review article

Claims and accomplishments of applied catastrophe theory

Raphael S. Zahler* & Hector J. Sussmann†

Several representative attempts to apply catastrophe theory to biological and social science problems turn out on close analysis to be characterised by incorrect reasoning, far-fetched assumptions, erroneous consequences, and exaggerated claims. Catastrophe theory seems to have made no significant contributions to biology and the social sciences, and to have no advantage over other better-established mathematical tools which have been used to better effect.

EMBRYOLOGY, ethology, ecology, and geology; physics, economics, dynamics, and linguistics; prison riots, literary symbolism, and the Vietnam war—these are some of the subjects to which catastrophe theory is said to be applicable. Its novel mathematical apparatus seems to be a near-universal tool, according to its proponents: "Properly understood and exploited, this ever-expanding web of concepts promises mankind a unique weapon against ignorance and a profound insight into the universe"[1].

We disagree. And because we feel that the many researchers now being attracted to catastrophe theory stand to gain nothing but disappointment and wasted time, we have written a critical study of applied catastrophe theory[2]. Our conclusion is that the claims made for the theory are greatly exaggerated and that its accomplishments, at least in the biological and social sciences, are insignificant.

This is because catastrophe theorists have misused the basic mathematics in ways that lead to incorrect reasoning; they have offered models which are based on unreasonable assumptions and which lead to erroneous conclusions; and they have made predictions which are either vacuous, tautologous, vague or impossible to test experimentally. We do not say that catastrophe theory cannot possibly be applied. There may be legitimate uses in areas of physics and engineering, and some have suggested that the use of topological methods will be conceptually stimulating to researchers. So far, however, its record is poor.

We stress that we are not discussing the correctness or importance of catastrophe theory as a purely mathematical subject: we are sceptical only about its usefulness as a tool for extramathematical applications. We present this critical picture because we are excited about the prospect of new applications of mathematics, and concerned that many will be disenchanted with all of modern mathematics when they discover, as we have, that catastrophe theory is a blind alley.

This article is organised round a list of ten main kinds of defect found in catastrophe theory models in biology and social science. Each is illustrated briefly by one or two examples. For more detail, the reader should consult our longer paper[2]. We are grateful for the advice of R. FitzHugh, J. C. Scanlon, H. Othmer, C. S. Hui, B. Goodwin, J. Sturtevant, G. Velicelebi, S. Mabrey, J. F. Chlebowski, E. S. Crelin, J. F. G. Auchmuty, N. Van Arkel whose comments have been of great help in the preparation of this paper.

The cusp catastrophe

Most applied catastrophe theory is based on the 'cusp catastrophe' (Fig. 1). In this picture, the horizontal plane represents the possible values of two control parameters a and b, and the behaviour of the system is plotted on the vertical x-axis. For example, suppose we place a globular protein in solution and study its denaturation in terms of the concentration a of a denaturant and the temperature b. The extent of denaturation x_0 (as measured by the negative of the optical rotatory dispersion) as a function of denaturant concentration a_0 and temperature b_0 can be plotted as a point in the vertical line through (a_0, b_0); all such points form the curved 'cusp' surface shown, according to Kozak and Benham[3].

So far this is just standard analytical geometry. The alert reader, however, may have noticed that above each point (a_0, b_0) in the shaded region R of the control plane lie not one but three different points of the behaviour surface; which one represents the actual behaviour at (a_0, b_0)? The answer, given by the delay rule, is that it depends on how (a_0, b_0) is approached. As a and b vary continuously, x is to vary continuously in the cusp surface as long as possible. When continuous variation of x is not possible, then x is to jump to another sheet of the surface. For example, suppose the control parameters vary in such a way that the point (a, b) moves from Q_1 to Q_2 along the path P_1, shown in Fig. 1. Initially, there is only one point in the cusp surface lying above each point in P_1, so we follow the line shown from Q_1 towards J. Even when we pass point J the delay rule keeps us on the bottom sheet. But, when we reach point K, we must jump to K_2 on the upper sheet as shown. This is how sudden changes of behaviour in response to smooth changes of controls occur in catastrophe theory models.

Why the cusp? Catastrophe theorists argue that, according to a "deep mathematical theorem" of René Thom, essentially any system where sudden changes occur and where two control parameters appear can be described by the cusp; it is an inevitable, universal paradigm. According to Kozak and Benham, in fact, Fig. 1 correctly predicts the denaturation behaviour of RNAse or collagen subject to the influence of temperature and $CaCl_2$ concentration. For example, following path P_1 from Q_2 to Q_1, or P_3 from Q_3 to Q_4 gives a sigmoid curve for x, which corresponds to the appropriate experimental data. By distorting the cusp M somewhat (which is permissible

*104 Hubinger Street, New Haven, Connecticut 06511.
†Department of Mathematics, Butgers College, Hill Centre for the Mathematical Sciences, Busch Campus, New Brunswich—New Jersey 08903.

in the theory), data from other systems can be modelled, they say.

Confusion about continuity

But when we look more closely at the model, things begin to come apart. First of all, are the transitions from one conformation to another really discontinuous jumps? The authors themselves admit that the experimental curves are not vertical, but this is only part of the problem. For a given small protein such as RNAse, Van't Hoff's relation

$$\Delta H = RT^2 \frac{\partial \ln K}{\partial T}$$

implies that the enthalpy change ΔH sets a precise bound to how steep the temperature denaturation curve can be, even in a two-stage denaturation model (under the usual assumption that the molecules do not interact). Thus for a given protein

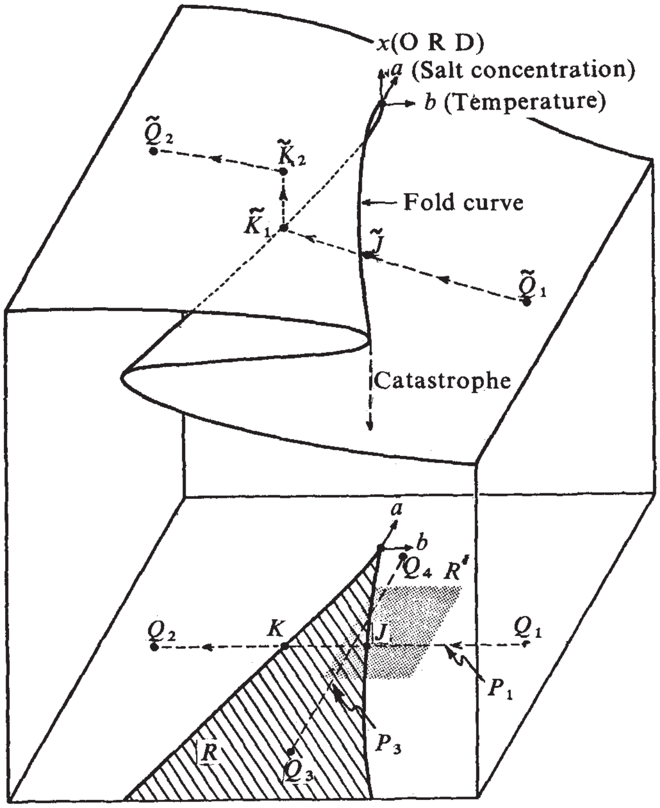


Fig. 1 An example of the cusp catastrophe, as it has been used to model protein denaturation.

we cannot even say that discontinuous jumps represent a limiting case. Now one of the main claims of catastrophe theory is that it is a unique way of applying mathematics to discontinuous phenomena. We have just seen that the reality of denaturation is inherently continuous; the same can be said for most biological situations that catastrophe theory has tried to model. It is therefore not surprising that most model-familiar to scientists are continuous. In these situations catastrophe theory gives no advantages in exchange for its greater complexity. Furthermore, in social science models, as we will see below, catastrophe theory often makes the reverse mistake, turning an obvious dichotomy into a continuous variable.

These continuity questions are not just mathematical fine points. By freely confusing the intuitive notion of 'jump' as a rapid change with the precise mathematical notion of a jump discontinuity, catastrophists are prone to construct false or misleading arguments (see, for example, the discussion of embryology below).

Our next criticism is more serious. Having plotted experimental data that correspond to the part of the cusp above the region R′ (Fig. 1), the authors claim that for the two-variable situation, "the mathematical theory states that if a change of morphology from one state to the other is observed", then the system must be described by a cusp. And even more: "if one has, say, two or three representative points from the bifurcation set of a given denaturation experiment, it should be possible to construct the surface M, and thereby to predict the behaviour of the system for values of the constraints not studied experimentally". Surely a theory that would allow a scientist to make two or three experimental observations on a laboratory system and then, with no other information or assumptions, deduce the behaviour of the system in all other conditions would be a great achievement. However, it is too good to be true: the authors have made serious blunders on at least three levels.

Use of Thom's theorem to justify extrapolation

The suggestion that Thom's theorem makes it possible to deduce the complete shape of a behaviour surface from some partial information about it is made by E. C. Zeeman[4], the leading applied catastrophe theorist. (For example, "through catastrophe theory we can deduce the shape of the entire surface from the fact that the behaviour is bimodal for some control points".) However, Zeeman's statements are false. Take, for instance, the surface M of the denaturation model. Mathematically, it is simply not true that Thom's theorem forces M to look like Fig. 1. There might be many cusps or folds, and they need not be located at the origin nor oriented as shown. Furthermore, Thom's theorem cannot be used to infer the shape of an entire surface from some partial knowledge of it. Quite the contrary: any surface is arbitrarily close to a surface which satisfies the conclusion of Thom's theorem, and therefore (since all observations have a finite error) invoking this theorem can tell us nothing new about behaviour. This conclusion, which applies to all CT models, is not really surprising: suggesting, as Zeeman does, that one can deduce non-trivial facts about "the shape of all possible equilibrium surfaces" from a theory that makes no physical or behavioural assumptions is tantamount to claiming that the world can be deduced by pure thought—a claim which few scientists would accept.

Prediction contrary to fact

Biochemically, the extension of the surface from the region R′ to the entire plane is not only mathematically unsupported but factually wrong, because it leads to implications that conflict with experimental data. For example, if we follow path P_1 in the right-to-left direction, we saw above that, according to the delay rule, this drop in temperature will cause renaturation at a temperature below that at which denaturation took place. But such hysteresis simply does not occur with collagen or RNAse, the systems Fig. 1 is supposed to apply to. Thus the cusp is not a suitable model for the denaturation of these or most other simple proteins. It is true that there are some large proteins which exhibit hysteresis, but one cannot build a general theory of denaturation on atypical examples. In fact, the authors' attempt to extend their idea to other simple systems forces them to construct a model (Fig. 2) which is not even diffeomorphic to a cusp (they seem not to have noticed the vertical line above the origin), and therefore falls outside the realm of catastrophe theory altogether.

Lack of true testable predictions

Practically, the catastrophe theory analysis, even if it were correct, tells us nothing. Though the authors refer to the sigmoid curves obtained from paths P_1 and P_3 of Fig. 1 as 'predictions', this is quite untrue; they are merely graphs of the experimental data. Similarly, 'predictions' made by Zeeman in CT studies of embryology[5] and neurophysiology[6] are either (1) contained in the data or (2) purely unverified hopes, or (3) independent of CT or (4) just wrong.

Consider, for example, Zeeman's studies of how homogeneous tissue of an embryo differentiates into two types separated by a frontier, a phenomenon he calls divergence or differentiation[5]. The 'main theorem' of this paper is worth quoting in full: "Homeostasis, continuity, differentiation, and repeatability imply the existence of a primary wave. In other words a frontier forms, moves and deepens, then slows up and stabilises, and finally deepens further".

It is hard to imagine what we could learn from such a breathtakingly vague statement, but we need not ponder this very long, since its proof is wrong as well.

Misuse of genericity

For one thing, the surface eventually chosen to model the cellular differentiation is not dictated by Thom's theorem but is obtained by a series of arbitrary choices which are justified—if at all—by appealing to 'genericity'. This mathematical concept is a way of excluding 'exceptional' or 'degenerate' cases. (For instance: 'generically, the axes of an ellipse are not equal' means that circles are an exceptional class of ellipse or, more intuitively, that a 'randomly chosen ellipse' is quite unlikely to be a circle.) In the course of his proof, Zeeman arbitrarily translates the vague word 'repeatability' into the precise concept of genericity. But then, all his theorem says is that if nothing exceptional happens, then the frontier moves.

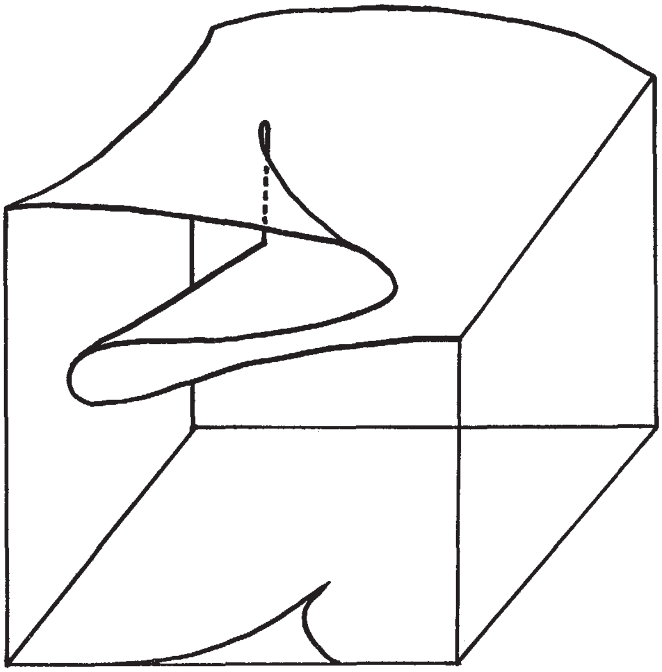


Fig. 2 Distorted cusp catastrophe; see ref. 3, Fig. 2.

Zeeman's 'proof' consists of no more than the observation that, if the frontier did not move, that would be quite exceptional. To evaluate this kind of reasoning properly, notice that the same logic, if correct, would apply equally well not just to frontiers of tissues but to anything whatsoever. So, Zeeman's reasoning 'proves' that everything moves except for those exceptional objects that do not. (For details, see ref. 2 §12 a).

Later in the same paper, Zeeman 'proves' that the frontier moves initially at constant speed. His proof is as follows: the frontier can be replaced initially, to first order, by its tangent, QED. Again, there is nothing in this reasoning that restricts its validity to frontiers of tissues, or to the time when the frontier forms. So Zeeman is really proving that everything moves at constant velocity. But let us apply the same logic once again. Why not use the second degree Taylor approximation? And why not argue about the acceleration as Zeeman does about the velocity, and conclude that the acceleration is non-zero, because it would be exceptional if it were zero? This gives us the conclusion that everything moves with non-constant velocity.

Finally, why can we not take the zeroth order approximation? The conclusion then is that nothing moves at all.

We see that using these methods, everything, no matter how absurd, can be proved.

More than two thousand years ago, Zeno formulated his celebrated paradox of the arrow. Consider an arrow in its flight. 'At each instant of its flight the tip of the arrow occupies a definite position. At that instant the arrow cannot move, for an instant has no duration. Hence, at each instant the arrow is at rest. Since this is true at each instant, the moving arrow is always at rest. This paradox is almost startling. It seems to defy logic itself. '[7]

The development of Calculus has resolved this paradox, and taught us how it is possible for things to change and, at the same time, be what they are at each instant. The position of the tip of the arrow is what it is at each time, but it is different at different times. Hence the arrow can have a definite position at each time, and not be at rest. What is true of the position is also true of velocity. The velocity has a value at each point, but this does not mean that it is constant. Zeeman, a Twentieth Century Zeno, is reformulating the paradox as a corollary, thus ignoring more than two millennia of mathematics.

But Zeeman's main theorem is wrong for other reasons as well: it is easy to show by means of counter examples that Zeeman's conclusions that the frontier does not stabilise or deepen do not follow from his assumptions. When these counter-examples were brought to the attention of catastrophe theorists, the theorem was defended by disclosing new interpretations of the terminology and the hypotheses. For instance, the hypothesis of 'differentiation' is now taken to mean that after some time two different types of tissue are found and no more changes occur. If so, then after some time the frontier no longer moves, so the frontier does indeed stabilise. But of course this 'proof' of stabilisation is transparently circular.

In general, the hypotheses on which many catastrophe-theory arguments are based are often vague. When terms like 'differentiation' and 'repeatability' are used without any precise translation, the correctness of the 'proofs' is hard to assess. Catastrophe theory papers consistently violate one of the most basic rules of the scientific method: state clearly what you mean and do not change definitions in the middle of your reasoning.

Misleading use of mathematics

The most basic criticism of Zeeman's catastrophe theory of embryology, however, is the way the mathematical foundations are used. Others have pointed out that the 'deduction' that differentiation will occur as a pattern of primary and secondary waves arises not from catastrophe theory itself but from the conditions assumed to exist before the primary differentiation process begins (for example a gradient of cell states). In fact, then, most of the results of this paper, regardless of their merit, have nothing to do with catastrophe theory at all.

To see why this is important, we must remind the reader of what catastrophe theory is and is not. First, events in which apparently 'catastrophic' changes occur are not necessarily 'catastrophes' in the technical mathematical sense. Catastrophe theorists agree that the term 'catastrophe' is reserved for certain kinds of singularity of smooth maps, seven of which have been described and classified elegantly by Thom. The keystone of catastrophe theory, in fact, is Thom's profound theorem. It is what is supposed to give catastrophe theory its deductive powers, because it allows one to conclude that a given situation must be represented by a cusp, or one of the other elementary catastrophes.

As we have illustrated, however, most catastrophe theory models make no use of Thom's theorem. Thus the cusp is no longer inevitable or unique.

How, then, does the cusp arise? Often, as in the embryology models, the cusp arose because the hypotheses were carefully chosen to make sure that it would. In other cases, however, experimental data are plotted and found to resemble a cusp[3,8]. This is not surprising: if we plot an ordinary hysteresis loop in

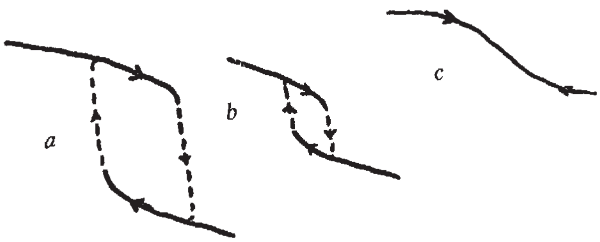


Fig. 3 Hysteresis loops of decreasing size.

two dimensions (Fig. 3*a*), in which fast vertical jumps alternate with slower horizontal movements, and then imagine a continuum of such loops getting smaller (Fig. 3*b*) and finally vanishing to form a continuous curve (Fig. 3*c*), we get a surface resembling the cusp of Fig. 1. Thus we can regard the cusp as a nice way of picturing a system with variable degrees of hysteresis. But it is certainly not unique in this: a surface with a double fold whose projection was any smooth curve (a parabola, for example: see Fig. 4) would do as well. So we must ask again: what is special about a model that looks like a cusp? Although some have found such pictures aesthetically satisfying, or a source of insight, we can only state that the fact that certain data happen to form a cusp-like shape tells us nothing new about the system.

Careless discussion of evidence

Catastrophe theorists have asserted that there is experimental evidence for some of their models. For instance, Zeeman[4] writes, "I have constructed CT models of the heart beat, the nerve impulses and the formation of gastrula and of somites in the embryo. Recent experiments by J. Cooke and T. Elsdale appear to confirm some of my predictions". The facts are as follows. (1) No experiments have been made to test the predictions of Zeeman's nerve impulse models. As for the heartbeat, Zeeman is said to have made experiments in 1972, but the results have not been published (Zeeman, personal communication). (2) The catastrophe theory nerve impulse models disagree with experimental voltage-clamp data[9] in several important aspects, deny the universally accepted concepts of the sodium leak and the independence of sodium and potassium channels, and lead to the wrong propagation speed for the action potential. (3) Zeeman's embryology paper[5], besides being mathematically wrong, betrays the author's inexperience in embryology. For example (p. 27), Zeeman likens the embryonic neural tube to a roll of stiff paper which tries to maintain its curl. But experiment shows that cut neural tube persistently tries to unroll[10]. (4) T. Elsdale *et al.*[11] write: ". . . we do not yet conclude that the observations here presented have confirmed Cooke and Zeeman's model to the exclusion of others". (5) J. Cooke (personal communication) writes: "I, at least, do not regard any of the predictions of the model in which I am involved as being deeply distinctive to catastrophe theory".

Stewart[1] repeats the untrue assertion that Zeeman's embryology predictions have been "recently verified by experiment". Regarding the use of catastrophe theory in social sciences, Stewart writes: "Although most such models still lack precise data, an interesting exception is a study by Zeeman and several collaborators of how tension and alienation among prison inmates influence disorder. A cusp catastrophe fits the data very well, the sudden jumps being riots and truces". In fact, when Zeeman and his coworkers plot points in a plane, and look for a cusp curve which fits them, they do not succeed in fitting one curve, so they use two, and claim that the cusp must have moved during the process. Finally, they do not use any statistical techniques to determine whether or not their pair of cusp curves gives a good fit (compare Fig. 5).

Dixon Jones[8] has made a catastrophe theory model for budworm infestation. His model relies on a 'cusp' which is really not a cusp in the correct mathematical sense. His model predicts a fast rise of a variable, but the data rise slowly. Jones then rescues his model by making a logarithmic change of scale!

At this point the reader may suspect that we have chosen the weakest catastrophe theory papers as targets for our criticism. The contrary is true: the best biological applications are contained in the papers just cited. Others[12,13] consist of long expositions of the mathematics of catastrophe theory, copious explanations of elementary biology, and vague speculations on how the two could be brought together.

Unreasonable or ambiguous hypotheses

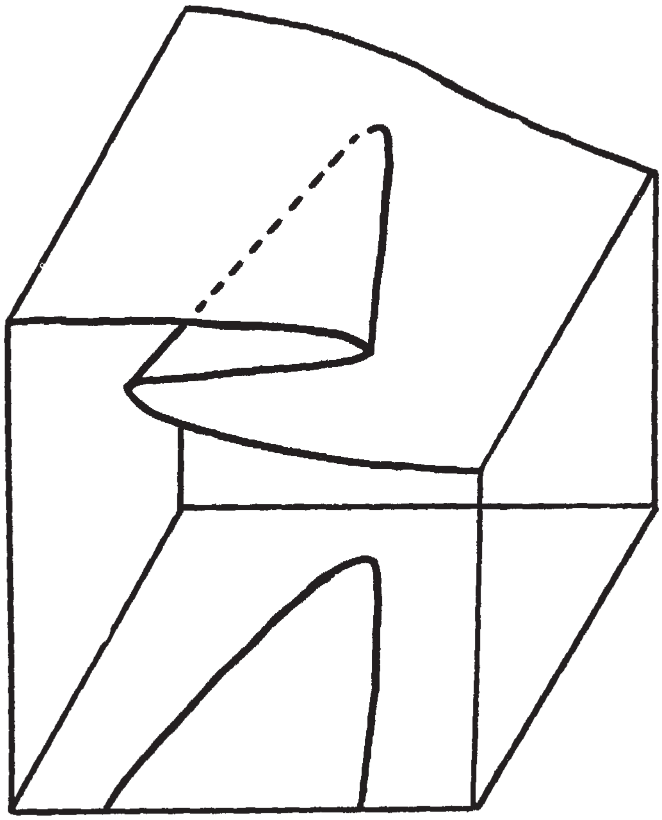
Isnard and Zeeman[14] have a model for how a country makes decisions about going to war. It is assumed that public opinion depends on the 'cost' of the war, and the (perceived) 'threat' posed by the enemy nation.

Among the five hypotheses of the war model are: Hypothesis 2: If the cost of the war is low, then opinion will be unified, and the greater the threat, the greater will be the level of military action called for.

Hypothesis 3: If the cost is high, and the threat moderate, then opinion will be split between doves and hawks.

Now in a given conflict situation, no sensible person would advocate a level of military action so low that the enemy is sure to win an easy victory. Either one wants no war at all, or

Fig. 4 A folded surface which is not a cusp since the projection of the fold curve on the control plane does not have a sharp point.



one wants to use as much military force as he thinks is needed to win. (War will follow when at least one side miscalculates, and both sides think they can win.) So, the dichotomy war–no war is a discrete one, and this is so whether the cost is high or low. But, if this is taken into account, we are left with a better theory which, however, does not involve a cusp.

Not only are the hypotheses of the war model unreasonable but they do not lead to the conclusions reached by the authors. Their main claim—that the hypotheses imply, via Thom's theorem, that the behaviour set is a cusp surface—is once again dubious, since the proof, which is not given, is presumably

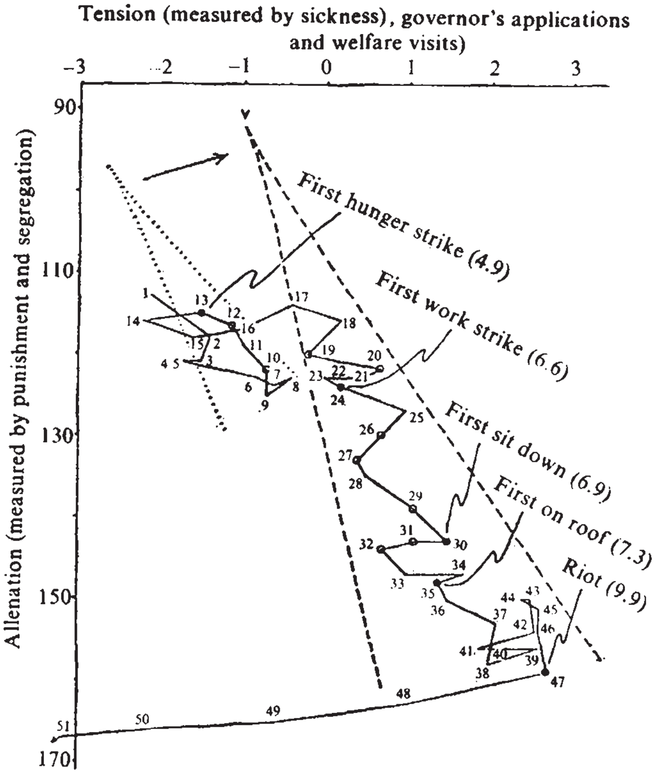


Fig. 5 The 'analysis of the data' of the paper by Zeeman *et al.* on prison riots[17]. These data are supposed to show that a cusp catastrophe gives a good fit.

based on some translation (also not given) of the vague hypotheses into precise language. It is not hard to show, however, that depending on one's interpretation of the hypotheses, either the conclusion does not follow; or the result does not depend on Thom's theorem, and is essentially contained in the hypotheses.

In fact, the only possible part Thom's theorem might play is to exclude a rounded bifurcation curve (such as the parabola of Fig. 4) in favour of a cusp (Fig. 1). But since either Fig. 1 or Fig. 4 would lead to the conclusions stated by Isnard and Zeeman, there is really no need to use Thom's theorem after all.

Spurious quantification

Catastrophe theorists often attempt to make a discrete variable into a continuous one so that CT can be applied. In Zeeman's dog aggression model[4], for example, the level of aggression of a dog is considered as a continuous variable x, ranging "from outright retreat through cowering, avoidance, neutrality, and growling and snarling to attacking". He also states, however, that an 'attack catastrophe' occurs when the value of x jumps upwards from one sheet of the behaviour surface to another. Despite this ambiguity, it can be shown that with either interpretation the attack is embedded in a continuous family of behaviours. But this is absurd: the idea of a 'semi-attack' by a dog, or of a snake gradually attacking a person, is utterly meaningless. The same reasoning applies to the 'level of military action' in the war model, as we have shown.

Better alternatives

Defenders of catastrophe theory claim that it provides, for the first time in history, a mathematical method for modelling discontinuous phenomena. This is false because, as we have shown, catastrophe theory does not lead to satisfactory models. But it is also false because it ignores the whole body of discrete mathematics, the study of shock waves, bifurcation theory, and the mathematics of quantum mechanics. Better alternatives certainly exist.

Besides, no mathematical theory, no matter how elegant it may be, can serve as a substitute for the hard work of learning the facts about the world. Catastrophe theory is one of many attempts that have been made to deduce the world by thought alone. It offers to mathematicians "the hope of applying mathematics without having to know anything but mathematics"[16]. An appealing dream for mathematicians, but a dream that cannot come true.

Appeal of the theory

Why has catastrophe theory acquired such widespread popularity? One possible reason may be its impressive claims of universality and usefulness, backed up by a large number of (mostly unrefereed) publications praising each other extravagantly; another might be the unusual nature of the mathematics used, combining concepts that are completely inaccessible to anyone who is not a professional mathematician with the use of some pictures of amazing simplicity, so that the result is simple to grasp intuitively but difficult to criticise.

It may be said that catastrophe theory is a new theory and that, while all the applications proposed so far have serious flaws, each one has benefits that make it worthwhile. Our criteria for judging a theory (or method) are rather generous. Even if a theory rests on questionable assumptions, or is based on faulty reasoning, or leads to false conclusions, or deals with ambiguous concepts, or does not make true testable predictions, we are ready to accept that it may be valuable, so long as it does not have all these faults at once.

The catastrophe theory models that we have examined, however, combine all these faults. The assumptions on which they are based are unreasonable and/or vague. The reasoning used to draw conclusions from the hypotheses is mathematically incorrect. The conclusions are either trivial (for example "a frightened dog, if angered, may attack"[4]) or false.

The possibility that, in the future, catastrophe theory may produce solid applications, cannot be dismissed *a priori*. However, its spectacular failures should suffice to raise serious doubts. For a method that has been said to have "the potential for describing the evolution of forms in all aspects of nature", existing evidence is indeed disappointing. The scientific community must remain sceptical until the proponents of catastrophe theory succeed in substantiating their claims. The burden of the proof is on them.

1. Stewart, I. N. in *Encyclopedia Britannica Book of the Year* (Encyclopedia Britannica, New York, 1977).
2. Sussman, H. J. & Zahler, R. S. *Synthese* (in the press).
3. Kozak, J. J. & Benham, C. J. *Proc. natn. Acad. Sci., U.S.A.* **71**, 1977 (1974).
4. Zeeman, E. C. *Sci. Amer.*, **234**, 65 (1976).
5. Zeeman, E. C., in *Lectures on Mathematics in the Life Sciences* **7**, 69 (1974).
6. Zeeman, E. C. in *Towards A Theoretical Biology* **4**, 8 (Edinburgh University Press, Edinburgh, 1972).
7. Kline, M. *Mathematics in Western Culture* (Oxford University Press, New York, 1974).
8. Jones, D. D. *The Application of Catastrophe Theory to Ecological Systems* (International Institute for Applied Systems Analysis, Schloss Laxenburg, Austria, 1975).
9. Hodgkin, A. L. & Huxley, A. F. *J. Physiol., Lond.* **117**, 500 (1952).
10. Crelin, E. S. *J. exp. Zool.* **120**, 547 (1952).
11. Elsdale, T., Pearson, M. & Whitehead, M., *J. Embryol. exp. Morph.* **35**, 625 (1976).
12. Woodcock, A. E. R. *Ann. N. Y. Acad. Sci.* **231**, 60 (1974).
13. Dodson, M. M. *Math. Biosci.* **28**, 243 (1976).
14. Isnard, C. A., Zeeman, E. C., in *Use of Models in The Social Sciences* (Tavistock, London, 1974).
15. Blainey, G. *The Causes of War* (Macmillan, New York, 1973).
16. Kolata, G. *Science*, **196**, 287 (1977).
17. Zeeman, E. C., *et al. Br. J. math. stat. Psych.* **29**, 73 (1976).