Bewegungen unzulänglich. Sie reicht eben nur bei reiner Vorwärtsbewegung hin, jede Richtungsänderung zu erzielen, ein Vermögen, das sich wohl stets bei einseitigem Ausfalle durch Kreisbewegung offenbart.

Im ganzen finden wir also bei Schnecken und Krebsen Gleiches ermöglicht, aber durch verschiedene Mittel, die sich der Verschiedenartigkeit der Organisation beider Tiergruppen angepasst haben.

Explanatory Remarks concerning the Normal Rate of Growth of an Individual and its Biochemical Significance.

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Recent publications by the author[1]) on the Normal Rate of Growth of an Individual and its Biochemical Significance have called forth certain criticisms which, although for the most part devoid of significance, are nevertheless such as might mislead those who lack technical knowledge of the questions at issue, — questions which are, for the most part, mathematical in character.

Enriques[2]) in a recent number of this journal, has devoted some space to pointing out that other formulae besides my formula $\log \frac{x}{A - x} = K(t - t_1)$ where x is the amount of growth after time t and A, K and t_1 are constants, might be applied to the growth of organisms with equal success. Hence, he argues that there is no reason why the formula which I suggest should be regarded as representing the growth of organisms rather than one of the other formulae which he quotes or suggests. He overlooks the well-known mathematical fact that any regular (non-discontinuous) plane curve can be represented with close approximation to accuracy by a great number of very different formulae[3]). Thus, for example, any continuous plane curve whatever can be represented to any desired order of accuracy by some formula of the type $y = a + bx + cx^2 + dx^3 + \ldots\ldots\ldots$, provided, only, that we include in our equation a sufficient number of terms. Hence, were we to carry out En-

1) T. Brailsford Robertson. „On the Normal Rate of Growth of an Individual, and its Biochemical Significance." Arch. f. Entwickelungsmech., 25 (1908), p. 581. — „Further Remarks on the Normal Rate of Growth of an Individual and its Biochemical Significance." Ibid. 26 (1908). p. 108.

2) Paolo Enriques. „Wachstum und seine analytische Darstellung". Biol. Centralbl., 29 (1909), p. 331.

3) Although this number is exceedingly small compared with the infinite number of formulae which will not represent a given curve.

riques' type of reasoning to its logical conclusion we should exclude quantitative evidence from natural science; for all quantitative evidence, depends, in ultimate analysis, upon the agreement between some theoretical formula connecting variables and the relationship between these variables which is actually observed, — but unquestionably the experimental relations could invariably be represented, within the order of accuracy of the observations, by other and quite different formulae. Thus, to quote a familiar example, Rudolphi's and van't Hoff's equations for the dependence of the dissociation of strong electrolytes upon their dilution are very different from one another, Rudolphi's being $\frac{\alpha^4}{(1-\alpha)^2 V} = K$ where α is the degree of ionisation and V the volume in which one gramme-molecule of the substance is dissolved, while van't Hoff's equation is $\frac{\alpha^3}{(1-\alpha^2) V} = K^1$, where α and V have the same meaning as in Rudolphi's equation. Yet these equations yield equally good constants when applied to the same experimental determinations; which equation, therefore, should we prefer? At present there is no evident theoretical foundation for either equation, — they are „empirical" formulae. But if it should transpire that one of these formulae could be anticipated upon theoretical grounds, that is unquestionably the formula which we would prefer. To quote another example, when the experimental relation between the amount of material transformed (= x) and the time (= t) in a chemical reaction obeys the formula $\log \frac{a}{a - x} = Kt$, where a and K are constants, we conclude that only one molecule is undergoing transformation, because that is the relation which would be theoretically anticipated if only one molecule were engaged in the reaction; but, arguing from Enriques' standpoint, physical chemists have been mistaken in drawing this inference, because the experimental relations could certainly also be represented by the formula $x = a + bt + ct^2 + dt^3 + \ldots\ldots\ldots$, and there is no reason, or Enriques percieves no reason, for assigning preference to the one formula rather than to the other. Or, to quote yet another example, a limited portion of the curve $y = \log x$ can be represented by the formula $x = a + bx + cx^2 + dx^3$, and a still larger portion of the curve by the formula $y = a + bx + cx^2 + dx^3 + ex^4$ and so on, — from which we should conclude, following Enriques' type of argument, that we are not justified in assuming that a table of logarithms is really and truly a table of logarithms.

When a certain relation, subsisting between experimentally determinate variables, is deduced from theoretical considerations, the relation thus predicted is only one among an infinite number of relations which might be written down haphazard. Among an enormous number of chance relations or formulae which might be written down, only one or two would be found to represent, even approximately, the experimental relations observed. If, therefore, a relation which is deduced from theoretical considerations represents even approximately the experimental relation, the probabilities are enormously in favour of the theoretical considerations being correct, since it is excessively unlikely that a formula chosen by chance will represent, even approximately, the given relation between the variables.

The degree of approximation to the experimental relations which we demand as evidence of the probability that our theoretical assumptions are valid depends upon the nature of the variables under consideration and upon the simplicity or complexity of the relations subsisting between them. Thus we demand a much greater precision of agreement between theory and experiment in an astronomical problem, where the variables are few and can be measured with the utmost precision, than in a biological problem, where the variables are many and diverse and can be measured only with approximate accuracy owing to the non-homogeneous character of our experimental material.

In a recent paper Pearl[4]) has raised the objection that in many of my comparisons between the theoretical and experimental curves of growth the experimentally determined curve lies to a greater extent on one than on the other side of the theoretical curve. This objection of Pearl's would be a perfectly valid one provided (I) that there were no systematic errors in the experimental determinations (II), that there were no disturbing factors such as deposition of fat, senile decay etc. and (III) that the constants of the theoretical curve were computed from the experimental determinations by the method of least squares. Not one of these conditions is, however, fulfilled in the present investigation, and Pearl's criticism is therefore deprived of its value. It is a complex and excessively tedious matter to compute constants in a transcendental equation by a least squares method and the computation, unless the experimental determinations attain the greatest precision, is a very uncertain one. Having regard to the innaccuracy which necessarily attaches to quantitative determinations upon living material, when these are not carried out in a strictly statistical manner upon an enormous number of individuals all under like conditions, it did not appear to me worth while to expend the amount of labour necessary to secure a probably fictitious

4) Raymond Pearl „Biometrics", The American Naturalist, 43 (1909), p. 302.

appearance of precision[5]). One instance to which Pearl points with emphasis is not chosen fairly. The instance in question is one (Table I in my first paper, cited above) in which the total deviation from theory is very large (642) when the observed ordinate is greater than the calculated ordinate and only small (19. 6) when the observed ordinate is less than the calculated ordinate. Pearl omits to mention, however, that 578 of these units of total deviation, when the observed ordinate is greater than the calculated, occur in a portion of the particular curve of growth under consideration to which, as is expressly stated in my paper, my equation does not apply. Possible reasons for this lack of agreement are adduced in the text accompanying my table; from Pearl's statement one would gather the impression that I regarded this part of the experimental curve as lending confirmation to the theory.

The remarks put forward above in relation to Enriques' criticisms also apply, it is needless to say, to the criticisms formulated by Pearl.

Towards the conclusion of his remarks concerning my publications Pearl states that „it would appear to be impossible to form any just and significant estimate, on the basis of the only kind of evidence which Robertson presents, namely the comparison of curves, as to the value of his theory as a general theory of growth . . . Can not evidence of another and more convincing kind than that adduced in the present papers be brought forward in its support?“ Pearl has evidently overlooked Ostwald's paper

5) It may be mentioned, in passing, that Pearl cites, in his criticism, only those comparisons in my paper which utilise, as experimental data, the observations of Donaldson upon white rats and upon the growth of the brain in the frog. Pearl says „The tables which have been chosen as illustrations of the point under discussion have been taken in preference to others for two reasons; one that they were long tables, involving a fairly large number of ordinates, the other that the observational data in these tables were obtained by most careful and painstaking measuring and are absolutely trustworthy. On such data, if anywhere, a theoretical curve may fairly be expected to give good results.“ Without for a moment calling in question the accuracy of the measurements I nevertheless cannot agree with Pearl that „on such data, if anywhere, a theoretical curve may fairly be expected to give good results.“ We are dealing with living, that is to say with excessively variable material in other words, for example, the constants A, K und t, in the curve of growth differ widely in different individuals. No matter how precise our measurements may be, trustworthy results possessed of physico-chemical meaning can only be obtained if the determinations are performed upon a very large number of individuals so that the mean group of individuals can be accurately ascertained and the growth of the mean individual accurately followed. Now Donaldson's determinations were carried out upon only 19 individuals and the individual departures from the average weight frequently amounted to from 30 to 50 % of the average! I consider that the determinations of the curve of growth in human beings, cited in my first paper, probably come most near to satisfying the requirements enumerated above, and in these, although the conditions are exceptionally complex, since there

in which he independently and almost simultaneously suggested the same theory of growth as the author and adduced much evidence of a qualitative kind in its support[6]). But, in addition, I contend that a comparison of curves, in the sense implied by Pearl, is not the only kind of evidence adduced in my papers. Perhaps the following recital of facts, which are quite independent of the existence or non-existence of algebraic identity between the theoretical curve of growth proposed by me and the curve of growth which has been experimentally determined, may assist in enlightening Pearl. The experiments of Peter[7]) and of Loeb[8]) upon the temperature-coefficient of Growth have shown that the velocity of growth is determined by the velocity of chemical reactions. Now the growth of an organism, as the results of a very large number of investigators have shown, undergoes, in the first part of a growth-cycle, positive acceleration and, later, negative acceleration with time; the curve of growth therefore possesses a point of inflexion. Now, as I have pointed out elsewhere[9]), only two groups of chemical reactions are known which display positive acceleration, — the one group consists of the autocatalysed reactions and the other of certain catenary reactions. But the curve expressing the extent of transformation with time, in a catenary reaction, is almost invariably markedly assymetric about its point of inflexion, whereas that expressing the extent of transformation with time in an autocatalytic reaction is symmetrical about its point of inflexion. Simple inspection of the numerous published curves of growth is sufficient to assure us that the curve of growth, in any given growth-cycle, is almost invariably notably symmetrical about its point of inflexion. We cannot, I think, avoid the conclusion, om these considerations alone, that the growth of living tissues and organisms is the expression of an autocatalysed chemical reaction.

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are two or more catenary cycles of growth, the agreement between theory and experiment which are cited by Pearl. As regards Donaldson's determinations of the growth of the brain in Frogs, these measurements, as a cursory glance at Donaldson's paper will suffice to show, were subject to very considerable error.

6) Wo. Ostwald. „Über die zeitlichen Eigenschaften der Entwickelungsvorgänge.“ Vorträge und Aufsätze über Entwickelungsmechanik der Organismen. Herausgeg. von Wilh. Roux, Heft 5, Juli 1908.

7) Karl Peter. „Der Grad der Beschleunigung tierischer Entwickelung durch erhöhte Temperatur.“ Arch. f. Entwickelungsmech. 20 (1906), p. 130.

8) Jacques Loeb. „Über den Temperaturkoeffizienten für die Lebensdauer kaltblütiger Tiere und über die Ursache des natürlichen Todes.“ Arch. f. d. ges. Phys. 124 (1908), p. 411.

9) T. Brailsford Robertson. „Sur la dynamique chimique du systeme nerveux central.“ Arch. Internat. de Physiol. 6 (1908), p. 388.

Verlag von Georg Thieme in Leipzig, Rabensteinplatz 2. — Druck der k. bayer. Hof- und Univ.-Buchdr. von Junge & Sohn in Erlangen.

Zeitschrift/Journal: Biologisches Zentralblatt

Jahr/Year: 1910

Band/Volume: 30

Autor(en)/Author(s): Robertson Thorburn Brailsford

Artikel/Article: Explanatory Remarks concerning the Normal Rate of Growth of an Individual and its Biochemical Significance. 317-320