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THE RELATION OF BIOLOGY TO PHYSICS[1]

IT came to me as a great surprise that a biologist should be invited to give an address at the dedication of a physical laboratory. I realized my unworthiness to represent the many sides of biology on such a significant occasion, but I dared not decline on these grounds, because there are certain things that I want to say, and your invitation gives me a chance to say them.

First of all, I should like to point out that one of the most important developments in recent times is the recognition of the need in the biological sciences of workers trained in physics. It seems to me that the reverse is also true—that a biologist is needed in physical laboratories. Perhaps to-day is the first time that this has been recognized, and Vassar has been foremost to recognize this fact, even though the biologist is allowed to remain for only one short hour.

When we dedicate a new building to *biology* at Columbia I shall see to it that one of your physicists is invited to respond to the sentiment, "the relation of physics to biology." As there is no immediate prospect of this building there will be several years to think it over.

If the president of Vassar or the program committee or whoever arranged these dedication exercises had intended that the speaker to-day should analyze the relation of *physiology* to physics, he or they, I am sure, would have picked out a physiologist. The physiologist would have had an easy and even a delightful time, for physiology has long since been wedded both to physics and to chemistry. A modern physiological laboratory is scarcely to be distinguished from a physical laboratory, having borrowed its instruments, at least, from the former.

There is, however, another side of biology that is conspicuous by its absence from most physiological laboratories. The traditional physiology of the schools is interested in the functioning of the organs. It takes the *organism* for granted and tries to find out how its organs work in health and in disease. The close relation of functional physiology to medicine explains and justifies this limitation of its field of interests.

[1] One of a series of addresses given at the inauguration of the new physical laboratory at Vassar College in October, 1926.

But there is another physiology, one that is concerned with the coming into existence of those organs whose function the physiologist studies. This coming into existence may have taken place in the past (we call it, then, evolution), or it may relate to the development of each individual each time it comes from an egg.

Now evolution has been largely treated as a historical question, one which in the last fifty years zoologists, botanists and paleontologists have studied from various angles.

But what reason is there to suppose that evolution really came to an end with the appearance of man on the earth? Is evolution not going on at the present time; and if so, is it not more open to investigation than in the past? The answer is, I think, that there is every probability that evolution is still going on as merrily as ever, and that man himself is not the finished product of evolution that he likes to believe.

It is, however, the other field of research relating to the creative process that is open to most direct attack. It may have taken millions of years for the present fauna and flora to have developed; it may take only a few days for an egg to develop into an organism. The millions of years that it took to evolve the kinds of individuals living to-day seem to have been condensed into a few hours. We can see taking place before our very eyes every single change in the development of an egg. What is not visible we can make visible with our stains and microtomes.

Now it had been supposed, for a hundred years, that we could interpret embryonic development as a historical series of events, condensing an eternity into a day. We know better now. At the time when the phylogenetic interpretation of embryology was in full swing, and before it became generally admitted that we could not hope by this means to discover how the egg develops into an organism—at that time a new school arose which directed attention to the need of a physical and chemical, *i.e.*, a causal interpretation of the underlying changes that take place during embryonic development. Its advocates called the new movement "developmental mechanics."

Unfortunately, William Roux, who is generally recognized as the foremost advocate of the new method, dealt largely in causal philosophical discussions of the supposed principles of developmental mechanics. Roux himself was primarily an anatomist and not a physiologist. The single pioneer experiment of note that he made consisted in killing one of the first two cells of the dividing egg of the frog. The other living cell produced a half-embryo. From this slender basis of fact Roux built up an elaborate theory of development. Driesch, on the basis of other observations, discovered that when the first two cells of the dividing egg of the sea urchin and ctenophor are separated, each develops in one case into a whole embryo, and in the other case into a half-embryo. From these and other observations that consisted mainly in isolating different parts of the egg Driesch elaborated another theory of development that was ultra-physical. He tried to show that back of the physical and chemical changes that take place during development there must be present a guiding principle, a *vis essentialis,* which he identified as the Aristotelian entelechy. Driesch's interesting speculations reached a wide audience, in part because of the brilliant logic by which he supported his views, and, in part, because an appeal to mysticism has in itself a fascination for the human mind untrained in scientific methods.

All this is past history, and little more than an episode in the process of biological thought. There was, in fact, a quick reaction from the stagnation that was the immediate outcome of this attempt to remove the problem of development from the field of physics to that of philosophy. From then onwards a study of development from a chemico-physical standpoint began, and forged steadily ahead until to-day the pack is in full cry.

The most immediate result of this new work was to reveal how completely ignorant we were of the constitution of living materials, and eggs in particular. It was obvious that we didn't really know what we were talking about, and that the problems of development are complex and need to be unraveled patiently, one by one, before we can hope to have the faintest idea of what is taking place.

This has led to two results. We realize how futile it is to attempt to make up grand philosophies of development, and we realize that only through an exact knowledge of the chemical and physical changes taking place in development can we hope to raise the study of development to the level of an exact science. In a word, we realize that we need all the help that physics and chemistry can give if we are ever to pass beyond the transcendental point of view that dominated the earlier work. Biologists have come, as I have said, to realize that there are problems in development to be found at every turn, which the physicist and the chemist confess they are not yet able to handle. In part this is due, I think, to their unfamiliarity at first hand with the problems themselves, because they did not realize that organic matter presented such problems. That is one of the reasons why I said a little while ago that biologists were needed in physical laboratories.

I should like to illustrate the need of physical knowledge in biological work by a few very simple examples which, in a general way, are familiar to you, yet will serve, I hope, to bring home the need for the cooperation for which I am pleading.

The egg is a cell, and the first step in development is taken when the egg divides into two parts. Cell-division is one of the most general phenomena of living things. The first indication of division in a living egg is a constriction that appears on the surface, which gradually spreads and encircles the egg. It cuts into the interior until two hemispheres result that flatten against each other. After a pause of less than an hour, a new division appears at right angles to the first, dividing the material into quadrants.

This process continues until a thousand or more cells may be produced before any of the embryonic organs are laid down. Our microscopes reveal, even in a transparent egg, only a small part of what is happening inside the egg. By means of an elaborate technique the interior changes have been made out. This technique consists in staining the substances of the egg in various stages of division. The exploration of the interior is further carried out by cutting the egg into hundreds of thin slices—as many as five thousand to an inch. Such sections show in the middle of the egg an inner sphere, or nucleus. The walls of the nucleus dissolve just before division is to take place, and a number of tiny rods or chromosomes reveal themselves. There is a characteristic number of these for each species of animal or plant. Moreover, they often differ in shape and size. Whenever differences are present we find that there are two chromosomes of each size or shape.

The next step is the appearance of a spindle-shaped figure near the chromosomes. Into the middle of the spindle the chromosomes move, or are carried, and there they arrange themselves in an equatorial plate.

Even before this time we discover a clear line running through the length of each chromosome. Each has split throughout its length and two daughter halves are present.

Each half of each chromosome then moves to one pole and its sister half to the opposite pole.

It is about this time that the constriction appears on the surface of the egg. As it deepens it cuts through the middle of the spindle separating the daughter chromosome groups from each other.

Around each group of chromosomes a fluid accumulates, and the chromosomes begin to lose their staining property. Suitable stains reveal that each chromosome becomes branched and the branches have the appearance of forming a network in the new nucleus that is now formed.

A resting stage of about half an hour follows, and then the same process repeats itself—the nucleus wall in each cell disappears, the chromosomes reappear, a spindle develops, the chromosomes again split lengthwise into daughter halves.

I have given the briefest outline of the process of cell-division that is described in every text-book of biology. What does it all mean? What, to begin with, causes the constriction to appear on the egg at the moment when the chromosomes have already divided and separated? The division of the cell impresses us as a simple physical phenomenon. Many attempts have been made to account for it, but none are satisfactory, because, I think, we do not know as yet enough of the physical constitution of the materials of the egg to permit more than provisional guesses.

This, however, is only the first problem that presents itself! What makes each chromosome split lengthwise? The chromosomes are too deeply imbedded in the egg for us to invoke external agents. It must seem that some sort of a molecular event is taking place, whose nature is entirely unknown to us, and yet, who will doubt that it, too, may be a very simple physical process.

How do the chromosomes reach the equator of the spindle? What moves the daughter halves to opposite poles? When they reach the poles why do they undergo a reverse series of changes and pass once more into a resting stage? What are they doing while resting? Probably each is growing to its original size, but what is the nature of this growth? Here we meet with a dozen questions, all calling aloud for answers. It seems that no one but a physicist can hope to solve them.

There is another important question connected with the chromosomes for which we have no answer: I refer to the union or conjugation of the chromosomes that takes place once, and once only, in the cycle of the life of each individual organism.

When the germ-cells, that is, the egg-cells and the sperm cells, after having passed through many ordinary divisions (such as I have just described), reach their final stage of maturity a strange thing happens. Although the chromosomes have remained apart through a long series of cell-divisions, now they come together in pairs.

The two members of each pair approach each other and come to lie side by side. It looks as though they had fused and reduced the visible number of chromosomes to half the original number. But we have many reasons for thinking that they do not fuse but only lie closely apposed.

There is another fact connected with this union of like-chromosomes that was at first wrongly interpreted: one member of each pair has come from the father of the individual, the other from the mother.

It was supposed, wrongly as I have said, that the conjugation of the chromosomes had something to do with their origin—in a word, that they mated because one had come from a male, the other a female.

Now we know that this is not the cause of their union, but that they mate because they are like each other—in fact they may be identical. Here is a fine opportunity for metaphysical discussion, but I like to think rather that the event is purely physical, even although I must confess that we do not know what kind of an attraction draws like-chromosomes together, and not even if it is an attraction.

I want again to emphasize that these chromosomes have been living together, literally, for months or even for years and have shown no tendency to come together. Then suddenly they all forget, as it were, their repugnances, and become attracted to each other, but only within strict class limits, for no chromosome makes the mistake of uniting with a wrong one.

Having gone so far, you will permit me to go one stage further and consider briefly some further information that we now have concerning the intimate constitution of the chromosomes themselves.

This evidence has come from a study of genetics. It carries us far beyond anything that even the highest powers of our microscope reveal. I can not attempt to discuss the sources of this evidence, or to weigh the value of the method by which the conclusions are reached. But the result is so simple that there is no difficulty in presenting the conclusions without the arguments for or against them.

The evidence from heredity, then, has revealed that each chromosome is made up of a chain of discrete particles which we call genes—because they are, as it were, the basis of the genealogy of every individual.

There are probably thousands of these genes in every chromosome, and no two genes appear to be alike. Each individual animal or plant is the product of the activity of all these genes, and heredity is the result of the shuffling of these genes in each generation.

In several plants and animals we can refer these genes to particular chromosomes, and in one animal at least about four hundred genes have been placed. On the assumption of their relative positions with respect to one another, we can predict what the numerical results will be in inheritance for some four hundred different characters. The theory justifies itself in that it allows one to predict the outcome in terms of numbers for all these four hundred characters whose genes have been located.

If there were time, I should like to go much further and give an account of the chromosomes in relation to their constituent elements, but I can add only one further point, namely, that the genes divide when the chromosomes divide, and collectively their division is what we see when each chromosomal rod splits throughout its length. It is the genes that come together during the conjugation of the chromosomes. They must come together with extraordinary precision, which implies probably that we are dealing with events of a molecular order. We can go no further until physics has furnished us with a key to unlock these extraordinary events.

One other example I should like to cite: one of the most successful attempts to apply the methods of physics to a biological problem of fundamental significance. The turning of plants and animals towards, or away from, the light has been found to obey the laws of a physical reaction known as the Bunsen-Roscoe law. This law states that a given amount of radiant energy produces a definite photochemical effect provided the product of the intensity of the light and the time of its action remain constant. That is to say: the same end is reached with a high intensity and a short exposure as with a low intensity and a long exposure.

The particular mechanism by means of which the reaction or orientation is carried out is a matter of secondary interest, for the movements may be carried out by the six legs of an insect, the four legs of a vertebrate, the body wall muscles of a worm, the cilia of a protozoon, or by differential growth on the opposite sides of a plant. The details of the machinery are of biological interest, of course, but the discovery that the reaction that sets them in motion obeys a simple law of physics is of the greatest significance, for it brings with it the hope that we may discover the kind of physical phenomena that lie at the bottom of the orienting reactions of living things.

Perhaps it has occurred to you that I have for a moment wandered away from my immediate subject, for tropisms furnish an example of a purely functional response. They belong to functional physiology rather than to development. This is true. The organism that responds is already there, it is given; the reaction is as perfect the first time it is formed as at any later time. This holds to a high degree in all those reactions that we classify as instincts. When the machine is finished, that is, when it has developed, the response is automatic. Until the last bolt is put in there is no reaction. When the bolt is in place the machine is then ready to perform perfectly the specific movements which it is designed to carry out. This simplifies the functional problem immeasurably.

But biologists are familiar with another set of reactions in animals, in which the whole or large parts of the nervous system and sense organs are involved, that are concerned with a kind of development phenomenon. These types of reactions characterize especially the vertebrates, although they are by no means exclusively confined to them. I refer to the formation of "associations." Here we find the coming-into-being of new things, or conditioned reflexes, or whatever name we may choose to give to the phenomena. What we want to discover is the physical nature of the nervous material that makes possible the building up of such complex types of substitutions and combinations on which the life of each individual may come to depend. We are ourselves, we suspect, largely creatures of our associations and conditioned reflexes that are formed and reformed every day of our lives, but moulded, and set perhaps, at an incredibly early age. We call it training, or education, or ethics—we have many names for the process—but the background of it all is not understood. Here perhaps is a new physics—a whole new world, from which biologists so far have been excluded because, I suspect, the purely physical aspects of the reaction system are unknown. Whether the nature of the association process will be revealed in a physiological or in a physical laboratory remains to be found out. In order to study it our best chance will be to put some physicists in the biological laboratory, and some biologists in the physical laboratory. Meanwhile, we shall have to get along with the kind of arguments that philosophy, metaphysics and introspective psychology have supplied.

Even the wonderful work that the physiologist Pavlow has done by applying objective quantitative methods to the study of associations takes the material basis—the nervous system—as given.

Two opposing and apparently irreconcilable movements have been going on for years in the biological sciences, that relate to the living and the dead. The most courageous leaders of one school assert boldly that there is a fundamental difference between biology and physics; the more timid speak of an irreducible minimum of living phenomena that is not explicable on physical principles and then magnify this minimum into some sort of principle. It would carry us far into the realm of philosophy to attempt to follow out the fine-spun dialectic and logic that has been used in the discussion of these speculations. To-day we are concerned with more practical and less abstruse questions; but there is undoubtedly something at stake here that concerns physics and biology. It involves the method of science.

We speak loosely of experimental science. What is this science? Does it apply only to the physical sciences, astronomy, physics and chemistry, or does it apply equally to the biological sciences? I need not dwell on the absurd idea that to reduce life to physics is crass materialism. This way of condemning all attempts to apply the method of the physical sciences to the phenomena of life is so transparent that it is seldom resorted to any longer. For what is less materialistic to-day than the most advanced views of physicists themselves concerning what used to be called matter, meaning a stone or a brick?

Among the illuminati, however, the objection has shifted to a more refined comparison. The physical world is mechanistic; the biological something more. What the "moreness" is depends on the metaphysical system that each philosopher has up his sleeve.

All this need not detain us, but may be left to the realm of the higher dialectics; for in these comparisons I am trying to make we are not concerned with absolutes and finalities, but with the more prosaic endeavor to discover if possible whether, and in what sense, it is worthwhile to carry on the biological sciences by the application of the methods that experience has taught to be profitable in the physical sciences—profitable in the broadest use of the term. What, in a word, has given to physics her recognized leadership in science? Fortunately there is a pretty general consensus of opinion on this score. Physics has progressed because, in the first place, she accepted the uniformity of nature; because, in the next place, she early discovered the value of exact measurements; because, in the third place, she concentrated her attention on the regularities that underlie the complexities of phenomena as they appear to us; and lastly, and not least significant, because she emphasized the importance of the experimental method of research. An ideal or crucial experiment is a study of an event, controlled so as to give a definite and measurable answer to a question—*an answer in terms of specific theoretical ideas,* or better still an answer in terms of better understood relations. This sounds, I fear, somewhat pompous. Let me give a more concrete illustration. If I set a house on fire I am not performing an experiment. I am simply acting foolishly; but I can employ heat to advantage in a different way. If I study the rate of a chemical reaction at different temperatures, I may discover some relation between the rate of change at different temperatures and be able to deduce, from an inspection of the measurements, certain constant relations between heat and the rate of reaction. I may find that, within a certain range, at least, a definite relation holds. By extending the experiment to other substances I may find the same relation to hold between different temperatures, and feel safe then in predicting for other unknown substances that the same correlation will hold. Surprising as it may seem, this has been found to be true not only for physical events but for those in the biological field. It is the discovery of constants such as these in the biological field that assures us that the method of physical science is applicable to living matter.

Why, then, should anyone hesitate to apply the method of physics to the living world? Living things, as we know, contain no chemical elements that are not found in the inorganic world. They depend for their very existence on an interchange of material with the outside world. The law of the conservation of energy applies to an animal or man as much as to a piece of coal. If we went no further than this, might it not be worth while to find out all we can in the way of method from physics and chemistry and see what they will give us and not indulge in ultra-physical speculations in our present state of ignorance about the simplest conditions relative to living things?

It may be conceded, at once, that in the organic world the conditions are more complex than those with which the physicist has to deal. It is much more difficult to make real experiments that reveal constants, although comparatively easy to set the house afire. Yet a beginning has been made and the success has been as great as was to be looked for. There are, however, two questions of importance concerning the methods of biology that are so significant and fraught with so much danger for the future of experiment in biology that they can not be safely ignored. I refer first to the rôle of mechanism or arrangement in the functional as well as in the developmental phenomena of living organisms; and second, to the disparagement of all attempts to find uniform constants in the reactions and behavior of living things.

My good friend Jennings has, in a recent address at the opening of the Whitman laboratory of experimental zoology at the University of Chicago, stressed one side of the question. I am in close agreement with much that he has to say in the admirable presentation of the method of experimental science as applicable to biology, but there are certain implications in his discussion with which I do not find myself in complete sympathy. Since these questions are intimately bound up with my topic to-day, I should like to devote a few minutes to their discussion.

Jennings points out that something more is needed in the study of living things than a knowledge of the environment and a knowledge of the chemical constitution and physical conditions of the substances that make up the organism. This something-more is its structure, both gross and minute, which is sometimes vaguely spoken of as its organization. Jennings says that, "in the days before experimentation, zoologists had given a romantic and mystical turn to the phenomena of structure in organism; 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 physiologist 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."

Now, all this is a very entertaining account of what happened to morphology, but the picture is overdrawn, and in my opinion not quite fair to physiologists in particular. For while it is true that the student of functional physiology paid scant attention to the evolutionary speculations of morphologists, they can not be said to have neglected the organization of the animals and plants on which they experimented—not only as to their gross structure, but as to the microscopic structure of the cells and tissues also. Recall, for example, the attention that physiologists have given to the structure of the heart as a pumping organ and to the distribution of the blood vessels, even down to the capillaries. Recall again how closely the physiology of the nervous system and sense organs kept in touch with the morphology and minute structure of the brain, nerves and organs of sensory perception. Physiologists accepted these structures as they found them and kept close to their arrangements. It is true, as I have already said, that physiologists have paid little attention to the problem of development, or the coming into existence of the more visible organs and functions during development. They left these problems to the embryologists who, for a long time, were under the lethal influence of the doctrine that "ontogeny repeats phylogeny." But at last a few physiologists did wake up to the situation and began to apply physiological methods to embryological development. They scandalized embryologists by assuming that the egg was little more than a bag of jelly. They spoke in terms of chemistry and physics and quantitative methods but made wide guesses as to the kind of jelly they were dealing with. They often showed an appalling lack of concern as to the visible changes in the egg. They were willing, despite their boasted quantitative method, to call an embryo anything and everything that swam 'round in their finger bowls. It is, I think, neglect of this kind of information that Jennings must have in mind when he says they threw the baby out with the bath.

But even before all this went on, embryologists too had seen the futility of the phylogenetic interpretations and turned their attention to the study of the changes that take place in the egg when it begins to differentiate its regional parts, and they had discovered that, far from being a bag of jelly impinged upon by a salt solution, extensive rearrangements of materials take place that are correlated with the important developments that follow. All this may not be physiology, as yet, but no one who is familiar with the literature of that period will doubt that it is essential to know something about these intrinsic movements if an adequate basis for an understanding of the essential problems of development is ever to be gained. I stand therefore with Jennings if this is what he means when he says that a knowledge of structure is important for the study of development. But when this is agreed to, it still seems to me that we have done little more than prepare the ground for a real physical and chemical study of development, for the essential questions still remain. What, for instance, causes the redistributions of the materials of the egg? It does not suffice to know that a new distribution is present. What we need to find out is how this redistribution brings in its train the local differences that take place. I fail to see how further and further accumulations of this sort can ever lead us beyond a pure description of what takes place in each kind of egg. For these reasons I am not sure that I can follow Jennings when he goes on to say, that "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 one can not directly transfer the experimental results that we have gotten in one field to another field. . . . To this is due the maxim that 'what one organism can't do, another can.' 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 the question proposed."

There are two implications in this statement that may very easily be misleading. In the "early experiences" referred to there were no scientific "principles" at all deduced. A few facts were added and much speculation. It is the *absence* of principles that makes it impossible to transfer the "results" from one field to another. In the second place, there were no "crucial experiments" carried out at all—if this term means what physicists mean when they use it.

Let me quote what Poincaré said not a few years ago. "We all know that there are good experiments and poor ones. The latter will accumulate in vain; though one may have made a hundred or a thousand, a single piece of work by a true master, by a Pasteur, for example, will suffice to tumble them into oblivion. Bacon would have well understood this; it is he who invented the phrase *Experimentum crucis*."

Now, I can not go further into a discussion of these questions. But while I agree with Jennings most heartily that, in the study of organisms, we can not neglect a single detail of their structure, our real problem is not to discover how many kinds of structures exist, but whether there are common principles that run through them all. If there are no such principles, then we are indeed headed towards chaos. We have not gone very far in our analyses it is true, but far enough, I think, to encourage us to go on. Take, for example, cell-division in all its diversity: are we to go on studying the variations resulting from slight differences in structure between the eggs of every species; or is it not better to attempt to discover whether there are not relatively few simple physical *principles* involved in cell-division. If so, then the differences depending on the details of structure may take care of themselves.

There is another question raised by Jennings which, as stated by him, may be easily misunderstood in its implications, although I am confident that Jennings would in general readily agree with much that follows. Jennings cites as typical of scientific method the progress of modern genetics. "At first," he says, "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 thus 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 of 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."

Any one not familiar at first hand with the history of genetics since 1900 might be led to suppose from Jennings' statements that Mendel's laws had been abrogated by the extension that took place as the subject progressed. In fact, however, Mendel's conclusions stand to-day exactly as Mendel stated them. They at least have certainly not become "more arbitrary, more unintelligible." Mendel pointed out that the numerical results that he obtained in his crosses with peas could be explained by two simple hypotheses that we may call the law of segregation and the law of independent assortment. The first law has been found to have wide application. It is the fundamental principle on which all later discoveries rest. It is a broad generalization based on numerical data and allows us to predict with accuracy the outcome of a given situation.

It is true that during the last twenty-six years there has been some progress in the study of genetics. We have found, for instance, that Mendel's second law applies only within certain limits that can be accurately stated, but his two laws still hold for the cases that Mendel studied and for the characters of the majority of animals and plants so far studied. But because linkage and crossing-over have been added to our equipment, and because with knowledge of these we can apply the laws of heredity over a much wider field, is this a reason for stating that because "the systems of structure are different the rules of heredity are different"? It is quite true that the information concerning the arrangement of the elements of heredity has helped us enormously to a fuller understanding of other types of heredity than those that Mendel first discovered, but of course Jennings does not mean to say that this wider knowledge made Mendel's laws more unintelligible, although a casual reader might interpret him in this way.

On the contrary, Mendel, by great insight in interpreting the results of experiments that were carefully and deliberately planned to determine how individual characters are inherited, arrived at two generalizations that are fundamental for all later work. His method was one that a physicist might have been proud of, and without his discoveries I think we might still be floundering around, as had the earlier hybridists, because they did not understand how to ask an intelligent question in order to get an intelligent answer. For it is not every experiment, so called, that will give the answer sought. Nature is sometimes as oracular as the priestess of Delphi. It took even physics a long time to find out how to question nature intelligently. In finding this out it was slowly discovered that certain methods of approach were more satisfactory than others. I have indicated what those methods are, and I think that we biologists can not do better than to borrow them from the physical sciences.

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SOLAR RECORDS IN TREE GROWTH[1]

THE freedom from undergrowth of the pine trees of northern Arizona and their exposure to the characteristic droughts of the country first suggested to the writer the idea that their variations in growth had a climatic and solar origin. Accordingly a long search was made for evidence of such relationship, which has resulted in the identification, dating and measurement of several hundred thousand rings in something under a thousand trees scattered widely about this and other countries. Many interesting results have been obtained and an extensive technique of ring-study has been developed. Of that technique the most important line has been the study of cycles which might have a solar cause.

It is no surprise that variations in climate can be read in the growth rings of trees, for the tree ring itself is a climatic product. In the spring the pines put on a rapid growth of soft white tissue, which continues till lack of moisture, through temperature or dryness, diminishes the growth and stimulates the tree to prepare for winter by putting on a red lignite ring. The growth gradually stops and in sufficiently dry or cold weather comes to an end till the warmth and moisture of spring revive it.

If successive years were exactly alike, the rings would all be of the same size with some alteration with age or injury. But successive years are not alike, and in that difference some factors appeal strongly to the trees. In northern Arizona, with its limited moisture and great freedom from pests and with no dense vegetable population, this controlling factor is identified as rainfall. So it is not astonishing that the pine trees, as tested near Prescott, Arizona, show a correlation with rainfall between 80 per cent. and 85 per cent.

The extent of the dependence of this southwestern rainfall on solar changes is still a point under discussion. Helmann found the sunspot cycle in the rainfall of North Germany, and it has been identified in many places, most notably perhaps in the well-known thirty-five-year Brückner cycle. But the strongest bit of evidence of solar effect on climate is in the trees themselves, for in this region where the trees are specially sensitive to rainfall they show the eleven-year sunspot cycle in a striking manner, and they show it in a double-crested curve just as the rainfall itself in southern California shows it. This has been brought out recently in a dramatic manner. From the start the sunspot cycle was sought in the Arizona pines, and during long parts of their growth it seemed perfectly evident, yet for scores of years it failed, and in 1914 I very nearly gave up the idea that the trees really show it, because near 1700 it failed entirely. Finally in 1919 (Carnegie Publication 289) I gave the facts about it, saying that from 1660 to 1720 the sunspot curve "flattens out in a striking manner," and again, "the sequoias show strikingly the flattening of the curve from 1670 or 1680 to 1727." Early in 1922 I received a letter from Professor E. W. Maunder, of England, calling attention to the prolonged dearth of sunspots between 1645 and 1715 and saying that if there were a connection between solar activity and the weather and tree growth, this extended minimum should show in the weather and in the trees. On receipt of the letter this period was immediately recognized as the interval referred to in which there was entire failure in attempting to trace

[1] Address of the retiring vice-president of Section D—Astronomy—American Association for the Advancement of Science, Philadelphia, December 29, 1926.