and theory agree in indicating that the universe is melting away into radiation. Our position is that of polar bears on an iceberg that has broken loose from its ice pack surrounding the pole, and is inexorably melting away as the iceberg drifts to warmer latitudes and ultimate extinction."

This is the old hypothesis of the "heat-death." It conflicts with no observed facts, and before the advent of Einstein it was a necessary consequence of the Second Law *provided the universe were treated as a closed system.* Scientists, however, have always objected that such treatment represents an extravagant and illegitimate extrapolation from our very limited mundane experience and modern philosophers and theologians have also objected on the ground that it overthrows the doctrine of Immanence and requires a return to the middle-age assumption of a *Deus ex machina.* Since the advent of Einstein it meets the further difficulty that it injects into modern thermodynamics one single process—the convertibility of mass into radiant energy—which violates the principle of "microscopic reversibility" required by the modern statement of the Second Law.

2. The second possible hypothesis is that of Stern,[3] Tolman[4] and Zwicky,[5] that the foregoing processes are all everywhere reversible. This hypothesis keeps the second law intact, including microscopic reversibility denied by Jeans' assumption, but so far as we can now see it does not avoid the "heat-death," and it is not favored by the evidence herewith presented that the atom-building processes that give rise to the cosmic rays do not seem to be taking place everywhere, *e.g.,* in the stars, but do seem to be taking place solely in the depths of space.

3. The third hypothesis—that herewith presented—is just as radical as 1, but no more so, in denying microscopic reversibility, but it provides an escape sought in vain by both 1 and 2 from the "heat-death." Also it is just as radical as 2, but no more so, in assuming that radiant energy can condense into atoms somewhere, but it is in better accord with the cosmic-ray evidence that the atom-creating processes seem to take place only in interstellar space.

But if the point of view developed in the foregoing is correct what sources of energy are there, then, for man to draw upon during the next billion years of his existence? The answer has already been given but it may be restated thus:

(1) The energy available to him through the *disintegration* of radioactive, or any other, atoms may perhaps be sufficient to keep the corner peanut and popcorn man going, on a few street corners in our larger towns, for a long time yet to come, but that is all.

(2) The energy available to him through the *building-up* of the common elements out of the enormous quantities of hydrogen existing in the waters of the earth would be practically unlimited provided such atom-building processes could be made to take place on the earth. But the indications of the cosmic rays are that these atom-building processes can take place only under the conditions of temperature and pressure existing in interstellar space. Hence there is not even a remote likelihood that man can ever tap this source of energy at all. The hydrogen of the oceans is not likely to ever be converted by man into helium, oxygen, silicon or iron.

(3) The energy supplied to man in the past has been obtained wholly from the sun, and a billion years hence he will still, I think, be supplying all his needs for light, and warmth, and power entirely from the sun. How best to utilize solar energy it is not the purpose of this paper to reveal. That subject is treated in masterly fashion in a paper by Edwin E. Slosson entitled "The Coming of the New Age of Coal," printed in the *Proceedings* of the International Conference on Bituminous Coal held from November 15 to 18, 1926—a paper to which I refer the reader for the next chapter on "Available Energy." The present paper serves merely as an introduction to Dr. Slosson's.

(4) When the matter of the sun has all been stoked into his furnaces and they are gone altogether out another sun will probably have been formed, so that on this earth or on some other earth—it matters not which some billion of years hence—the development of man may still be going on.

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[3] O. Stern, Zeit. f. Elektrochemie, *31*, 448, 1925.

[4] Richard C. Tolman, Proc. Nat. Acad. Sci., *12*, 67, 1926; *14*, 268, 348, 353, 1928.

[5] F. Zwicky, Proc. Nat. Acad. Sci., *14*, 592–597, 1928.

THE RELATION OF PHYSIOLOGY TO OTHER SCIENCES—II

SCIENTIFIC ASPECTS

PHYSIOLOGY takes its place as a science in proportion as its data are accurate and its principles fall into line with those in the other sciences. My great teacher Starling said that science has only one language, that of quantity, and but one argument, that of experiment. The qualitative observations of one generation tend to become quantitative at a later stage of development of a science, and the degree of development of a science can indeed to some extent be judged by the extent to which it falls into a scheme of the unity of science by giving results which are capable of mathematical treatment and of expression in broad general principles.

I recollect that when I first took up the study of chemistry the acquaintance of most chemists with any of the branches of mathematics was so slight that there was on the market a book on arithmetical chemistry. Shortly after that time the progress of physical chemistry on the Continent had become so definite that it came to be considered quite a useful thing for a chemist to acquire some knowledge of the higher mathematics, and the appearance in Britain of a textbook of higher mathematics for students of chemistry and physics rendered great service by introducing the kind of mathematics that was likely to be of value in application to these subjects.

What has happened in physics and chemistry may be reasonably expected to happen in biology so soon as it is able by improvement in the accuracy of its methods, and by progress in the formulation of its problems, to employ mathematics with profit in the manipulation of data and in the construction of those generalizations which are landmarks of progress in all the sciences; indeed we are, I think, now witnessing the commencement of such a phase in the development of our own subject. The many facets of physiological inquiry make it incumbent on all of us to possess some knowledge of one or more related subjects, and I know of no more promising collateral subject which a young physiologist could take up at the present time, as an alternative to chemistry or biology, than the study of mathematics. But those who do take it up should do so for the purposes of utilizing it in their own experimental work, not merely for the purpose of surveying results obtained by others, and still less in order to "lend an air of verisimilitude to an otherwise bald and unconvincing narrative." Mathematics is a most valuable aid to reasoning, and it can be of no real use to physiology except when it leads to clarification of thought both of an author and of his readers. Under any other circumstances its introduction into biological literature is, I think, of extreme danger, because of the superstition, common alike to those who write and those who read, that anything expressed in mathematical form must be accepted as correct without any further question.

Mathematics and mathematical physics have been of considerable use to physiology in increasing the accuracy of its experimental data, and this in two ways. First, by bringing the accurate experimental and intellectual methods of physics to bear on the construction and use of the numerous physical instruments which it employs. It has been said by Professor A. V. Hill that many of the early investigations on muscle were in reality studies of the properties of levers, and it is certain that similar remarks apply to only too many investigations in which the properties of the apparatus used have not been suitably investigated. As illustrations of the value of mathematical-physical study of apparatus one may mention the classical investigations of Frank on hæmadynamical recording apparatus, the fundamental treatment of string galvanometers and similar instruments by Einthoven, the correction of capillary electrometer records by Keith Lucas, and the vast improvements in galvanometer systems effected by Downing and Hill.

Even when the apparatus at the disposal of the physiologist is unexceptionable, however, it is often the fact that, owing to the nature of the subject, results are not susceptible of repetition with the same ease and certainty as are those of chemical or physical experiments. The variability of the results is due in such cases to what are called accidental circumstances, a term which in reality means circumstances over which we have no control, owing either to our ignorance of their nature, or else to our inability to alter them. In those cases where further study provides methods of more fully understanding and therefore more adequately controlling these circumstances, valuable results follow almost at once. For instance, certain of the obscure causes of different behavior under particular conditions are inborn, and can be controlled by the use of inbred strains of animals such as those of the standard inbred white rats; or, again, one may mention the far-reaching results of the observation by Pavlov that the utmost care must be exercised when studying the conditioned reflexes to exclude all stimuli however trivial they may appear except the one under consideration.

Under the most favorable conditions, however, it has up to the present been usual to find a considerable unavoidable margin of variation in the results of many physiological experiments. By regarding these provisionally as "chance" variations, considerable help may be obtained by the application of the theory of errors, based on the theory of probability. In reality this is an empirical method of which Poincaré has said that "everybody firmly believes in it, because mathematicians imagine that it is a fact of observation, and observers that it is a theorem of mathematics," but nevertheless, although it can not, as seems sometimes to be assumed, be used to replace accurate observation, it does enable a result to be brought out which might otherwise be obscured by small variations beyond our control. Research by such statistical methods provides a useful method of investigation, as, for instance, in the study of the toxic or other action of drugs, the data of the œstrus cycle, etc. An elementary deduction which can be drawn from the consideration of these facts is that, where only a few experiments of any kind are performed, important conclusions can not be drawn unless it can be shown that the conditions are so controlled, and the accuracy of the actual observations so high that the sum of the individual "chance" variations must be small. Observation of this precaution would, in my opinion, reduce the bulk of contemporary physiological literature very materially, with a corresponding improvement in its quality.

Lastly, as a means for evolving generalizations out of experimental data, and of bringing these into relation with the generalizations of other branches of science, the use of mathematics is incontestable. One need only mention as examples the fresh outlook which has been provided for further investigation by the exact study of the data relative to the segregation and recombination of hereditary factors, the beautiful investigations of L. J. Henderson on the equilibria in the blood, the theoretical study of the phenomena of excitation, the employment of thermodynamics and the numerous other applications of physicochemical theory.

Certain applications of physics to physiology are quite clear-cut and need no further comment; but in many respects conventional physics has for our purposes serious limitations, which the physiologist must try to make good by his own investigations. For instance, many hydrodynamical problems of a specialized kind are connected with the study of the circulation. The physical theory of the flow of homogeneous liquids in wide, rigid, unbranched tubes is fairly well established, though, I understand, somewhat abstruse. But when we come to study the physical aspects of a pulsatile flow of a heterogeneous mixture like blood along tubes which are branched and of varying degrees of elasticity, of diameters which in the same system range from several centimeters down to a few microns, and these subject to variations, we can expect little help from orthodox physics, which is not in the habit of working with so many independent variables.

It follows that much of our physics, if it is worth calling that, must of necessity be empirical for the present. This is not a defect in physiology—it is a defect in physical knowledge.

Chemistry and physiology having both originally sprung from the art and practice of medicine, it is little matter for surprise that such a rich harvest has been reaped by their reunion in the form of biochemistry. Although these developments were foreshadowed by the intuition, if not by the actual achievements, of the iatro-chemists of the sixteenth century, little advance was possible until chemistry had, by separation from medicine, established its position as an independent science. So that it was not until about 1840 that organic chemistry and biochemistry were able, chiefly owing to the inspiration of Liebig, to make rapid progress, at least on the Continent. There is probably no branch of chemistry that is entirely without interest to physiology, but of course preference must always be given to organic and physical chemistry. It is significant that at the present time a steadily increasing number of young highly trained organic chemists consider it worth their while to turn to biochemistry; their welcome entry into our ranks gives us fresh hope and faith in our future, as well as in theirs. Already one can point to many achievements of the organic chemist applying himself to our problems, the work of Fischer on the carbohydrates, purine bodies and proteins and amino acids, the more recent work on adrenaline, the identification of carnosine, glutathione, the structure of thyroxin and the natural bases, of which histamine threatens to rival or even to eclipse lactic acid in its importance to the physiologist. As is usually the case, rapid developments in biochemistry have followed improvements of technique; the advances in micro-methods of analysis, without which insulin would probably not have been discovered, or the constitution of thyroxin made known, have played a very important part; the same applies to the whole subject of physical chemistry, much of which, like colloid chemistry and the theories of buffer action, has been built up in response to biochemical requirements. Since the central problems of biochemistry are dynamical, most of its subject-matter must be treated from that standpoint, and here again the debt to physical chemistry must be recognized, particularly in regard to the study of enzyme action, and more recently of interfacial and membrane equilibria, of the molecular structure of surfaces, and of the phenomena of activation and the thermodynamics of oxidation-reduction phenomena.

Whether a biochemist should be primarily a chemist or a biologist is a question which has been much debated in private, though little in public. Personally I see no reason why he should not be both. If he must have one label, it is better that of the chemist, provided always that the biochemist works in the closest possible association with the physiologist. This is most essential if both are not to be deprived of much valuable interchange of ideas and, on a lower plane, of materials and apparatus. In fact, I am convinced that within the limits of administrative possibility the greater the variety of workers brought together the better the results.

So much for the exact sciences. Their value to physiology is immense. They help us to interpret phenomena, but not to predict. In a word physiology is something more than biochemistry and biophysics; it is, and will always remain, a biological subject.

As its nearest neighbor among the biological sciences, zoology should have the closest relations with physiology, yet it is curious that during several decades, for reasons which need not now be discussed, those two subjects were as the poles apart. The newly distinterred subject of comparative physiology, however, bears witness to returning interest of zoologists in the experimental study of function as against mere morphological classification, as well as of physiologists in comparative function as a valuable means of throwing light on their own special problems. For there can surely be no more fruitful means of studying that response to alterèd conditions which we know as structural adaptation, and which we consider as only a special case of response to a stimulus, than the study by physiological methods of those examples of homology and analogy with which zoological science can so abundantly supply us.

With the science of botany, except in its most general principles, physiology has a less direct connection, though here too the demonstration of fundamental points of resemblance in the metabolism of plants and animals, and the fact of the mutual dependence of the animal and vegetable kingdoms on each other, reminds us that we can not afford to ignore the physiology of any living thing. Nor, in this connection, should we forget that many valuable suggestions have arisen from plant physiology—the discovery of the cell, of Brownian movement, of osmotic pressure, and the notion of the storage of food materials, for instance.

The relation of anatomy to physiology can best be understood if we recall the fact that when the time was ripe physiology separated off from anatomy, taking with it all those dynamic problems which concerned function, and leaving anatomy literally little but the dry bones. The stationary condition of anatomy during the last decades of the nineteenth century was similar to that of zoology, and indeed had similar causes, and was little relieved by the subsequent incorporation of anthropology and embryology. Histology had in most countries remained with anatomy, and had for the most part been content, like it, merely to describe the structure of preserved dead things. In Britain, it is true, histology had until quite recently everywhere remained with physiology, and had perhaps fared no better, for although the British, like their continental friends, did "nothing in particular," they did not do it very well, for we must admit that histology had degenerated into a merely descriptive subject, supplemented by training in a useful technique, and by the identification of specimens. Nevertheless, there were rays of hope, and occasional hints, as in Bowman's researches on the kidney, Hardy's study of the structure of protoplasm, Langley's investigation of the changes in glands during secretion, or more recently Herring's careful study of the pituitary body, that the problems of function had not been entirely lost sight of, and that the large mass of histological information which had been collected might become valuable if only the fundamental question as to the reality of the structures described could be settled.

At the present time some English schools have followed the American and Continental practice, and handed histology over to anatomy, and though I am personally not at all convinced of the justification of this step, yet in view of the indications of quickening in the subject of anatomy during the past two decades, it no doubt is best to suspend judgment as to the ultimate result of the transfer. The portents of the approach of a more live and scientific type of anatomy, of an anatomy of a kind far more useful to physiology and to medicine, are many. The study of the relations of organs in the living body, of the functional significance of structure, the newer experimental histology, as typified by studies with ultra-violet illumination, ultra-microscopy, micro-dissection of live cells, tissue culture, micro-chemistry and the remarkable development of experimental embryology bring to the physiologist joy and hope, and the conviction that the artificial line of demarcation between anatomy and physiology will happily soon be a thing of the past.

The relations of anatomy and physiology to pathology are, or should be, as close as those with each other. When the separation of physiology from anatomy took place many methods and problems which rightly belonged to pathology went with it—such problems of nutrition as inanition, rickets, diabetes, ketosis and acidosis, or jaundice, and of the circulation as heart-block, fibrillation, and so forth. These and many other problems were studied in the physiological laboratory by methods which physiology had come jealously to claim as its own; the dead study of anatomy led to a pathology of the dead in preference to that of the living, and the euphemism so common in the wards, "when this case comes to the pathologist," meaning "when this patient is dead," is significant of this state of affairs. Yet it must be quite apparent that pathology and medical science can only take as their starting-point the study of the normal individual as presented by physiology.

Instead of this, the experimental side of pathology has up to the present been almost entirely directed to the study of bacteriology, which, though well enough in its way, is too narrow and superficial, because it gives insufficient information as to the relation between bacteria, their products and the tissue cells on which either infection or immunity can be explained. Now that the subject of physiology is so far advanced, the time is ripe, if not overdue, I think, for the pathologist to come into his own, and for the subject of experimental pathology, with ramifications similar to those of physiology, to attract some of the best brains in the world of biological workers. And, if the knowledge of service rendered to their fellows be regarded as payment, they will be well paid.

The subject of psychology was until recently included at the British Association as a sub-section of physiology. As a science psychology must always retain the closest links with physiology, and I think that in the future these links will be strengthened rather than weakened. The researches of Pavlov on the conditioned reflexes will undoubtedly revolutionize the study of physiological psychology, and I need offer no further comment on their scientific excellence, or on the general approval they have won, beyond reminding you that they have already been condemned by Mr. Bernard Shaw.

I have, I hope, said enough to lend emphasis to my principal point, which is that the subject of physiology has the most intimate and vital contact with all biological subjects, with the fundamental sciences, and with medicine. It is, in fact, one of the best possible illustrations of Herbert Spencer's idea that "the sciences are arts to one another." It has often been said that science knows no frontiers and no nationalities. If we apply this a little nearer home we shall all look forward to the day when departments will merely indicate administrative boundaries and not intellectual compartments. In the meantime it is to be hoped that increasing numbers of young people specially trained in other sciences will think it worth while to try to understand what physiology is and what it is striving for, and that they will come to our aid with their own special implements and standpoints.

Philosophical Position

Although the application of those sciences which are called "exact" is of immense value to physiology, we must be under no misapprehension as to their real relation, which is merely that they enable the phenomena of life to be described more accurately. They in no way furnish an explanation of those phenomena or enable us, without direct reference to physiological facts, to forecast them. The so-called exact sciences appear to be so because of the simplifications of which they are capable, by reason of which problems can readily be formulated and attacked. Disturbing conditions can provisionally be ignored or allowed for, and a first approximation reached which can be corrected later. In biology this can less readily be done. It is the failure to appreciate this elementary fact which leads some of those trained only in the methods of the exact sciences into the most palpable and unpardonable blunders when they attack biological problems. To take a simple illustration, no amount of pure physics, chemistry and mathematics would have enabled the intricate and beautiful physico-chemical adaptations which have been shown by L. J. Henderson to happen in blood, to have been predicted, because these adaptations depend, among other things, on the presence of membranes round the red cells, fashioned by the living cells and having properties incapable of prediction. The investigation of the equilibria themselves, in their physiological significance, was a necessary preliminary to the introduction of physico-chemical theory. When these phenomena, and deductions from them, became known, it was possible for the physical chemist to step in, apply the appropriate theories, and thus enable the phenomena to be more accurately described in his own language.

But the fact remains that this description turns entirely on the postulated physico-chemical properties of the membranes as deduced from their actual behavior under given conditions in what are in reality physiological experiments. It brings us no nearer to an explanation, perhaps, but it certainly does enable us to link up some of the phenomena of life with phenomena in the non-living, and so to describe them in terms which we think we understand better, because for some reason we regard physics and chemistry as more fundamental sciences than biology. Whether they are really more exact, however, is a point which might be debated.

The process of application of the exact sciences to physiology consists in reality of studying the phenomena themselves and then adopting the most plausible explanation capable of formulation in terms of the exact science. There is no other way. But let us be under no illusion about finding final explanations of what life is by this or any other methods.

The enormously rapid developments of physics in recent years strike the uninitiated onlooker dumb with an almost religious awe. Matter and energy are as fleeting as time, and the ingenuity of man has spanned the mighty extent of the known universe. Matter, energy, time and space are in the melting-pot, and out of it will come we know not what of strange relations of one to another. Of one thing we may be sure—that no final explanation will follow. Lines of separation previously held to be rigid will probably fade away, and there will be found to be a continuity between matter and energy, between living and non-living, between the conscious and the unconscious. But since philosophy can not arrive at an explanation of the nature of human understanding, the great mystery of the origin, nature and purpose of life will, I think, always remain to tease, stimulate or humiliate us.

Each must decide for himself what view he takes, and as many of our religious and philosophical beliefs are no doubt unconscious wish-fulfilments, I feel that it ultimately amounts to our decisions being dependent upon our individual temperaments, or, in other words, on our personal physiological make-up.

It was pointed out long ago by Claude Bernard that all *a priori* definitions of life, like those of time, space or matter, are futile, since they usually themselves imply the thing defined. Let us take one or two famous definitions of life as examples. Bichat in 1818 defined life as "the sum total of those functions which resist death." Here we have two opposed ideas, life and death. "All that lives will die; all that is dead has lived." For Bichat life is a struggle of the living thing against an environment which seeks to destroy it, but it is clear that the idea of life as opposed to death is implicit in the definition. This idea of an internal teleological principle, of entelechy, runs through all biological writings back to Aristotle, with whom we believe it to have originated. The amoeba which encysts itself does so in order to defy adverse conditions in its environment. The "calculating intelligence" postulated by Kant directs this response.

Another definition of life which has been much favored of late is the mechanistic one in various forms; "life is a special activity of organized things." Here again the definition implies the idea itself. The possession and maintenance of a definite structure can not any longer be held to be an outstanding feature of living matter as commonly understood, for recent researches in physics show us that, although electrons may come and go, the atomic structure of matter is relatively stable, even though under particular circumstances mutations may occur. Nevertheless the view of life as a mechanism created by and entirely dependent upon its environment gained strength owing to the developments in other sciences, particularly by reason of the synthesis of organic compounds, the principle of the conservation of energy and the introduction of the Darwinian theory of evolution. According to this view, a revival of that of Empedocles, teleological manifestations are accidental. As that thoughtful writer Hjort remarks, however: "When we, as human beings, call a thing accidental, it only means that we give up the hope of understanding it. . . ." "In the physical sciences those factors are termed accidental which we voluntarily disregard in the course of an investigation, or which we find we have omitted to notice." Kant, however, in his "Kritik of Judgment" calls the teleological "the link whereby our understanding can alone be supposed to find any agreement between the laws of nature and our own power of judgment."

Mechanistic interpretations tend in the long run to become arrogant and superficial, as vitalistic ones predispose to scientific nihilism. For, while it is inconceivable that living things do not obey the laws of nature, yet it is equally unthinkable that a chance encounter of physico-chemical phenomena can be the explanation of their existence. This being so, how can we, in Kant's words, "arrive at an understanding of nature"?

It seems clearly impossible to harmonize or to decide between these opposed views of the nature of life, and I do not think any final conclusion to be possible or even necessary. To quote Hjort once more, "Philosophy has no other starting point than a problem, and the current results of scientific research; it never leads to any absolute conclusion. It grows with the science of nature, since in reality it comprises the most general results of that science and comprises nothing more. It does not explain the nature of the human understanding, and provides no means of getting behind the understanding itself . . . the existence of which is the first and necessary condition for the existence of science at all."

Physiologists, in attempting to know what life is, have in my opinion attempted too much, and I think that a new standpoint is essential. One of the greatest of contemporary thinkers, L. J. Henderson, has recently submitted an argument with which I venture humbly to agree. The idea of adaptation, urged by Claude Bernard, should be adopted by physiology as its basal principle, as the chemist accepts the conservation of matter or the physicist the conservation of energy. We need not seek to know why it is so: that is the province of the philosopher; all our experience tells us that it is so. It is not a definition of what life is, but a brief statement of its way, which is valuable, stimulating and true. But we must treat the organism and its environment as one if we are to gain a proper insight into the adaptations manifested by the former. Life is conserved by adaptation, and I venture to think that this conception will be useful alike to general biology, to physiology and perhaps most of all to pathology. For there is no fact in biology, pathology or therapeutics which may not profitably be viewed from this fundamental physiological standpoint. An essentially similar standpoint has been reached by Haldane, who says: "We can reach no other conclusion than that it is the very conceptions of matter and energy, of physical and chemical structure and its changes, that are at fault, and that we are in the presence of phenomena where these conceptions, so successfully applied in our interpretation of the organic world, fail us." It is the concern of physiology to study the normal functions, and here the normal must be regarded as a statistical group. For particular purposes it is convenient to consider normals as of fixed value; thus the normal man has a body temperature of 37.5° C., a pulse rate of 70, a systolic arterial pressure of 120 mm. Hg, a red cell count of 5,000,000 per cubic mm. or an alveolar carbon dioxide pressure of 40 mm. Hg, etc., and we can investigate the means by which this constancy is reached. But for other purposes it is equally convenient to regard each of these in turn as variable, to study its variations and find how they are produced. When we do so we find with increasing clearness, the more deeply the subject is investigated, that the variability and the constancy are closely related, the fixed value of one thing being due to the interplay of the variables of others. Thus the constancy of the alveolar CO_2 pressure may be regarded as due to the interaction of such variables as hydrogen ion concentration of blood, body temperature, ventilation rate, oxygen pressure, etc., by which a state of equilibrium is maintained.

We have in the study of physiology many beautiful examples of this closely woven texture of interdependent phenomena. Modify any condition concerning any one of them, and you at once set the machinery moving in such a way as to counteract what you have done. And this is not what life is but what it does, which distinguishes it—it adjusts the organism to its environment.

There is a striking though superficial resemblance between this principle of biological adaptation and the principle of Le Chatelier of "the opposition of a reaction to further change" which is expressed "when any system is in a state of physical or chemical equilibrium, a change in one of the factors of equilibrium will cause a reverse change within the system."

In living things, however, as Donnan has remarked, "the activities, and indeed the very existence, of a living organism depend on its continuous utilization of an environment that is not in thermodynamic equilibrium. A living organism is a consumer and transformer of external free energy, and environmental equilibrium means non-activity and eventual death." Nevertheless, as Claude Bernard believed, and as Henderson has strikingly illustrated, the internal environment is maintained very constant in certain respects, and this constancy is the outcome of special activities which characterize life.

Glancing now towards the future, what may we say represents in a few words the trend of modern physiology? In many ways a great future lies before it. Utilizing the other sciences as its tools and itself reacting powerfully on them, we can confidently predict progress to undreamed-of heights, an enormous development of experimental pathology and medicine, and far-reaching effects on economic and sociological conditions. Yet, implicit in these very potentialities, there is another and a gloomier side to the picture. The rapidly accumulating wealth of detailed knowledge and of special technique demands an increased specialization; unless there is a periodic intellectual stocktaking there must inevitably be a loss of perspective and of grasp of great general principles. But how can this stocktaking be done? Can team work ever reach that harmony of action which distinguishes the individual? Any scientific subject is capable of indefinite expansion, and with the biological sciences it is hard to foresee what the ultimate end of *mere* expansion can be. How will scientific literature develop? Will there have to be abstracts of abstract journals and reviews of reviews? Will the subdivision of the subject necessitate in the long run the creation of lectureships or professorships to deal, for example, with the special physical chemistry of heterogeneous equilibria in biological systems, with intermediary metabolism, with the problems of hemodynamics or growth or reproduction? If so, how will the results of their special investigations be brought to common ground if no great unifying principles come to light? Can we expect that such unifying principles will appear: if they do not, will the progress of science be brought to an end by the accumulation of its own products?

The establishment of special research professorships, however profitable in isolated cases, can not in my opinion make good this growing specialization, because it will tend to divorce research and teaching and place the teaching professor on a level of real or apparent inferiority. The idolization of research for the sake of the advancement it brings is another of the dangers which threaten us. If there is one thing worse than "a mediocrity who does no research" it is "a mediocrity who does." There are at the present time a large number of junior research posts available, but not enough well-trained people adequately to fill them. This is all to the good provided that those who on trial show no aptitude for the work can be ruthlessly eliminated. As they often can not, there are in consequence a number of young people who drift from one research scholarship to another, perhaps not aimlessly, but with no better objective than the manufacture of papers designed to justify their employment. The hapless editors of each of the swelling tide of journals are coaxed, hoodwinked and, if necessary, bullied, to ensure that these papers see the light of day. In the fullness of time the list of short-time research posts is exhausted, and the young investigator must now either turn to some entirely different occupation or else, as one of my friends expressed it, "subside into a professorial chair" for which, incidentally, he is probably entirely unfitted. The pursuit of science is nowadays, perhaps unfortunately, a career, and one in which moreover it pays to advertise. Science, we are often told, is the cream of civilization. If we believe this let us use all our endeavors to ensure that it be not a whipped cream, specious, puffed up with wind, and presenting a fictitious appearance of solidity.

CHARLES ARTHUR LOVATT EVANS

MEMORIAL OF ALPHEUS HYATT

ON the request of the executive committee of the Marine Biological Laboratory, Mrs. Alfred G. (Harriet Hyatt) Mayor, daughter of Alpheus Hyatt, prepared a *bas-relief* and memorial tablet in bronze of her father, which was unveiled in the reading-room of the laboratory on September 4, 1928. In presenting the tablet to the laboratory, on behalf of Mrs. Mayor and her family, Professor E. G. Conklin, of Princeton University, made the following remarks:

Alpheus Hyatt was the leader in the movement which resulted in the establishment of the Marine Biological Laboratory at Woods Hole. It is often said that this laboratory is the lineal descendant of the Anderson School of Natural History, established by Louis Agassiz on the island of Penikese in 1873; but this is true only in the sense that several persons who were associated with that school were instrumental in founding this laboratory.

In 1870, before the establishment of the school at Penikese, Professor Hyatt had organized a Teacher's School of Science which continued under his guidance for more than thirty years. Lectures and laboratory work for teachers were given by him and by several other distinguished scientists. "Science Guides," which were the precursors of our "Laboratory Directions," were prepared by him and his associates for this work, and more than twelve hundred teachers received instruction. In furtherance of this work, Professor Hyatt, with the aid of the Woman's Education Association of Boston and the Boston Society of Natural History, maintained a sea-side laboratory at Annisquam, Massachusetts, from 1880 to 1886. After the session of 1886, Professor Hyatt called a meeting at the Boston Society of Natural History of those residents of Boston interested in the founding of a more permanent and better equipped laboratory, and in March, 1888, the Marine Biological Laboratory was incorporated by ten residents of Boston, the first name on the list being that of Alpheus Hyatt. Professor Hyatt was for two years president of the corporation and was a leading member of the board of trustees. It is most appropriate, therefore, that we should commemorate in beautiful and enduring bronze our debt of gratitude to the man who more than any other one person was the founder of this laboratory.

Since a generation has arisen that knew him not, it is well on this occasion to recall some of the salient features of his life and work.[1] Born in 1838 at Washington, D. C., he died suddenly at Cambridge, Massachusetts, in 1902. He was a student of Louis Agassiz at Harvard and graduated from the Lawrence Scientific School in 1862. Among his fellow students were Alexander Agassiz, Scudder, Putnam, Shaler, Verrill, Morse and Packard. After graduation he served in the Union Army throughout the Civil War and was retired with the rank of Captain.

In 1867 he was associated with E. S. Morse, A. S. Packard and F. W. Putnam in the Peabody Institute at Salem. In 1870 he was appointed custodian of the Boston Society of Natural History and in 1881 he became curator and continued in that office until his death. From 1870 to 1888 he was professor of zoology and paleontology in the Massachusetts Institute of Technology, and from 1877 until his death he was professor of biology in Boston University. He was one of the founders of the American Society of Naturalists, a member of the National Academy of Sciences, and an honorary member of many foreign scientific societies.

His scientific work was both extensive and intensive; he was a stimulating teacher of zoology and paleontology, a distinguished museum administrator, an organizer of scientific societies, schools and laboratories; but in addition to all these he was an important contributor to knowledge. His greatest works were on fossil cephalopods, culminating in his monumental monograph, "Genesis of the Arietidae" (Smithsonian Contributions to Knowledge, 1889); but he also made important contributions to our knowledge of sponges, bryozoa, pelecypoda, gasteropoda and insecta. Almost all these studies have to do with the evolution and genetic relationships of these groups of animals. He said of himself that he had been an evolutionist since 1859, the year of the publication of Darwin's "Origin of Species," and, incidentally, the year in which he became a student under Agassiz. His most important contributions to evolution consisted in detailed comparisons of the stages of ontogeny with those of phylogeny, for which study the fossil cephalopods were peculiarly favorable since the stages of the individual life history as well as the geological succession of species were represented in the characters of the skeleton. He divided the whole course of ontogeny into ten principal stages and he pointed out the resemblances between these stages in the life history of the individual and the life history of a species. Among the many generalizations which he developed from these studies, perhaps the best known is his "law of embryonic acceleration," ac-

[1] Much of what follows has been drawn from the "Memorial of Professor Alpheus Hyatt" published in the *Proceedings* of the Boston Society of Natural History, 30: No. 4, June, 1902; and from Dr. Robert Tracy Jackson's paper entitled, "Alpheus Hyatt and his Principles of Research," *The American Naturalist*, 47: April, 1913.