" to the appropriate cells.⁹ This *may* be a description of what happens; but inasmuch as it fails to connect the process of ontogeny with physiological processes of which we have definite knowledge, it does not to me seem a convincing explanation.

For myself I can only say that I am not satisfied with Weismann's theory of heredity or of ontogeny. As regards the first, I incline to deny the distinction between germ and soma, to insist on the plain facts that the soma is continuous with the germ-cell, and that the somatic cells may have the same reproductive qualities as the germ-cells (as is proved by the facts of regeneration); that, in fact, the germ-cell

⁹ "The Evolution Theory," English translation, I., 373 *et seq.*

is merely a specialized somatic cell and has the essential qualities of the soma. With regard to ontogeny, I have already pointed out that Weismann does not seem to explain its automatic character.

THE MNEMIC THEORY

If the mnemonic theory is compared with Weismann's views it is clear that it is strong precisely where these are weakest—namely, in giving a coherent theory of the rhythm of development. It also bears comparison with all theories in which the conception of determinants occurs. Why should we 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 we have positive knowledge (though we do not know their precise nature)? We know positively that by making a dog sit up and then giving him a biscuit we build up something in his brain in consequence of which a biscuit becomes the stimulus to the act of sitting. The mnemonic theory assumes that the determinants of morphological change are of the same type as the structural alteration wrought in the dog's brain.

The mnemonic theory—at any rate that form of it held by Semon and by myself—agrees with the current view, viz., that the nucleus is the center of development, or, in Semon's phraseology, that the nucleus contains the engrams in which lies the secret of the ontogenetic rhythm. But the mode of action of the mnemonic nucleus is completely different from that of Weismann. He assumes that the nucleus is disintegrated in the course of development by the dropping from it of the determinants which regulate the manner of growth of successive groups of cells. But if the potentiality of the germ nucleus depends on the presence of engrams, if, in fact, its function is comparable to that of a nerve-

center, its capacity is not diminished by action; it does not cast out engrams from its substance as Weismann's nucleus is assumed to drop armies of determinants. The engrams are but cut deeper into the records, and more closely bonded one with the next. The nucleus, considered as a machine, does not lose its component parts in the course of use. We shall see later on that the nuclei of the whole body may, on the mnemonic theory, be believed to become alike. The fact that the mnemonic theory allows the nucleus to retain its repeating or reproductive or mnemonic quality supplies the element of continuity. The germ-cell divides and its daughter cells form the tissues of the embryo, and in this process the original nucleus has given rise to a group of nuclei; these, however, have not lost their engrams, but retain the potentiality of the parent nucleus. We need not, therefore, postulate the special form of continuity which is characteristic of Weismann's theory.

We may say, therefore, that the mnemonic hypothesis harmonizes with the facts of heredity and ontogeny. But the real difficulties remain to be considered, and these, I confess, are of a terrifying magnitude.

The first difficulty is the question how the changes arising in the soma are, so to speak, telegraphed to the germ-cells. Hering allows that such communication must at first seem highly mysterious.¹⁰ He then proceeds to show how by the essential unity and yet extreme ramification of the nervous system "all parts of the body are so connected that what happens in one echoes through the rest, so that from the disturbance occurring in any part some notification, faint though it may be, is conveyed to the most distant parts of the body."

A similar explanation is given by Nägeli.

¹⁰ E. Hering in Ostwald's *Klassiker der exakten Wissenschaften*, No. 148, p. 14; see also S. Butler's translation in "Unconscious Memory," p. 119.

He supposes that adaptive, in contradistinction to organic, characters are produced by external causes; and since these characters are hereditary there must be communication between the seat of adaptation and the germ-cells. This telegraphic effect is supposed to be effected by the network of idioplasm which traverses the body, in the case of plants by the intercellular protoplasmic threads.

Semon faces the difficulty boldly. When a new character appears in the body of an organism, in response to changing environment, Semon assumes that a new engram is added to the nuclei in the part affected; and that, further, the disturbance tends to spread to all the nuclei of the body (including those of the germ-cells), and to produce in them the same change. In plants the flow must be conceived as traveling by intercellular plasmic threads, but in animals primarily by nerve-trunks. Thus the reproductive elements must be considered as having in some degree the character of nerve-cells. So that, for instance, if we are to believe that an individual habit may be inherited and appear as an instinct, the repetition of the habit will not merely mean changes in the central nervous system, but also corresponding changes in the germ-cells. These will be, according to Semon, excessively faint in comparison to the nerve-engrams, and can only be made efficient by prolonged action. Semon lays great stress on the slowness of the process of building up efficient engrams in the germ-cells.

Weismann¹¹ speaks of the impossibility of germinal engrams being formed in this way. He objects that nerve-currents can

¹¹ Weismann, "The Evolution Theory," 1904, Vol. II., p. 63; also his "Richard Semon's 'Mneme' und die Vererbung erworbener Eigenschaften," in the *Archiv für Rassen- und Gesellschafts-Biologie*, 1906. Semon has replied in the same journal for 1907.

only differ from each other in intensity, and therefore there can be no communication of potentialities to the germ-cell. He holds it to be impossible that somatic changes should be telegraphed to the germ-cell and be reproduced ontogenetically—a process which he compares to a telegram despatched in German and arriving in Chinese. According to Semon¹² what radiates from the point of stimulation in the soma is the primary excitation set up in the somatic cells; if this is so, the radiating influence will produce the same effect on all the nuclei of the organism. My own point of view is the following. In a plant (as already pointed out) the ectoplasm may be compared to the sense-organ of the cell, and the primary excitation of the cell will be a change in the ectoplasm; but since cells are connected by ectoplasm threads the primary excitation will spread and produce in other cells a faint copy of the engram impressed on the somatic cells originally stimulated. But in all these assumptions we are met by the question to which Weismann has called attention—namely, whether nervous impulses can differ from one another in *quality*?¹³ The general opinion of physiologists is undoubtedly to the opposite effect—namely, that all nervous impulses are identical in quality. But there are notable exceptions, for instance, Hering,¹⁴ who strongly supports what may be called the qualitative theory. I am not competent to form an opinion on the subject, but I confess to being impressed by Hering's argument

¹² Semon, "Mneme," ed. I., p. 142, does not, however, consider it proved that the nucleus is necessarily the smallest element in which the whole inheritance resides. He refers especially to the regeneration of sections of *Stentor* which contain mere fragments of the nucleus.

¹³ I use this word in the ordinary sense without reference to what is known as *modality*.

¹⁴ "Zur Theorie der Nerventhätigkeit, Akademische Vortrag," 1898 (Veit, Leipzig).

that the nerve-cell and nerve-fiber, as parts of one individual (the neuron), must have a common irritability. On the other hand, there is striking evidence, in Langley's¹⁵ experiments on the cross-grafting of efferent nerves, that here at least nerve impulses are interchangeable and therefore identical in quality. The state of knowledge as regards afferent nerves is, however, more favorable to my point of view. For the difficulties that meet the physiologist—especially as regards the nerves of smell and hearing—are so great that it has been found simpler to assume differences in impulse-quality, rather than attempt an explanation of the facts on the other hypothesis.¹⁶

On the whole it may be said that, although the trend of physiological opinion is against the general existence of qualitative differences in nerve-impulses, yet the question can not be said to be settled either one way or the other.

Another obvious difficulty is to imagine how within a single cell the engrams or potentialities of a number of actions can be locked up. We can only answer that the nucleus is admittedly very complex in structure. It may be added (but this not an answer) that in this respect it claims no more than its neighbors; it need not be more complex than Weismann's germ-plasm. One conceivable simplification seems to be in the direction of the pangenes of De Vries. He imagines that these heritage-units are relatively small in number, and that they produce complex results by combination, not by each being responsible for a minute fraction of the total result.¹⁷ They may be compared to the letters of the alphabet which by com-

bination make an infinity of words.¹⁸ Nägeli¹⁹ 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 (*loc. cit.*, p. 59) the idea of a combination of symbols to the telegraphic quality of his idioplasm. He suggests that as the nerves convey the most varied perceptions of external objects to the central nervous system, and there create a coherent picture, so it is not impossible that the idioplasm may convey a combination of its local alterations to other parts of the organism.

Another theory of simplified telegraphy between soma and germ-cell is given by Rignano.²⁰ I regret that the space at my command does not permit me to give a full account of his interesting speculation on somatic inheritance. It resembles the theories of Hering, Butler and Semon in postulating a quality of living things, which is the basis both of memory and inheritance. But it differs from them in seeking for a physical explanation or model of what is common to the two. He compares the nucleus to an electric accumulator which in its discharge gives out the same sort of energy that it has received. How far this is an allowable parallel I am not prepared to say, and in what follows I have given Rignano's results in biological terms. What interests me is the conclusion that the impulse conveyed to the nucleus of the germ-cell is, as far as re-

¹⁵ *Proc. Roy. Soc.*, 1904, p. 99. *Journal of Physiology*, XXIII., p. 240, and XXXI., p. 365.

¹⁶ See Nagel, "Handbuch der Physiologie des Menschen," III. (1905), pp. 1-15.

¹⁷ De Vries, "Intracellular Pangenesis," p. 7.

¹⁸ I take this comparison from Lotsy's account of De Vries's theory. Lotsy, "Vorlesungen über Deszendenztheorien," 1906, I., p. 98.

¹⁹ Nägeli's "Abstammungslehre," 1884, p. 73.

²⁰ For what is here given I am partly indebted to Signor Rignano's letters.

sults are concerned, the external stimulus. Thus, if a somatic cell (A) is induced by an external stimulus (S) acting on the nucleus to assume a new manner of development, a disturbance spreads through the organism, so that finally the nuclei of the germ-cells are altered in a similar manner. When the cellular descendants of the germ-cells reach the same stage of ontogeny as that in which the original stimulation occurred, a stimulus comes into action equivalent to S as regards the results it is capable of producing. So that the change originally wrought in cell A by the actual stimulus S is now reproduced by what may be called an inherited stimulus. But when A was originally affected other cells, B, C, D, may have reacted to S by various forms of growth. And therefore when during the development of the altered germ-cell something equivalent to S comes into play, there will be induced, not merely the original change in the development of A, but also the changes which were 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.

If the view here given is accepted, we must take a new view of Weismann's cases of *simultaneous stimulation*, i. e., cases like Fischer's experiments on *Arctia caja*, which he does not allow to be somatic inheritance. If we are right in saying that, the original excitation of the soma is transferred to the germ-cell, and it does not matter whether the stimulus is transferred by "telegraphy," or whether a given cause, e. g., a low temperature, acts simultaneously on soma and germ-cell. In both cases we have a given alteration produced in the nuclei of the soma and the germ-cell. Nägeli used the word *telegraphy* to mean a dynamic form

of transference, but he did not exclude the possibility of the same effect being produced by the movement of chemical substances, and went so far as to suggest that the sieve tubes might convey such stimuli in plants. In any case this point of view²¹ deserves careful consideration.

Still another code of communication seems to me to be at least conceivable. One of the most obvious characteristics of animal life is the guidance of the organism by certain groups of stimuli, producing either a movement of seeking (positive reaction²²) or one of avoidance (negative reaction). Taking the latter as being the simplest, we find that in the lowest as in the highest organisms a given reaction follows each one of a number of diverse conditions which have nothing in common save that they are broadly harmful in character. We withdraw our hands from a heated body, a prick, a corrosive substance, or an electric shock. The interesting point is that it is left to the organism to discover by the method of trial and error the best means of dealing with a sub-injurious stimulus. May we not therefore say that the existence of pleasure and pain simplifies inheritance? It certainly renders unnecessary a great deal of detailed inheritance. The innumerable appropriate movements performed by animals are broadly the same as those of their parents, but they are not necessarily inherited in every detail; they are rather the unavoidable outcome of hereditary but unspecialized sensitiveness. It is as though heredity were arranged on a code-system instead of by separate signals for every movement of the organism.

It may be said that in individual life the penalty of failure is pain, but that the

²¹ See Semon, *Archiv f. Rassen- und Gesellschafts-Biologie*, 1907, p. 39.

²² See Jennings, "Behavior of the Lower Organisms."

penalty for failure in ontogenetic morphology is death. But it is only because pain is the shadow cast by Death as he approaches that it is of value to the organism. Death would be still the penalty of creatures that had not acquired this sensitiveness to the edge of danger. Is it not possible that the sensitiveness to external agencies by which structural ontogeny is undoubtedly guided may have a similar quality, and the morphological variations may also be reactions to the edge of danger. But this is a point of view I can not now enter upon.

It may be objected that the inheritance of anything so complex as an instinct is difficult to conceive on the mnemic theory. Yet it is impossible to avoid suspecting that at least some instincts originate in individual acquirements, since they are continuous with habits gained in the lifetime of the organism. Thus the tendency to peck at any small object is undoubtedly inherited; the power of distinguishing suitable from unsuitable objects is gained by experience. It may be said that the engrams concerned in the pecking instinct can not conceivably be transferred from the central nervous system to the nucleus of the germ-cells. To this I might answer that this is not more inconceivable than Weismann's assumption that the germ-cell chances to be so altered that the young chicken pecks instinctively. Let us consider another case of what appears to be an hereditary movement. Take, for instance, the case of a young dog, who in fighting bites his own lips. The pain thus produced will induce him to tuck up his lips out of harm's way. This protective movement will become firmly associated with, not only the act of fighting, but with the remembrance of it, and will show itself in the familiar snarl of the angry dog. This movement is now, I presume, hereditary in dogs, and is so strongly inherited

by ourselves (from simian ancestors) that a lifting of the corner of the upper lip is a recognized signal of adverse feeling. Is it really conceivable that the original snarl is due to that unspecialized stimulus we call pain, whereas the inherited snarl is due to fortuitous upsets of the determinants in the germ-cell?

I am well aware that many other objections may be advanced against the views I advocate. To take a single instance, there are many cases where we should expect somatic inheritance, but where we look in vain for it. This difficulty, and others equally important, must for the present be passed over. Nor shall I say anything more as to the possible means of communication between the soma and the germ-cells. To me it seems conceivable that some such telegraphy is possible. But I shall hardly wonder if a majority of my hearers decide that the available evidence in its favor is both weak and fantastic. Nor can I wonder that, apart from the problem of mechanism, the existence of somatic inheritance is denied for want of evidence. But I must once more insist that, according to the mnemic hypothesis, somatic inheritance lies at the root of all evolution. Life is a gigantic experiment which the opposing schools interpret in opposite ways. I hope that in this dispute both sides will seek out and welcome decisive results. My own conviction in favor of somatic inheritance rests primarily on the automatic element in ontogeny. It seems to me certain that in development we have an actual instance of habit. If this is so, somatic inheritance must be a *vera causa*. Nor does it seem impossible that memory should rule the plasmic link which connects successive generations—the true miracle of the camel passing through the eye of a needle—since, as I have tried to show, the reactions of living things to their surroundings ex-

hibit in the plainest way the universal presence of a mnemonic factor.

We may fix our eyes on phylogeny and regard the living world as a great chain of forms, each of which has learned something of which its predecessors were ignorant; or we may attend rather to ontogeny, where the lessons learned become in part automatic. But we must remember that the distinction between phylogeny and ontogeny is an artificial one, and that routine and acquisition are blended in life.²³

The great engine of natural selection is taunted nowadays, as it was fifty years ago, with being merely a negative power. I venture to think that the mnemonic hypothesis of evolution makes the positive value of natural selection more obvious. If evolution is a process of drilling organisms into habits, the elimination of those that can not learn is an integral part of the process, and is no less real because it is carried out by a self-acting system. It is surely a positive gain to the harmony of the universe that the discordant strings should break. But natural selection does more than this; and just as a trainer insists on his performing dogs accommodating themselves to conditions of increasing complexity, so does natural selection pass on its pupils from one set of conditions to other and more elaborate tests, insisting that they shall endlessly repeat what they have learned and forcing them to learn something new. Natural selection attains in a blind, mechanical way the ends gained by a human breeder; and by an extension of the same metaphor it may be said to have the power of a trainer—of

an automatic master with endless patience and all time at his disposal.

FRANCIS DARWIN

*THE ANALYST, THE CHEMIST AND THE
CHEMICAL ENGINEER*¹

LET us consider that the terms, the analyst, the chemist and the chemical engineer, represent those members of the chemical profession who devote their time to the practical and industrial aspects of the science, as contrasted with the teachers of chemistry and the workers in abstract research.

The teacher of chemistry and the man of abstract research may be compared to the exciter, the industrial chemist to the dynamo, which supplies whatever power is to be derived from the science of chemistry, to the industrial world.

It is essential that the industrial chemist and the teacher should work closely together, that each should know the aims and needs of the other, if the power of chemical science is to be developed to its full capacity.

There is no more important member of the community to-day than the chemist. I doubt that there ever were more important members of the community even in the more primitive conditions of society than the men who smelted the iron, and tanned the leather, or the women who wrought and burned the earthen pots and dyed the fibers for weaving. And these technologists were the early representatives of the chemical profession, they were the industrial chemists of those early times—chemists to this extent: they knew the properties of certain substances and the chemical transformations in certain directions which these substances were capable of undergoing.

The soldier, the priest and the medicine

¹ Address delivered before the New Haven meeting of the American Chemical Society.

²³ This subject is dealt with in a very interesting manner in Professor James Ward's forthcoming lectures on the "Realm of Ends." Also in his article on "Mechanism and Morals" in the *Hibbert Journal*, October, 1905, p. 92; and in his article on Psychology in the "Encyclopedia Britannica," 1886, Vol. XX., p. 44.