# SCIENCE

VOL. LXIV JULY 30, 1926 No. 1648

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**SCIENCE: A Weekly Journal devoted to the Advancement of Science, edited by J. McKeen Cattell and published every Friday by**

**THE SCIENCE PRESS**

**Lancaster, Pa. Garrison, N. Y.**

**New York City: Grand Central Terminal.**

**Annual Subscription, $6.00. Single Copies, 15 Cts.**

**SCIENCE is the official organ of the American Association for the Advancement of Science. Information regarding membership in the Association may be secured from the office of the permanent secretary, in the Smithsonian Institution Building, Washington, D. C.**

**Entered as second-class matter July 18, 1923, at the Post Office at Lancaster, Pa., under the Act of March 3, 1879.**

## BIOLOGY AND EXPERIMENTATION[1]

Opening a new laboratory for experimenting on living things gives a thrill to any one who has pursued that adventurous occupation. The typical experiment on living things, according to the maxim of the older zoologists, is to kick a dog; the outcome is likely to be stirring, it may be astonishing and perturbing. And as biological experimenters we are in the blessed time of youth; we have not gone far enough to know what to expect. Great regions are still almost without a preliminary survey. General principles are still unsettled. Anything may happen.

What shall we try to do in our new laboratory? Where can we best take hold? What may we hope to accomplish? Why do we work by experimentation? What *is* experimentation, indeed? What are its foundations, its principles? What must we look out for in experimenting on living things?

In thinking over these questions, it helps to look over the experiences that zoologists have had so far as they have gotten in experimenting on things alive. Although intentionally or unintentionally men have always experimented with living things, the use of experimentation as a systematic method of research in zoology, employed on a large scale, is very recent. But even in that brief period, we *have* found out something about the peculiarities of experimentation on living things; about how to experiment and how *not* to experiment. Application of experiment to living things turns out, with a thorough-going consistency, to be itself a great experiment; a proceeding by trial and error, like that of a rat in a maze. To learn how to experiment, the only method *is* to experiment; to make errors, and then later to avoid the errors. The errors are an essential part of the process; no errors, no advance. But after they are made they must not be repeated; no elimination of errors, no advance. And to eliminate them we must mark them.

Living men, here present, can remember when zoologists did not work by experimentation. When I became conscious of the science, zoologists were doing descriptive work, and drawing far-reaching conclusions from that. Mainly these conclusions were as to the course that had been followed in their evolution by particular animals and by particular systems of

[1] Address at the dedication of the Whitman Laboratory of Experimental Zoology at the University of Chicago, June 4, 1926.

organs. But at a certain period, within a few years, almost every one stopped that, and turned into experimental work. The reason for that is a pretty reason; and worthy of meditation. About the course followed in evolution by particular kinds of animals, they had drawn conclusions that were impressive; these they had developed into histories as fascinating as the myths of the gods of Greece and Rome; we may find them still in the journals of that period. But on comparison, it was found that they were not all in a tale; in fact their tales disagreed radically. They tried for a long time to convince each other, but failed. And the reason was that there was no way of deciding which, if any, of the tales were correct. But what hath the man of science of all his labor and of the vexation of his heart, if it leads to no general agreement, to nothing that can be demonstrated? And so the zoologists gave it up; they looked upon the works that their hands had wrought, and behold all was vanity and vexation of spirit. Henceforth, they said, we must so work that our results and conclusions can be tested; can be verified or refuted. We must be able to say: Such and such things happen under such and such conditions, and if you don't believe it you may supply the conditions, you may try it for yourself, and you will find it to be true. But that is precisely experimentation; and so they flocked with enthusiasm into experimentation.

So *that* is what we are doing when we experiment; at least it is one thing we are doing; we are trying to get results and conclusions that can be verified by repeating the essential conditions.

How men fared in the first pursuit of this idea will tell us something further about biological experimentation. When they gave up trying to establish the course followed in evolution by particular organisms, they had the world before them. What should they do next? There was a feeling of relief, of expansion. Says an investigator of that time, von Uexküll, "When in biology one has freed himself from the notion of evolution—a notion at last hunted to death—so that one is again in a position to look upon each animal as a unity complete in itself, instead of as the last chance product of an ancestral series that has been speculated together—then form and function gain a new interest and a heightened brilliance." This and similar pronouncements, which were not uncommon for a time, were meant, I take it, not as denial of the fact of evolution, but as expressions of relief that one may now think and work on something else.

And so work scattered in many directions. Some attacked by the new methods the question of how the egg develops into the adult; others studied behavior; others the internal physiology of organisms; others heredity and variation. Enthusiasm was at a high pitch, that at last the right way to work had been found; now the way was open for steady advance.

But in that advance there were unexpected adventures; some of the sort to remind you of the traditional biological experiment of kicking the dog. The main body of workers attacked the problem of development from the egg. We can't, they said, establish the course by which the organism has developed in its evolution, for that is past and gone. But we certainly can find out just how and according to what laws it develops from the egg: how it differentiates into diverse parts, diverse tissues and organs, and produces an adult; for we have that process before us and can experiment on it. For this movement a dashing leader arose in young Driesch, a brilliant investigator and analyst; he became the voice of the zoological modernists, their high priest and prophet. Analytical experimentation, he proclaimed, is the one and only possible way of salvation for biological science; the one and only way of getting that knowledge of the *causes* of things which constitutes science. He not only preached, but practiced; the appearance of a new experimental paper by Driesch was the sensation of the times.

Now mark what happened to Driesch and to his fellow enthusiasts in experimentation! They were trying to find out how the egg develops into the adult with its diverse parts, and to so do this that their results should be verifiable. There were clearly two possibilities. One was that the egg contains a lot of diverse things, a lot of determinants, each of which produces one of the later organs or tissues or parts of the body, so that the egg is a mosaic of diverse parts. These are distributed during development to the different cells, to the different regions of the body. As a result, one region produces head, another body; one region produces the right half of the animal, another the left; one part produces an eye, another the heart. If development is thus by distribution of these diverse parts, then perhaps one could take the egg to pieces and get the pieces to develop. In that case each will produce its own part; we shall have separate right and left halves, separate heads, arms and legs scattered about in our experimental hatcheries.

The alternative possibility is that the egg is not composed of any such diverse parts, and that the way it develops and what it produces depend on the relations of the different regions to each other, and to the surroundings. The parts fit themselves to the situation and work harmoniously together, so as always to produce a unified organism. Separate halves, separate heads and legs would be an absurdity.

Experimentation was bound to tell which of these possibilities is correct. Separate the parts of the developing egg—say the first two cells; or destroy one, and see what the separate cells will produce, a half animal or an entire one. Here is a plain and simple question so put to nature that she is bound to give a plain and simple answer, which can be verified by anyone who will repeat the experiment.

The difficult experiment was carried out. Roux did it with the amphibian egg and found that the first alternative was the correct one; a half egg produces a half animal; each region produces its foreordained part; the mosaic idea is verified. Driesch did it, with the sea urchin egg, and found that the second possibility was the correct one; a part of the egg produces the entire animal; any part can produce any part of the animal or the whole animal. Obviously one or the other was mistaken; to find out which, many investigators took a hand, trying it on many different organisms. Some found that Roux was right, some that Driesch was right. Years passed, with acrimonious controversy. Then Driesch, along with Morgan, finds an animal in which Roux's results are correct. Enormous sensation! Others find that in one and the same organism, sometimes Roux's result is correct, sometimes Driesch's.

And this was the general upshot. Some organisms operated in accord with one of the alternatives, some with the other; some parts of an organism in accord with one, some with the other. What one animal or one part couldn't do another could. All the conflicting reports were correct. The situation was that of the Gilbertian comic opera chorus, "For you are right and I am right and he is right and all is right."

But what can you make of a verdict like that? Where is your clear, definite and verifiable answer that experimentation was to give? What advantage does it show over the old methods of work? What's to be done about it?

Look what *was* done. Driesch, the apostle of experimentalism, gave it up, finally and completely; he turned fundamentalist. He decided not only that we do not know how the egg develops, but that now we know that we never can know, in experimental terms. Conditions discoverable by experimentation are not what determine the happenings in living things. The experimental method is not adequate to biological reality; it is fundamentally a failure. Driesch withdraws from it, and attempts to get at the nature of reality by other methods.

And so that's *one* thing that may happen to the biological experimenter! But must it happen? And if not, why not? *Is* experimentation essentially inadequate, a failure, as applied to living things?

Suspending for a time these questions, what did the rest of the experimenters do? They didn't despair so quickly as Driesch. By refined methods, by centrifuging and the like, they rearranged the parts of the developing egg. The results became still more incomprehensible and confusing. And then, along about 1910, with a few exceptions, they quit. From that time, for a long period, experimental contributions on the nature of development from the egg almost disappear from zoological publications. Most of the other experimenters didn't follow Driesch into fundamentalism, but they decided that, practically, experimentation on development from the egg didn't lead now to concordant and intelligible results that we could all agree on. They had gotten into a blind alley, and they backed out.

And so that is another thing that may happen to the biological experimenter!

What was the trouble in all this? What was really found out in this campaign? We discover that to plain and simple experimental questions we do not always get plain and simple experimental answers. We discover that general principles which beforehand seem obvious may be shown by the event to be wrong. We discover that when we demand of nature on which of two mutually exclusive alternatives she operates, she shows us some operations on one, some on the other, some on a mixture of the two. We find that we can not naively transfer the results and principles that we obtain by experimenting on one organism to another organism; or even to another part of the same organism. We find that what one organism can't do, another can. We find that what a given organism doesn't usually do, it may do when put to it.

But these are maxims of anarchy, of denial. They appear to vindicate Driesch, the fallen angel of experimentalism; they appear to justify his henchman, von Uexküll, who declares that there is an unresolvable contradictoriness in biological nature.

For any constructive suggestion from all this, the best that the invincible optimist can do is to moralize somewhat as follows: For rules or principles of general aplication, we can not naively generalize the responses given to our experiments by the first organism we work on, or by any single organism. The method of crucial experiments is a snare and a deception. If we are to get truths of general validity, we must compare the answers given by many different organisms, to many different experiments, and seek for some principle that includes as special cases all the discordant details.

But is there really hope in such a quest? What have the further adventures of experimentalists to say on this?

There came early into experimental zoology a powerful impulse from another body of workers. These had already developed in biology another experimental discipline, one at that time curiously limited in its objects of work, in the factors with which it dealt, and in its outlook; but a really experimental science; namely, what was called physiology. This dealt mainly with the chemistry and physics of the substances found in organisms and with the action of recognized chemical and physical agents upon them. These two categories of things were urged upon experimental zoology as the essential and exclusive material for experimentation. This impulse centered largely in the study of movements and behavior. Other experimental zoologists had attacked the question: What makes organisms behave as they do? What makes them go in a certain direction? What makes them gather in a certain region? The physiologists set forth that organisms are masses of a certain kind of material, to-wit, protoplasm. Men had already experimented on the movements of masses of matter, and had learned generally applicable principles concerning them. Masses, both organic and inorganic, are impinged upon by physical and chemical agents from the surroundings: by light, heat, chemicals, electricity. The masses are moved by these agencies, in ways depending upon the nature of the particular agent. Argal, the way to understand the behavior of organisms is to find out just what external agents or forces are impinging on them, the directions from which these come; and the particular movements produced by each agent. Then the mathematical resultant of these movements is what we call the behavior of the organism.

This chain of reasoning contains true elements of permanent value. One of the fundamentals of biological experimentation *is* to know and control the environmental agents. But look now at the results of its application. The program was carried out. Crucial experiments were performed. Favorable masses of organic substances—lower organisms that could be had in large numbers—were subjected to light, to heat, to chemicals, and the like. The results, on the grouping of the masses, and on the direction of motion required to produce this grouping, were determined and classified. This yielded the tropisms: simple, direct, uniform movements. By application of this concept of tropisms the movements of organisms were explained; their goings out and comings in, their gatherings and groupings, their behavior. A triumph of the clear and simple expositions that come from really enlightened work in experimental science.

But mark again what happened. Certain investigators desired to know the "particular go" of these things. They studied, under the influence of single agents, single individuals of particular kinds of lower organisms, one after another, following the details of their motions. The movements were not simple and uniform. Different species under a given impinging agent acted quite diversely, as do dogs and cats and squirrels; each had his specific way of responding. Different methods of response were correlated with different peculiarities of structure. How the organisms behaved depended, not alone on the agent impinging on them, and on the kind of material of which they were made, but also upon the way that material was arranged; as is true of a bell or a typewriter or an automobile. These arrangements were varied and numerous; the result was not uniformity of behavior, but heterogeneity and variety. We have come here upon another principle that is of fundamental significance for the biological experimenter. The physics of diverse *arrangements* of substances is as essential as the physics of the homogeneous substances taken singly.

And a still further fundamental principle showed itself in this work. Even the single individuals, acted upon by a single agent, as a diffusing chemical, did not move uniformly and directly, like iron filings under pull from a magnet. On the contrary, each went through a number of diverse motions and moved in many directions, before they all had gotten collected in a certain region. The final result was due to the cessation, the elimination, of many of the motions; and continuance of others. The essential question becomes: What causes the elimination of the motions that cease? We have come upon another one of the things that play a major rôle in biological experimentation: the phenomenon of selective elimination.

Now in dealing with this subject of behavior, I realize that I am treading on a lava stream still hot from the fires of controversial eruptions, so I will hasten to step off. Arrived at a safe distance, I wish to try to point out certain general features in the landscape. The history just set forth appears typical for biological experimentation. At first the general features, the beginning and the end, of "crucial" experiments on "typical" organisms form the material of our science. The science is now clear, uniform, simple, intelligible. Then other organisms are examined, and the steps intervening between the beginning of the experiment and its final result are studied. As this is pursued, the critical and decisive parts played by diversity of arrangement, of constitution; and by selective elimination, become manifest. Variety, diversity, takes the place of uniformity and simplicity. Such has been the story in other fields; for example, in the progress of genetics, the study of heredity and variation. At first there are laws of inheritance, abstract and mathematical; they hang in the air. These laws as they are followed become more varied, more arbitrary, more unintelligible. And then it is found that their form and content is the resultant of the operation of special arrangements of the organic material; certain systems of structure, the chromosomes. Where these arrangements are different, the rules of heredity are different. These rules become intelligible only through understanding these arrangements and their operation. And further, the processes of genetics reveal themselves as the production, on an immense scale, of diverse combinations, diverse systems, giving the widest scope for selective elimination and selective persistence. It is a history which, with variations in detail, has unrolled in many fields of experimental zoology.

And now that we have gotten in hand at least a few of the main threads that weave themselves into the complex tissue of biological experimentation, let us look at that tissue; let us examine the interweaving of the threads that make it up; let us weigh the significance of each.

Two of the fundamentals for biological experimentation were, as we saw, from the beginning emphasized by the physiologists who did so much to promote experimentation in zoology. The first is the analysis of the environment. We must know the outer agents that act upon organisms; we must study and control them in detail; we must be physicists and chemists for their sake; we must experiment analytically with them; we must know their effects on organisms, singly and in combination. Here is one of the most extensive fields for experimental zoology; here particularly is one of its great opportunities for influencing the practice of human life. Agriculture, hygiene, medicine, are largely outgrowths of such work. A laboratory of experimental zoology must be a laboratory for control of the environment.

Yet this is not the only requirement. Biological experiments that limit themselves to analyzing the environment and cataloguing the immediate effect on organisms of its components, will not lead far into biological science. Only when combined with adequate consideration of the other fundamentals does it become an instrument for such insight; without this it may be and has been an instrument of deception.

The second fundamental, likewise emphasized by the early physiological impulse, is the study of the physics and chemistry of the substances that make up organisms; the study of colloids; of nuclear compounds; of secretions; of hormones; of tissues. This is so fully recognized and so practically established that there is a special type of institution for it; laboratories of physiological chemistry. I need not dwell upon it here; it is now riding the crest of the advancing wave.

The third fundamental is logically but an aspect of the second, an aspect of the physics of the organism. But it requires separate consideration, both because of its extreme importance for zoological experiment; and because it was minimized, nay, despised and rejected of the physiological impulse in zoology. This is the rôle of physical arrangements of material in organisms; gross physical arrangements as well as minuter ones; what is variously called organization or structure. Structure had become the object of one of those epidemic phobias that beset scientific men as they do other men. In the days before experimentation, zoologists had given a romantic and mystical turn to the phenomena of structure in organisms; they built upon it a great edifice which was called morphology. They discovered in organic structure plans, styles, comparable to the diverse styles of architecture; to Gothic, Romanesque, Classical and the rest. But the physiologists said: This may be pretty, but is it Science? It is not. Out with it. We shall have nothing to do with morphology; it is fantastical. And throwing away the baby with the bath water, they largely rejected also the rôle of structural arrangements, even in experimentation. This it was that led to most of the adventures or misadventures of the sort I have recounted, in the progress of experimental zoology. It is important that this phobia should no longer dominate our work. Consider for a few moments the rôle of arrangements or organization in experimental work, and the consequences of its neglect.

Structural arrangement, organization, is of course physics; we find it playing a very great rôle in physics as that science advances. The properties of atoms depend upon the arrangement of the electrons; of molecules on the arrangement of atoms, of crystals on the arrangement of molecules. In organisms there is a great extension of this. They are bodies in which the arrangements have become complex and differentiated, and have passed into the grosser, the visible features as well as in the finer details. They are bodies in which there is an almost infinite variety of these arrangements, as we pass from species to species. They are systems of structures. In consequence, their properties and the way they respond to experiments, depend largely on these systems.

This puts a very great limitation upon the adequacy of experimentation and conclusions when only our first two fundamentals are taken into consideration. Knowledge and control of the environmental agents impinging upon organisms, and of the physics and chemistry of the separate substances of which they are composed, does not suffice for understanding what happens in them. For, the same materials, under the action of the same agents, respond in most diverse ways, depending on how the materials are arranged. This is as true for physics as it is for biology. The same lot of materials, under the action of the electric current, may in one arrangement act as a clock and tell time; in another act as a typewriter and spell

John; in a third act as a computing machine and give the product of 9 times 17. A laboratory of experimental zoology is a very museum of such diverse arrangements, responding diversely to the same conditions, with no necessary corresponding diversities in the constituent materials, aside from their arrangements. As a result, the responses of organisms need not at all correspond in their diversity to the diversities of the agents which act upon them.

Again, physical arrangements are readily made which, like an organism, may respond in one and the same way to the most diverse and opposed agents. Such a one may react in some single way—as by ringing a bell, or lowering a window—to heat and to cold, to acid, to alkali and to neutral salt; to mechanical shock, to light and to electricity. The world of organisms is a world of such arrangements. It will not do therefore, as has been so often done, to take the gross responses of such arrangements as typical and general for the substances concerned, irrespective of their arrangement; as yielding in that sense a general physiological or physical law. The axoltol transforms under the influence of the thyroid secretion. It has an adequate amount of that secretion in active condition. But it does not transform, for some special arrangement prevents the secretion from coming to action. Of such is the kingdom of organisms.

How far may we trace the decisive rôle of arrangement of parts? It is as pronounced in most lower organisms as in higher ones; the responses of infusoria are the workings of complex systems, diverse in different species. Is it true for Amoeba? For the fluid protoplasm within cells? May these move and react diversely, in different instances, as their structure is diverse? May there be as great diversities in the finer details of structure as in the grosser ones—so that different instances of protoplasmic flow may be as diverse as locomotion by legs and by wings? All this appears possible. Is there indeed a limit to this? Are there any properties of organisms in which special arrangements, organization, play no part? These are unsettled questions, but of the greatest importance for the experimental biologist.

It is largely the habitual neglect—nay, the contemptuous rejection—of these relations by some biological experimenters, that has so often led the experimental method to grotesque failure where triumph was expected. This is what has so often led the biological experimenter into a land of Cockayne; a land of romantic and unsubstantial phantoms, as mystical as any creations of the older morphologists; and fading away at the touch of reality. This it is that led to the tragedy of Driesch; it is the different structural arrangements in diverse organisms that bring about their diverse responses to the experiment of separating the parts of the egg.

In general, it is to this decisive rôle of diverse arrangements that are due the seemingly anarchistic principles which we deduced from the early experiences of experimenters. To it is due the fact that we can not directly transfer the experimental results that we have gotten in one field to another field. We can not transfer them uncritically even from one organism to another. And *a fortiori* it will not do to transfer uncritically the results of experimentation on inorganic things to organisms; the arrangements of parts are different. To this is due the maxim that what one organism can't do, another can; a maxim verified in every field of experimentation. To this is due the deceptiveness of the method of crucial experiment so much employed—the single experiment that is to give a generally valid answer to a question proposed. It is largely because of this that it is only through comparison of experiments on many diverse organisms that we can hope for truths of general validity.

But can we hope for truths of general validity? If we must stop with the truth that organisms are diverse arrangements, and therefore act diversely, respond diversely, is not biology incurably pluralistic, a heap of heterogeneous details? If we must stop there, surely it is. To picture it so, as Driesch urged in rejecting diverse arrangements as the explanation of the discordant results of experimentation, is merely to photograph the situation with all its complexities; is merely to state at once all the difficulties that we are working to overcome. A solution can lie only in accounting for the diversity of arrangements, of structures; in discovering how arrangements are changed, how new arrangements are produced, how from one arrangement come many. The problem of evolution we have thrown out of the window and we have locked the door, but it returns at the keyhole. To discover how organisms come to be arrangements; to be diverse arrangements; to discover how organic arrangements are produced and transformed and differentiated and conserved, is the final, the fundamental problem for the biological experimenter.

Changes of arrangements, of structure, we find as we experiment; these must be our port of attack. Some of these changes of arrangement seem stereotyped and automatic, like a shift in a typewriter, causing it to print capitals in place of lower case letters; a mere working of the mechanism already existing. So we may find in behavior that an infusorian responds to a continued stimulus by a whole series of motions, one touching off the next; inorganic arrangements acting in this way through the principle of the shift are readily made.

So, too, do we find a chain of diverse arrangements produced in the development from the egg. We start with a complex mechanism, the chromosomes with their many diverse substances or genes systematically arranged; the cytoplasm; the environment. The system so operates as to change its own organization, and thereby its own responses to experiment. Development of the individual is a gradual series of transformations of the arrangement of parts; hence showing a series of diverse responses to given conditions, to experiment. Different organisms begin as different systems with different kinds and degrees of arrangements of their parts, hence they respond diversely to experimentation. Thence it was that arose the troubles and defeats of the early experimenters on development.

Again, in genetics, in the processes occurring at the production and union of germ cells we have a continual and kaleidoscopic production of new arrangements, new combinations, occurring in a systematic and perhaps predictable manner, as the working out of existing mechanism.

But in these cases where transformation seems but the working out of a complex mechanism already present we do not feel sure that we are getting light on the production of structure where it did not before occur: the change from one structure to another in a way not stereotyped. How does this occur? We find it in several sets of phenomena. During the lifetime of the individual we find it in what we call the formation of habits. Here is an actual change of organization that so far as we can see is not stereotyped; not a repetition of what has before occurred. How does it take place? We do not know. Here is fundamental work for the experimenter.

Changes of organization are induced too when we subject the developing organism to special or unusual conditions; a head may be caused to appear where a tail should have occurred, and similarly of other induced changes; they may extend to the fine details of organization. In such changes of organization we seem to approach the final secret of biology.

But the changes we have mentioned disappear with the individuals in which they have occurred. They do not, so far as we have been able to discover, produce alterations in their descendants; "acquired characters are not inherited." And therefore they do not account for the permanent differences of system that we find among organisms; it is these permanent, these hereditary, diversities that form the deepest problem of biology.

In some way such lasting diversities of organization do occur. All different animals, or a very great number of them, are originally pieces of the same material; the animal kingdom is essentially one organism which, like a mycetozoan, becomes separated into small pieces. As time has passed, these pieces have transformed; transformed in their most intimate nature, so that the substances which make up the germ cells—what we call the genes—have become diverse. For formerly these genes and germ cells all produced organisms much alike. Were they Amoebas, perhaps? But now some of them still produce Amoebas, some of them crayfish, some tape worms, some frogs, some black birds, some horses and some men. How and according to what rules have these changes occurred? How and according to what rules are they still occurring?

Experimenters that are at work upon this question are engaged upon the most fundamental of all the questions of biology. Other aspects of biological experimentation are of extreme interest, theoretical and practical. We must know the environment and the effects it produces on organisms; the diverse effects it produces on diverse organisms. We must know the chemistry and the physics of the diverse substances that make up organisms. We must know how these are arranged; how they are combined into systems. We must know the diversity of these systems in different organisms; and how this results in diversity of response to environment, to experiment. But finally, we must know how it happens that organisms are diverse systems of structures and functions; what are the laws of the production of these diversities. Only this knowledge can bring the whole of experimental biology to a unity.

Having examined as it were a map for biological experimentation, let me turn for a moment to another one of its fundamentals; something not to be located at a particular region of the map; but pervading the whole; something inconspicuous, impalpable, yet potent, in physics, in chemistry, and above all in biology; "a mighty darkness filling the seat of power." This is what I touched briefly in the experiences of experimentalism with behavior; it is the operation of selective elimination, with its complement, selective persistence. This is the very Mephistopheles of biological experimentation, filling it, if unrecognized, with chimeras and deceptions. In our experimental material, many diverse combinations are formed, diverse chemicals, diverse motions, diverse genes, diverse systems. Experimental conditions cause the elimination of some of these, while others persist. At the end the material before us has changed. Our experimental agent appears to have worked a transformation; in fact it has worked only an elimination. If we do not see the details, the production of many combinations, the elimination of certain sorts, we shall enunciate laws of action, of transformation, that are delusive phantasms. Again and again has this happened in biological experimentation. Stock is subjected to given environment. After a few generations it is found to be changed; the change is inherited even upon restoration to the usual environment. Behold!

We have discovered the inheritance of acquired characters. And then selective elimination is found lurking beneath the surface, and we know not what we *have* discovered. But for it, the inheritance of acquired characters has been overwhelmingly demonstrated. It is the very evil genius of the biological experimenter. To it are due the teleological fantasies of biology. To it are due specific false doctrines in many concrete fields of work. Wherever in experiments there is superabundant production, whether of motions, of chemicals, of genes, of germ cells, of individuals, so that only a part continue—beware, for in such does the demon of selective elimination lurk. Particularly in the fundamental problem of biological experimentation—the formation and transformation of biological systems—does it play the master rôle.

The experimenter who is not perpetually conscious of it and of the possibilities of its action is in danger. To ignore it, as many have done, is to court disaster. It must be dealt with explicitly; it must be seized and controlled; it must itself be made the subject of investigation; only thus is there security. That this is difficult does not make it the less necessary.

The opportunity before the new generation of biological experimenters, those that shall work in this laboratory, is an enticing one. The first generation of experimenters in zoology were ill prepared. Those of us who came from the older zoology were hampered by inadequate preparation in the first two fundamentals—in the physics and chemistry of the environmental conditions, and of the organic materials; this has been a heavy handicap. Those who entered experimental zoology from physiology were equally hampered by inadequate appreciation of the second two fundamentals—the great and decisive rôle of diversities of organization; and the equally great but insidious rôle of overproduction with selective elimination; the taboo placed by the physiologists upon these things has been to them a heavy handicap. The new generation need suffer under neither of these handicaps; it can deal adequately with the one pair of fundamentals without failing to deal adequately with the other.

To such, to men who will have done with taboos and phobias, who will be physicists and chemists without failing to be also zoologists, the field is ripe to the harvest. To lay out a specific program for an experimental laboratory is the function of those that shall work therein. But a glance at some large features of the concrete situation, at the opportunities before us, is not foreign to our purposes.

Study of the environmental components and their effects on the organism; and study of the physics and chemistry of the organism are bound to form a large part of the concrete work of any experimenter. Carried out with enlightenment and with thoroughness they lead into every problem of biology; they will cast light upon every problem. This work is in full swing; I need not dwell upon it.

Genetics has of late been one of the most fruitful fields for experimentation, starting from the question of the distribution of inherited characteristics. With Morgan, American experimenters can say with pride that *that* part of the problem is in principle solved; and by the work of Americans.

But how the genes operate to produce the results that they do; how they interact with each other, and with other components; how they interact with the environment; in a word, how development occurs, from the egg to the adult—this is the field that is now open for conquest. At its first attack on this, biological experimentation, as we saw, fell back repulsed; its approaches had been ill prepared and unsystematic. Now a secure foundation has been laid by the work in genetics. Intermediate products, between the genes and the later characteristics, are laid bare by the work on hormones. Through the work of Spemann, of Child, of Harrison; through that of Lillie and Moore, and of others, the walls of the fortress have been undermined or demolished; the way is open. An immense amount of experimental work is yet required, and it must lead to truths of fundamental significance. A more inviting prospect can not be imagined.

On the still more fundamental problem of the production of permanent alterations in stocks; of permanent alterations in genes, the way seems to me less clear; this matter has the allurement of difficulty as well as of importance. Lasting alterations of genes, of stocks, have been observed under experimental conditions, but their causation, their physiology, and their relation to the general transformations of stocks are obscure or totally unknown. How does it happen that in different organisms the same diversity of characteristics is produced in one case by environmental differences, in another by gene differences? This is true even for so deep-lying a feature as the diversity of sex; it seems to be true for every kind of characteristic. *Is* there in some way a transition from environmental determination to gene determination? And if there is, how is it brought about? What *is* the relation of environment to changes of genes, to changes of stocks? It is little short of a scandal that we know so little of this; so little even of the problem of the direct injury of genes by environmental conditions. To such a body of evidence as Kammerer presents for the inheritance of acquired characters we can respond with little more than a gesture of incredulity; or with vague suggestions as to selective elimination. What *is* the rôle of selective elimination in all this? What the rôle of mating, of biparental reproduction? The problem of the relation of environment to changes in stocks is one on which depends the answer to many pressing human problems; at the same time it is the one that contains the key to the unity of biological science. This question alone might well constitute the program of a great experimental institution.

H. S. JENNINGS

THE JOHNS HOPKINS UNIVERSITY

## THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

### THE MILLS COLLEGE MEETING OF THE PACIFIC DIVISION

MILLS COLLEGE proved to be an ideal place to hold the tenth annual meeting of the Pacific Division of the American Association. The accommodations were excellent in every respect. Large auditoriums for the general sessions, well lighted and ventilated class rooms for the meetings of affiliated societies well met the purely physical needs of the convention. The cordial hospitality and thoughtful attention of the faculty and officers of the college, combined with the delightful environment of trees and glades, quaint architecture, flowers and sequestered paths made the occasion one to be very pleasantly remembered. The spirit of aspiring young womanhood seemed indeed to pervade the place. While the sciences figure prominently in the instruction offered at Mills, the inscription over a monumental doorway in its beautiful art gallery perhaps sounds the keynote for the harmony which prevails throughout the campus, "Art remains the one way possible of speaking truth"—a sentiment which at first thought might give a scientist pause, but with reflection and in such surroundings would be sure to win his assent.

The total registration was 402. While the attendance was largely drawn from the membership in the Bay region, including Berkeley, Oakland, San Francisco and Stanford, analysis of the balance shows a geographic distribution as follows: Northern California, outside of Bay district, 47; Southern California, 65; Canada, 2; Hawaii, 2; Mexico, 2; Nevada, 9; Oregon, 16; Philippines, 1; Utah, 3; Washington, 4. Besides, attendance was registered from Delaware, Illinois, Iowa, Massachusetts, Michigan, Minnesota, New Jersey, New York, Vermont, Washington, D. C., China, Egypt, England, Germany, Ireland, Russia and Sweden.

#### RESEARCH CONFERENCE

The general sessions in which the entire convention participated opened with the research conference at luncheon on June 16. The relation of the college to research was discussed. President Aitken presided and introduced the following speakers:

Professor Howard E. McMinn, Department of Botany, Mills College.

Professor Albert Schneider, dean of the School of Pharmacy, North Pacific College, Oregon.

Professor Vernette L. Gibbons, Department of Chemistry, Mills College.

Professor Philip A. Munz, Department of Botany, Pomona College.

Stress was laid upon the importance of inciting interest in research work among undergraduates, and various methods by which this could be done were advanced by the speakers.

#### SYMPOSIUM ON THE CONSTITUTION OF MATTER

Following the luncheon, adjournment was taken to Lisser Hall, where the symposium on "The Constitution of Matter" was presented. The various phases of this fascinating subject were discussed and recent contributions to the solution of the problem were described and interpreted in a series of four papers as follows:

(1) "The Elements and their Composition." DR. T. R. HOGNESS, of the University of California, Chemistry Department, Berkeley, California.

(2) "Atomic and Molecular Structure." DR. HERTHA SPONER, of the Physical Institute of the University of Göttingen, Germany.

(3) "The Nature of the Atom as explaining and as exhibited by Lines in the Stellar and Solar Spectra." DR. H. H. PLASKETT, of the Dominion Astrophysical Observatory, Victoria, British Columbia, Canada.

(4) "The Structure of Matter as elucidated by X-Rays." MAURICE L. HUGGINS, Department of Chemistry, Stanford University, California.

#### ADDRESS OF THE PRESIDENT

The address of the retiring president, Robert G. Aitken, was given on the evening of June 16.

Following a graceful address of welcome by President Aurelia Henry Reinhardt, of Mills College, to which response was made on behalf of the membership by Vice-president Joel H. Hildebrand, President Robert G. Aitken, associate director of Lick Observatory, delivered a scholarly address on the "Solar System: Some Unsolved Problems."

Prefacing his remarks with a plea for better instruction in astronomy in the secondary schools, urging that "every child has a right to be introduced to the stars as ever present friends" the speaker advanced to his theme, in which he showed a fine appreciation of the requirements and limitations of a popular address on an abstruse subject. He spoke of the