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Inhalt: Mast, Loeb's Mechanistic Conception of Life. — Correns und Goldschmidt, Die Vererbung und Bestimmung des Geschlechtes. — Sečerov, Die Zweckmäfsigkeit des Lebens und die Regulation der Organismen. — Klatt, Experimentelle Untersuchungen zwischen Kopulation und Eiablage beim Schwammspinner. — v. Reichenau †.

Loeb's Mechanistic Conception of Life.

By S. O. Mast.

The "analysis of life [psychical and ethical as well as physiological] from a purely physico-chemical view-point" has been Loeb's aim in practically all of his work. In a recent volume, consisting of a number of so called essays, he has made an attempt to present in popular form the more important of the results attained. A book containing what may truly be called the essence of the life-work of a man with a reputation such as Loeb has, especially when it deals with a subject of such profound significance as the phenomena of life, can not fail to be of universal interest.

Practically every fundamental problem of biology is raised in some form or another in this volume altho the author deals specifically with only a few. Fertilization, heredity, morphogenesis and behavior, including psychical and ethical, are the principal problems discussed; they occur repeatedly in the different chapters sometimes with little variation. Regarding these problems Loeb draws the general conclusion based largely on the results of his own investigations, that they have been or can be reduced to purely physico-

1) From the Zoological Laboratory of the Johns Hopkins University.

2) The Mechanistic Conception of Life, by Jacques Loeb, The University of Chicago Press, 1912. 232 pages.

chemical principles, and he maintains that since these problems are among the most fundamental all others can be analysed in the same way. He says (p. 23) "It is not possible to prove in a short address that all life phenomena will yield to a physico-chemical analysis. We have selected only the phenomena of fertilization and heredity, since these phenomena are specific for living organisms and without analogues in inanimate nature; and if we can convince ourselves that these processes can be explained physico-chemically we may safely expect the same of such processess for which there exist a-priori analogies in inanimate nature, as, e. g., for absorption and secretion."

It is evident that everything in this conclusion depends upon the meaning of mechanical and physico-chemical. It will therefore be necessary, first of all, to attempt to ascertain clearly the sense in which Loeb has used these terms. He does not specifically define them, strange as it may seem, especially in a book whose whole argument is rooted in them. We can therefore only infer the ideas he intends to convey by their use. His aim in all of his work is to obtain methods for controlling vital phenomena. He says (p. 195): "It was perhaps not the least important of Darwin's services to science that the boldness of his conceptions gave to the experimental biologist courage to enter upon the attempt of controlling at will the life phenomena of animals." and (p. 196) "All the writer could hope to do was to bring together a few instances of the experimental analysis of the effect of environment, which indicate the nature and extent of our control over life phenomena". I assume that he holds that if such phenomena can be controlled, we have a physico-chemical explanation and that such an explanation is the foundation of a mechanical conception of life. Mechanical and physico-chemical are evidently used synonymously. In fact according to Loeb there is but one kind of explanation; to explain means the same thing as explaining mechanically. He says (p. 58): "All 'explanation' consists solely in the presentation of a phenomenon as an unequivocal function of the variables by which it is determined." As for metaphysics, our author repeats again and again, it is a "mere play on words". (p. 73) Metaphysicians "employ the wrong methods of investigation and substitute a play on words for an explanation by means of facts". and (p. 3) "In certain of the mental sciences . . . everything rests on argument or rhetoric and . . . what is regarded as true today may be expected with some probability to be considered untrue tomorrow".

This definition, like a number of other statements in our volume, appears to me to be exceedingly vague. Superficially it looks clear enough, but as soon as one attempts to apply it to actual cases it assumes a different aspect. It seems to mean nothing more than a statement that nature is orderly and that an explanation of any phenomenon consists merely in ascertaining the position of the phenomenon in the whole series of natural events, that is, in ascertaining the order of events in nature. Probably nearly every one would, at least in a limited sense, accept this as a definition of an explanation, but very few indeed would follow our author in the implied assertion that such an explanation is necessarily physico-chemical or mechanical. It thus appears that these terms have been used in a very loose sense, in my opinion a wholly unjustifiable sense; and much of the controversy regarding Loeb's apparent dogmatic ultra mechanistic tendencies center in this unfortunate circumstance. Not only is mechanism used synonymously with physico-chemism, it is also used synonymously with determinism.

If however such a definition of mechanism be accepted some of Loeb's conclusions necessarily follow, but others appear, even on this basis, to be without foundation. Some of these I wish to consider now. In selecting only questionable matter for treatment in our review it should be emphasised that it is taken for granted that the author's splendid achievement in certain fields is generally recognized, and that limitations of space prevent the consideration of much in which the reviewer would be in agreement with the author.

Loeb maintains that the function of the sperm is twofold. It causes the egg to develop and it serves to transmit male characters. Both of these phenomena, our author holds, have already been largely reduced to physico-chemical principles. He says (p. 20): "The problem of sex determination has, therefore, found a simple solution, and simultaneously Mendel's law of segregation also finds its solution." and (p. 14) "The process of the activation of the egg by the spermatozoon, which twelve years ago was shrouded in complete darkness, is today practically completely reduced to a physico-chemical explanation. Considering the youth of experimental biology we have a right to hope that what has been accomplished in this problem will occur in rapid succession in those problems which today appear as riddles."

What has in reality been solved in connection with these problems? It has been demonstrated, (1) that certain eggs will develop without sperm, some under normal conditions, others when subjected to certain chemical or physical changes in the environment, (2) that the development will not begin in the absence of oxygen, (3) that after development begins the elimination of carbon dioxid increases, (4) that unfertilized or inactive eggs live longer in the absence of oxygen than in its presence, (5) that transmission of hereditary characters including sex is associated with the chromosomes. These facts, admittedly of great importance, or any other facts that have been established in regard to the sperm, do not appear to me to warrant Loeb's conclusions stated above. Practically nothing regarding the chemical changes in the egg preceding and accompanying activation is known. How then can it be maintained, except in the most superficial sense, that this process has been practically completely reduced to physico-chemical principles? The same is true with reference to heredity. The establishment of the fact that heritable characteristics are associated with chromosomes does, indeed, mark a great advance in the study of inheritance. But the statement that this whole problem is practically solved and that we may hope that all the riddles still connected with it as well as all those connected with all other biological phenomena will disappear in rapid succession, must be looked upon largely as the personal opinion of an enthusiast. Many riddles will undoubtedly disappear but some bid fair to stay with us, for example certain features concerning the association of hereditary characters with chromosomes and specific changes within them. Even if we succeed in discovering every chemical and every physical change in every chromosome and precisely how each character is connected with them — and we shall no doubt be able to do much along this line of the greatest value both practical and theoretical — there still remains the riddle as to why they are thus associated. This riddle and others of a similar sort are clearly beyond mechanics, even in the loose sense in which Loeb has used this term, for they involve not only the question of order in nature but also the question of why there is order. What hope is there then in our author's "mechanistic conception" for the solution of such problems.

It is however in the fields of morphogenesis and behavior that anti-mechanists have found the most fertile source of material for their arguments. And it is therefore not surprising to find these subjects rather extensively treated by our author.

He discusses morphogenesis in two different sections and appears to arrive at quite different conclusions as to an explanation of this phenomenon.

He found experimentally, that the place of origin and the direction of growth in a number of organisms is dependent upon gravity and contact. And he concludes (p. 91): "The circumstances that determine the forms of animals and plants are only the different forms of energy, in the sense in which this word is used by the physicist, and have nothing to do with natural selection." In this same section he maintains (p. 108) that the reason why the egg of the sea-urchin normally gives rise to only one embryo "is due simply to the geometrical shape of the protoplasm, which, under normal conditions, is that of a sphere" (in other words if the form were not spherical, there would be more than one) and that the limit to the number of embryos that can arise from one egg "is not due to any preformation, but to other circumstances, the chief one being that with too small an amount of protoplasm the formation of a blastula — from merely geometrical reasons, as there must be a minimum size for the cleavage-cells — becomes impossible." Neither of these conclusions seems to be in accord with the facts. The experiments of Pflüger, Hertwig and others show that when frog's eggs are flattened they still develop into but one embryo; and the work of Conklin, in particular, shows that preformation in the egg has much to do with the determination of the number of embryos that can develop from it.

In this section then Loeb maintains that the form of organisms, including the interrelation of different parts, is regulated directly by the action of gravity, light, surface tension, etc. He rejects in unmistakable terms, quoted above, the idea that natural selection has anything to do with it. In another section however his whole discussion of this problem is in complete harmony with the theory of natural selection.

In this section he postulates, under normal conditions, numerous variations in form, structure and arrangement of parts due to hybridization and maintains that in a large percentage of the individuals which arise thus, these features are of such a nature that they soon die leaving only those which are adapted to their environment. He says (p. 24): "The number of teleosts at present in existence is about 10000. If we accomplish all possible hybridizations 100000000 different crosses will result. Of these teleosts only a very small proportion, namely about one one-hundreth or 1 per cent, can live . . . It is, therefore, no exaggeration to state that the number of species existing today is only an infinitely small fraction of those which can and possibly occasionally do originate, but which escape our notice because they cannot live and reproduce." The whole matter is summed up in the following startling sentence (p. 25): "Disharmonies and faulty attempts in nature are the rule, harmonically developed systems the rare exception." No one to my knowledge ever sketched the theory of natural selection with bolder strokes. Loeb settles the whole question of adaptation and the origin of species in the space of a few paragraphs. What is more, he shows that all of these phenomena are only the product of "blind forces". "Nobody doubts", he says, in concluding a brief argument, (p. 26) "that the durable chemical elements are only the product of blind forces. There is no reason for conceiving otherwise the durable systems in living nature."

Thus it appears that in this section in attempting to account for morphogenesis, Loeb makes use of the fundamental principles underlying the theory of natural selection which was definitely rejected in another section. But his whole argument in support of his contention rests on highly speculative premises, that is, that hybrids occur extensively under natural conditions and that only relatively very few organisms produced thus can live. Moreover, this hypothysis has no bearing on organisms which reproduce only asexually.

His final conclusion regarding the reduction of form production and adaptation to "blind forces" hinges on the meaning implied by the expression "blind forces"; and in the absence of a definition of this extremely vague term, it would be folly either to agree or to disagree with the conclusion. It may be said however that if the author intends to maintain that we have any real insight into the reason why certain chemical elements are durable and others are not (quoted above), further than the fact that this has been observed to be so he will be supported by few if any of those competent to judge in the matter.

The treatment of the problem of behavior occupies nearly one-fourth of the entire volume. Loeb holds that the elements of all behavior both in plants and in animals, including all psychic phenomena, are tropisms. These, he maintains, have been mechanically explained and since the elements have been reduced to mechanical principles all of the compounds constructed from them can be similarly reduced.

A tropism is, according to Loeb, a process of orientation due to the continuous action of the stimulating agent on symmetrically situated sensitive tissues. He says (p. 219): "In . . . heliotropic animals in which the symmetrical muscles participate equally in locomotion, the symmetrical muscles work with equal energy as long as the photochemical processes in both eyes are identical. If, however, one eye is struck by stronger light than the other, the symmetrical muscles will work unequally." In another connection (Dynamics of living Matter, p. 135) he says specifically that tropisms are "a function of the constant intensity". Reactions in accord with these ideas would, of course, result in orientation. The question is are the observed orienting reactions actually of this nature; are they controlled by the continuous action of the stimulating agent? One or both of these questions have been conclusively answered in the negative for the following organisms: *Euglena*, *Stentor* and all of the other unicellular organisms, with the possible exception of *Ameba*, in which the process of orientation has been studied, *Volvox* and all other colonial forms tested, *Hydra*, fireflies, fiddler-crabs and toads. (See Mast, "Light and the Behavior of Organisms".) In most of these forms it has been shown that orientation is dependent upon the time rate of change in the intensity of the stimulating agent on the sensitive tissue and not on its continuous action as Loeb's theory demands. In the rest of them it has been demonstrated that stimulation of symmetrically located sensitive tissue is not necessary in the process of orientation. Loeb maintains (p. 220) that circus movements caused by the destruction of the sensitive tissue on one side of certain animals as observed by Holmes, Parker and others support his theory, but he fails to consider the fact that in a number of these cases the animals after some time tended to recover and orient normally with the sensitive tissue functional on only one side, reactions directly in opposition to his theory. In not a single case has it actually been demonstrated that the response of any organism is ever in accord with Loeb's theory of tropisms, with the possible exception of certain reactions to electricity. This whole theory of tropisms must consequently be relegated to the realm of pure speculation. How then can it it be maintained that tropisms are elementary reactions which have been mechanically explained? And what possible support can the assumption that all behavior is founded on such hypothetical reactions lend to the thesis that behavior is capable of mechanical explanation?

Loeb holds that the orienting reactions in plants and animals are identical, and that this supports his mechanistic ideas on behavior. He says (p. 28): "In a series of experiments I have shown that the heliotropic reactions of animals are identical with the heliotropic reactions of plants." He brings forth several points of identity in support of this contention. All of these are of essentially the same nature. We shall discuss but two of them, the effect of different colors and the effect of different chemicals on reactions.

(1) "In plants", our author says, (p. 29) "only the more refrangible rays from green to blue have . . . heliotropic effects, while the red and yellow rays are little or less effective; and the same is true for the heliotropic reactions of animals." This statement holds, if at all, only in a very general and superficial sense. The region of maximum stimulation in the solar prismatic spectrum for all green plants, as far as tested, is either in the violet or the indigo. Blaauw (1908) found that in the region of maximum stimulation (indigo 465 $\mu\mu$), for oat seedlings, the efficiency on the basis of equal energy is 2600 times greater than in the red, yellow or green. In the unicellular and the lower multicellular animals, as far as investigated, the distribution of stimulating efficiency is similar to that in green plants. Engelmann gives for *Euglena*, blue, 470—490 $\mu\mu$, Harrington and Leaming, and Mast for *Ameba*, violet, indigo, blue; Wilson for *Hydra* violet, indigo, blue, 430—490 $\mu\mu$. But in the higher animals the distribution is not in agreement with this. Lubbock e. g. found the maximum for

Daphnia in the yellow and green. Loeb (1910) confirmed this result, using Lubbock's methods, altho earlier (1905) he had sarcastically rejected Lubbock's results intimating that his methods were faulty. Many other experimental results could be cited in support of the criticism of Loeb's contention stated above.

(2) Loeb says (p. 223): "The writer has shown that the experiments on the effect of acids on the heliotropism of copepods can be repeated with the same results in *Volvox*. It is, therefore, erroneous to try to explain these heliotropic reactions of animals on the basis of peculiarities (e. g., vision) which are not found in plants." Loeb refers here to the fact that when a trace of acid is added to the solution *Volvox* and a number of different copepods have been found to become strongly positive in their reactions to light. But *Volvox* is on the border-line between plants and animals. It is claimed by botanists as a plant, by zoologists as an animal. What support then can this fact lend to the contention that the orienting reactions to light in plants and animals are identical! especially when this is the only known point of similarity in the reactions of these forms and when it is known that the process of orientation in the copepods is radically different from that in *Volvox* and that changes in temperature have precisely opposite effects on the reactions to light in these forms?

It is, however, in the treatment of those forms of behavior known as moral action that our author seems to have wandered farthest on the paths of mysticism and vague dogmatic speculation.

In attempting to reduce ethics to mechanical principles he assumes that all instincts are purely mechanical and says (p. 31): "Our instincts are the root of our ethics and the instincts are just as hereditary as is the form of our body. We eat, drink, and reproduce not because mankind has reached an agreement that this is desirable, but because, machine-like, we are compelled to do so. We are active, because we are compelled to be so by processes in our central nervous system; and as long as human beings are not economic slaves the instincts of successful work or workmanship determines the direction of their action. The mother loves and cares for her children, not because metaphysicians had the idea that this was desirable, but because the instinct of taking care of the young is inherited just as distinctly as the morphological characters of the female body. We seek and enjoy the fellowship of human beings because hereditary conditions compel us to do so. We struggle for justice and truth since we are instinctively compelled to see our fellow beings happy." Thus morality is supposed to rest directly on instinct and heredity. In another section, however, the author appears to arrive at a quite different conclusion. He says (p. 62): „The highest manifestation of ethics, namely, the condition that human beings are willing to sacrifice their lives for an idea is comprehensible neither from the utilitarian standpoint nor from that of the categorical imperative. It might be possible that under the influence of certain ideas chemical changes, for instance, internal secretions within the body, are produced which increase the sensitiveness to certain stimuli to such an unusual degree that such people become slaves to certain stimuli just as the copepods become slaves to the light when carbon dioxide is added to the water." And he concludes after referring to Pawlow's work, "it no longer seems strange to us that what the philosophers term an 'idea' is a process which can cause chemical changes in the body."

Thus he begins with an attempt to found ethics on instincts, which are assumed to be purely mechanical, and ends with the surprising statement, apparently diametrically opposed to this, that chemical reactions in the body are "caused" by ideas. The whole argument intended to reduce ethics to mechanical principles seems to amount to but little more than would a statement that ethical phenomena are mechanical because they are. It certainly must be classified as speculation of the vaguest sort.

Finally our author maintains that all natural phenomena, including our existence, are "only a matter of chance . . . based on the blind play of forces". Precisely what is here implied by this expression I am unable to ascertain, but I assume the author intends it to be synonymous with the phrase "fortuitous concourse of atoms" so much used some fifty years ago. Now, whatever else this phrase may mean it seems clear that it has ordinarily been used with the intention to convey an idea in direct opposition to the fundamental principle of mechanism which is undoubtedly determinism. How can anything that is definitely determined (mechanical) be a mere matter of chance dependent upon the play of blind forces! How can a mechanist maintain that our existence is purely fortuitous! Our author scornfully rejects all metaphysical speculation with the statement that it is a mere play on words and yet he implies in the phrase just quoted that force is a causal agent, a purely metaphysical concept. Even "stereotropism" is clothed with mysterious power to regulate the movement of organisms. "Negative stereotropism," says Loeb (p. 92), "forces the polyps to grow away from the ground into the water, and hence parts surrounded by water form polyps only. Positive stereotropism forces roots in contact with the ground to hold to it, hence parts in contact with the ground give rise to roots only."

It probably is true that all biological phenomena, including ethics, are mechanical in the very loose sense in which our author appears to have used this term, meaning merely orderly, and it may possibly be true that they are mechanical in the strict sense of the term, but the evidence presented in favor of either of these contentions is anything but convincing.

Leaving now the question of the reduction of life processes to mechanical principles, let us consider a few instances in which Loeb's work seems to be open to criticism from other points of view.

In attempting to establish the idea "that vision is based on the formation of an image on the brain" he makes use of two lines of evidence, (a) results obtained in operations on the brain, and (b) observation on the pattern adaptation in fishes.

He says (p. 79) that the experiments of Munk on the brain of dogs show that "there exists a projection of the retina on a part of the cortex" designated as the visual sphere and he maintains in this connection, that these experiments have been confirmed by Henschen and by Minkowski, but on page 35 he says, referring to these same experiments: "Five years of experiments with extirpations in the cerebral cortex proved to me without doubt that Munk had become the victim of an error." His principal source of evidence in support of the thesis in hand is however, he maintains, found in Sumner's work on changes in the pattern of the skin of certain fishes so as to continuously harmonise with the background.

Loeb holds that this work shows that the retinal image is reproduced in the skin. He says (p. 81): "There exists, therefore, a definite arrangement of the images of the different luminous points of the ground on the retina and a similar arrangement of the images of the luminous points on the skin of the fishes", and concludes that "vision is a kind of telephotography".

A careful examination of Sumner's excellent photographs of patterns produced in the skin of flatfishes by different backgrounds shows clearly that the spacial arrangement of light and dark areas in the skin is similar in all. It is essentially the same in fishes over a background consisting of alternate black and white squares as it is over one consisting of alternate black and white stripes or black spots on a white field or white spots on a black field or an irregular arrangement of high light and shadows as is found in nature on gravel bottoms. Sumner says (p. 468): "Squares, cross-bands, circles, etc., were never copied in any true sense, by the fishes." The size of the dark and light areas in the background have a profound effect on the nature of the pattern, but I can find no evidence indicating that their form or their spacial arrangment has any. Where then is there any foundation for Loeb's speculation on the mechanics of vision? What evidence is there that images on the retina are reproduced as such in the brain?

Loeb says (p. 207): "It has often been noticed by explorers who have had a chance to compare the faunas in different climates that in the polar seas such species as thrive at all in those regions occur, as a rule, in much greater density than they do in moderate or warmer regions of the ocean." He holds that the results of a number of investigators show that it requires an increase of about 10^0 in temperature to double the rate of development in organisms, but maintains (p. 209) that the length of life of sea-urchin eggs (fertilised and unfertilised) is doubled if the temperature is decreased only 1^0. And he says: "Lowering the temperature by 10 degrees therefore prolongs the life of the organism 2^{10}, i. e., over a thousand times, and a lowering by 20 degrees prolongs it about one million times. Since this prolongation of life is far in excess of the retardation of development through a lowering of temperature, it is obvious that, in spite of the retardation of development in Arctic seas, animal life must be denser there than in temperate or tropical seas."

If there is anything in this theory we should expect in the temperate zone that pelagic life would be much more abundant in winter than in summer. We should expect our ponds to swarm with microorganisms when they are covered with ice. Of course every one knows that this is not true.

In this instance we have an illustration of a peculiar method of reasoning not rarely found in Loeb's works. He finds that a change of 1 degree at a given temperature produces a given effect on a sea-urchin egg and concludes that a change of 1 degree will produce the same effect in practically all organisms over a wide range of temperatures.

Another illustration of the same tendency to excessive generalization is found in the following statement (p. 45): "Every animal is continually producing acids in its cells, especially carbonic acid and lactic acid; and such acids increase the tendency in certain animals to react heliotropically . . . Fluctuations in the rate of the production of these substances will also produce fluctuations in the heliotropic sensitiveness of the animals. If, for instance, the active mass of the photosensitive substance in a copepod is a relatively small, a temporary increase in the production of carbonic acid can increase the photosensitiveness of the animal sufficiently to cause it to move for the period of a few seconds directly toward the source of light. Later the production of carbonic acid decreases and the animal again becomes indifferent to light and can move in any direction. Then the production of carbonic acid increases again and the animal goes again, for a short time, toward the light." Thus it is clear that our author holds that variability in the response of animals to light is due to variability in the production of acids within them. Superficially this appears to be a very simple and plausible explanation; but Loeb himself found that the reactions to light can be changed by certain alkalies, salts and narcotics as well as by acids, and others have discovered that the same changes can be produced by mechanical stimulation and by changes in temperature. Why then select acid as the controlling factor? Moreover he assumes that substances within the body have the same effect as they do when outside, an assumption which, as far as I am aware. has no foundation in facts.

Of a similar nature is the argument (pp. 96—99) leading to the conclusions that "Growth in animals is determined by the same mechanical forces which determine growth in plants" and "Activity plays the same rôle in the growth of a muscle that the temperature plays in the growth of the seed."

The consideration of only a portion of the known facts regarding many phenomena has made it possible for Loeb to offer extremely simple and attractive explanations for them, explanations which appear superficially plausible especialle to those not thoroly grounded in the subject.

Aside from those already referred to our author has made a number of statements, direct or implied, which, altho of no great consequence in the discussion, are of doubtful validity. For example, (p. 41) "Experiments on the perception of light by our retina have shown that the effect of light equals the product of the intensity into the duration of illumination." (p. 43) Copepods have retinas. (p. 50) More species react to light than to the electric current. (p. 52) Animals are aggregates of independent hereditary qualities. (p. 53) There is no indicatien of adaptation in the reactions of animals to light. If they are positive at all they are positive to all intensities above the threshold. (p. 54) Sudden decrease of intensity of light causes a decrease in movements in planarians. (p. 55) Hypotricha are sensitive to light. (p. 54) Haberlandt, Nemec and F. Darwin do not attempt to explain plant tropisms physico-chemically. (p. 196) "All as a rule or the majority of individuals of a species in a given region spawn on the same day." (p. 174) "The egg membrane in Fundulus possesses a small opening, the so-called micropyle, through which the spermatozoon enters into the egg." (p. 74) "It can be shown that Infusoria, Coelenterates, and worms do not possess a trace of associative memory." (p. 14) "The problem of the beginning and the end of individual life is physico-chemically clear."

After criticising former theories of fertilization as too vague to be useful Loeb says (p. 115): "If we want to make new discoveries in biology, we must start from definite facts and observations, and not from vague speculations." I know of no biologist who would not whole-heartedly subscribe to this doctrine, but with all due respect for our distinguished author I am compelled to say that his "mechanistic conception" appears to me to contain so much vague speculation based upon so few well founded facts, that I fear his practice imperfectly conforms to his precept.

Correns und Goldschmidt. Die Vererbung und Bestimmung des Geschlechtes.

2 Vorträge, Berlin 1913. 72 + 76 S., 10 + 45 Abb., 4,50 Mk.

Das Bändchen enthält die auf der Naturforscherversammlung zu Münster 1912 gehaltenen Vorträge in erweiterter und durch Hinweise auf neuere Veröffentlichungen ergänzter Fassung. Correns bespricht unter dem Titel Experimentelle Untersuchungen über Vererbung und Bestimmung des Geschlechtes die fast ausschließlich durch Bastardierungsversuche gewonnenen Einblicke in die Tendenz der Keimzellen, das eine oder andere Geschlecht hervorzubringen. Kein anderer Forscher hat auf diesem Gebiete so große Erfolge zu verzeichnen wie der Verfasser, dem es durch sorgfältig durchdachte Anwendung der Mendel'schen Spaltungsgesetze gelungen ist, einige besonders günstige Fälle klarzulegen.

Da ursprünglich wohl überall beim Auftreten der Sexualität beide Geschlechter demselben Individuum zugeteilt waren und sich dieser Zustand bei den Pflanzen meist erhalten hat, während die Metazoen zur Getrenntgeschlechtigkeit übergegangen sind, müssen wir den tierischen Hermaphroditismus da, wo er sich findet, als sekundär erworben ansprechen, während er bei den Pflanzen primär ist. Es ist das wohl ein Grund dafür, dass sich Blütenpflanzen als besonders geeignet für derartige Versuche erwiesen.

Einen Anhalt zum Eindringen in die vorliegenden Fragen gaben jene Pflanzen, die selbst zweigeschlechtig zwitterige Verwandte besitzen, mit denen sie gekreuzt werden können. Denn ähnlich wie eine Analyse der Erbeinheiten eines Organismus auf Grund der Spaltungsregel erst dann möglich wird, wenn eine Bastardierung mit Individuen vorgenommen werden kann, die in irgendeiner oder mehreren Erbeinheiten abweichen, so lässt sich auch die Vererbungstendenz einer Keimzelle nur aus ihrer Wirkung auf eine in der Verteilung der Sexualität verschiedene Spezies entnehmen. Solche Verschiedenheiten in der Geschlechterverteilung finden sich z. B. in den Gattungen *Satureia, Melandrium* und *Bryonia*. Besonders die letztgenannte bietet relativ einfache Verhältnisse, da *Bryonia alba* zwitterig, *Bryonia dioica* getrenntgeschlechtig ist. Wird eine der beiden Arten mit sich selbst bestäubt, so erhält sich das Verhältnis der Geschlechter, indem *Br. alba* lauter Zwitter, *Br. dioica* zur Hälfte Männchen, zur Hälfte Weibchen gibt. *Br. alba* ♀ + *Br.*

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