Book Reviews

A Universal Topology

Stabilité Structurelle et Morphogénèse. Essai d'une Théorie Générale des Modèles. René Thom. Benjamin, Reading, Mass., 1972. xii, 362 pp., illus. $28. Mathematical Physics Monograph Series.

"One of the central problems posed to the human spirit is the problem of the succession of forms . . . the spectacle of the universe is an incessant movement of the birth, the development, the destruction of forms. The business of all science is to foresee this evolution of forms and if possible to explain it." Thus Thom begins his book, which has few precedents. Written in French it is published in the United States. Written by a mathematician it addresses itself to philosophy, biology, physics, linguistics, and other subjects. It is concerned with notions and theories unfamiliar to most readers who are not specialists in differential topology, while its philosophical and biological excursions leave most mathematicians behind. It challenges reductionism and other sacred cows of biology and the prevailing methodology of physics. This may sound like madness or a prescription for a publishing disaster. Yet the book will be impossible for mathematicians, theoretical biologists, and philosophers of science to ignore.

Thom's main point is that with our prevailing theories and methods we have been singularly unsuccessful in explaining forms of all kinds, most visibly in biology but in inanimate nature as well. Thom proposes a general method for dealing with forms. It is geometrical, qualitative, topological. The study of forms is to some degree incompatible with the calculation of precise trajectories. (The argument for this is only sketched. Besides involving notions of differentiable dynamical systems, which pervade the book, it makes use of the notion of the structural stability of the domains of existence of analytic functions of several complex variables.) Thom complains that in the face of this dilemma modern physics has chosen calculability over forms: "Present-day physics has sacrificed structural stability [and hence forms] to calculability; I strongly believe that it will repent this choice" (p. 48).

Thom's mathematics is not of the kind usually found in applied mathematics: pages full of formulas, many integrals and differentials. In fact the book contains few formulas. Instead it has pictures, mainly of the singularities of seven specific potential functions. The mathematics is conceptual and geometric and Thom's method is qualitative rather than quantitative. "The use of the term qualitative in science—above all in physics—has a derogative aspect; and a physicist repeated to me, not without vehemence, the remark of Rutherford, 'Qualitative is nothing but poor quantitative' " (p. 20).

> . . . the natural philosopher's mistrust of the qualitative is of historical origin; at the end of the 17th century controversy raged between followers of the physics of Descartes and that of Newton. Descartes, with his vortices, his hooked atoms, explained all and calculated nothing. Newton with his gravitation law in terms of $1/r^2$ calculated everything and explained nothing. History has given favorable judgment to Newton and relegated the Cartesian constructions to the rank of products of fancy and relicts in a museum. . . . But I am not sure that in a universe where all phenomena are governed by a mathematically coherent scheme, but deprived of imaginable content, the human spirit would be fully satisfied. Wouldn't that be pure magic? [p. 21].

From the Pre-Socratics to Descartes speculative, qualitative reasoning was reputable. Its fault, according to Thom, lies not in its qualitativeness but in its naiveté, in that the proposed schemes all rest on intuition about solid bodies in the Euclidean space of three dimensions. "This intuition, as natural as it may be, is probably shaped by the genetic heritage of our species through manipulation and the construction of primitive utensils, and is quite certainly insufficient to give a satisfactory account of most phenomena, even on the macroscopic scale" (p. 22).

To generalize the geometric forms of the three-dimensional Euclidean space of human intuition, Thom considers a Euclidean space (or differentiable manifold) E (of any dimension) and a group G (or pseudogroup) acting on E. A G-form is an equivalence class of closed subsets of E. This concept goes back to Felix Klein's "Erlanger Programm," in which Klein suggested pursuing geometry as a theory of invariants under the action of groups. Thom adds to this the notion of structural stability: A form is structurally stable if the totality of points of E belonging to the same equivalence class is an open set. The nature of the equivalence depends upon the subject of study. Structural stability insures that the forms tolerate small perturbations without failing to be recognizable as such.

Because of perturbations in the initial conditions that enter into experimentation of any kind Thom asserts that "the hypothesis of structural stability . . . appears as an implicit postulate of all scientific observations." "Every model [this includes theories like quantum mechanics or classical mechanics] is a priori composed of two parts, kinematics, whose object it is to parametrize the forms or states of the process under consideration, and dynamics, whose object it is to describe the temporal evolution of the forms." The points of the space E that represent structurally stable forms constitute an open subset of E. The points of the complement K of this set represent unstable forms or "catastrophes." The unstable forms fall into two classes: those that have a chaotic, complicated internal structure, and those that are composed of a small number of identifiable elements whose association in the same object is in some sense contradictory. The former are called generalized catastrophes, the latter bifurcation catastrophes.

It is the study of the bifurcation catastrophes which are describable as singularities of certain potential functions that makes Thom important for biology, namely for embryology. It is in this field (other than mathematics) that Thom's ideas have stirred the greatest interest.

Given a dynamical system by means of a vector field X on a manifold M: When a trajectory approaches the catastrophe set K, then a discontinuity in the appearance of the system results, the preexisting forms change, the system undergoes morphogenesis. The dynamics of the system determines the catastrophe set. Often, however, the dynamics is not known. "In fact in the majority of cases one proceeds in an inverse sense: By macroscopic examination of the morphogenesis of a process, by the local or global study of its singularities, one endeavors to get at the dynamics which cause it" (p. 24). This endeavor is of particular excitement for embryology, where the changing forms can easily be observed but the underlying dynamical process has eluded understanding in spite of a prodigious amount of experimental labor.

For the case where the vector field X is a gradient field of a scalar potential function there are up to homeomorphisms only seven different kinds of bifurcation catastrophes in four-dimensional space-time. Thom links these elementary catastrophes to symmetry-breaking events in embryology: gastrulation, neurulation, and others. Since the book was written Thom has carried this part of his theory even further (*1*).

Biology is only one of many applications of Thom's theory of forms, which has an a priori character:

> One could therefore create a theory of morphogenesis in abstracto, purely geometrical, independent of the substrate of the forms and the nature of the forces which created them, *voilà* . . . this may seem difficult to admit, above all for experimenters who are accustomed to toil in the ardent and continuous struggle with a reality that resists them. The idea nevertheless is not new and one finds it formulated quite explicitly in the classical treatise by D'Arcy Thompson, *On Growth and Form*. But the theories of this great precursor, which were too much ahead of their time to take hold, are expressed in a very naive geometrical manner, and are totally lacking in mathematical justification, which only the recent progress in topology and differential analysis is likely to bring.

Perhaps the strongest statement of Thom's philosophy is the following (p. 295):

> When the mathematician Hermite wrote to Stieltjes: "Numbers appear to me to exist outside of us and they impose themselves with the same necessity, the same fatefulness as sodium and potassium," he did not go far enough, to my taste. If sodium and potassium exist, it is because there exists a corresponding mathematical structure which assures the stability of the atoms Na and K; this structure one can make explicit in quantum mechanics for a simple entity like the hydrogen atom; it is much less well known for the atoms Na and K but there is no reason to question its existence. I believe that likewise in biology there exist formal structures—in fact geometrical entities—which prescribe the only forms possible that can represent a dynamics of self-reproduction in a given environment.

Thom's method is universal. It is applicable wherever forms appear. This is reflected by the great variety of subject matter discussed in his 13 chapters. A work of such universality is bound to cause controversy. It is easy to criticize almost any of the applications. To traditional practitioners of painstaking quantitative analysis of specialized phenomena, Thom's flight of generalities and pronouncements about everything from the interpretation of quantum mechanics, embryology, evolution, archetypes, consciousness, free will, and semantics to the circulation of money in an economy may cause anguish. Yet qualitative methodology may be the only possible way for dealing with models (systems) of great complexity. Biological, economic, and social systems are of this kind. Even if a model were to describe such a system with unlimited accuracy and even if there were no uncertainty in the initial conditions the system trajectories would nevertheless remain not completely known, except in special cases. The reason is that trajectories are, with rare exceptions, described by transcendental functions of the system parameters that cannot be expressed in closed form. Trajectories can be computed numerically, but computation is a physical process that is limited not only in practice but in principle (by quantum effects). For complex systems (such as the chemical dynamics of a living cell) the physical restrictions on computability make the precise knowledge of system trajectories *transcomputable*. Thom's qualitative theory dispenses with exact knowledge of the trajectories; instead it is concerned with their topological equivalence classes and transitions between these classes. Manfred Eigen, in his recent work on "self-organization of matter and the evolution of biological macromolecules" (*2*), comes to the same conclusion about the predictability of evolution processes: "What the theory does explain is the general principle of selection and evolution at the molecular level . . . . What the theory may explain is how to construct simple molecular models representing possible precursors of living cells . . . . What the theory will never explain is the precise historical route of evolution." Eigen's base of departure is not topology but experimental work on biochemical reactions. Yet it leads to the same kind of theory: a qualitative theory of the succession of metastable forms.

The mathematical content (especially chapters 3 to 7) consists of numerous results, problems, and challenges. Students of dynamical systems will find a wealth of ideas and inspiration even though, or perhaps because, the book is not written in the customary style of definitions, theorems, and proofs. For the general reader the appendix should be helpful (the notions of group, pseudogroup, and group action unfortunately are missing).

Missing from Thom's book are any references to automata and to pattern recognition. The latter, a branch of computer science, has for years struggled with related problems, namely with the automatic recognition of representatives of forms. In particular the notion of fuzzy set could eliminate some of the difficulties that Thom encounters when trying to apply his notions to forms that have no threshold.

Automata have strong formal similarities to dynamical systems theory. In fact, Arbib (*3*) has given a formulation that comprises differential dynamical systems as well as finite state automata as special cases. Both are valid models of reality. As Thom himself points out: Experimental data are always a finite set of points in a bounded time interval. Describing them with a differentiable manifold is a matter of taste, not of logical necessity. Perhaps Thom is influenced in his preference by the great traditional models of physics, which are continuous models. Computer science, obviously, tends toward finite state automata. Yet many fundamental problems are isomorphic. Thom's notions of structural stability, attractors, and catastrophes are important for finite state automata. A systematic transfer of results from continuous dynamical systems could prove fertile for automata theory.

Conversely, some very fundamental and powerful results for automata have been obtained by John Rhodes (*4*). These are results about decomposition and complexity of automata. The complexity of forms is discussed by Thom but Rhodes seems to have deeper results. A unification of dynamical systems and automata theory remains largely to be done.

The work of Thom and Rhodes may well be a turning point for mathematical biology. There have been many useless and meaningless formalisms. Now the promised land may be in sight at last. The publication of Thom's book is a major event. A new paradigm for the foundations of physics and biology has been proposed. There should be vigorous discussion. Many arguments that are merely sketched should be made rigorous. Whatever the fate of individual points, I can only agree with Waddington, who writes in his introduction to the book: "Whether he is justified or not [in certain specific applications of his method] 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 [and other subjects]. 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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3. M. I. Arbib, in R. E. Kalman, P. L. Falb, M. A. Arbib, *Topics in Mathematical System Theory* (McGraw-Hill, New York, 1969).
4. J. Rhodes, "Application of Automata Theory and Algebra to Biology, Physics, Psychology, Philosophy, Games, Codes" (notes distributed by the Department of Mathematics, University of California, Berkeley, 1971).

Condensed Matter

Introduction to the Theory of Liquid Metals. T. E. FABER. Cambridge University Press, New York, 1972. xiv, 588 pp., illus. $37.50. Cambridge Monographs on Physics.

Liquid Metals. Chemistry and Physics. SYLVAN Z. BEER, Ed. Dekker, New York, 1972. x, 732 pp., illus. $35. Monographs and Textbooks in Material Sciences, vol. 4.

It cannot be a simple enterprise to write a text on liquid metals. Dense liquids fall within the confines of the condensed state of matter, and the foundations of the theory of these complex systems cannot, as yet, be regarded as part of the entrenched dogma of condensed-matter physics. Metals (or at any rate free-electron-like metals) are, in their regular solid forms, perhaps one of the better understood classes of matter, but this is certainly to a large extent a direct consequence of crystallinity.

Faber's text attempts to come to grips with the problem of expounding the nature of dense conducting fluids and their propinquity to both liquids and metals. Given the imposition of some rather stringent boundary conditions his effort is, on the whole, successful. The structure of the book is close to what one might expect: a lengthy introductory section on the electron theory of simple metals, two chapters dealing with the structural and dynamical aspects of simple liquids, a chapter devoted to the nature and manifestation of electron levels in disordered systems followed by one on the transport properties of liquid metals, and finally a discussion of the properties of alloys including more bizarre systems of which the mercury amalgams and liquid semiconductors are endemic.

The subject matter is exhaustively researched and the style is lucid, occasionally sardonic, and with a tincture of skepticism that eases one gently through the more jejune but unavoidable aspects of the lore of liquids. It is therefore regrettable that from the standpoint of the student (and, one might observe, his teacher) the price is prohibitive.

Many of the topics touched on in Faber's text are dealt with in much greater detail in the review papers included in the volume edited by Beer. In addition to extensive articles on both electron and mass transport, structure and dynamics, and equilibrium and nonequilibrium properties, there is contained in the book a considerable body of information of a more chemical and thermodynamic nature. Occasionally one has the impression that approaches which are successful in classifying the experimental information from insulating fluids (and their mixtures) will, *mutatis mutandis*, automatically apply in metallic systems. In this respect the importance of the interacting-electron gas on the gross properties of conducting fluids is more clearly exposed in Faber's text, and indeed many of the claims made by the contributors to Beer's volume can be found with acerbic and illuminating critiques in Faber's concordance.

As a compendium of papers on the nebulous forefront of research in liquid metals and alloys, Beer's book will have a certain value as a reference text. Unevenness of style is difficult to avoid with some 20 or so authors, but there is also a more troublesome variation in quality with which to contend.

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Paleogeophysics

Palaeomagnetism and Plate Tectonics. M. W. MCELHINNY. Cambridge University Press, New York, 1973. x, 358 pp., illus. $27.50. Cambridge Earth Science Series.

The measurement of the permanent or "fossil" magnetism of rocks in order to study the earth's magnetic field in the past is one of the few examples, and certainly the most significant example, of paleogeophysics. Not surprisingly, therefore, it is this branch of geophysics which has contributed most to geology. In particular it formed the basis of the so-called "revolution in earth sciences" within the past decade—a revolution not only in our understanding of the earth but also in that it has brought together the formerly rather disparate disciplines of geology and geophysics. The field itself provides an interesting blend of both geology and physics (as reflected in this text) which attracts students and experts alike.

In view of how much this subject has contributed to the earth sciences, within a comparatively short life-span of little more than 20 years, there are remarkably few texts which cover it adequately. Only one other has attempted to be as comprehensive as McElhinny's, that by Irving (*Paleomagnetism and Its Application to Geological and Geophysical Problems*, Wiley) published in 1964. Both books are classic and definitive texts, but needless to say Irving's has dated considerably as a result of developments relating to reversals of the earth's magnetic field, sea-floor spreading, and plate tectonics and of the acquisition of a great number of, typically higher-quality, paleomagnetic data. In many ways the title of Irving's book would be equally or more appropriate for this one, which is primarily a text on paleomagnetism rather than plate tectonics.

After outlining the nature of the earth's magnetic field and the assumptions and potential of paleomagnetism the author provides further background material in chapters on the basic principles and theory of rock magnetism and the procedures and techniques for the collection and measurement of samples and the analysis of results. This section ends with a particularly useful and detailed consideration of field and laboratory tests for the stability of remanent magnetization and a listing of minimum criteria for the reliability and hence acceptability of paleomagnetic results. Subsequently a chapter is de-