STRUCTURAL STABILITY and MORPHOGENESIS

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An Outline of a General Theory of Models

Translated from the French edition, as updated by the author, by

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With a Foreword by

C. H. WADDINGTON

1975

W. A. BENJAMIN, INC.
Advanced Book Program
Reading, Massachusetts
London · Amsterdam · Don Mills, Ontario · Sydney · Tokyo

Structural Stability and Morphogenesis
An Outline of a General Theory of Models

First English Edition, 1975

Originally published in 1972 as
Stabilíté structurelle et morphogénèse
Essai d'une théorie générale des modèles

by W. A. Benjamin, Inc., Advanced Book Program, Reading, Massachusetts 01867

Library of Congress Cataloging in Publication Data

Thom, René, 1923-
Structural stability and morphogensis.

1. Biology--Mathematical models. 2. Morphogenesis--Mathematical models. 3. Topology. I. Title.
QH323.5.T4813 574.4'01'514 74-8829
ISBN 0-8053-9276-9
ISBN 0-8053-9277-7 (pbk.)

American Mathematical Society (MOS) Subject Classification Scheme (1970): 92A10/57A20

END PAPERS: The hyperbolic umbilic in hydrodynamics : waves at Plum Island, Massachusetts.

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Manufactured in the United States of America.

Foreword

to the English Edition

When I was asked, several years ago, to write a Foreword to the French version of René Thom's *Stabilité Structurelle et Morphogénèse*, his work was very little known among biologists, and since Thom often insisted on its importance for biology it seemed not inappropriate that a biologist should introduce it. By now, many more biologists have at least heard of Thom's theories and are ready to consider their import. This translation, so admirably and clearly made by David Fowler, will make them readily available to a wide audience. Readily—but perhaps not easily: it is important to emphasise that Thom's work is a part of mathematics, not in a direct way a part of biology. And it is difficult mathematics. At the same time, it is a branch of mathematics developed with a subject matter in view which is a definite aspect of reality. For the sake of dealing with this subject matter, Thom is ready to forego an insistence on all the possible niceties of rigour. For instance, on p. 43 he writes "... when the catastrophes are frequent and close together, each of them, taken individually, will not have a serious effect, and frequently each is so small that even their totality may be unobservable. When this situation persists in time, the observer is justified in neglecting these very small catastrophes and averaging out only the factors accessible to observation." A scientist is likely to feel, on reading such a passage, that it was written by one of his own colleagues rather than by a mathematician of the traditional kind he is accustomed to meeting.

What, then, is this subject matter, to which Thom will devote such arduous and subtle thought, even at the sacrifice of some mathematical purity? It is nothing less than one of the major problems of epistemology. Thom defines it in his first few sentences: "... it is indisputable that our universe is not chaos. We perceive beings, objects, things to which we give names. These beings or things are forms or structures endowed with a degree of stability; they take up some part of space and last for some period of time." And, Thom emphasises, they have boundaries; a boundary implies a discontinuity; and the mathematics used in almost all science so far is based on the differential calculus, which presupposes continuity. This is the lacuna which Thom is attempting to fill.

I stress again that his attempt is that of a mathematician, not of the experimental scientist whose imagination is largely confined to the real four-dimensional world of observables. Quite early in his exposition, Thom points out that "we therefore endeavor in the program outlined here to free our intuition from three-dimensional experience and to use more general, richer dynamical concepts, which will in fact be independent of the configuration spaces." The biologist or other scientific reader must continually strive to ensure that he is not interpreting Thom's words in too simple and conventional a manner. For instance, Thom speaks much of "morphogenesis," a word biologists commonly employ to refer to the shaping of developing tissues into recognisable forms such as those of particular bones, muscles, and the like. Thom, however, is using the word in a considerably wider sense: "Whenever the point m meets K, there will be a discontinuity in the nature of the system which we will interpret as a change in the previous form, a *morphogenesis*" (P. 7). Thus the "morphogenesis" of Thom's title is a more inclusive concept than that familiar to the biologist. The reader must not be surprised to find himself confronted with discussions not only of physics—a subject to which we have all had to try to acclimatise ourselves—but also of human communication and linguistics, in the last chapter, one of the most stimulating in the book.

Undoubtedly Thom sees in biology, and particularly in developmental biology, the main area for the application of his ideas. The manner of his writing is not, perhaps, always persuasive of the need to view his theories as abstract mathematical statements rather than as straightforward descriptions of the geometry of solid bodies. It is easy to accept that the chemical composition of differentiating cells needs for its description a multidimensional function space, and that the sudden transitions and sharp boundaries between one tissue and another are examples of Thomian catastrophes. His fundamental theorem, that in a four-dimensional world there are only seven basic types of elementary catastrophe, which he proceeds to describe, provides many very provocative ideas in connection with the known processes of embryonic differentiation. I personally feel that I have not adequately thought through the argument whereby Thom moves swiftly from a four-dimensional space of chemical composition to the four-dimensional world of extended material structures in time. The key point is perhaps most clearly stated on p. 169: "Suppose that, in the model, the space U into which the growth wave is mapped, and which parametrizes the average biochemical state of each cell, is a four-dimensional state $\mathbf{R}^4$ identified with space-time. This is not so restricted an assumption as it may seem at first sight, for, if U actually has many more dimensions and we suppose that the growth wave $F(B^3,t)$ describing the evolution of the embryo is an embedding, the only effective part of U (in normal epigenesis) will be a four-dimensional domain." I think that much of the debate about the particularities of Thom's biological suggestions will turn on the justification of that remark.

The mentally straitlaced and orthodox should perhaps also be warned that Thom clearly takes a certain mischievous pleasure in provoking them. "This attitude," he remarks on p. 205, "is defiantly Lamarckian; it supposes that, on the whole, *the function creates the organ*...." In an evolutionary context, there is nothing in this notion that need upset any believer in natural selection unless he has been brainwashed into accepting that Lamarckism is a dirty word; the organ is in fact evolved by selection pressures acting in directions dictated by the function. Here I suspect that Thom himself is not quite clear in his own mind whether he is talking about the evolution or the embryonic development of the organ; the fault is perhaps to some extent with the French language, which is very uncertain in making this important distinction. There are many other examples of the highly individual nature of Thom's thought processes, characterised often by extremely long intuitive leaps.

I have mentioned some of the oddities of Thom's writings to warn readers that they will not find only what they already expect, and to advise them as earnestly as I can not to allow themselves to be sidetracked from the main argument. I am convinced that Thom's book is one of the most original contributions to the methodology of thought in the last several decades, perhaps since the first stirrings of quantum and relativity theories. In the particular field of embryological morphogenesis, which is so central to it, it is in my opinion more important than D'Arcy Thompson's great work, *On Growth and Form*. Thompson's contribution was to apply well-known types of mathematical thinking where they had not been applied before, whereas Thom invents not only the applications, but the mathematics as well. Just as much of the detail in D'Arcy Thompson has turned out to be invalid, or at best incomplete, so it is likely that not all Thom's suggestions will prove acceptable; but in neither case does that constitute any reason to overlook the importance of the new insights which these authors have given us.

C.H. WADDINGTON

Foreword
to the French Edition

I am honoured to have been invited to write a *preface* to Dr. René Thom's *Stabilité structurelle et morphogénèse*. I cannot claim to understand all of it; I think that only a relatively few expert topologists will be able to follow all his mathematical details, and they may find themselves less at home in some of the biology. I can, however, grasp sufficient of the topological concepts and logic to realise that this is a very important contribution to the philosophy of science and to theoretical general biology in particular.

The state of biology at the present time seems to call for concerted attempts, from several sides, to develop with greater rigour and profundity the concepts and logical systems in terms of which we can usefully consider the major characteristics of life processes, at all levels. It is a remarkable fact that, although theoretical physics is a well-recognised discipline, served by its own journals and with separate academic departments in many universities, there is no similar acceptance of any discipline called theoretical biology. Is this, perhaps, because no such subject exists or is called for? It might be argued that, in their essence, living beings are no more than very complex physicochemical systems, and do not call for the development of any general theory other than that which can be borrowed from the physical sciences. Proponents of such a view would admit that, there are many special aspects of biological processes which require the elaboration of bodies of appropriate special theory. It is obvious that the hydrodynamics of body fluids, the membrane permeabilities involved in nerve impulses and kidney function, the network systems of nervous connections, and many other biological phenomena require theoretical developments in directions which have not previously been forced on students of nonliving things, but which physicochemical theoretists can accommodate without much difficulty in the types of thinking to which they are accustomed. These are, however, clearly only theories of parts of biology, not of general biology as a whole.

Could one go further and argue that biology does not demand the development of any general theoretical biology, but can be dealt with adequately by a number of separate theories of biological processes, each no more than an extrapolation and development of some appropriate physical theory? In the recent past, the consensus in practice has amounted to an acceptance of this point of view, since little attempt has been made to construct any more general theory. Until the last decade or so, the orthodox view was that the most fundamental characteristic of living things was the existence of a metabolism in apparent (but only apparent) conflict with thermodynamics; but it was held that this was merely a rather elaborate chemistry, demanding little addition to existing theory except the recognition that many proteins can act as enzyme catalysts. More recently geneticists have urged that heredity and evolution are still more basic living processes, but the brilliant discoveries of molecular biology have made it possible to argue that these phenomena essentially fall within the realm of a chemistry expanded only by a few additional theorems about the template activities of nucleic acids.

There is a good deal of weight behind these arguments. Indeed, so long as we regard living things as scientific objects, that is to say, as objectively observable, neglecting any considerations of subjectivity and consciousness, in a certain sense they must be "mere physics and chemistry." There is nothing else but the physical and chemical raw materials out of which they could be constructed; and if at any time our knowledge of the physical sciences is not enough to account for some biological phenomenon, such as the catalytic activities of polyaminoacids or the template properties of polynucleotides, then what we have to do is to develop further our physical and chemical theories. But, all this being granted, there is still a case to be made for the desirability of constructing a general theoretical biology in addition.

This case is most obvious in certain areas of biology which deal with phenomena a long distance removed from the physicochemical processes that must in the last resort be their ultimate base. The evolution of higher organisms is an outstanding example. It would be outrageously clumsy to try to express the problems directly in physicochemical terms, and we have in fact developed quite an elaborate theory of population and evolutionary genetics. There are similar, though not quite so obtrusive, needs for specifically biological theories in other major areas, such as development and metabolism, though as yet these are not so definitely formulated as evolutionary theory. Such biological theories might perhaps be compared with the general theories of particular types of physical phenomena, such as aerodynamics, electronic circuitry, and optics. They would still not demand the formulation of any general theoretical biology, were it not for the fact that they share certain characteristics; and it is the exploration of the common characteristics of the various types of biological theory that constitutes the main topic of theoretical biology.

When a category of biological processes, such as evolution or development, leads to the formation of an appropriate and specifically biological body of theory, it does so because it exhibits two characteristics; it involves entities which have a certain global simplicity and definiteness of character (e.g., a given species of animal or plant, an organ such as the heart or liver, or a cell type such as a muscle or nerve cell); but if one attempts to analyse these entities into basic constituents, such as genes or molecules, they turn out to be of unmanageable complexity. The logical structure of important biological concepts is almost always an actual simplicity (exhibited in their relations to other concepts in the theoretical scheme), included within which is an extreme complexity (revealed on reductional analysis). If there was no simplicity, there would be nothing to make a theory about; if the complexity remained manageable, physicochemical theories would suffice.

Because of their inherent analytical complexity, biological concepts in general imply a multidimensionality. To specify a liver cell, the kidney, or an evolving population would require—if it could be done—the enumeration of a large number of parameters—10^2? 10^3? 10^4? We do not know. Such concepts can therefore be properly related to one another only within a logicomathematical framework which can handle a multiplicity of dimensions. Statistical mechanics is one such system, but the most general is topology. I hope I may be excused for remarking that as long ago as 1940, in my book *Organisers and Genes*, I urged the need to develop a topology of biology. In the intervening years, as not even an amateur mathematician, I have been quite unable to follow my own prescription. I am all the more grateful, therefore, to René Thom, who has now entered the field with such a strong and extended effort. Thom has tried to show, in detail and with precision, just how the global regularities with which biology deals can be envisaged as structures within a many-dimensioned space. He not only has shown how such ideas as chreods, the epigenetic landscape, and switching points, which previously were expressed only in the unsophisticated language of biology, can be formulated more adequately in terms such as vector fields, attractors, catastrophes, and the like; going much further than this, he develops many highly original ideas, both strictly mathematical ones within the field of topology, and applications of these to very many aspects of biology and of other sciences.

It would be quite wrong to give the impression that Thom's book is exclusively devoted to biology. The subjects mentioned in his title, *Structural Stability and Morphogenesis*, have a much wider reference; and he relates his topological system of thought to physical and indeed to general philosophical problems. I have little competence to make any comments on these aspects of the book. In biology, Thom not only uses topological modes of thought to provide formal definitions of concepts and a logical framework by which they can be related; he also makes a bold attempt at a direct comparison between topological structures within four-dimensional space-time, such as catastrophe hypersurfaces, and the physical structures found in developing embryos. I have not yet made up my mind whether I think that, in these efforts, he is allowing his desire to find some "practical results" of his theories to push him further than he ought to go in literal interpretation of abstract formulations. Whether he is justified or not in this makes little difference to the basic importance of this book, which is the introduction, in a massive and thorough way, of topological thinking as a framework for theoretical biology. As this branch of science gathers momentum, it will never in the future be able to neglect the topological approach of which Thom has been the first significant advocate.

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PREFACE

translated from the French edition

This book, written by a mathematician, is hopefully addressed to biologists and specialists of other disciplines whose subjects have so far resisted mathematical treatment. Although the radically new methods that are here developed will not require more than a very crude mathematical formalism, they do demand close acquaintance with concepts and objects basic both to differential topology and classical mechanics: differential manifolds, vector fields, dynamical systems, and so forth. I will not deny that communication will be difficult, all the more so since there is as yet no accessible modern introduction to these ideas. The nonmathematical reader can approach them through the mathematical summary at the end of this book and omit at first reading the more technical portions, Chapters 3 and 7. This may do little to ease communication, but my excuse is an infinite confidence in the resources of the human brain!

Although no one can say that this book lacks originality, many will surely point to its precursors, both known and unknown to me. I would like to mention the following that I know: the most classical, *On Growth and Form* by D'Arcy Thompson, of which this book attempts a degree of mathematical corroboration; the books of C. H. Waddington, whose concepts of the "chreod" and the "epigenetic landscape" have played a germinal part in the formation of my theory; and among physiologists there is J. von Uexküll (*Bedeutungslehre*) and K. Goldstein (*Der Aufbau des Organismus*).

In a work with such comprehensive aims as this, the text should not be weighed down by all the possible references. Accordingly, I have retained only those involving necessary details of specific technical facts and have refrained from citing any reference where general or philosophical statements are concerned; for this I apologize to any writers who feel slighted.

I cannot express sufficient gratitude to the many colleagues who helped me with my task, especially Ph. L'Héritier, Etienne Wolff, and C. H. Waddington, who gave so much of their time and their valued conversation. I should also like to thank my colleagues at the University of Strasbourg, P. Pluvinage and his assistant, M. Goeltzene, who helped me in their laboratory to produce the photographs of caustics for this book.

RENÉ THOM

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Translator's Note

The translation has been vastly improved by the time and effort that M. Thom devoted to it. He removed the blunders, obscurities, and *contresens horribles* with which it was infested, brought some of the material up to date, and added some new sections and notes. I should like here to express my gratitude. I am also indebted to my wife, who is still patiently trying to teach me French, to Christopher Zeeman, who encouraged and helped me with this translation, to the many other people who provided help, in particular Klaus Jänich and Stephen Stewart, and finally to those who typed the various versions, most particularly Mademoiselle A. Zabardi, who reduced a mountain of scribble to a beautiful typescript.

DAVID FOWLER

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