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Driesch's harmonic equipotential systems in form-regulation.

C. M. Child.

The importance of „harmonic equipotential systems“ in Driesch's autonomistic hypothesis is known to all who are familiar with his later work. One of his „Beweise der Autonomie der Lebensvorgänge“ is based upon the supposed method of regulation in such systems.

My own experimental work has, up to the present, suggested conclusions very different from those of Driesch and I have called attention from time to time to facts which seemed scarcely in accord with his views. Driesch's replies to some of my criticisms are polemical rather than critical; in other cases, as I have pointed out (Child, 1907e), his statements of my experimental results and conclusions are incorrect, in consequence of misapprehension, and in some instances he has cast doubt upon the correctness of my observations, or has ignored them. His polemics require no further consideration, and as regards many other points, reference to the earlier discussions (Driesch, 1905b, 1908: Child, 1907e) is sufficient for the present.

It seems desirable, however, in the interests of both theoretical biology and the experimental method, to examine in the light of the available facts Driesch's conception of the harmonic equi-

potential system, together with certain other features of his hypothesis, in order to determine if possible whether the present status of our knowledge actually justifies the position which he maintains. I venture, therefore, to present the following consideration, in spite of the risk involved of eliciting from Driesch a further expression of his somewhat unflattering opinions with which I am already familiar. The frequency with which the critics of this author have been formed with opinions of this sort serves in some degree to lessen their effect. Reference to Driesch's criticisms will be made only so far as they duely concern the subject in hand.

I. Certain of Driesch's assumptions.

The consideration of Driesch's (Driesch, 1893, 1894 etc.) ideas of organic form as expressed in his earlier writings is beyond the scope of the present paper, though we find here certain assumptions and views which, if accepted, lead very naturally and logicaly to his later conclusions. For example, in his „Analytische Theorie" (1894, p. 128) appears the following: „Man hat nun wohl die Ansicht geäußert, dass jedes Stadium der Ontogenese die notwendige Folge des vorhergehenden und die Ursache des folgenden sei; dieser Satz ist aber nicht ohne weiteres zuzugeben, denn erstens ist die Ontogenese kein einheitlicher Vorgang, sondern ist aus vielen teilweise voneinander unabhängigen und in eben dieser Unabhängigkeit gegebenen Vorgängen zusammengesetzt, zum andern verstehen wir, wie erörtert, keinen dieser Vorgänge aus seiner Ursache auch nur einigermaßen." It is of course quite true that at certain stages different processes of ontogeny may proceed independently of each other, but it does not follow that each stage is not the necessary result of the preceeding and the cause of the following, not is it necessarily true that ontogeny is „kein einheitlicher Vorgang", at least as regards the determination and initiation of the various processes. Processes occurring independently at a certain stage may have been initiated in direct or indirect relation to each other, or they may be the results of processes so initiated. It is impossible to determine whether ontogeny is an „einheitlicher Vorgang" or not until we know it from the beginning. Present inability to understand the processes from their causes may well be, and doubtless is, because we know the causes only in small part. Such assertions as this of Driesch's are, to say the least not at present justified by the facts.

Turning to the development of the conception of the harmonic equipotential system, we find that this is likewise connected with certain assumptions. In „Die Lokalisation morphogenetischer Vorgänge" (Driesch, 1899a, p. 77) it is pointed out that the localization of ontogenetic processes becomes a peculiar problem when

„harmonisch-äquipotentielle Systeme mit primärer Richtungsorganisation sich bei Ausschluss äußerer Ursachen differenzieren. Unter dem Ausdruck „äußere Ursachen" sind hier nicht nur im strengen Sinne die Faktoren der Außenwelt verstanden, sondern wenn die Differenzierung eines Systems in Frage kommt, das Teil eines Organismus ist, auch alle etwa von anderen Teilen desselben auf ihn geschehenden Beeinflussungen, wie sie z. B. in attraktiven, taktischen Reizwirkungen, bei Berührungen etc. vorliegen; auch solche Reizarten müssen also, damit unser Problem auftrete, ausgeschlossen sein.

„Sind auch sie, und damit die äußeren Ursachen überhaupt, ausgeschlossen, so wird eben deshalb die Lokalisation der Differenzierungsvorgänge ein Problem, weil sich mit Hilfe der von uns wissenschaftlich gekannten Ursachsarten nicht verstehen lässt, wie es nun an jenem in sich prospektiv gleichartigen Material zum Eintritt einer Verschiedenheit gerade an diesem oder gerade an jenem Ort komme."

Here Driesch apparently becomes involved in serious error. If I understand him, his argument is essentially that there is nothing among the causes known to science to account for the localization of morphogenetic processes in material which is prospectively alike and which differentiates with exclusion of all factors external to it. This argument seems to involve the tacit assumption that where prospective likeness exists present likeness must also exist, for if differences exist within the material then a basis for further localization exists, at least a possible basis, and Driesch's problem cannot become a real problem until it is demonstrated that such existing differences are not the basis of further localization. Actually, however, present likeness does not follow from prospective likeness. Material at present widely different may still possess the capacity for producing like effects under conditions different from those now existing. In an organism the parts may possess similar prospective potences but may at present differ from each other widely, since they are or have been under different conditions. Moreover, the past or present relations may play a part in determining which of the prospective potences shall be realized in a particular case.

A simple illustration will serve to make this point clear: All units (theoretically all atoms) of a given substance, e. g., oxygen, are prospectively alike, i. e., they possess the same potentialities, yet oxygen in combination with carbon (CO or CO_2) is different from oxygen in combination with hydrogen (H_2O). Moreover the oxygen in these different substances may behave differently when the substances are placed under identical conditions, i. e., different potentialities may be realized in the two cases. Yet under these and other conditions the prospective potences of the oxygen remain the same.

As long as a given part of an organism remains a constituent of the whole it is affected more or less by processes and conditions arising from other parts, and these may induce specific localized differences in it, but without necessarity altering the prospective potence of the various regions. When the part is isolated the effects of other parts upon it cease, but the localized differences existing may determine the localization and character of following processes. In other words, the past history of material which is „prospektiv gleichartig" may establish a basis for the localized realization of different potentialities in different parts of material in a particular case. In Planaria, for example, the regulatory processes occur in visible and in part experimentally determined relation to visible localized differences which exist in the piece, and in cases where visible differences do not exist, there is no reason for assuming that they ar not present.

Moreover, it is absolutely impossible to exclude entirely what Driesch calls external conditions in the separation of a part from other parts. The act of separation itself alters the complex of external conditions to which the part is subjected, both in that it eliminates all influence of other parts and in that it brings new regions into direct relation with the surrounding medium.

Assuming that prospective likeness is equivalent to present likeness, and that external factors in the widest sense can be excluded, the problem of localization is simply the problem of the self-production of heterogeneity from homogeneity. But Driesch has never presented any real evidence to show that any case of ontogenetic or regulatory development is of this kind. When we look for proof we find merely assertions that it is so, but without evidence, and none of his experiments constitutes proof. That such assumptions must be made „damit unser Problem auftrete" I readily grant, but it still remains to be proven that the problem is real and not a „Scheinproblem", ,and I believe that such proof is at present impossible.

As regards Tubularia Driesch apparently does not even consider the possibility of localized differences which are not visible. He says: „Ist doch die einzige am Stamm in der Längsrichtung vorhandene Differenz die, dass er an einem Ende eine Wundfläche hat, am anderen nicht (i. e., after the removal of the hydranth). In bezug auf diese Wundfläche, aber nicht an ihr wird eben in typischer Lokalisierung, die Hydrantenanlage gebildet, das ist alles, was wir sagen können; mehr können wir auf Grund dessen, was wir über formative Mittel wissen, eben nicht sagen, und so tritt uns denn die elementare Natur des Problems der Lokalisation wieder in ganzer Schärfe auch hier entgegen" (Driesch, 99a, p. 51). The facts in the case are that no visible differences exist except

the wounded surface at one end and that the tentacle-ridges, which are the most conspicuous parts of the hydranth-primordium do not arise at the cut surface, but the distal end of the hydranth is at the cut surface in all cases. If Driesch's conclusion is justifiable on the basis of these facts then we are justified in concluding that every scientific problem which is at present unsolved is elementary in nature.

But let us examine the case of Tubularia somewhat more fully, in order to determine whether possible factors of localization exist, and whether any of the data of experiment afford any evidence with regard to such factors. In the first place, every piece which is capable of becoming a new whole possesses some degree of polarity, and experimental data indicate that this polarity consists in differentiation or specification along the axis (Child, 1907g): in Tubularia polarity disappears completely so far as morphogenesis is concerned in pieces below a certain size. There is no ground for believing that polarity is a fundamental property of protoplasm and that each element or particle possesses it. If polarity is simply axial difference in specification or in correlation of parts, then it may afford a basis for localization, though perhaps insufficient alone to account for all details.

Secondly, the act of separation establishes a new „end" or terminal region and the new hydranth forms at this end. The tip of the manubrium coincides in position with the end of the piece. The first visible changes leading to the formation of a new hydranth begin at the end, and the phenomena in short pieces suggest that the various parts of the new whole are localized in succession, beginning at the end (Child, 1907d, 1907f). For Driesch these small pieces which give rise to partial structures are simply „atypical", a sort of „freak of nature" and can teach us nothing concerning „normal" phenomena. But if we attempt to analyse the process of morphogenesis in Tubularia on the basis of all the phenomena instead of a part, the assertion that the elementary nature of the problem appears clearly becomes, even with our present knowledge, almost absurd.

The possible „mechanistic" factors involved in the formation of a whole from a part are therefore, when reduced to the lowest termes: first, the constitution of the piece 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, i. e., from the cessation of the action of other parts upon it, and these are undoubtedly different in different regions; third, the changes in relation to the extra-organic environmant, resulting from formation of terminal regions by the act of separation, or the exposure of new surfaces to direct contact with

the medium, etc. These also are localized to a greater or less extent. None of these possible factors can be excluded in actual experiment. Until Driesch has analyzed these and demonstrated that they are insufficient, his hypothesis deserves but little consideration as a proof „Lebensautonomie" so far as form regulation is concerned.

II. Earlier definitions of the harmonic equipotential system.

Driesch's systems are discussed and defined for the first time in his „Lokalisation morphogenetischer Vorgänge" (Driesch, 1899a). The definition of the harmonic equipotential system given here is as follows: „Jedes Element kann Jedes" (p. 73), and as a further characterization, „es steht hier nämlich jeder der möglichen Effekte zu jedem anderen in einem ganz festen relativen Lageverhältnis, auch tritt, mit wenigen Ausnahmen, jeder Effekt nur einmal oder doch in bestimmter Zahl auf: eben seine Zahl und sein Lageverhältnis zu jedem anderen Effekt ist hier ein ganz wesentliches spezifisches Merkmal" (p. 73).

In another paper (Driesch, 1899b) he says concerning these systems: „die Proportionalität alles Geschehens bleibt bei beliebiger Entnahme von Material des Systems bewahrt."

In „Die organischen Regulationen" (1901) his definition is as follows: „Es kann an solchen Systemen jedes Element jede der überhaupt möglichen Einzelheiten gleichermaßen leisten" (p. 171), and further: „dass Alles was kraft der vorliegenden Potenzen wirklich, in jedem einzelnen Falle entsteht, zueinander in ganz bestimmte Beziehungen gesetzt ist" (p. 172).

In all of these definitions the idea of proportionality is apparently important: at least, I so interpret the expressions „ganz festen relativen Lageverhältnis" and „ganz bestimmte Beziehungen" and the assertion that „seine Zahl und sein Lageverhältnis zu jedem anderen Effekt ist hier ein ganz wesentliches spezifisches Merkmal."

Moreover in „Die organischen Regulationen" (1901, pp. 172—179) Driesch develops certain mathematical formulae as expressions of certain characteristics of harmonic equipotential systems. We are concerned here only with the formula for change in localization with change in absolute size. Driesch begins his consideration of this point as follows: „Fragen wir uns zunächst, ob sich nicht ein Ausdruck finden lässt, der etwas darüber aussagt, wie sich die Lokalisation eines bestimmten Effektes mit der absoluten Größe des Systems ändert. Wir wollen dabei den einfachen Fall der Hydrantenreparation die Tubularie zugrunde legen, an der wir nur ebene Verhältnisse zu berücksichtigen brauchen, nämlich nur die Abstände der in Betracht kommenden Teile vom distalen Ende.

Es ist hier konstatiert, dass von einer bestimmten Stammlänge an abwärts, die Längen der Gesamtanlageareale verschiedener Objekte sich annähernd verhalten wie ihre Stammlängen, und dass ferner ein bestimmtes Anlagegebilde, etwa der Anlagering des proximalen Tentakelkranzes, zu einem anderen Anlagegebilde oder zur Gesamtanlage des gleichen Individuums immer annähernd in derselben Längenproportion steht.

„Wir wollen annehmen, dass diese Proportionalitäten ganz strikte bewahrt bleiben (was sie natürlich wegen der Vernachlässigung des Räumlichen und namentlich deshalb, weil die Tubularia wegen ihres festen Perisarks ja in allen Versuchen einer Serie denselben Stammdurchmesser, bei allein wechselnder Länge aufweist, nicht tun)." The development of the formula does not concern us at present, but merely the formula itself, which is $\frac{x}{g} = A$ or $x = gA$, in which x is the distance of the distal end of a given organ-primordium from the distal end of the stem, g is the length of the piece and A is a constant.

According to Driesch this formula shows us that „die Örtlichkeit des Geschehens bei der Differenzierung harmonisch-äquipotentiellen Systems ist in einfachen Fällen ihrer Größe direkt proportional" (1901, p. 176). He also says: „Auch der Ausdruck für das eigentlich Lebensautonome oder wie der übliche, besser zu vermeidende Ausdruck lautet, für das „Vitalistische" am Geschehen ist in dem $x = gA$ . . . enthalten (1901, p. 178).

As regards this formula it is to be noted that it rests wholly on two assumptions; first, that the length of the whole hydranth-primordium in pieces below a certain size is strictly proportional to the length of the piece; and second, that strict proportionality exists between the various parts of the hydranth-primordium with decrease in size of the whole. Driesch himself admits as quoted above that these assumptions do not correspond with the facts, yet he regards the result as an expression „für das eigentlich Lebensautonome . . . am Geschehen." In short, the facts themselves do not give us such an expression. But again Driesch assumes that strict proportionality is not maintained simply because hydranths of different sizes develop within a perisarc of approximately the same diameter and the measurements concern only linear dimensions of space. In the following section our present knowledge concerning proportionality in Tubularia is briefly considered.

III. Proportionality in Tubularia.

A. Proportionality in Regulation to Length of Piece.

In one of his studies on regulation in Tubularia, Driesch (1899 b) showed that in pieces below a certain size a decrease in

the length of the primordium occourred with decrease in the length of the piece, and that this decrease in length of the primordium amounted to about 50 % when the length of the piece was double the length of the typical hydranth-primordium. He also showed that the two tentacle-areas of the primordium maintain approximate proportionality during this decrease. He did not show, however, that the length of the hydranth-primordium decreases proportionally to the length of the piece, or even approximately proportionally, yet he asserts, as quoted above, that in pieces below a given length the lengths of the whole primordia in different pieces are approximately proportional to the lengths of the respective pieces. His experiments showed simply that in pieces below a given length decrease in length of the piece is accompanied by decrease in the length of the primordium formed in it, but they did not even show that such decrease was approximately proportional; in fact measurements show clearly that it is not proportional, though Driesch did not make use of the facts which concern this point.

In my study of Tubularia I found from a considerable number of measurements that the length of the hydranth-primordium decreases less rapidly than the length of the stem in all cases (Child, 1907 f, p. 289, Table III). Moreover, I found, on examination of Driesch's measurements, that they showed exactly the same thing. I have given the results of Driesch's measurements in Table IV of my paper (Child, 1907 f, p. 289). They show very clearly for all groups compared that the length of the hydranth-primordium decreases less rapidly than the length of the piece.

In Driesch's recent criticism of my work (Driesch, 1908 [1]),

1) So far as a critical consideration of this paper is desirable it will be found in the following pages. Some considerable part of it, however, consists either of reassertion of the authors views, without actual criticism of the results of my work, or of condemnation of that work, and to these portions no reply is necessary.

In support of my earlier criticism (Child, 1907 e) that Driesch had failed at various points to state both the results of my experiments and my conclusions correctly, it is perhaps worth noting that Driesch's second attempt to state the results of my work on Leptoplana (Child, 1904 a, 1904 b) is a failure like his first. His first statement was: „Nach Child nimmt bei Leptoplana das Regenerationsvermögen nach hinten zu ab" (Driesch, 1905 b, p. 694). I called attention to the incorrectness of this statement (Child, 1907 e, p. 142), and pointed out that my experiments on Leptoplana showed that posterior regeneration is qualitatively complete at all levels (I should have added, posterior to the cephalic ganglia), but is quantitatively incomplete in the absence of food: that anterior rsgeneration, however, is always qualitatively incomplete at all levels posterior to the cephalic ganglia, as I had stated in my original paper. In his reply to my criticisms Driesch corrects his earlier statement as follows: „Nach Child nimmt bei Leptoplana das Vermögen, die qualitativ richtigen Regenerate quantitativ vollständig auszubilden, nach hinten zu ab, wenn keine Nahrung gereicht wird (Driesch, 1908, p. 408). This statement is not even correct for posterior regeneration, for as a matter of fact posterior regeneration in the absence of food is much more nearly complete quanti-

p. 413) he such as this, and this is by no means an isolated case, make Driesch's frequent imputations to others of carelessness

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tatively in the posterior than in the anterior regions. Moreover, Driesch has not yet stated at all the rather important result that regeneration in the anterior direction is always not merely quantitatively, but qualitatively incomplete at all levels posterior to the cephalic ganglia, whether food is present or not. Repeated incorrect statements, says: „Wo also in aller Welt habe ich von mathematisch-strikter Proportionalität geredet? Das wüsste ich wahrlich gern. Und ferner wäre mir lieb zu wissen, wo bei seinen Proportionalitätsmessungen an Hydrantanlagen von Stammstücken verschiedener Größe oder verschiedener regionaler Herkunft oder verschiedener Polarität denn Child auch nur irgend etwas gefunden hat, was von meinen wahren Befunden und Aussagen, wie sie vorliegen, abweicht?" The answers to these questions are to be found in the above quotations from Driesch's „wahren Befunden und Aussagen, wie sie vorliegen", and my remarks concerning them. Driesch's formula $x = gA$ for „das eigentlich Lebensautonome — am Geschehen" is impossible without the assumption of strict mathematical proportionality. Moreover, he has nowhere stated that the lengths of the primordia decrease less rapidly than the lengths of the pieces. What he did state was „es ist hier (i. e., for Tubularia) konstatiert, dass von einer bestimmten Stammlänge an abwärts, die Längen der Gesamtanlageareale verschiedener Objekte sich annähernd verhalten wie ihre Stammlängen" (Driesch, 1901, p. 174). I must still believe that this statement does not express my own results, for I found that under certain conditions the length of the piece might decrease more than twice as rapidly as the length of the primordium.

Driesch has also accused me (Driesch, 1908, pp. 412—413) of imputing to him the belief that a mathematically exact proportionality exists in Tubularia. This I have never done, I have maintained and must still do so on the basis of the above quotations, that Driesch's conception of the harmonic equipotential system is based on the assumption of mathematically exact proportionality. The formula $x = gA$ has been discussed above: in the definitions quoted we find the expressions „ganz festen relativen Lageverhältnis" and „ganz bestimmte Beziehungen". If words possess any definite meaning, it seems to me that we are forced to interpret these various expressions as signifying exact proportionality. I was familiar with Driesch's various statements to the effect that only approximate proportionality existed in Tubularia, but I cannot find that he has ever stated that there are characteristic regional, polar and dimensional differences in the proportions of the primordia.

But Driesch (1908, p. 413) still asserts, in spite of my data, that approximate proportionality exists in Tubularia. Final decision upon this point is impossible until Driesch defines approximate proportionality. But a brief reference to some of my data will show the basis for my conclusions. In my measurements (Child, 1907f, p. 289, Table III), where pieces of given lengths compared, I found that in pieces of 4 mm and 2 mm respectively the difference in the lengths of the primordia was in one series about 9 %, in another 21 %, i. e., the length of the pieces hat decreased 50 % and the length of the primordia, in one case 9 %, in the other 21 %.

In pieces of 6 mm and 4 mm respectively, a decrease in length of the pieces, of 33,3 %, the decrease in length of the primordia was 5 % and in pieces of 6 and 2 mm respectively, a decrease in length of the pieces of 66,7 %, the decrease in length of the primordia was 25 %. Is this approximate proportionality between the length of the primordium and the length of the piece?

Driesch, in his measurements (Driesch, 1899 b, Table VIII, p. 119), grouped pieces of three different lengths in eech class, e. g., he compared pieces, 3, 4, and

and misunderstanding somewhat less effective than they might otherwise be.

As regards my „reaction energy" (See Child, 1907b, 1907d: Driesch, 1908, pp. 414—415), it is perhaps not far removed from the facts as Driesch's „entelechie".

Driesch (1899b, pp. 120—121) has attempted to explain teleologically the proportional decrease in length of the primordium with decrease in the length of the piece. His explanation is essentially, that the new hydranth must posses a stem in order to emerge from the perisarc, and that it is also „zweckmäßig" that a region capable of future „reparation" should be present. Consequently when the piece is to short to permit the formation of a hydranth-primordium of full length and also a stem, the length of the primordium decreases. But, as the measurements — both Driesch's and my own — show, the shorter the piece the less the relative length of the stem proximal to the hydranth. Consequently in pieces sufficiently short, we may expect to find hydranths alone produced, without any stem, or even distal parts of a hydranth without proximal portions. All who are familiar with the phenomena of regulation in short pieces in Tubularia know that very short pieces do actually produce structures of these kinds. In short the phenomena in longer pieces with decreasing length foreshadow those which occur in very short pieces. Driesch disposes of the phenomena in very short pieces as follows: „In der Tat unterbleibt ja, wie früher geschildert, bei sehr weit oralwärts dem ursprünglichen Individuum entnommenen Stückchen, die sich sehr rasch reparieren, jene quantitative Regulation: aber was resultiert daraus? eine durchaus atypische Bildung oder zwar ein ganzer Kopf, der aber wegen fehlenden oder zu kleinen Streckstückes nicht aus dem Perisark frei wird, oder auch, wenn er etwa durch die Bewegungen seiner Tentakeln sich frei macht, bald abstirbt und keine reparative Potenzen besitzt" (1899b, p. 121). These are results which we do not „teleologisch sehr wohl verstehen", but this apparently is of no importance to Driesch, for they are either „ganz atypische

5 mm with pieces 1,5, 2 and 2,5 mm in length without separating the different absolute lengths. But since the decrease in length of the primordium depends in part upon the absolute lengths of the pieces compared and not merely upon their relative lengths, Driesch's measurements give simply average results for relative decrease of 50% in pieces of different absolute length. Consequently his figures, when treated in the same way as my own show in general greater decreases in the lengths of the primordia for a given decrease in the length of the pieces (Child, 1907, p. 189. Compare Tables III and IV). If he had compared pieces of a single given length with pieces of another given length his actual percentages would have been different and much nearer my own, and he would not have found that the decrease in the length of the primordium was 50% in pieces double the length of the typical primordium, but much less than that.

Bildungen" or else they „soon die and are incapable of reparation". According to Driesch's definition of regulation, it includes only cases where a return or approach to the „normal" condition occurs (Driesch, 1901, p. 92), hence abnormal or atypical results are unimportant. Nevertheless, I believe that these pieces present certain problems of considerable importance: first, why does any localization and development occur in pieces too short to form wholes? second, why do partial structures of relatively large size occur in some pieces, while others of the same length give rise to wholes of smaller size? third, why do partial structures always represent the distal portions of hydranths, never the proximal? Driesch offers no solution to these problems but says merely: „klar ist nur, dass irgendwelche „innere Mittel" hier in Frage kommen" (Driesch, 1908, p. 414). He does not even mention my results and suggestions but refers merely to his and my own „ziemlich wenig besagende Vermutungen".

With respect to the regional occurence of partial and atypical structures in the stem, Driesch says in this paper: „Sie tun es nicht (i. e., they do not represent harmonic equipotential systems), wenn sie gar zu jungem Gewebe entstammen, also dem Originalstamm sehr weit distal entnommen sind" (Driesch, 1908, p. 414).

In view of the fact that both Morgan and I have shown positively that stemless hydranths and partial structures may appear in any region of the stem, provided the pieces are sufficiently short (for literature see Child, 1907 f), it is somewhat surprising to find Driesch repeating his original views on this point without reference to the observations of others. If he has read my paper he cannot be ignorant of the facts. Does he believe that the observations of Morgan and myself are incorrect or does he simply prefer to ignore them?

I have shown (Child, 1907 f) that there is a complete gradation between results that Driesch regards as typical and the stemless hydranth and distal partial hydranth. Shorter pieces are necessary for the formation of partial structures and stemless hydranths in the proximal than in the distal regions of the stem, and this fact is correlated with the well known fact that shorter hydranths appear in pieces from the proximal regions, whether the pieces are long or short, than in pieces from the distal regions. In short, there is every reason to believe that the same factors which give rise to wholes in longer pieces give rise to the stemless hydranths and the partial structures in the shorter pieces.

To my mind the formation of distal partial structures and stemless hydranths in very short pieces from any region of the stem constitutes in itself a very serious objection to Driesch's hypothesis: it is impossible, as Driesch practically admits, to under-

stand these pieces teleologically. What is it that determines that localization shall occur at all in these pieces? It certainly cannot be the „entelechy", and if not then typical localization of morphogenetic processes is possible in Tubularia without the guidance of the entelechy. If it is possible in these short pieces, why not in other long pieces?

The phenomena of regulation in these short pieces show most clearly that Driesch's sharp distinction between the „typical" and „atypical" in form is not only useless but false as regards Tubularia, for structures wich Driesch regards as typical, i. e., the hydranths of reduced length with stems actually show the same characteristic, viz., a greater decrease in proximal than in distal structures with decreasing length of the stem, which leads in still shorter pieces to the formation of stemless hydranths and distal partial structures.

To sum up concerning short pieces: Both Driesch's measurements and my own show that in pieces below a certain length the localization of each „effect" or part of the primordium differs typically in its relative position in the piece with every further decrease in the length of the piece. Proportionality between the length of the primordium and the length of the piece is therefore not even approximately maintained if I unterstand the word „approximately" but changes in a typical direction.

It should also perhaps be noted that this change cannot be accounted for by the fact that hydranth-primordia of different sizes arise within the perisarc, which is of approximately the same diameter in all cases (cf. Driesch, 1901, p. 174). If the differences were due to this factor, we should expect the measurements to show that the length of the primordia decreased more rapidly than the length of the piece, but as a matter of fact, the measurements show exactly the opposite. I mention this point because Driesch, in his criticism of my work (Driesch, 1908, p. 413f footnote 1) has called attention to his earlier statements upon this point. The quotations which Driesch gives from his earlier papers (Driesch, 1908, p. 411) do not help the matter, so far as I can see, for they do not concern the real point involved. I believe I have answered his questions fully as to where he has made the assertions concerning which he inquires. (Schluss folgt.)

Nochmals Mimikry und Schutzfärbung.

Von Dr. Franz Werner.

(Schluss.)

IV.

Es ist selbstverständlich, dass ein Tier besser geschützt ist, wenn es eine Anpassungsfärbung trägt, als im anderen Falle. Aber welchen Tieren fehlt sie?