Biologisches Centralblatt.

Unter Mitwirkung von

Dr. K. Goebel und Dr. R. Hertwig

Professor der Botanik Professor der Zoologie

in München,

herausgegeben von

Dr. J. Rosenthal

Prof. der Physiologie in Erlangen.

Vierundzwanzig Nummern bilden einen Band. Preis des Bandes 20 Mark.
Zu beziehen durch alle Buchhandlungen und Postanstalten.

Die Herren Mitarbeiter werden ersucht, alle Beiträge aus dem Gesamtgebiete der Botanik an Herrn Prof. Dr. Goebel, München, Luisenstr. 27, Beiträge aus dem Gebiete der Zoologie, vgl. Anatomie und Entwickelungsgeschichte an Herrn Prof. Dr. R. Hertwig, München, alte Akademie, alle übrigen an Herrn Prof. Dr. Rosenthal, Erlangen, Physiolog. Institut, einsenden zu wollen.

Bd. XXVIII. 1. Oktober 1908. № 19.

Driesch's harmonic equipotential systems in form-regulation.

C. M. Child.

(Schluss.)

B. Proportionality in Relation to Position.

As regards the proportionality of primordia from different regions and poles, I showed (Child, 1907b) that in primordia form different regions and poles of the stem typical differences in proportionality exist. These differences consist in differences in the relative lengths of different parts of the primordia and may reach almost 50% (cf. Child, 1907b, p. 427, Table XIII and adjoining text). To take a concrete example: The distal tentacle-area (area II) is about 48% longer in aboral primordia from the extreme proximal regions of long stems, than it is in oral primordia from the extreme distal region of the same stems. In the same cases the proximal tentacle area (area IV) is 31% longer in the aboral proximal than in the oral distal primordia. Other examples may be found in the table referred to.

Such differences are not merely chance or irregular differences, but are characteristic, and typical in direction, and their amount depends chiefly on the distance in the original stem between the regions where the primordia compared are formed and on the polar position of the primordia. Moreover, these differences cannot be

due to the fact that primordia of different sizes arise in a perisarc-tube of approximately the same diameter, for the changes in the distal tentacle-area (area II) are nearly double those in the proximal tentacle-area (area IV), as is shown in my table (Child, 1907b, p. 427, Table XIII). Driesch states (1908, p. 413) that I find that „die Proportionalität in den positiven Anlageteilen des neuen Hydranten sehr strikte bewahrt ist und dass nur die Zwischenstücke in ihrer Länge gegen sie verstoßen." I thake the „positiven Anlageteile" to be the tentacle-areas (incidentally why should they be called „positive"?). But I have shown that the changes in the distal tentacle-area are in most cases nearly twice as great as those in the proximal tentacle-area.

Can we speak of approximate proportionality when such differences appear as those which I have shown to exist? I believe we must conclude that the proportions of the parts of the hydranth-primordium are typically different according to the position of the primordia in the stem, i. e., their distance from the oral end, and also according to their polar position. In short the localization of the parts of the primordium is not exen approximately constant in relation to the length of the primordium. Tubularia then does not agree with the characteristics of Driesch's harmonic equipotential systems which have been quoted above.

And finally, in the last of my papers on Tubularia, I showed that it is possible to control experimentally to some extent the proportions of the hydranth-primordium in Tubularia, or in other words to determine the formation of primordia of typical different proportions by different experimental conditions (Child, 1907g, pp. 317—322). These experiments are briefly as follows: the four areas of primordia developing from the oral end of the stems after removal of fully developed hydranths were measured and compared with those of primordia developing from the oral ends of stems after the removal of primordia. Primordia developing after the removal of primordia are somewhat shorter than those developing after the removal of fully developed hydranths (Child, 1907g, p. 318, Table I), but comparison of their proportions shows most clearly that the distal portions, especially area I (Child, 1907g, p. 319, Table II), are relatively considerably longer, and the proximal regions, especially area IV, are relatively considerably shorter in primordia following primordia than in those following fully developed hydranths. Evidently the development of a hydranth induces changes in the stem proximal to it. When we remove developing primordia, as in my experiments, these changes have affected the distal part of the region where the new primordium will form, more than the proximal, but by the time the hydranth has attained full development the changes in the stem proximal to it extend at

least over an area equal to the whole length of the new primordium. Consequently a primordium following a primordium has disproportionately long distal parts as compared with a primordium following a hydranth.

The differences in this case are certainly not due to the development of primordia of different sizes in the perisarc-tube of approximately the same size, neither are they due to stretching of the stem proximal to the primordia removed, for at the time of their removal the stretching had not begun.

In these cases it is very clear that conditions or processes in one part of the stem play a part in determining the character of morphogenesis in another part. The proportions of the primordium differ according to the conditions to which the stem has been subjected before its isolation. In this case, then, external factors in Driesch sense (See p. 579) are not and cannot be excluded, and if they play a part in this case, there is every reason to suppose they do in others. In short, these experiments show positively that the proportions of the primordia in Tubularia are determined in some degree by conditions external to them, and, what is also important, conditions which cannot be excluded in any experiment.

As to the use of mathematics in cases like that Tubularia, I agree perfectly with Driesch (Driesch, 1908, p. 412). My measurements of Tubularia were made for exactly the same purpose as his own. I was of course well aware that Driesch did not regard proportionality in Tubularia as mathematically exact: anyone who examines a few primordia cannot fail to be convinced on this point. On the other hand, Driesch did not measure all of the different areas of the primordia separately, and he confined his measurements to primordia from certain regions of the stem. Consequently his measurements fail to show what mine show very clearly, viz., a typical and practically constant difference in proportion between primordia from different regions and poles. His data are simply insufficient to establish his conclusion that proportionality is approximately maintained in Tubularia, and more complete series of measurements would have made such a conclusion impossible, unless Driesch's idea of what constitutes approximate proportionality is very different from my own. But in any case Tubularia does not agree with the definitions of harmonic equipotential systems, which have been quoted above. We do not find that „es steht hier nämlich „jeder" der möglichen Effekte zu jedem anderen in einem ganz festen relativen Lageverhältnis" (Driesch, 1899a, p. 73).

IV. Later definitions of harmonic equipotential systems.

Driesch's later definitions of harmonic equipotential systems seem to differ rather widely from those which have already been

quoted. For example, in one of his reviews of developmental physiology he says: „Bekanntlich nenne ich harmonisch-äuquipotentielle Systeme solche Formganze, bei denen eine Differenzierungs- oder Wachstumsgesamtleistung in ihren Einzelheiten jeweils einzelnen Elementen des Ausgangsganzen zufällt, derart, dass jedes Einzelne dieses Ganzen jedes Einzelne jener Leistung vermag, alles Einzelne aber derart in Harmonie steht, dass die Leistung selbst ein Ganzes ist" (Driesch, 1905b, p. 679).

And again in his discussion of my work: „Ein harmonisch-äquipotentielles System liegt vor, wenn von dem Fragment eines originalen Ganzausgangs das morphogenetische Resultat als Ganzes geliefert wird, derart, dass die zu diesem Resultate führenden Einzelleistungen auf die Einzelelemente (Zellen) des Fragments, so wie es da ist, verteilt werden. Das Resultat ist „ganz", wenn ihm kein wesentlicher Teil seiner Zusammensetzung fehlt und die Anordnung der Organisationskonstituenten im großen und ganzen die normale ist" (Driesch, 1908, p. 414).

These definitions contain nothing concerning proportionality. Moreover, on another page of this latest paper the following reference to the matter of proportionality appears in connection with the discussion of my measurements of Tubularia: „Nur ganz unbestimmt und allgemein hat unter solchen Umständen das Wort „proportional" einen Sinn: es soll nur ausdrücken, dass z. B. bei allgemeiner Verkleinerung jeder einzelner Teil kleiner wird, und nicht etwa der eine größer wird und der andere unverändert bleibt" (Driesch, 1908, p. 412).

I find it quite impossible to reconcile this statement concerning proportionality with those which have been quoted from Driesch's earlier papers. Moreover, according to the definitions of harmonic equipotential systems just quoted not even approximate proportionality is necessary provided the result is a „whole". These seem to me to be very different definitions from those quoted in section II of this paper in which he says: „es steht hier nämlich jeder der möglichen Effekte zu jedem anderen in einem ganz festen relativen Lageverhältnis — eben seine Zahl und sein Lageverhältnis zu jedem anderen Effekt ist hier ein ganz wesentliches spezifisches Merkmal." And again: „dass Alles was — in jedem einzelnen Falle entsteht, zueinander in ganz bestimmte Beziehungen gesetzt ist." So far as I am aware Driesch has nowhere called attention to this difference in his different statements. Are we then to regard „ein ganz festes relatives Lageverhältnis" of the various parts to each other as „ein ganz wesentliches spezifisches Merkmal" of a harmonic equipotential system, or are those cases in which, in addition to the other characteristics, „die Anordnung der Organisationskonstituenten im großen und ganzen die normale ist" har-

monic equipotential systems? If the earlier definitions are correct Tubularia is certainly not a harmonic equipotential system; if these new definitions are correct then all that Driesch has said concerning the importance of proportionality in these systems is valueless. For example, the formula $x = gA$, which Driesch regards as the expression „für das eigentlich Lebensautonome" in localization with decrease of size in Tubularia (Driesch, 1901, pp. 174—180; also p. 583 above) can be obtained only by assuming mathematically exact proportionality in Tubularia. If Driesch believes that „nur ganz unbestimmt und allgemein hat unter solchen Umständen das Wort „proportional" einen Sinn" what possible significance can a formula derived from the assumption of mathematically exact proportionality posses for Tubularia? If the formula has no significance then it is impossible to determine that the „Entelechiekonstante A" exists in the case of Tubularia: or in other words, it is impossible to discover „das eigentlich Lebensautonome" in the phenomena of regulation with decrease of size in this species.

Actually then, the differences in Driesch's different definitions of the harmonic equipotential system leave us in a dilemma as to what such a system actually is. Furthermore, if typical regional, polar and dimensional differences in localization occur, as they do in Tubularia, they must be the result of some physiological factor or factors existing at the different regions or poles, i. e., of an „extensive Mannigfaltigkeit" or a „machine" in Driesch's sense and cannot be due to the „intensive Mannigfaltigkeit" or „entelechie". If, notwithstanding these differences, Tubularia is still a harmonic equipotential system, then it is impossible to obtain evidence from it or from any other such system in which such regional or polar or dimensional differences appear, for the hypothesis of „Lebensautonomie".

In short, if exact proportionality is an essential characteristic of a harmonic equipotential system, Tubularia is not such a system, and if proportionality is not essential then the entelechy-constant cannot be derived from the phenomena of regulation in such systems, since the derivation of this constant assumes exact proportionality.

V. Fact and regards hypothesis in certain special cases.

As regards another so-called harmonic equipotential system, viz. the sea-urchin egg[2]) Driesch himself (Driesch, 1900, 1902b)

2) I am glad to acknowledge an error of statement contained in the words; „others have shown that the eggs of the sea-urchin and the ascidian are not harmonic equipotential systems as Driesch supposed them to be" (Child, 1907e, p. 145). This statement is of course entirely incorrect so far as it concerns the sea-urchin egg, as Driesch has pointed out (Driesch, 1998, p. 409). As regards the ascidian egg it is not incorrect, though I should have mentioned the fact that

discovered an apparent inequipotentiality in the polar direction, „eine gewisse Differenz des Eiplasmabaues in „animal-vegetativer" Richtung —, welche zwar nicht ausreicht, dem Keim den Charakter eines harmonisch-äquipotentiellen Systems zu nehmen, aber doch der Sonderentwickelung einzelner Elemente desselben Widerstände verschiedener Intensität entgegensetzt" (Driesch, 1900, p. 407). He suggests by way of interpretation that „ein allmähliches Starrerwerden des Plasmas" takes place. In later papers he has referred to this case as showing an „Einschränkung", „Verundeutlichung", or „Maskierung" of the harmonic equipotentiality. The question at once arises as to the differences in actual experiment between an inequipotential system and an equipotential system with „masked" equipotentiality. How is the one to be distinguished from the other? So far as I am aware Driesch has not given us a basis for such distinction.

It seems to me important to distinguish between fact and assumption in this case. The fact is that the sea-urchin egg is not equipotential in certain respects. Driesch assumes that the equipotentiality is „masked" or made indistinct by certain physical characteristics of the cytoplasm. What reasons are there for such an assumption? The only one which I am able to find is that Driesch must interpret visible inequipotentiality in this manner, i. e., as a „masking" of the real equipotentiality, or else must admit that the sea-urchin egg is not an equipotential system in the „animal-vegetative" direction. In other words this interpretation is merely an attempt to save the general hypothesis as applied to this particular case; the sea-urchin egg is assumed to be equipotential, and visible inequipotentialities must then be assumed to be something non-essential.

If Driesch admits that my observations on Tubularia show anything that cannot be regarded as approximate proportionality, he may readily interpret them as a „masking" of the equipotentiality, if proportionality has any connection with it. If, however, as Driesch seems at present to believe, proportionality is involved in only the most vague indefinite sense, then I see no possibility of distinguishing between equipotentiality and inequipotentiality.

Driesch's conception of the „normal" or typical form as a „Zweck" toward which the course of morphogenesis is directed involves a sharp distinction between „normal" and „abnormal" or „atypical". The entelechy determines the normal form and individual departures from the norm or failures to attain it are due to incidental physical or chemical conditions, or are „errors" etc. It is not difficult to

---

the results of Conklin and Driesch concern different genera. The error on my part was due, however, to inadvertence, not to ignorance of the literature as Driesch so kindly suggests.

see that facts may become of relatively slight importance in the light of such conceptions, for facts can neither prove nor disprove their correctness. But it is also easy to see that these conceptions do not afford a basis for „proof" of the „Autonomie der Lebensvorgänge", for while the facts can be interpreted in this way, such interpretation is far from necessary. The sea-urchin egg, for example, is not a harmonic equipotential system along the polar axis in fact, but merely ex hypothesi.

In his various discussions of harmonic equipotential systems Driesch has referred repeatedly to Planaria as constituting such a system. In connection with these references it is stated that „regeneration" in the stricter sense does not occur or is very slight in Planaria. For example, he says that „eigentliche Regenerationserscheinungen — hier gerade nicht vorliegen" (Driesch, 1899a, p. 54), and again „wird eine Planaria der Quere nach in Stücke geschnitten, und wird dann eines der Operationsprodukte kontinuierlicher Beobachtung unterzogen, so sieht man, wie an diesem Stück neue Wachstumsvorgänge nur in ganz geringem Maße auftreten — leider gestattet die Natur des Objektes eine intimere histologische Untersuchung nicht" (Driesch, 1899a, p. 55). And in a later paper (1901, pp. 180—181) he again speaks of the „geringfügige Regenerationsprozesse."

It is of course a matter of personal opinion as to what the limits of „geringfügige Regenerationsprozesse" may be, but Driesch's assertion quoted above, that eigentliche Regenerationserscheinungen — hier nicht vorliegen" is certainly incorrect. Examination of the living animals shows very clearly the formation of an „Anlage" composed of new tissue formed at the cut surface and this undergoes gradual differentiation. Moreover, Flexner (1898) found abundant mitosis in the region of the cut surface: in Morgan's work (1898, 1900, 1901) it is sufficiently clear that at least the terminal regions of the parts removed are replaced by new tissue in all cases. Bardeen (1902) found both mitosis and amitosis involved in the development of the new tissue, and I have been able to confirm his results (Child, 1907h). It is also clear from the work of Morgan and other later investigators that the whole head and often a considerable region posterior to it and in many cases almost the whole, in short pieces from the anterior region the whole, of the postpharyngeal region, i. e., half of the body, are formed by true regeneration in Driesch's sense. And finally, Driesch's assertion that the nature of the object does not permit a more exact histological investigation is certainly far from correct, for as a matter of fact it is not in the least difficult to determine that there is really very little „Substanzverlagerung" so far as actual cells are concerned, though of course a transfer of substances

in the form of nutrition does occur. Even the new pharynx, when formed in the old tissue, as is commonly the case, is the result of localized cell division, at first chiefly mitotic, later chiefly amitotic, i. e., even in this case an „Anlage" is formed by cell division and later undergoes differentiation. In her recent paper on this subject Stevens (1907) has assumed that cell migration occurs extensively in addition to cell division. Steinmann (1908) has pointed out the insufficiency of her evidence for cell migration, and since frequent cell division has been observed by Flexner, Bardeen and myself the assumption of cell migration is unnecessary.

But, leaving these later results out of consideration, it was sufficiently evident from the earlier work that regeneration is an important factor in regulation in Planaria.

This particular case has constituted a considerable difficulty to me in my attempts to obtain a clear idea of Driesch's exact conception of the harmonic equipotential system. In my work on Cerianthus, I decided after some hesitation that, so far as the occurrence of true regeneration was concerned, this form corresponded as closely to Driesch's of harmonic equipotential systems general definitions as did Planaria, since in Cerianthus the actual amount of regeneration is under most conditions proportionally much less than in Planaria. I therefore called attention to the fact that as regards proportionality Cerianthus does not correspond to Driesch's definition since proportionality is not maintained with decrease in size of the piece (Child, 1905). In one of his reviews of literature Driesch says concerning this work: „Wir glauben zwar vornehmlich in seinen Beobachtungen an *Cerianthus*, bei dem anfänglich unproportionale Regenerate in ihrem zu dicken Stammabschnitt allmählich dünner und länger werden, doch Restitutionen unseren Sinnes erblicken zu müssen" (Driesch, 1905 b, p. 68). This statement appears in a section devoted to the consideration of harmonic equipotential systems, and so far as I can see, means that Driesch regards Cerianthus as a harmonic equipotential system. But elsewhere in the same paper he says that „Child den analytischen Begriff des harmonisch-äquipotentiellen Systems gänzlich missverstanden hat, wenn er in der Meinung, diesen Begriff ad absurdum zu führen, beibringt, dass Cerianthus von Bruchstücken aus, umbekümmert um deren Größe, gleiche Anteile nach beiden Richtungen regeneriere, dass also keine Proportion zwischen Stamm und Regenerat bestehe. Bei allen Regenerationen handelt es sich ja eben um komplex-äquipotentielle Systeme in meinem Sinne" (Driesch, 1905, p. 791). Later I attempted to defend my position by comparing Cerianthus with Planaria (Child, 1907 e, pp. 140—141), and to this Driesch has again replied: „Ich habe

den Begriff des harmonisch-äquipotentiellen Systems ja gerade konstruiert für solche Restitutionserscheinungen, die nicht Regenerationen sind" (Driesch, 1908, p. 410).

The facts of the case are then these. Driesch has included Planaria in his list of harmonic equipotential systems, but has practically ignored the part which true regeneration plays in regulation in this form. Cerianthus, which is certainly as much a harmonic equipotential system as Planaria, so far as method of regulation is concerned, Driesch regards at one point as such a system, while elsewhere he takes the opposite view because regeneration occurs in Cerianthus. I am quite willing to admit that I find myself unable to arrive at Driesch's real meaning with regard to this point.

I note also that Moszkowski (1907) regards the actinian body as a harmonic equipotential system. His conclusions are based on a study of *Actinia aequina* and *Actinoloba dianthus* in which restitution occurs in essentially the same manner as in Cerianthus.

It is beyond the scope of the present paper to discuss the essential differences between harmonic equipotential and complex equipotential systems, though I believe it can readily be shown that what Driesch regards as the essential differences are either hypothetical or purely differences of degree. In fact, if I understand Driesch's latest definition of the harmonic equipotential system, its actual existence seems to me somewhat doubtful. In this definition (Driesch, 1908, p. 414) it is stated that „die zu diesem Resultate (i. e., the whole) führenden Einzelleistungen auf die Einzelelemente (Zellen) des Fragments, so wie es da ist, verteilt werden". It is the clause „so wie es da ist" of whose meaning I am uncertain. If it means that no changes which play a part in determining the morphogenetic result are brought about by the act of isolation, then I believe that the harmonic equipotential system does not exist in nature, for the data of experiment indicate that the changes in the piece resulting from its isolation are, at least in many cases and probably in all, very important factors in the following localization and differentiation. If the clause means that localization is accomplished in such cases without the formation of new cells or new material, then certainly Planaria is not such a system, and in Tubularia cell division occurs to a considerable extent before visible differentiation. As regards the other adult forms the data ore not conclusive, but isolated blastomeres (e. g. in the sea-urchin) evidently become wholes without the formation of new cells, and the question as to whether new material is formed in these cases is at present idle.

VI. Driesch's first „Beweis der Lebensautonomie".

This first „proof" is briefly stated as follows in „Die Seele" (Driesch, 1903, p. 74): „Eine Maschine bleibt nicht dieselbe, wenn man ihr beliebige Teile nimmt oder ihre Teile beliebig verlagert; deshalb kann das sich auf Basis harmonisch-äquipotentieller Systeme abspielende Formbildungsgeschehen kein maschinelles chemisch-physikalisches Geschehen sein."

In a later work the argument is given somewhat more fully. „Es ist nun klar, dass sich „Entwickelung" allgemein, wie sie von einem natürlich gegebenen, ungestörten, ganzen Keim aus vor sich geht, wohl möchte prinzipiell physikalisch-chemisch verstehen lassen, falls man sich den Keim als eine zwar in ihren Einzelheiten unter der Grenze selbst mikroskopischer Sichtbarkeit liegende Maschinerie außerordentlich komplizierter Art vorstellte.

„Ist aber zugegeben, dass der Ausgang, aus dem sich ein so kompliziertes Ganzes, wie ein Tier, entwickelt, nur eine äußerst komplizierte Maschine allenfalls sein könne, so muss notwendigerweise solche Maschine auch für jeden Ausgang eines relativ gleichen „Ganzen", also auch für jeden der „Ausgänge", welche bei unseren harmonisch-äquipotentiellen Systemen zu „verkleinerten Ganzen" führen können, gefordert werden."

„Diese „Ausgänge" aber sind beliebig, und jedes Element derselben kann jedes, wobei die Harmonie bewahrt wird."

„Also müsste jedes Element unserer Systeme gleichzeitig jeden Teil der supponierten Maschine, ja sogar jeden Teil in jeder beliebigen Größe darstellen."

„Das ist sinnlos."

„Damit ist aber gezeigt, dass die „Differenzierung harmonisch-äquipotentieller Systeme" überhaupt nicht, jedenfalls nicht nur auf Basis einer aus chemisch-physikalischen Faktoren kombinierten Maschine, dass sie also nach anderer Gesetzlichkeit, als sie aus dem Anorganischen bekannt, also „autonom" vor sich geht" (Driesch, 1904, p. 115).

The argument is also stated in much the same form in the „Vitalismus" (Driesch, 1905, p. 201—208).

Several points require consideration in connection with this „proof". In the first place, according to Driesch the development of a harmonic equipotential system becomes an „autonomistic problem" only when external conditions in the broadest sense, i. e., including all effects arising from other parts, are excluded, and when all elements of the part are prospectively alike (See Section I of this paper). But, as I pointed out, it is absolutely impossible to exclude external conditions in Driesch's sense completely in any case, and moreover, a machine, i. e., a „typische chemisch-

physikalische Spezifitätskombination (Driesch, 1901, p. 187) may exist whose parts are prospectively alike. Such a machine may exist in a isolated part and constitute the basis for future localization. In other words the conditions under which morphogenesis in harmonic equipotential systems becomes an „autonomistic" problem cannot be realized in nature. Driesch's „proof" applies, therefore, only to a postulated not a real case.

It is evident from Driesch's argument quoted above that he makes no distinction between a prospective potential „machine" and a real machine.

The reason for his failure to make such a distinction apparently lies in his belief that external conditions including the relations with other parts play no part in the development of harmonic equipotential systems. I have shown that external conditions cannot be excluded in any case, but even if we admit for the sake of argument that they may be excluded, it is not necessary to accept Driesch's conclusion that each element of such a system according to the mechanistic hypothesis must represent at the same time every part of every machine which it is capable of forming in the future. Conceivably at least localized differences, i. e., parts of a real machine, visible or invisible may exist which are capable in consequence of their relations to each other of bringing about new localizations, i. e., of forming a new machine. But this new machine does not exist as such until it is formed. Up to that time it exists merely as the „properties" and relations of the parts of the old machine and these do not posses the typical space configuration which the machine itself possesses when it is formed.

But as a matter of fact there is no case in regulatory development in which these factors alone are involved. As I pointed out above (p. 581) the possible mechanistic factors involved in the formation of a whole from a part are in their lowest terms: first, the constitution of the part and the regional differences which exist in it in consequence of its previous differentiation as a part of an organism; second, the internal changes which result from its isolation, which are undoubtedly localized; third, the changes in relation to the extra-organic environment which result form the formation of new surfaces of contact with the medium, new terminal regions etc., and these are also localized to a greater or less extent. Similar possible factors, though differing in specific character are involved in those cases where a whole is formed after dislocation of the parts. And it is impossible to exclude any of these factors in any experiment.

In short Driesch's assertion that the mechanistic hypothesis must assume that all parts of all potential machines are present as such, i. e., in typical space configuration at the beginning is not

correct. A mechanistic interpretation is possible if each part constitutes a part of only one machine at any given time, and there is no reason to suppose that it constitutes anything more than this.

To put the matter briefly, the act of separation of the part, or of dislocation of the parts establishes directly or indirectly the conditions for the formation of a new machine, which did not exist as such before this act. Before there existed merely a potence, i. e., a possibility of forming this new machine under certain conditions: this potence exists in the constitution of the original machine and in the conditions, but it does not possess the space configuration of the new machine itself. The formation of new machines which did not exist as such previously is a familiar phenomenon in inorganic nature, e. g., in the formation of a flame from a combustible substance under certain conditions.

But whether the new machine shall be similar to the old in cases of regulatory development, and indeed whether any new machine shall be formed, depends both upon the constitution of the part or parts and upon the conditions which are present. It is possible, for example, to prevent the formation of a hydranth in a piece of Tubularia stem in many different ways, e. g., by inserting the end in sand, by closing it with wax etc. If the harmonic equipotential system which Tubularia is supposed to be can accomplish the processes of development independently of external conditions the reason for the failure to develop under these conditions is not apparent.

Driesch states the case as if the formation of the new whole occurred in all cases, but this is very far from being true. I am of course aware that Driesch assumes the entelechy acts in conjunction with „mechanical" conditions or employs these conditions as „Mittel", but I fail to see the necessity for assuming the existence of the entelechy and the assumption of „Mittel" seems to be merely a second hypothesis for the purpose of rendering the first plausible.

Driesch's use of the word „beliebig" is a good illustration of the case in point. In the first statement of his „proof" quoted above he says: „eine Maschine bleibt nicht dieselbe, wenn man ihr beliebige Teile nimmt", in the second „diese Ausgänge aber sind beliebig".

In experiment, however, it is not in the least difficult to isolate pieces of Tubularia or of any other of Driesch's harmonic equipotential systems which are incapable of forming a whole, i. e., which do not remain the same, or as I should prefer to put it, do not possess the same potences as the whole. Evidently then the organism, like Driesch's postulated machine does not remain the same „wenn man ihm beliebige Teile nimmt". In all cases so

far known a minimal size exists, below which no piece is capable of forming a whole. Driesch is of course perfectly familiar with this fact, but it plays no part in his general conclusions concerning these systems. But the existence of such a minimal size-limit is of itself a very strong argument for the existence of an „extensive Mannigfaltigkeit" i. e., a „machine" as the basis of the processes leading to the formation of a new whole. It should also be noted that the minimal size-limit is not determined solely by the amount of material present. It may differ widely in different regions of the body: in Planaria, for instance, minimal pieces from certain regions are several times as large as those from other regions and I have found that the size of the minimal piece differs with age (these results are not yet published). In Tubularia also (Child, 1907 f) the size of minimal pieces differs considerably in different regions of the body, being much less in proximal than in distal regions.

Moreover, in certain cases, e. g., in Planaria, axial heteromorphosis is particularly characteristic of certain regions of the body, and the same is true, though in less degree of Tubularia. Such facts as these cannot be ignored or interpreted as mere incidents in any consistent and logical hypothesis of regulation.

Driesch's second „proof" of „Lebensautonomie" is as follows: „Eine nach den drei Dimensionen typisch spezifisch verschiedene Maschine bleibt nicht ganz, wenn sie geteilt wird, deshalb liegt der Genese äquipotentieller Systeme mit komplexen Potenzen im Bereiche des Formbildungsgeschehens kein maschinelles, chemisch-physikalisches Geschehen zugrunde" (Driesch, 1903, p. 74). This „proof" is also given in later works (Driesch, 1904, pp. 116—118; 1905 a, pp. 208—211).

This is open to the same objections as the first: the original machine in the organism does not remain the same when it is divided, but the act of division results under certain conditions in the realization of a new machine which previously existed only as a potence or possibility.

Driesch's argument for „beinahe unendliche" complexity of the machine which, according to the mechanistic hypothesis, must underlie the phenomena of „normal" development (Driesch, 1904, p. 115; 1905 a, pp. 206—207) also fails to take account of the fact that the complexity is largely potential at the beginning of development and is therefore not necessarily represented at that time by a corresponding „typische chemisch-physikalische Spezifitätskombination". Herbst's conclusion that „wir sind also nicht imstande, nachzuweisen, dass die Zahl der Verschiedenheiten im Anfange der Entwickelung geringer ist als die Gesamtzahl der im Laufe der Ontogenese stattfindenden Differenzierungsprozesse; d. h.

alle maschinellen Einrichtungen, welche zu letzteren notwendig sind, müssen bereits im Ei gegeben sein" (Herbst, 1901, p. 117), rests on a similar basis. Moreover, it is not necessary to conclude that the egg cannot be simpler than the adult organism because we cannot at present prove that it is simpler. And when we take into account all the facts, i. e., the conditions of development as well as the constitution of the egg, the assumption that the egg is simpler than the organism attains a higher degree of probability than any other.

We are then, I believe, justified in concluding that, so far as it concerns form regulation, Driesch's „Autonomielehre" with its „proofs" is simply a hypothesis, and a hypothesis which at present has no solid basis in facts. A few of its characteristic features may be summed up as follows: it proceeds at various points as if the present status of our knowledge of the physiology of organic form were final; it assumes the exclusion of factors which cannot actually be excluded in any experiment; it seems to assume that the capacity for future or prospective likeness in the parts or elements is equivalent to present likeness, so far as morphogenesis is concerned; it practically ignores „atypical" results of experiment; in certain cases, e. g., as regards Tubularia, it assumes that visible localized differences are the only localized differences existing; it interprets actual inequipotentiality in certain cases as „masked" equipotentiality; and finally different statements concerning certain points, e. g., proportionality in harmonic equipotential systems, are not consistent.

We are indebted to Driesch for many facts and analytical concepts of great value, but not as yet for a proof of „Lebensautonomie" in form regulation.

Hull Zoological Leaborotory. University of Chicago. June 1908.

Bibliography.

Bardeen, C. R., 1902. Embryonic and Regenerative Development in Planarians. Biol. Bull. Vol. III, Nr. 6, 1902.

Child, C. M., 1904 a. Studies on Regulation. V. The Relation between the Central Nervous System and Regeneration in Leptoplana: Posterior Regeneration. Journ. Exp. Zool. Vol. I, Nr. 3, 1904.

— 1904 b. Studies on Regulation. VI. The Relation between the Central Nervous System and Regulation in Leptoplana: Anterior and Lateral Regeneration. Journ. Exp. Zool. Vol. I, Nr. 4, 1904.

— 1905. Form-Regulation in Cerianthus. IX. Regulation, Form, and Proportion. Biol. Bull. Vol. VIII, Nr. 5, 1905.

— 1907 a. An Analysis of Form-Regulation in Tubularia. I. Stolon-Formation and Polarity. Arch. f. Entw.-Mech. Bd. XIII, H. 3, 1907.

— 1907 b. An Analysis etc. II. Differences in Proportion in the Primordia. Arch. f. Entw.-Mech. Bd. XXIII, H. 3, 1907.

— 1907 c. An Analysis etc. III. Regional and Polar Differences in the Re-

lation between Primordium and Hydranth. Arch. f. Entw.-Mech. Bd. XXIII, H. 3, 1907.

Child, C. M., 1907 d. An Analysis etc. IV. Regional and Polar Differences in the Time of Hydranth-Formation as a Special Case of Regulation in a Complex System. Arch. f. Entw.-Mech. Bd. XXIV, H. 1, 1907.

— 1907 e. Some Corrections and Criticisms. Arch. f. Entw.-Mech. Bd. XXIV, H. 1, 1907.

— 1907 f. An Analysis of Form-Regulation in Tubularia. V. Regulation in Short Pieces. Arch. f. Entw.-Mech. Bd. XXIV, H. 2, 1907.

— 1907 g. An Analysis etc. VI. The Significance of Certain Modifications of Regulation: Polarity and Form-Regulation in General. Arch. f. Entw.-Mech. Bd. XXIV, H. 2, 1907.

— 1907 h. Amitosis as a Factor in Normal and Regulatory Growth. Anat. Anz. Bd. XXX, Nos. 11 and 12, 1907.

Driesch, H., 1893. Die Biologie als selbständige Grundwissenschaft. Leipzig 1893.

— 1894. Analytische Theorie der organischen Entwickelung. Leipzig 1894.

— 1899 a. Die Lokalisation morphogenetischer Vorgänge. Arch. f. Entw.-Mech. Bd. VIII, H. 1, 1899.

— 1899 b. Studien über das Regulationsvermögen der Organismen. 2. Quantitative Regulationen bei der Reparation der Tubularia. Arch. f. Entw.-Mech. Bd. IX, H. 1, 1899.

— 1900. Die isolierten Blastomeren des Echinidenkeimes. Arch. f. Entw.-Mech. Bd. X, H. 2—3, 1900.

— 1901. Die organischen Regulationen. Leipzig 1901.

— 1902 a. Über ein neues harmonisch-äquipotentielles System und über solche Systeme überhaupt. Arch. f. Entw.-Mech. Bd. XIV, H. 1—2, 1902.

— 1902 b. Neue Ergänzungen zur Entwickelungsphysiologie des Echinidenkeimes. Arch. f. Entw.-Mech. Bd. XIV, H. 3—4, 1902.

— 1902 c. Neue Antworten und neue Fragen der Entwickelungsphysiologie. Ergebn. d. Anat. u. Entwickelungsgesch. Bd. XI (1901), 1902.

— 1903. Die „Seele" als elementarer Naturfaktor. Leipzig 1903.

— 1904. Naturbegriffe und Natururteile. Leipzig 1904.

— 1905 a. Der Vitalismus als Geschichte und als Lehre. Leipzig 1905.

— 1905 b. Die Entwickelungsphysiologie von 1902—1905. Ergebn. d. Anat. u. Entw.-Gesch. Bd. XIV (1904), 1905.

— 1908. Über einige neuere „Widerlegungen" des Vitalismus. Arch. f. Entw.-Mech. Bd. XXV, H. 3, 1908.

Flexner, S., 1898. The Regeneration of the Nervous System of *Planaria torva* and the Anatomy of the Nervous System of Double-headed Forms. Journ. of. Morphol. Vol. XIV, Nr. 2, 1898.

Herbst, C., 1901. Formative Reize in der tierischen Ontogenese. Leipzig 1901.

Morgan, T. H., 1898. Experimental Studies of the Regeneration of *Planaria maculata*. Arch. f. Entw.-Mech. Bd. VII, H. 2—3, 1898.

— 1900. Regeneration in Planarians. Arch. f. Entw.-Mech. Bd. X, H. 1, 1900.

— 1901. Growth and Regeneration in *Planaria lugubris*. Arch. f. Entw.-Mech. Bd. XIII, Heft 1—2, 1901.

Moszkowski, M., 1907. Die Ersatzreaktionen bei Actinien (*Actinia aequina* und *Actinoloba dianthus*). Arch. f. Entw.-Mech. Bd. XXIV, H. 3, 1907.

Steinmann, P., 1908. Untersuchungen über das Verhalten des Verdauungssystems bei der Regeneration der Tricladen. Arch. f. Entw.-Mech. Bd. XXV, 1908.

Stevens, N. M., 1907. A Histological Study of Regeneration in *Planaria simplicissima, Planaria maculata Planaria* and *morgani*. Arch. f. Entw.-Mech. Bd. XXV, H. 2, 1907.