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Catastrophe theory: *Selected papers*, 1972–1977, by E. C. Zeeman, Addison-Wesley, London, Amsterdam, Ontario, Sydney, Tokyo, 1977, x + 675 pp., $26.50 (hard binding), $14.50 (paper binding).

For the general public, catastrophe theory (or CT) has become the biggest thing in mathematics. René Thom and Christopher Zeeman are the two leaders of this field. L'Express (October 14–30, 1974) asserts that the "new Newton" is French (i.e. Thom). An announcement of Zeeman's lecture at Northwestern University in the spring of 1977 contains a quote describing catastrophe theory as the most important development in mathematics since the invention of calculus 300 years ago. Newsweek has given similar comparisons. Zeeman juxtaposes Newton and Thom in the volume under review (briefly ZCT), p. 623. Thom writes "... CT is–quite likely–the first coherent attempt (since Aristotelian Logic) to give a theory on *analogy*." [p. 637, ZCT]. On the back cover of Thom's book, *Structural stability and morphogenesis* [English translation, Benjamin, 1975 or Thom's SSM], is the quote from the London Times review, "In one sense the only book with which it can be compared is Newton's *Principia*."

Recently however, the importance of CT has been sharply and publicly challenged by Hector Sussman and subsequently by Sussman and Raphael Zahler [*Catastrophe theory as applied to the social and biological sciences*: *A critique* to appear in Synthese]. A critical story on CT by Gina Kolata in "Science", April 15, 1977, is headed: *Catastrophe theory*: *The emperor has no clothes*. A front page story on the New York Times, November 19, 1977 focuses on the challenges to CT.

To write a review in this environment has a very personal side for me. On one hand my own work on dynamical systems is closely connected to the origins of CT. I have had a long and close personal and professional relationship with both Thom and Zeeman. More than 20 years ago I was discussing singularities of maps, transversality, and immersions with René Thom. Thom tried to interest me in an early draft of chapters of his book *Structural stability and morphogenesis* in 1966.

On the other hand I have remained skeptical and aloof from CT, perhaps due to my conservatism in science. While my colleagues and students were showing enthusiasm for CT, I gave critical lectures, one at the University of Chicago in 1974, one at the Aspen Institute of Physics in 1975. More recently I have been quoted negatively in the "Science" and New York Times references above. This is the first time I have written on the subject, and I should warn the reader of this negative bias, far from shared by many of my fellow mathematicians.

Some of the mathematics underlying CT, especially transversality, and singularities of maps, has played a constructive role in outside disciplines, and is destined to play an ever increasing role. On the other hand I feel that CT itself has limited substance, great pretension and that catastrophe theorists have created a false picture in the mathematical community and the public as to the power of CT to solve problems in the social and natural sciences. It is to the credit of Sussman and Zahler that they have seriously challenged this false picture.

On this matter of warning to the reader, there is also the problem of my "quoting out of context". In fact the quotes I have made above and will make below are often imbedded in more prosaic language, perhaps tempering the brief and sometimes sharp sounding statements I refer to.

At this point, I would like to acknowledge my debt to J. Guckenheimer, C. Zeeman, H. Sussman, and M. Hirsch among others for very useful conversations on the subject here. Besides the work of Thom and Zeeman, on Catastrophe Theory, I have also learned much from that of Sussman and Zahler.

Just what is Catastrophe Theory? Thom writes that CT "... has to be considered as a theory of general morphology" 633, ZCT and Zeeman "CT is a new mathematical method for describing the evolution of forms in nature." (1, ZCT).

However the new mathematics associated to CT really is contained in what is called elementary catastrophe theory (ECT) by Thom and Zeeman. Very briefly, ECT studies a smooth real valued map f as a function (often called a "potential function") of a state x and parameter μ. Here the state lies in some Euclidean space and μ also varies in a (usually low dimensional) Euclidean space. The problem is to find local canonical forms for "generic" f, using a smooth change of coordinates in the variables x, μ, $y = f(x, \mu)$. In this case, "generic" means for an open dense subset in a suitable function space. A local canonical form is defined in a neighborhood of a pair (x_0, μ_0) at which f is singular. ECT solves this problem when μ is in a low ($\leqslant 5$) dimensional space. In particular, when μ lies in a 4 dimensional space (e.g. space-time), Thom found seven canonical forms. We will shortly describe the case of the cusp catastrophe where x is in $\mathbf{R}$ and μ in $\mathbf{R}^2$.

Is CT much more than ECT? Here Thom and Zeeman differ. Thom writes "... that Christopher's criticisms arise basically from a strict dogmatic view of CT which he identifies with ECT ..." 633, ZCT. Zeeman replies (same page) that his emphasis on ECT has been mainly because of its usefulness in applications. In fact, the book under review, although titled CT, deals almost completely with ECT (its mathematics and applications).

In my mind, when CT goes beyond ECT it loses pretty much any direct touch with mathematics. It is true that Thom refers to nonelementary CT as "generalized catastrophes, composed map catastrophes, G-invariant catastrophes" etc. 633, ZCT. But here the relationship of the mathematics to nature in the CT literature becomes tenuous and infrequent. For example in Thom (SSM), the words "generalized catastrophe" are used to describe situations where no mathematical model is proposed. More particularly under the picture of "feather buds on a chicken embryo", Thom writes "a generalized catastrophe and example of symmetry in biology" (Figure 21, Thom's SSM). He labels a picture of "the Crab nebula, the remains of the explosion of a supernova" with "a partially filament catastrophe in astrophysics" (Figure 17, Thom's SSM). Thom described the filament catastrophe as a certain "codimension two" type of generalized catastrophe, but the mathematics is not specified.

No doubt it is in this spirit Thom writes (p. 189 in *Structural stability, catastrophe theory and applied mathematics*, SIAM Rev., April, 1977) "The truth is that CT is not a mathematical theory, but a body of ideas, I dare say a state of mind."

In ECT, one can take the gradient of the "potential" function with respect to the state variable to obtain a dynamical system $x' = \operatorname{grad} f_\mu(x)$ parameterized by μ, $f_\mu(x) = f(x, \mu)$. It is in this context that Thom introduced ECT in his book SSM, Chapter 5. He takes μ to be in $\mathbf{R}^4$ or space-time. Elementary catastrophes are defined there as the points μ of space-time for which the qualitative dynamics of $x' = \operatorname{grad} f_\mu(x)$ changes in a neighborhood of the attractors (stable equilibria) of that dynamics. Generalized catastrophes are defined analagously where the gradient dynamical systems are replaced by a more general class of dynamical systems.

However the mathematical theorems that Thom states (later proved by Mather) actually give the classification of elementary catastrophes according to the theory of singularities of maps in contrast to the theory of dynamical systems. Guckenheimer subsequently pointed out (Bull. Amer. Math. Soc. **79** (1973), 878–890, and in *Bifurcation and catastrophe*, Dynamical Systems (ed., Peixoto) Academic Press, 1973) that the two classifications differ already when μ is in $\mathbf{R}^3$ (and certainly for μ in $\mathbf{R}^4$). I believe this weakens much of the scientific or philosophical foundations of CT which is a theory based on dynamical systems (Thom's SSM). If even the elementary catastrophes don't correspond to the dynamical classification, what of the generalized catastrophes where the mathematics in SSM deals in only a few known examples?

In fact ECT seems much closer to what economists call comparative statics than to dynamical systems.

One can sense a corresponding change of perspective in the work of the catastrophe theorists. As we have noted, the starting point of CT in Thom's SSM was the "bifurcations" of dynamical systems parameterized by space-time. In Zeeman's work, these parameters changed from "space-time" to "control parameters". Now in his SIAM article (cited above) Thom also speaks of control parameters and seems to de-emphasize the dynamical system foundations of CT, especially relative to space-time.

The cusp catastrophe is the most important example of a catastrophe and much if not most of the book under review (ZCT) centers around the cusp catastrophe and its applications. Zeeman with Isnard (ZCT, 329 in *Some models from catastrophe theory in the social sciences*) describes the canonical model as follows. In 3 dimensional space $\mathbf{R}^3$, variables (a, b, x), let M be the cubic surface defined by $x^3 = a + bx$. Here a, b are horizontal axes and are the controls; x is the vertical state variable. The fold curve F is where the vertical lines are tangent to M and is given by $3x^2 = b$. The projection of F onto the control space is called the bifurcation set B. The equation of B is $27a^2 = 4b^3$ and has a cusp at the origin. It is supposed that M is given by $\partial P/\partial x = 0$ where P is some (probability or potential) function $P\colon \mathbf{R}^3 \to \mathbf{R}$

and that a sociologically (or physically) meaningful subset G of M is the set of local maxima (or local minima) of P.

The state of the system stays on G and is controlled by the choice of (a, b). Thus when (a, b) crosses B, the state may be forced to make a discontinuous jump to remain on G.

This is all laid out clearly in the following Figure 330, ZCT from Zeeman and Isnard.

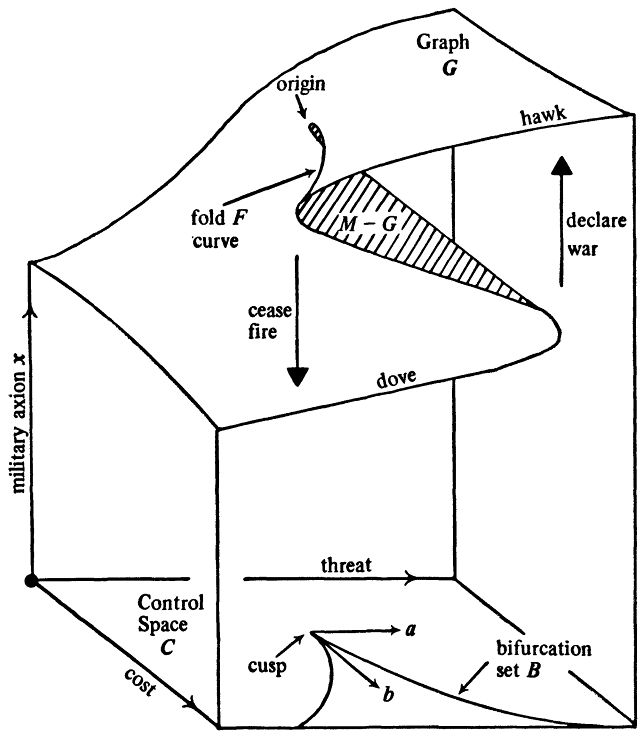

FIGURE 11*

In the model under discussion, a is a numerical representation of a threat, b the cost and x represents military action. The discontinuity, representing a jump in military action could be thought of as a declaration of war.

Thus what Zeeman and Isnard have given us is a model for the study of a nation deciding upon its level of action in some war. I consider this paper the most developed of Zeeman's papers on CT and the social sciences and he writes "And I believe that sociology may well be one of the first fields to feel the full impact of this new type of applied mathematics, . . . " (627, ZCT).

Sussman and Zahler have discussed this model in detail. Here I would like to focus on the question of its *justification* as a model of military decision

*Reprinted from the chapter entitled "Some models from catastrophe theory in the social sciences" by C. A. Isnard and E. C. Zeeman in *The Use of Models in the Social Sciences* (Social Issues in the Seventies Series) edited by Lyndhurst Collins, Tavistock Publications, London, copyright © 1976 Seminars Committee of the Faculty of Social Sciences of the University of Edinburgh, Scotland.

making; in this I believe the authors have failed. Their efforts in this direction lie on the mathematical side. Zeeman and Isnard write "... we shall introduce sociological hypotheses, and translate them into mathematics. The deep theorems of CT will enable us to synthesize the mathematics. We can then translate the synthesis back into sociological conclusions. It is not immediately apparent, without the use of the intervening mathematics, that the sociological hypotheses imply the sociological conclusions, and that is the purpose of using catastrophe theory." (314, ZCT).

The trouble is with the sociological hypotheses. Evidence for them, or for the model, should be given, for example from military history, studies in decision making, or sociological, political studies in general. There is much theory and data from history and social sciences relevant to the model of Zeeman and Isnard. None of this finds its way into the paper directly or indirectly save for brief references to Tolstoy on calculus in *War and peace* and Lorenz, *On aggression*.

Let us look at some of the sociological hypotheses that Zeeman and Isnard in fact do make. Summarized into mathematics, they express these hypotheses graphically by:

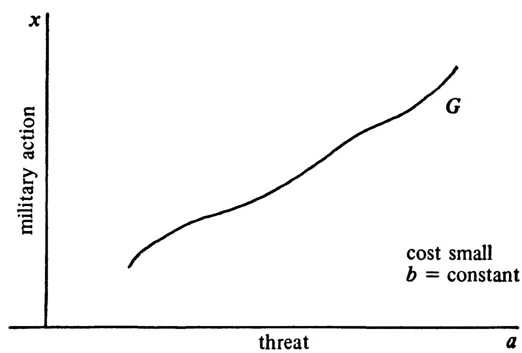

FIGURE 6, 316, ZCT

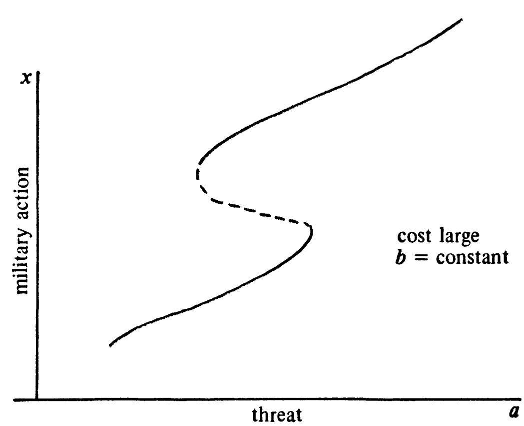

FIGURE 7, 317, ZCT

Zeeman and Isnard then ask how does Figure 6 evolve into Figure 7. "The main theorem of CT tells us that qualitatively there is only one way for this evolution to occur" [p. 318, ZCT], and they deduce Figure 11, cited above.

One trouble is the definition for "small" cost and "large" cost that Zeeman and Isnard use. They mean there exists b_0 such that "small b" means $b < b_0$ and "large b" means $b > b_0$, the same b_0 for "small" and "large". Thus Figures 6 and 7 (the sociological hypotheses) already describe the model for all b save b_0; Figure 6 applies if $b < b_0$, Figure 7 if $b > b_0$ [p. 331, ZCT]. No evidence or justification is given for this sociological hypothesis, that such a b_0 exists (there are arguments given earlier however to justify a "delay rule" vs. "Maxwell's rule").

In an earlier paper by Zeeman alone on a model for the stock exchange, (paper 11 in ZCT), Zeeman uses the words small and large in the same way as above [p. 364, ZCT]. Thus his hypotheses already give, without using any mathematics at all, the structure of the model as a surface in E^3 save for a 2-dimensional plane described by one control parameter being constant. Nevertheless Zeeman refers to these hypotheses as "disconnected" and "local". He states: "Summarising: we insert seven disconnected elementary local hypotheses into the mathematics, and the mathematics then synthesises them for us and hands us back a global dynamic understanding." [p. 362, ZCT] and elsewhere in that paper, "We now use the deep classification theorem of Thom to synthesise the information acquired so far into a 3-dimensional picture of the surface S . . . " [p. 366, ZCT]. Note that Zeeman is referring to the same theorem that Thom credits to Whitney in "Nature", December 22, 1977. Thom referring to Sussman and Zahler writes: "I would also like to point out a misquotation by the authors. The classification theorem for the "Cusp catastrophe" erroneously quoted as "Thom's theorem" is in this specific case due to H. Whitney."

No justification for the hypotheses of Zeeman's stock exchange model is given in terms of existing data and/or theory of stock exchanges, price theory, etc. In fact no reference to any economic literature is given in this paper.

A defense might still be made that even though Zeeman doesn't justify the stock exchange or war models, still it is good that he has proposed them. Doing so introduces topology (or geometry) into the social sciences and with luck sociologists or economists will be able to develop the models, test them, verify them, etc. Indeed it is important for scientists, social or otherwise, to be aware of mathematical possibilities for models. A positive aspect of all the publicity given to CT is that it may have increased this kind of awareness.

On the other hand good mathematical models are not generated by mathematicians throwing models to sociologists, biologists, etc. for the latter to pick up and develop. Both Thom and Zeeman seem to fit this caricature sometimes in their work or when they give their views on the future of CT in science. Good mathematical models don't start with the mathematics, but with a deep study of certain natural phenomena. Mathematical awareness or even sophistication is useful when working to model economic phenomena for example, but a successful model depends much more on a penetrating study and understanding of the economics.

On the other hand around CT, not only does mathematics come first but one sees a sort of mathematical egocentricity; understanding the world is a mathematical (even geometrical) problem. Thom's position on this is clear; e.g. "... Eliminate the "obvious" meaning and replace it by the purely abstract geometrical manipulation of forms. The only possible theoretisation is Mathematical." (638, ZCT) or "... I agree with P. Antonelli, when he states that theoretical biology should be done in Mathematical Departments; we have to let biologists busy themselves with their very concrete–but almost meaningless–experiments; in developmental Biology, how could they hope to solve a problem they cannot even formulate?" (636, ZCT).

Along with this mathematical egocentricity there is a kind of mystification of the subject that is being created by both Thom and Zeeman. Zeeman does this when he speaks of the "deep classification theorem of Thom" as above and elsewhere in his papers. Presenting this picture to nonmathematicians and even nontopologists has an intimidating effect. Thom does this by using technical mathematical terms without explanation when addressing nonmathematical audiences, and often writing obscurely. Then Zeeman deepens the mystifying power of CT by explaining Thom's obscurities with: "When I get stuck at some point in his writing, and happen to ask him, his replies generally reveal a vast new unsuspected goldmine of ideas" (622, ZCT).

Some defenders of CT may accuse me of discussing very special examples not characteristic of the literature of the subject. I feel that the problem of lack of justification discussed above, is also found in Zeeman's other models. Furthermore Thom's models are even less specific and less developed. On the other hand, Thom's work in CT covers many subjects; in this connection Zeeman writes in his Scientific American article, April 1976, p. 65: "The method has the potential for describing the evolution of form in all aspects of nature, and hence it embodies a theory of great generality."

Corresponding to the universality of CT is a certain superficiality, almost in a dual sense! But one can find even more local maxima in nature than cusps.

It is Thom and Zeeman who have brought CT to the attention of the scientific community with their studies mainly in biology and the social sciences. Thus I can't go along with Ian Stewart's assertion, in "Nature", December 1, 1977, p. 382, "The case in favour of CT rests not on speculative models in the social sciences, but on successful applications to the physical sciences."

I would like to make it clear that I find merit in the Catastrophe theorists use of modern calculus and geometric techniques in models in science. In particular discontinuities can often best be understood via this kind of mathematics. For example it would be important to find a calculus oriented model for the computer, a machine which is intrinsically discrete. Such a calculus model would not be exact, but it could give great insight to automata theory.

There is much value to science in some of the underlying mathematics of CT. I think especially of transversality theory and the theory of singularities of maps. The idea of transversality goes back in history, but took a good development with Pontryagin and Whitney. Thom had used transversality in his early work and by 1956 was putting the concept into a powerful systematic use with his theorem of transversality of jets. In its modern form, *transversality* for a smooth map $f\colon V \to W$, relative to a submanifold M of W means that whenever $f(x) = y \in M$, $T_x(W) = T_y(M) + Df(x)(T_x(V))$. In other words the derivative of f at x has image complementing the tangent space of M at y. In this case the inverse function theorem implies that $f^{-1}(M)$ is a submanifold of the expected dimension.

Transversality has many ramifications, especially when f itself is a derivative map (or jet map) of some order of another map. These ideas give form to the study of singularities of maps. Again it was Thom who, after fundamental work of Whitney, substantially enriched that theory. Now transversal maps have the property of being an open and dense subset of an appropriate function space. The openness of transversality often can be used to show that corresponding models have the important property of robustness. A model is *robust* if the properties under study remain after perturbation. The approximation properties of transversality imply that one might expect to find transversality present in smooth models. For these reasons it is important for mathematically oriented scientists to be aware of transversality and such examples as cusps. In particular, workers in bifurcation theory from a classical point of view should be (and to some extent are) studying these modern ideas.

Elementary catastrophe theory studies a particular class of maps, (the target space is 1-dimensional, but parameters are allowed) from the above point of view. Sometimes, as in classical mechanics, this situation arises naturally. For example, statics in a simple mechanical system (M, K, V) studies the local minimum of the potential. Here K is kinetic energy or a Riemannian metric on configuration space M while $V\colon M \to \mathbf{R}$ is the potential energy. Suppose that V depends on a parameter μ in $\mathbf{R}^k$ (i.e., comparative statics). Then ECT is natural for such a study as in ZCT, #17, *A Catastrophe model for the stability of ships*. While this kind of mathematics has a legitimate and even important place in physics, I must object to Zeeman's assertion in the introduction of his ship article (442, ZCT): "it (this example SS) is a prototype revealing catastrophe theory as a natural generalization of Hamiltonian dynamics."

More generally the theory of singularities of maps has a constructive role to play in the physical, biological and social sciences. For example comparative statics, as in Paul Samuelson's *Foundations*, studies economic equilibrium prices p satisfying $f(p, \mu) = 0$ where for each parameter value μ, $p \to f(p, \mu)$ is a map from $\mathbf{R}^n$ to $\mathbf{R}^n$. The problem is: how does a solution vary with μ, even for small μ. Since economic theory has shown pretty clearly that the excess demand (in some form) f is not derived from any potential function, CT itself is not relevant. On the other hand, the problem naturally fits into the theory of singularities of maps.

We end this review by a remark on history. Catastrophe theorists often speak as if CT (or Thom's work) was the first important or systematic (CT is "systematic" as a certain study of singularities, but not as a study of discontinuous phenomena) study of discontinuous phenomena via calculus mathematics. My view is quite the contrary and in fact I feel the Hopf bifurcation (1942) for example lies deeper than CT. The Hopf theory shows how a stable equilibrium bifurcates to a stable oscillation in ordinary differential equations. Moreover, there is the reference *Theory of oscillations* by Andronov and Chaiken, 1937, with English translation in 1949 published by the Princeton University Press which is never referred by Thom or Zeeman. This book besides giving an early account of structural stability, gives a good account of dynamical systems in two variables with explicit development of discontinuous phenomena, quite close to Zeeman's use of the cusp catastrophe. Examples from physics and electrical engineering are studied in some depth.

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Bornologies and functional analysis, by Henri Hogbe-Nlend, Mathematics Studies no. 26, North-Holland, Amsterdam, New York, Oxford, 1977, xii + 144 pp., $19.50.

The author states in his introduction that functional analysis is analysis over infinite dimensional spaces. This is a fact. But concrete infinite dimensional spaces, e.g. function spaces, are more important than the reader would gather from the book.

Hard functional analysts evaluate and prove a priori inequalities. Their topologies are related to the problems they study, to the inequalities they prove. The solution of a concrete problem is the main emphasis. If this solution involves the consideration of a half dozen topologies on a given space, well it does, but the problem is solved.

The soft functional analyst does not find these proofs elegant. Some proofs may even be "clumsy", the hypotheses being too strong. Of course, the examples to which the "better" proof applies are fairly artificial, but that does not affect the general principle. Elsewhere, a "main theorem" can be proved, its proof involves one single topology or convergence on the space. The other topologies only serve to bridge the gap between the main result and the applications. The hard functional analyst does not appreciate the progress since the main result is only justified by its applications.

Bornology is a chapter of soft functional analysis.

Locally convex space theory is a well established subject. We all know a half dozen or more classes of examples of locally convex spaces. These examples put the flesh on the skeleton when we, or our students, read a textbook on locally convex space theory.

Bornology is not as well established. The reader of a text on bornology may not know how it can be used to help the hard analyst. It is the author's responsibility to lead his reader to the applications. These applications are the final test in judging the value of a soft analytic theory.

In this book, the author places more emphasis on the easy parts of bornology, or in the chapters where functional analysts used bornologies before they were invented than on the chapters where the consideration of bornologies really brings something to functional analysis. A senior