Some Corrections and Criticisms.

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In a recent review of the literature of developmental physiology DRIESCH ('05) has discussed and criticised various conclusions presented in recent papers of my own. Some of these criticisms appear to be due to a misunderstanding of my views and in other cases DRIESCH does not seem to have considered the facts with sufficient care. The critique includes an attempt to bring the new facts into line with his own conclusions. Since certain points which seem to me important in connection with analysis and interpretation have either escaped his notice or have not been made sufficiently clear I take the present opportunity to call attention to some of these, and to offer a few corrections and some criticisms of DRIESCH's position.

In the first place DRIESCH has failed to understand my use of the terms »function« and »functional« in connection with regulatory processes and results. These terms are somewhat ambiguous since they are employed in both a special and general sense. A given structural complex, for example, is said to possess a certain »function« when it brings about certain typical changes in its environment. These changes involve the expenditure of a certain amount of energy. Changes produced in the structure which are connected with this expenditure of energy are designated »functional« changes and those structures for which an expenditure of energy, typical in kind and amount, is a necessary condition of existence are said to be »functional« structures.

On the other hand we speak of the functions of protoplasm and of physiological systems, etc. Here the term includes the sum total of the dynamic conditions existing in the system or complex under

consideration and has no necessary relation to visible morphological complexes.

In short these two different uses of the term are based on two different points of view. In the first case certain effects which the morphological complex as a whole produces upon its environment are regarded as its »function« and the reaction of the complex to certain features of this environment is considered as a »functional« reaction or a reaction to functional conditions. But these constitute only a part of the functions and functional conditions in the second sense. Every morphological complex represents a physiological system or a complex of such systems, and the functions of the former constitute only a part of the functions of the latter. In the case of a muscle, for example, the function in the first sense is contraction and the consequent performance of mechanical work. Strictly speaking, however, contraction is only a part of the function of the muscle-system. The sum total of the dynamic processes constitutes the function, or more properly the functions of the physiological system which is visibly represented by the structure. In short the »function« of a morphological complex is in any case only a part of the physiological, i. e., dynamic system of which it is the visible representative.

The distinction which ROUX has made between a formative and a functional stage of development is correct when the word »functional« is used in the special sense with reference to the morphological complex which is in process of formation or completed, but is not correct when the word is used in the general sense, for in this sense every organic system is functional in some manner at all times, and morphogenesis is the result of certain functional conditions in the system.

But since the »functional« relations of morphological complexes are only special cases of the relations of organic systems of various degrees of complexity, the use of the terms »function« and »functional« in the general sense seems to me more justified than their limitation to the special cases, and in the papers reviewed and criticised by DRIESCH they have been used in the general sense, except in certain cases where their special meaning was made clear by the context. My use of these terms should, I think, be clear from the following statement: »the term function is used here to include all the activities of the organism, all transference and transformation of energy« (CHILD, '05c, p. 278). This series of papers included a number of cases in which functional conditions in the special sense and parti-

cularly mechanical conditions connected with movement were found to be of considerable importance, and with respect to such cases the terms are employed in the special sense.

DRIESCH has apparently failed, however, to understand my use of the terms. For example he quotes a statement of mine »The new tissue develops according to the manner in which it is used by the animal or piece« with the following criticism: »Der Satz mag also für gewisse Verhältnisse gewisser Gewebe gewisser Plattwürmer richtig sein: als Allgemeinlehre ist er ganz sicherlich unrichtig« (DRIESCH, '05, p. 790). I agree perfectly with DRIESCH on this point. The statement was made with particular reference to certain *Turbellaria* and as far as I am aware there was no attempt on my part to regard it as »Allgemeinlehre«. But this is simply a special case of the determination of the morphological result by the functional conditions in the system.

DRIESCH ('05, p. 790) continues: »Um zunächst einmal auf einige Beispiele unserer harmonisch-äquipotentiellen Systeme einzugehen: wie stellt es sich CHILD vor, daß ein durchaus ruhendes Stück des Stammes einer *Tubularia*, des Stolos oder des reduzierten Kiemen-darmes einer *Clavellina* sich in proportionaler Richtigkeit ausgestaltet? Ist hier auch nur die entfernte Möglichkeit vorhanden, mit Funktional-reizen auszukommen, wo doch erst nach der Ausgestaltung funktioniert wird.

»Und eben solches gilt von echten Regenerationen: Was hier neu gebildet wird, soll doch erst funktionieren, kann also nicht durch das Funktionieren entstehen. Mag man immerhin mechanischen Zug auf die Ausgestaltung entstehender Bindegewebelemente eine formative Wirkung ausüben lassen (s. o.): Wie steht es mit den Nerven, wie mit dem Bindegewebe, welches neu für das Funktionieren entsteht? Gerade auf den nur mittelbaren, weil direkt nur funktionsbestimmenden, formativen Einfluß des Nervensystems legt ja CHILD einen großen Nachdruck: Welche ‚Funktion‘ bestimmt denn nun die doch wohl in seiner Gestaltung begründete ‚funktionsbestimmende‘ Tätigkeit des Nervensystems? Man sieht gerade hier, wie die CHILDSchen Allerklärungen durch Funktionsreize sich im Kreise drehen, selbst wenn man seine Ermittlungen über die nur mittelbar formative Rolle des Nervensystems für richtig hält. Woher diese Rolle? Woher, bei Restitutionen, Neubildungen typischer Art — nämlich die Restituierung des Nervensystems — für diese Rolle?«

It is evident from the quotation that for DRIESCH »function«, includes merely the activities of the completed structure or strictly speaking only certain of those activities. Apparently as regards the hydranth of *Tubularia* only movement of the morphological components is function, according to his idea. I certainly never intended to assert that the development of the primordium in *Tubularia* was determined by the »functions« of the fully developed hydranth. I do maintain, however, that the hydranth is the visible result of functional conditions or processes in a physiological system of typical character and I believe that other structures develop in similar manner. The development or regeneration of the nervous system is certainly not determined by the »function« of the fully developed nervous system; but I believe it is determined by the functional conditions in a system of which the regions which give rise to a nervous system form a part. In consequence of correlations, i. e., the effect of processes in one region on those in another, the nervous system becomes the chief means of transferring stimuli to certain parts and so determines their function in greater or less degree. But the fact that the nervous system becomes the chief organ for the transmission of certain stimuli does not exclude the probability that transmission occurred to some extent before a visible nervous system was present. In general, morphogenesis is to be regarded as a specialization of function rather than an appearance of new functions.

I had supposed it was sufficiently evident from my statements that relations of the developing part to the other parts of the system were regarded as playing a part in determining its fate. I endeavored to make this point clear in my papers on *Leptoplana* (CHILD, '04 b, '04 d, '04 e, '05 c) and again in later work on *Cestoplana* (CHILD, '05 d).

These comments are, I think, sufficient to show that DRIESCH's criticism of my view, which was quoted above, fails entirely of its purpose because it is based on misapprehension.

A somewhat similar misapprehension is evident in DRIESCH's account of the work of YOUNG and myself (CHILD and YOUNG, '03) on the regeneration of the legs of agrionid nymphs: »Wie die Sehnen an ihre Orte kommen, das wissen die Autoren nun freilich nicht: die Gliederung aber soll wieder nicht als echte Formativeleistung, sondern als Anpassung an die funktionelle Inanspruchnahme verständlich sein. Hier dürfte man denn wohl doch berechtigte Bedenken nicht unterdrücken können: man wird namentlich einwenden, daß eine durchaus typische Muskelkoordinationsbewegung doch gerade erst nach

Existenz der Gelenke möglich wird; wir werden einen ähnlichen, aber noch prinzipielleren Einwand, daß er nämlich Causalbeziehungen umdrehe, CHILD später noch in allgemeiner Form zu machen haben« (DRIESCH, '05, p. 756).

The facts in the case are these: It was observed that certain articulations fail to appear, except where the tendons of the muscles become attached. In some cases where one tendon attached itself and its antagonist failed to appear, the articulation developed on only one side of the leg, the other side remaining without a trace of the articulation. But nowhere in the paper is the assertion made that the articulation is formed in consequence of movement. The assertion was made, however (CHILD and YOUNG, '03, p. 576) that »there is nothing inherently improbable in the theory that the articulations may arise primarily as the direct result of the mechanical pull exerted on the parts to which the muscles attach«. This statement was not intended to imply that movement was necessary. Under normal conditions muscles exhibit what is known as tone. This tonic condition must arise at some time during development and the statement made above primarily concerns this fact. There is no reversal of causal relations here. It is a fact, moreover, that the articulation is at first a simple fold in the cuticle and of very different form from that which it acquires later after actual movement has taken place (CHILD and YOUNG, '03, p. 574). The changes are great and I have long desired to examine them in detail. It is impossible to resist the impression that a high degree of functional adaptation occurs during the development of the articulation, but that actual movement is necessary for the formation of the articulations was certainly not claimed and not believed by the authors of this paper. Here again apparently DRIESCH regards function as consisting in actual molar movement of parts.

No attempt was made in this paper to account for the attachment of muscles at certain points in the leg. Attention was merely called to the fact of the varying proportions of parts as indicating that the regions of attachment were not rigidly fixed (CHILD and YOUNG, '03, p. 577) though of course such evidence is merely an indication and not conclusive.

Regarding this paper DRIESCH comments further: »Übrigens verursacht es den Autoren selbst Bedenken, daß alte Nymphen, welche erst nach der nächsten Häutung auskriechen, trotz ihrer verkrüppelten Beine doch eine annähernd normale Gliederung derselben

erhalten, daß das Neue doch offenbar in der Ruhe angelegt wird (im Gegensatz zu den Polycladen) und daß, falls anfangs nicht alles normal ist, doch im Laufe mehrerer Häutungen meist der normale Zustand erreicht wird. Alles dieses spricht in der Tat durchaus nicht für diese Gestaltung des Normalen nur durch die Funktion« (DRIESCH, '05, p. 756).

So far as I am aware, the only »Bedenken« expressed with regard to the regeneration of legs in old nymphs, was the statement that »special regulative factors appear to be concerned« (CHILD and YOUNG, '03, p. 593) in these cases. This statement had reference merely to the fact that the development of muscles in these cases is accelerated, but is in no sense an implication that these cases do not belong in the same category as the others. That they afford any evidence for DRIESCH's conclusion just quoted I cannot see.

Moreover, the only approach to normal form after successive moults consists in further development. DRIESCH's statement that »im Laufe mehrerer Häutungen meist der normale Zustand erreicht wird« is absolutely incorrect, for it was shown that in a large number of cases the legs always remained typically abnormal, no matter how large the number of moults.

Turning now to other points: DRIESCH accuses me of creating confusion by an incorrect use of the word »form«. »CHILD stiftet aber unseres Erachtens Verwirrung dadurch, daß er jenen Umriß seiner Würmer kurzerhand als ihre ‚Form‘ bezeichnet. Organische Form ist ein typisches Zusammengesetztes, nicht eine bloße Raumkonfiguration, und es mag schon an dieser Stelle bemerkt sein, daß CHILDS Ermittlungen natürlich gar nichts darüber aussagen, warum denn überhaupt Neues restitutive gebildet wird: nur warum das schon Gebildete sich so ordnet, wie es tut, erklären sie bis zu gewissem Grade. Wie gar sollte die Restitution des Nervensystems selbst durch Aktivitäten erklärt werden, die von ihm erst inszeniert werden« — (DRIESCH, '05, p. 768).

My use of the word »form« with which DRIESCH finds fault was in connection with the changes in the shape or outline of the pieces of *Turbellaria* which MORGAN has termed »morphallaxis« (CHILD, '02, '03, '05c, '05e, '05f). It should, I think, be sufficiently clear from the context that the word was used in the sense of »shape« or »outline«. I readily admit that the shape of the whole is not the »form« of the whole, but DRIESCH's assertion that organic form is »ein typisches Zusammengesetztes« seems to me to require proof. It may

be so for him, but the assertion that form is something more than space-configuration seems to me to involve a wholly unwarranted use of the word.

As regards the second part of the quotation, viz., that my conclusions do not show why »Neues restitativ gebildet wird« it may be said that in these particular experiments I was not concerned with this question in any way, but merely with showing how in certain *Turbellaria* that which was formed acquired a certain typical shape. What purpose does it serve to point out the fact that certain experiments afford no evidence regarding points with which they are not concerned? But DRIESCH might have found in my paper certain suggestions as to why »Neues restitativ gebildet wird«. In No. IV of my Studies on Regulation (CHILD, '04b, p. 98 et seq.) certain suggestions on this point were offered, and the same point was touched on elsewhere. In a paper published since DRIESCH's review appeared (CHILD, '06b) I have endeavored to state more fully my views on this subject.

DRIESCH is inclined to doubt my conclusions regarding the effect of mechanical conditions in such cases as *Stenostoma* (CHILD, '02, '03), *Leptoplana* (CHILD, '04b, '05c) and *Cestoplana* (CHILD, '05d). In these cases I attempted to show that the elongation and decrease in width of the pieces were essentially similar to the changes in shape which would occur non-living material of the same degree of plasticity under the same conditions. After a brief consideration of my data in which some of the most important points are not mentioned, DRIESCH concludes: »so können wir die Gesamtheit des von ihm Gebotenen unsres Erachtens auch für die Polycladen nur im Sinne des Nachweises funktioneller Anpassung, nach Art von ‚Mechanomorphosen‘ auffassen« (DRIESCH, '05, p. 767). Here again proof, not assertion, is necessary. I have shown that in *Stenostoma* and *Leptoplana* for example, the change of shape is in the direction of the mechanical tension, that it increases and decreases with increase and decrease in the mechanical conditions, that its direction is reversed when mechanical conditions are reversed. I know of no case of mechanomorphosis in which direct proportionality between the mechanical conditions and the change of shape exists, moreover, such proportionality cannot be expected where complex reactions to stimuli are concerned. And finally, I see absolutely no reason for denying physical plasticity to such organisms as *Turbellaria*, and this is what DRIESCH's statement really amounts to: for he believes

that these changes of shape are reactions to mechanical stimuli — not direct mechanical distortions. I cannot understand how it is possible that an organism without hard parts, such as a Turbellarian, should fail to undergo changes in shape in direct consequence of mechanical pressures or tensions to which it is subjected. Moreover, if these mechanical conditions are more or less typical in direction and amount, it seems to me that such changes will be more or less typical. I have no doubt that various mechanomorphoses also occur in consequence of the mechanical conditions, and to this point I have referred repeatedly. For example in my discussion of the changes in shape of *Stenostoma* (CHILD, '02, pp. 231—234) attention is repeatedly called to the probable occurrence of reactive processes in connection with or in consequence of the change in shape. On p. 233 (CHILD, '02) the following statement occurs: »it is perhaps unnecessary to point out once more that various other regulative processes, reactive in nature, may be most intimately associated with mechanical regulation and that the mechanical change of form itself may serve as a stimulus for these. Nothing is further from the purpose of this paper than to attempt to explain morphallaxis or allied phenomena on a purely mechanical basis«.

In the same paper (p. 234) I showed that when specimens of *Stenostoma* are kept for a short time under a cover-glass so that the body is somewhat flattened, the flattened shape is often retained for half an hour or more after the animal is freed; the peristaltic movements which force the intestinal contents into various parts of the body and so cause pressure on the body-wall appear to be the chief factors in bringing about its disappearance.

Something more than DRIESCH's assertion is necessary to prove the incorrectness of these data. The burden of proof rests upon him. Such unproved assertions seem to me particularly unjustifiable in a review of this nature, where the reviewer is supposed to state at least all the data on which the author's conclusions are based. One is forced to the conclusion that DRIESCH is much more interested in interpreting in accordance with his own hypotheses the data presented by other authors, than in giving a complete and accurate statement of their data and conclusions.

I agree with DRIESCH ('05, p. 770) fully that the loss of intestinal branches in *Cestoplana* is in some cases »Inaktivitätsatrophie« but the atrophy is in my opinion due, at least in large part, to the absence of contents which distend the intestine rather than to the

absence of food. My data on these points have not been presented in full as yet, and cannot be presented here; but I believe they indicate very clearly that in certain cases the disappearance of the intestinal branches and their reappearance in a new position and arrangement a few days later are largely reactions to altered mechanical conditions. These changes are, in my opinion, true mechanomorphoses, at least in large part, and not simply mechanical changes in the shape of physically plastic material.

In similar manner DRIESCH gives only partial and incorrect statements of my data and conclusions regarding the relations between internal water-pressure and regulation in *Cerianthus*. For example he says regarding the tentacle-formation in *Cerianthus*: »CHILD erhielt bei *Cerianthus* durch seitliche Einschnitte die schon LOEB bekannten Objekte mit zwei Tentakel tragenden Mundscheiben. Wie nun nach CHILD die Tentakeln von *Cerianthus* überhaupt als Antwort der Gewebe auf die Ströme der Leibesräume zustande kommen sollen, so soll das auch hier der Fall sein: an der Wunde liege gar nichts, bedeutsam sei nur die Einrollung des Randes, damit eben die Ströme aufetwas treffen und so Tentakeln auslösen können. Doch schlugen alle Versuche, Tentakelbildung ohne Wunde bei künstlicher Rollung der Wand zu erzielen, fehl« (DRIESCH, '05, p. 776). I suggested (CHILD, '04b, pp. 273—276) that a relation between localized growth, i. e., tentacle-formation and definitely directed currents might exist, at least for the marginal tentacles. At the same time (p. 276) I called attention to the difficulties of such a hypothesis with respect to the labial tentacles. In following papers (CHILD, '04c, p. 152) I called attention to the necessity of caution in drawing conclusions along this line. In a still later paper (CHILD, '05a, p. 108) I predicted that the formation of tentacles might be brought about without the presence of a cut if the body-wall could be retained in the proper position for a sufficiently long time. On the other hand I called attention repeatedly to the fact that the tentacles appeared in my experiments in a typical relation to a region where a cut had previously existed. It seems to me that even if this relation shall be shown to be a necessary one it does not follow that the internal currents are not a factor in determining the localization of the tentacles. It is quite possible that only those regions which are within a certain distance from a distal terminal region are capable of reacting to the internal pressure by tentacle-formation. In later experiments with Actinians I have been able to produce forms

with many supernumerary tentacles, in part abnormally situated, which persist and show no tendency to return to the normal form. Such tentacles appear, even when the old tentacles are present, and both persist indefinitely. Evidently then the absence of tentacles is not a necessary condition for tentacle-formation.

DRIESCH emphasizes the fact that my attempts to produce tentacles in the absence of a cut surface failed. He might have done me the justice, however, to state what I stated in connection with these experiments, viz., that all specimens either separated into two parts in the plane of the ligature within a few hours, or if it was less tightly tied, crept cut. Thus the experiment has in reality never been performed, for the method was a failure. Consequently DRIESCH's statement as it stands is incorrect and gives an entirely wrong impression as to the facts.

On p. 791 DRIESCH ('05) asserts »daß 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, unbekümmert 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! Bei solchen Mißverständnissen in Hinsicht der fundamentalen Begriffe ist eine Würdigung komplizierter auf sie gebauter analytischer Gedankengänge allerdings unmöglich«.

According to DRIESCH's own definition of harmonic-equipotential systems which he has given in several papers ('99, '01, etc.) their properties are as follows: first, »es kann an solchen Systemen jedes Element jede der überhaupt möglichen Einzelheiten gleichermaßen leisten« (DRIESCH, '01, p. 171), or as he has put it elsewhere (DRIESCH, '99) »jedes kann jedes«; and second, »daß Alles, was kraft der vorliegenden Potenzen wirklich in jedem einzelnen Falle entsteht, zueinander in ganz bestimmte Beziehungen gesetzt ist« (DRIESCH, '01, p. 172), or as elsewhere stated »Die Proportionalität alles Geschehens bleibt bei beliebiger Entnahme von Material des Systems gewahrt« (DRIESCH, '99, p. 121).

Now I have shown that every transverse piece of *Cerianthus* above a certain minimal size is capable of giving rise to a whole. In other words »jedes kann jedes« just as truly as in *Tubularia* or *Planaria* which DRIESCH regards as typical harmonic-equipotential

systems. The only difference between these forms and *Cerianthus* is that the minimal size is relatively somewhat greater in the latter form in proximal than in distal regions. Similar regional differences occur in both *Tubularia* and *Planaria*. As I understand DRIESCH's definition, the fact that the missing parts are replaced by regeneration has nothing to do with the case. Regeneration occurs in *Planaria* and in *Clavellina* but nevertheless these are according to DRIESCH harmonic-equipotential systems. So far as this point is concerned *Cerianthus* is as truly a harmonic-equipotential system as these other forms, consequently DRIESCH's comments are wholly beside the mark. But in *Cerianthus* proportionality is not maintained. Short pieces of one sixth or less of the whole length give rise to tentacles of about the same length as those on pieces including the whole body except the disc. Here then is a case which does not fit DRIESCH's definition. In other words his definition does not hold good for all cases. A series of papers now in press will show that it does not hold for *Tubularia* and that the observations on which DRIESCH based his conclusions for this species were inaccurate and incomplete. But my remarks regarding *Cerianthus* still hold, and if DRIESCH's own statements are to be taken at their face-value they show a fundamental error in his definition of harmonic-equipotential systems. Possibly in time I may be able to appreciate the »komplizierte analytische Gedankengänge« with which DRIESCH has furnished us in so many instances.

In a footnote (DRIESCH, '05, p. 791) it is further asserted that I have overlooked the fact that »ich die Entelechie, meinen gestaltenden autonomen Naturfaktor, in steter Beziehung zu den übrigen Naturagentien sein lasse«. My statements which gave rise to this comment concern the changes of shape, viz., elongation and decrease in diameter, which occur in pieces of *Cerianthus*. Such changes occur only in pieces which are distended with water, while collapsed pieces change in the opposite direction and depart farther and farther from the normal proportions. It seems to me that if we assume the existence of an entelechy to account for the changes in the direction of normal proportions, which occur in pieces distended with water we should be logically justified in assuming the existence of another entelechy to account for the changes in the opposite direction. As I stated in the paper referred to: »it is possible to keep a specimen collapsed by means of a very small opening at one end or elsewhere, provided this be reopened frequently. It is difficult to understand how

such an opening should interfere with any internal or 'vitalistic' factor capable of causing return to the typical form. Here again experiment indicates the impossibility of explaining the facts with the aid of DRIESCH's hypothesis* (CHILD, '05b, p. 283).

I was quite aware when I made this statement that DRIESCH's entelechy was regarded by him as in constant relation to physical factors, but I still fail to see the necessity for assuming its existence in this case. When distension is present typical changes in one direction occur, i. e., toward the »normal proportions«; when it is absent typical changes in the opposite direction occur. If an entelechy determines one the other must, it seems to me, be similarly determined. But the assumption of its existence seems to me to be wholly gratuitous in both cases. DRIESCH fails to consider the fact that many phenomena which constitute departure from »normal« proportions or conditions are quite as typical as those involved in the return to »normal« proportions or conditions.

One or two other points of minor importance require brief consideration. On p. 694 DRIESCH ('05) states that »Nach CHILD nimmt bei *Leptoplana* das Regenerationsvermögen nach hinten zu ab«. This statement is incorrect. Regeneration in the posterior direction in *Leptoplana* is qualitatively complete at all levels posterior to the ganglia and is quantitatively complete if the specimens are fed (CHILD, '04d). In the absence of food, posterior regeneration decreases quantitatively with increasing distance of the cut surface from the ganglia but remains qualitatively complete in all cases. In short, *Leptoplana* is capable of restoring all parts removed at all levels posterior to the ganglia, but in the absence of food these parts never attain normal proportions.

Regeneration in the anterior direction is always incomplete at levels posterior to the ganglia but decreases slightly in amount with increasing distance from the ganglia (CHILD, '04e).

Regarding the proportional development of the pharynx in the regenerating parts of *Leptoplana* (CHILD, '04d, p. 492) DRIESCH says: »So erscheint ihm (CHILD) z. B. die proportional richtige Ausbildung des Pharynx der *Leptoplana* sehr seltsam« (DRIESCH, '05, p. 791). My statement was that it is difficult to answer the question as to why such proportionality exists. But DRIESCH fails entirely to consider the attempt which I made to answer it (CHILD, '04d, pp. 492—493), and until he has considered this and shown that it is a failure, he is not justified in placing this case, as he has done, among cases

which, as he asserts, have raised doubts in my mind concerning the possibility of a complete analysis of organic form on a functional basis.

Regarding another case his critique is as follows: »Von besonderem Interesse erscheinen ferner die Figuren 28—30 der betreffenden Arbeit (CHILD, '04 d, p. 495): Hier bildet er (CHILD) eine sogenannte Regeneration des vollständigen komplizierten Genitalapparates seiner Form aus Teilen der Genitalgänge ab: die Erklärung, daß der Druck des Inhalts der Gänge hier in funktionaladaptiver Weise alles mache, tritt mit Recht, doch sehr zaghaft auf. Es scheint uns, was sich freilich aus den Figuren nicht deutlich entnehmen läßt, als läge hier ein recht typischer Fall der Differenzierung eines harmonisch-äquipotentiellen Systems vor« (DRIESCH, '05, p. 791).

Here again DRIESCH has distorted my statements so that they are scarcely recognizable. In the first place, this is a case of regeneration not of the development of a harmonic-equipotential system, for the development proceeds from the ends of the remaining portions of the ducts. An early stage of the process was shown in Figure 29, and if I had supposed that a critic, without bringing any new facts, would doubt my observations on theoretical grounds, I should have given additional figures of the stages.

Secondly: as regards the factors involved in this regeneration my statements are as follows: »We know little regarding the conditions which may be concerned in the regeneration of these parts, but they are doubtless connected with the presence of other portions of the ducts in the old part and also with certain obscure physiological conditions on which the presence and periodical activity of the sexual organs is also dependent« (CHILD, '04 d, p. 494). And again: »There can be little doubt, I think, that the complete regeneration of the ducts and terminal organs of large size in the case just described was due first to the fact that parts of the ducts remained in the old tissue, and second and probably chiefly, to the fact that the ducts were in an active functional condition, i. e., filled with sexual products. Here again as in the case of the intestine, the possible effect of the pressure of the contents of the ducts upon the walls and of other functional conditions upon regeneration must be taken into account.«

Of these suggestions DRIESCH mentions only that regarding pressure of contents and puts it in positive form as if I had asserted that it explained all, whereas I actually referred to the »possible effect« of this pressure as one of the conditions to be considered.

However, as I showed from other experiments (CHILD, '04d, pp. 496—497) when all parts of the larger ducts are removed the terminal structures do not appear except in rudimentary form. If this were a harmonic-equipotential system the presence or absence of parts of the ducts in the old tissue should make no difference in the results.

In commenting upon certain of my observations and conclusions regarding *Cerianthus* DRIESCH says: »hier sei darum nur noch mitgeteilt, daß er (CHILD) ganz allgemein weder das Fehlen von etwas, noch die Wunde als solche, sondern einfache physikalische Faktoren für Auslösung jeder Restitution verantwortlich machen will. Nichts sei teleologisch oder adaptiv! Übrigens kann man eine gewisse Einschränkung solcher evident falscher und eigentlich undiskutabler Sätze vielleicht in dem Zugeständnis erblicken, daß Zellenbildung und Differenzierung zu jenen physiologischen Faktoren hinzukämen —« (DRIESCH, '05, p. 776).

It is necessary first to consider what DRIESCH means by »einfache physikalische Faktoren«. If physical as opposed to chemical factors are meant the statement is manifestly incorrect. If on the other hand, as I suppose, physical factors in the broader sense as opposed to teleological or »vitalistic« factors are meant, the statement expresses my views exactly. I believe the »absence of something« or the »presence of the cut surface« act, not »as such« but by bringing about physico-chemical changes of one kind or another in the parts remaining. Consequently I believe it is conceivably possible that somewhat similar changes may be brought about experimentally by other means. The fact that I have called attention to the importance of mechanical factors *sensu stricto* in determining the course of regulation and the fate of the parts formed in certain species does not involve the conclusion that all form-regulation is the product of such mechanical factors, neither does it exclude the possibility that these factors may be the most important and even the initiating factors in some cases. In short, widely different factors are involved in different cases. Moreover, the »normal« or »typical« form is in my opinion merely the result of physico-chemical activity of a given character in a given environment, and not associated with entelechies or other ultra-physico-chemical entities. In fact DRIESCH's argument for the existence of entelechies appears to me to be much like the course of reasoning through which the more primitive human mind arrives at the conclusion that the locomotive, the electric car, the

thunder-storm, or the eclipse are the results of witchcraft or magic or the acts of a supernatural personality. Such conclusions all belong in one category, they all postulate the existence of entelechies under one name or another, and finally, they are all simply ways of accounting for phenomena which have not yet been analyzed, or in other words they are expressions of ignorance.

I am well aware that DRIESCH asserts that he has analyzed the phenomena of form-regulation on which his conclusions regarding entelechies are in part based. As I shall show elsewhere, however, his analysis of these phenomena in *Tubularia* is incorrect and based on insufficient and inaccurate observations. Consequently his conclusions based on analysis of this case must be rejected, and doubt is thereby cast on other similar conclusions from other cases. Until DRIESCH furnishes a better basis for his hypothesis than his observations on *Tubularia* his »komplizierte analytische Gedankengänge« will not inspire great confidence in the reader. Moreover, others have shown that the eggs of the sea-urchin and of the ascidian are not harmonic-equipotential systems as DRIESCH supposed them to be.

The apparent amazement with which DRIESCH states that there is nothing teleological or adaptive in form-regulation according to my ideas reminds one of the incredulity with which some minds receive any attempt to demonstrate that certain natural, physical phenomena are not supernatural.

It is only to be expected that my views appear »falsch und eigentlich undiskutabel« to DRIESCH nor do I expect that his opinion will be altered by the present writing. The purpose of these comments is to accomplish what DRIESCH's review has not, viz., merely to place my views in their proper light before others, to whom such knowledge as DRIESCH possesses has not yet been granted, and to whom the question as to whether teleological factors exist in regulation still appears at least open to discussion. I believe we may with complete equanimity of mind leave DRIESCH to contemplate the universe from the height of his lonely scholastic eminence. We have but one request to make, and that is that when he considers the results of our labors he shall at least acquaint himself fully with those results and not merely with a part of them. In another criticism of certain of my observations and conclusions DRIESCH remarks: »Ich kann hier die ganz allgemein gedachte Bemerkung nicht unterdrücken, daß die so hochverdienten amerikanischen Autoren den Wert ihrer Arbeiten nur noch erhöhen würden, wenn sie . . . fremde Literatur stets wirklich

sachgemäß prüfen und erörtern würden« (DRIESCH, '04, pp. 232—233).
If DRIESCH would only take this good advice to himself!

Hull Zoological Laboratory, University of Chicago, March, 1907.

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