The γ form is produced when sodium stearate is formed by the reaction between stearic acid (Eastman catalogue number 402) and sodium alcoholate, followed by drying the precipitate at 105° C.

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OCCURRENCES OF "RED WATER" NEAR SAN DIEGO[1]

SINCE 1917 the Scripps Institution of Oceanography has given considerable attention to the phenomenon of "red water" and to the conditions of its occurrence. Although the information available concerning it is not much greater in detail in 1942 than it was at the time of previous reports,[2] certain aspects of the conditions appear to be growing more distinct.

1942 was characterized by two periods of "red water," although neither was so conspicuous as in one or two former years. The responsible organisms (dinoflagellates) differed at the two periods in 1942 in contrast to the fact that in the other years only one organism attained "red water" prominence in the year in and near La Jolla Bay. *Prorocentrum micans* Ehr. contributed the color in May, but *Goniaulax polyedra* Stein was causative in September. The duration of noticeable discoloration of the sea in May was about one week, in September about three weeks. The largest abundance recorded in May was 500,000 cells per liter, but probably streaks and patches of more than a million per liter were present. In September the numbers in routine catches at the Scripps Institution pier yielded a maximum of 1,000,000 cells per liter, but special catches showed abundances up to about 2,000,000 cells per liter.

In both May and September the conspicuous populations appeared to drift into the bay from the west. In some other years the invasion was from the northwest. However, in all years the evidence available clearly indicates origins outside of the local area. In 1924 the discoloration caused by *Prorocentrum* was first discovered by the institution boat at about ten miles directly off shore and it was not for several days that the appearance was distinct in La Jolla Bay. Differences in direction and speed of approach to La Jolla, considered in connection with the fact that there is no recognizable "nursery area" in the region, indicate rather strongly that growth of these populations to "red water" prominence is dependent upon conditions affecting particular masses of water rather than upon conditions affecting particular localized geographic areas.

Perhaps the most striking evidence concerning occurrences of "red water" acquired by the Scripps Institution in twenty-five years is that indicating zonation of conspicuous abundances. Most of this has come to hand since the institution began intensive operations at sea in 1938. From these and from certain earlier observations off shore, it appears certain that high abundance in Southern California does not occur as much as twenty-five miles from shore (possibly not more than fifteen) and that it does not reach to a depth of more than thirty meters (except in very rare instances). By way of contrast, the planktonic diatoms, which usually thrive under conditions apparently favorable to the planktonic dinoflagellates, have shown large populations far from shore, a hundred miles and more.

Here, of course, we have introduced a nice complexity of problems for marine hydrographers and chemists, etc., no less than for marine biologists. How can we account for such definite zonation with water boundaries of one and not the other of two groups closely associated? Still more difficult, how can we account for the fact that within these zones the "red water" organisms may show little prominence for years and then "suddenly" become conspicuous almost over night? Larger and smaller movements of water masses have a very definite place in the results, as do air conditions also, and there must be a long series of chemical and biological influences to run parallel with these when enormous development of numbers occurs. Until we know more about a number of these things our explanations of occurrences of "red water" must remain rather hazy except for some interesting generalities.

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[1] Contributions from the Scripps Institution of Oceanography, New Series, No. 180.

[2] W. E. Allen, SCIENCE, 78: 12–13, 1933; SCIENCE, 88: 55–56, 1938.

SCIENTIFIC BOOKS

GROWTH AND FORM

Growth and Form. By D'ARCY WENTWORTH THOMPSON. Pp. 1+1116. Cambridge University Press. New edition, 1942. $12.50.

THIS book, as the author writes in a "Prefatory Note," is a war effort. "I write this book in wartime, and its revision has employed me during another war. It gave me solace and occupation, when service was debarred me by my years. Few are left of the friends who helped me write it, but I do not forget the debt I owe them all." The general character of the work is shown by the subject-matter of the chapters: Introduction; Magnitude; The rate of growth; On the internal form and structure of the cell; The forms of cells; A note on adsorption; The forms of tissues, or cell aggregates; On concretions, spicules, and spicular skeletons; A parenthetic note on geodesics; The equiangular spiral; Spiral shells of the Foraminifera; The shapes of horns and of teeth or tusks, with a note on torsion; On leaf arrangement, or phyllotaxis; On the shapes of eggs, and of certain other hollow structures; On form and mechanical efficiency; On the theory of transformations, or the comparison of related forms; Epilogue.

There is indeed a continuity of thought in all this strange series of topics, but it comes from mathematics and not from biology. A book of this character and size would be thought to have a profound influence upon biological thinking, but that is not so. An inspection of a series of works upon theoretical biology shows, indeed, only a few references to it, and only rarely is it mentioned at all. How can a work which has cost so much effort have so little effect upon the subject with which it deals, particularly when it is remembered the charm with which it is written? Let us see what statements we can find that will throw any light upon this question.

In his "Epilogue" we find this statement: "For the same reason, with no formal and elaborate conclusion do I bring it to a close. . . . My task is finished if I have been able to shew that certain morphological aspects of morphology, to which as yet the morphologist gives little heed, is interwoven with his problems, conplementary to his descriptive task, and helpful, nay essential, to his proper study and comprehension of Growth and Form. . . . And while I have thought to shew the naturalist how a few mathematical concepts and dynamical principles may help and guide him, I have tried to shew the mathematician a field for his labor—a field which few have entered and no man has explored."

There are two men whom he has in mind here—the naturalist and the mathematician, and of the two it is undoubtedly the latter of whom he is thinking the most. Here is biology, with its many problems, and here is a tool, mathematics, that can solve them. Let us present the case and see what will come of it. And so he takes up one problem after the other and shows us what the application of mathematics does for its solution. But driven as he is with the belief in the possibilities of the mathematician as an explainer of the problems of the biologist, one wonders after all just how convinced he is of the value of this transfer of interest. One encounters every now and then traces of doubt—rarely any firm and unequivocal statements of belief. Thus in the "Introductory" chapter we find these statements: "It is but the slightest adumbration of a dynamical morphology that we can hope to have until the physicist and mathematician shall have made these problems of ours their own. . . . How far even then mathematics will suffice to describe and physics to explain the fabric of the body no man can forsee. It may be that all the laws of energy, and all the properties of matter, and all the chemistry of all the colloids are as powerless to explain the body as they are impotent to comprehend the soul," and then, instead of roundly asserting that this is not so, he mildly adds: "For my part, I think it is not so." He is well aware of the difficulty which the appeal to mathematics makes, for he says: "The introduction of mathematical concepts into natural science has seemed to many men no mere stumbling block, but a very parting of the ways." And in another place he is led to make a statement which raises doubts regarding his own position, for he says: "One does not come by studying living things for a lifetime to suppose that physics and chemistry can account for them all."

There are many other doubts that assail one's mind. One wonders most of all why the entire subject of cytogenetics is left untreated. Surely the significance of all the modern work on this subject must be appreciated, and yet there is no mention of genes and little of chromosomes. Undoubtedly these are of the utmost significance in the determination of growth and form in development. Again, in considering the relation between molecules and the living system there is no reference to supramolecular units, a conception of the greatest significance. Indeed, while the title of the work is "Growth and Form," the act of becoming is only incidentally treated. This the author recognizes, for he states: "My sole purpose is to correlate with mathematical statement and physical law certain of the simple outward phenomena of organic growth and structure, or form, while all the while regarding the fabric of the organism, *ex hypothesi,* as a material and mechanical configuration."

How far it is possible to go, considering only the ill-defined forces in the completed structure, it is difficult to say, but surely it can be asserted with little fear of contradiction that in the present work the author leaves us in the end pretty much where we were in the beginning. But even at that one can not blame the author, for neither in the beginning nor at the end, when all the arguments are made, does he make any definite promises. For us he has outlined a series of problems in biology, to which he has suggested mathematical solutions and, having done that, he dismisses the whole matter in the hope that some eminent mathematician will be inspired to take advantage of the opportunity to make of biology a true science.

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SPECIAL ARTICLES

PATHWAY OF INVASION IN A CYNOMOLGUS MONKEY AFTER ORAL APPLICATION OF POLIOMYELITIS VIRUS[1]

DESPITE a great deal of work on both human and experimental material, the portal or portals of entry of poliomyelitis through the body surfaces has not as yet been precisely determined. Present evidence shows that the olfactory system is not as a rule primarily implicated, and that invasion probably occurs in most cases through the alimentary tract, but it is not yet known whether the upper portion—the mouth and pharynx—or the lower portion—the stomach and intestines—is the more vulnerable to penetration by virus.

In studying this problem we have encountered certain technical difficulties which have apparently prevented others also from obtaining a clear answer. Among these may be mentioned the difficulty of confining the application of virus in the experimental animal to a particular region of the alimentary tract; and the difficulty, in cases of fully developed infection, of determining the portal of entry from the distribution of virus or of lesions in the central nervous system. The latter procedure appears to be better adapted to the exclusion of a given portal than to its positive determination, because, once virus has become implanted and the animal has developed typical symptoms of the disease its spread is remarkably rapid and extensive, even before paralysis has occurred.

It has become clear to us that the experimental animal must be sacrificed at the earliest possible moment of manifest infection or even before this, in order to obtain plain evidence of the primary localization. However, the adoption of routine and systematic examination of the peripheral nervous ganglia—suggested by McClure's recent work[2]—has proved to be helpful, a method the value and significance of which has been surprisingly late in gaining recognition. These ganglia contain the nerve cells whose axons supply the mucous surfaces through which the virus presumably first gains access to the interior of the body and it is highly probable that they are the primary site of multiplication of this strictly neuronotropic virus. Since most of the ganglia (with the exception of those of the vagus) supply a fairly limited portion of the mucous surfaces, the distribution of lesions or of virus in them should afford valuable clues to the portal of entry, provided as we have stated that the examination is made early in the disease. Particularly significant ganglia are the Gasserian, the geniculate and the petrosal, which supply the mucous membrances of the mouth and nasopharynx, and the celiac, which supplies the stomach and intestine. The sympathetic and spinal ganglia also have some localizing value, but the vagus ganglia (nodose and jugular) have such wide-spread connections in the entire alimentary and respiratory tracts as to give but little localizing information.

Using cynomolgus monkeys and Sabin's *Per* strain of poliomyelitis virus, we have applied virus to various parts of the alimentary tract and have killed animals shortly after the onset of fever or, in some cases, without any signs of infection. Tissues of the central and peripheral nervous system have been systematically examined for lesions, including the olfactory bulbs, brain stem and spinal cord; the ganglia of the V, VII (geniculate), IX, X cranial nerves; the sympathetic ganglia of the ganglionated cord at all levels; the spinal ganglia at all levels, and the celiac plexus. The results of the study as a whole will be reported later, but at this time we wish to present the data in one monkey as being of some special interest.

Cynomolgus 9. Capsules, covered with a digestible fat, containing dried virus amounting to about one third of a cynomolgus cord, were inserted into the esophagus on April 5, 1941, in such a manner as to avoid contamination of the mouth. On May 20 and again on September 15, 1941, after zinc sulfate olfactory blockade, the tongue was gently swabbed with a minute amount of 15 per cent. virus suspension. On January 22, 1942, a high enema of 5 cc of 20 per cent. virus suspension was administered. No symptoms and no fever occurred after any of these treatments. The olfactory mucosa was again treated with zinc sulfate on March 14, 1942, and on March 25 and on each of the 3 following days, the mouth was sprayed from an atomizer with 5 cc of a 10 per cent. suspension supernate. On March 30, 5 days after the first spraying, fever, slight weakness of the arms and mild head tremors were noted at 5 P.M. (none of these had been present that morning). It is highly probable that this animal would have become paralyzed. It was sacrificed about 15 minutes later, following our routine

[1] From the Department of Pediatrics, Stanford University School of Medicine, San Francisco, Calif. Sponsored by the National Foundation for Infantile Paralysis, Inc.
[2] G. Y. McClure, SCIENCE, 94: 307, 1941.