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APPLIED CATASTROPHE THEORY IN THE SOCIAL AND BIOLOGICAL SCIENCES

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Catastrophe theory is a mathematical theory which, allied with a new and controversial methodology, has claimed wide application, particularly in the biological and the social sciences. These claims have recently been heatedly opposed. This article describes the debate and assesses the merits of the different arguments advanced.

1. Introduction. The last two years have seen a most unusual explosion of controversy within a branch of mathematics. It is unusual in two senses: first, mathematicians can usually settle their differences without resort to rhetoric and invective; second, where deep differences do exist, they are usually decently confined to the purdah of the technical literature. Almost from its inception some fifteen years ago, catastrophe theory has seemed to break these precedents. Its catchy name, its consequent publicity and its widely advertised potential for applications combined to give it an image that was, before the evidence was in, one of bounteous promise.

Perhaps inevitably, a reaction set in. Catastrophe theory, it was claimed, was over-advertised, unrewarding, unhelpful, and even immoral. The controversy has become indeed a heated one; nor is it confined to the mathematical literature.

The present paper aims to set out the bases on which this dispute rests, to guide the reader through the various claims and counterclaims, to suggest methods whereby the debate might be arbitrated, and to present the author's own views.

There are now so many presentations of elementary catastrophe theory extant that another is felt to be redundant. The reader is referred to my articles (Deakin, 1977, 1978) and the literature there cited. An excellent paper, which came to my notice since the publication of Deakin (1977), is that by Chillingworth (1976). Two good and readily available popularisations of the theory are those by Stewart (1975) and Zeeman (1976a). This latter article, however, has attracted some trenchant criticism—so much so that its author took the unusual step of publishing the draft on which it was based (Zeeman, 1977a). Both versions are interesting and important. There is also available the recent text by Poston and Stewart (1978).

Many accounts (e.g. Deakin, 1977) distinguish between elementary catastrophe theory and generalised catastrophe theory, a dichotomy first introduced by Thom (1975). The present debate, and hence this paper, is concerned with the former of these. The claims, or more precisely, the expectations of generalised catastrophe theory are open to discussion—a discussion indeed likely to be more profound and useful than the current debate, but not yet joined, and thus outside the scope of this article.

Our present concern is with a number of quite precisely posed objections to a number of relatively precisely stated models produced in the context of elementary catastrophe theory. Indeed, almost all the furore resolves itself around the single special case of the *cusp* catastrophe, the simplest, if one excepts the mathematically trivial *fold*.

2. The Two-Fold Way. Thom (1976a) distinguishes two approaches to applied (elementary) catastrophe theory. On the one hand, there is the "physical way", characterised by our relatively secure knowledge of quantitative governing laws for the system under study. Here the part played by catastrophe theory is that of a powerful heuristic; it suggests new viewpoints and alternative formalisms and allows the use of a "ready-made" mathematical apparatus. In fact, the role of catastrophe theory here is precisely the role traditionally played by mathematics in the exact sciences.

Thom's "metaphysical way", by contrast, is speculative. Its domain of application tends to be the socio-biological sciences. In its pure form, it *postulates* the applicability of some elementary catastrophe, usually the cusp, and analyses the situation at hand in these terms. Such postulation is not, of course, unsupported, although the strength of the supporting evidence varies considerably from case to case. In practice, the analyses using this methodology differ considerably in their impact, and although one can say much in general terms, it remains necessary to evaluate each case individually.

Most of the present controversy concerns itself with applications of this more speculative type.

3. The Physical Way. The expanding corpus of its successful physical application is now generally recognised as the main argument in favour of the scientific importance of catastrophe theory. This aspect of the discipline has progressed most impressively in the last year or two. The first such application was the work of Berry on optical caustics (Berry, 1976), and Berry has remained in the forefront of application of the theory in this and related areas.

Particularly impressive here is a novel investigation of oscillatory integrals, due initially to Arnol'd (1972) and Duistermaat (1974). But this is now only one of a number of interesting studies, which include other optical studies, atomic scattering from crystals, molecular collisions, statistics of twinkling starlight, elastic buckling, gravity waves and other fluid phenomena, laser physics and the stability of ships. The recent text by Poston and Stewart (1978) concerns itself, as far as applications are concerned, almost exclusively with such studies, and includes an extensive reference list. Another important review in this area is that by Golubitsky (1978).

By and large, the physical applications are uncontroversial. This fact has led to their being emphasized by those involved in applied catastrophe theory. Recently, the widely mooted potential of catastrophe theory as a useful and applicable branch of mathematics has received trenchant (and at times intemperate) criticism. H. J. Sussman and R. S. Zahler (Sussman and Zahler, 1977, 1978; Zahler and Sussman, 1977) have led a determined attack on applied catastrophe theory. This has been the subject of a number of articles (see, e.g. Kolata, 1977; Guckenheimer, 1978) and has also generated debate in the pages of *Nature* and *Science*, (Senechal *et al.*, 1977; Sussman, 1977; Zeeman et al., 1977; Thom and Dodgson, 1977; Zahler, 1978). An earlier attack by Croll (1976) anticipated some of Sussman and Zahler's objections. However, most of these articles are concerned with the more speculative social and biological applications. Zahler (1978) explicitly states that the main objections are to the models in the biological and social sciences. The objections brought by Croll against the physical way are rather cautions against the use of models before they have been adequately tested. Thus, by and large, the physical applications are uncontroversial. There are now many of them, most to be found in Poston and Stewart's (1978) text or its references.

4. The Metaphysical Way. An application follows the metaphysical way if the analysis proceeds in terms of the properties of one of the elementary catastrophes whose relevance is *postulated.* This postulation is not, of course, a completely arbitrary matter, but proceeds from consideration of the likely behaviour of the situation under study. Such considerations rest less on the classification of potential functions than on notions of what is expected of a scientific theory.

At the very simplest level, catastrophe theory has served to emphasize that some of the procedures in sociological and biological analyses depend upon the fitting everywhere of one-valued functions and thus may be inadequate to situations of sudden change. A number of archetypes (notably the pleated surface associated with the cusp catastrophe) are now more widely available and have received useful publicity. The technical difficulties involved in fitting such surfaces are considerable (but see Cobb, 1978), and most current studies at this semi-empirical level restrict themselves to the adumbration of qualitative possibilities.

However, as indicated in my earlier article (Deakin, 1977) the much stronger claim is often made that the metaphysical way offers real promise of a new mathematical methodology in the socio-biological sciences. This suggestion rests on a complex interplay of mathematical and philosophical notions first advanced by Thom (1972) and since the subject of other expositions (Zeeman, 1972a; Poston, 1978, and in press; Poston and Stewart, 1978). Before embarking on an assessment of the metaphysical way, it is necessary to examine this body of theory. This will be done in the context of the cusp catastrophe, which is relatively simple and extensively used.

5. The Cusp Catastrophe. The cusp catastrophe arises most naturally from a consideration of the potential well defined by

$$y = x^4 + ax^2 + bx. \tag{1}$$

The shape of this well depends upon the values assigned to the parameters (*control variables*) a, b. The state of the system is uniquely given by x_p, the value of x that minimises y in (1)x_p is referred to as the *state variable.*

We thus have

$$4x_p^3 + 2ax_p + b = 0. \tag{2}$$

The dependence of x_p on a, b is then given by (2), represented by Fig. 1.

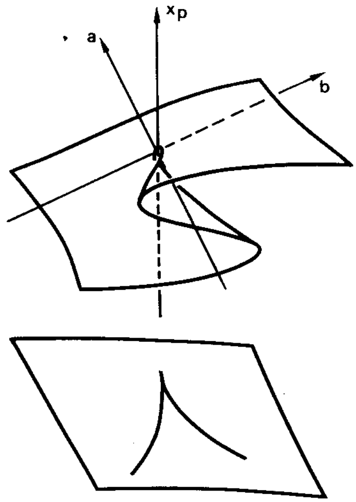

Figure 1. The standard diagram for the cusp catastrophe, indicating the dependence of the state variable x_p on the control variables a and b

Projection of this surface onto the a–b plane produces the cusped curve

$$8a^3 + 27b^2 = 0 \tag{3}$$

separating the "pleat" from the single-valued surface. Points on this surface may be classified into three types. These are:

(a) points for which (3) does not hold—here a tangent plane may be constructed, and the surface is perfectly regular;
(b) points (other than the origin) for which (3) holds—here the surface is locally parabolic in cross-section;
(c) the origin, characterised by the meeting of the two folds in the surface.

Equation (2) is the simplest surface exhibiting points of all three types. Other more complicated surfaces could be envisaged—for example, there could be two pleats. Indeed the surface could become more complicated in the sense that if (for example) two pleats both had their heads at the same point, a point of a type not listed above would be generated. Such a point, however, would be classed as "non-generic", i.e. a small perturbation to the form of the surface would separate it into two points of type (c).

It is also possible to have surfaces less complicated than that given by (2). If, for example, we considered

$$4x_p^3 + 2ax_p + b^3 = 0, \tag{4}$$

no point of type (c) would exist. This situation is shown diagrammatically in Fig. 2. For such a surface, the equality

$$8a^3+27b^6=0 \tag{5}$$

corresponds to (3). This is the parabola

$$2a+3b^2=0, \tag{6}$$

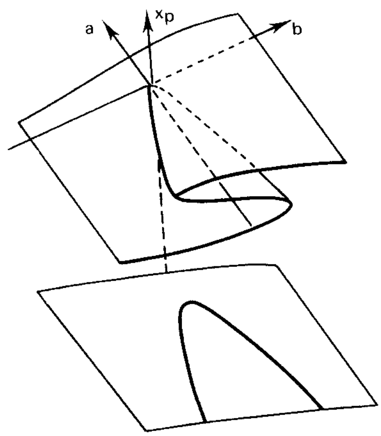

Figure 2. A surface akin to that of Fig. 1, but without a singular point. Redrawn after Zahler and Sussmann (1977)

whose smoothness eliminates the cusp of (3). However, such a surface is also classifiable as non-generic because the perturbation

$$4x_p^3+2ax_p+(b^3+\varepsilon b)=0 \tag{7}$$

replaces the parabola by the triply-cusped curve shown diagrammatically in Fig. 3 for the case $\varepsilon<0$ (for $\varepsilon>0$, another cusped curve appears). In the neighbourhood of any one of the cusps, the form (5) is generated locally.

The following theorems hold for a situation described by two control variables:

1. only one state variable is generically involved in the formation of a point where linear and quadratic approximations break down;
2. the most pathological behaviour that can be exhibited generically is that of the origin in (2);
3. in the neighbourhood of such a point, the surface can be reduced locally to that of (2).

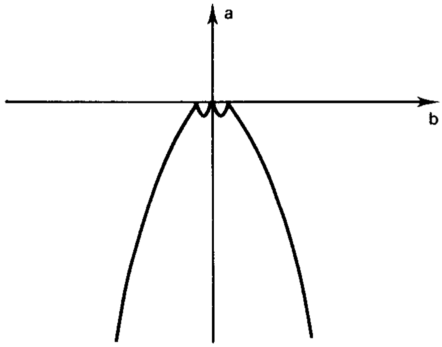

Figure 3. A generic perturbation of the parabola in Fig. 2.

[Fuller accounts are given in heuristic versions by Poston and Stewart (1976, 1978) and Lu (1976). Complete details are available in Bröcker's (1975) text among others.]

These results underly most of the claimed applications using the "metaphysical way".

6. A Methodological Principle. The rationale for the "metaphysical way" has been expounded by Thom (1972a, b, 1976a, b) and, perhaps more clearly, by Poston (1977 and in press). Thom (1972b) has expressed the underlying methodological principle as a "genericity assumption": "[Nature] realizes the local morphology which is the least complex possible with respect to the given local initial data". Elsewhere (Thom, 1976b), he has expressed the view that catastrophe theory is not so much a mathematical theory as an attitude of mind encapsulated in the phrase "Nature is almost everywhere well behaved". Poston (1978) has seen the principle involved as a "honing of Occam's razor". Zeeman, whose papers on applied catastrophe theory constitute the most determined attempt to demonstrate the utility of the "metaphysical way" has presented several analyses, of which the best is to be found in his paper on the heartbeat and the nerve impulse (Zeeman, 1972a).

In essence, the argument follows these lines. Suppose a phenomenon depends locally upon for example two control variables. Then, under plausible continuity and genericity assumptions, at most one state variable will fail to depend upon these in a smooth way. If such a variable exists, its local behaviour should, in the first instance, be sought in the graph of (2), as this alone of the possible surfaces we might draw, satisfies the genericity we expect of nature, and its employment of a single cusp is suggested by Occam's razor, i.e. the choice of the simplest available model.

This methodology does not, of course, guarantee secure results. As Thom (1975), has remarked: "In no case has mathematics any right to dictate to reality. The only thing one might say is that, due to such and such a theorem, *one has to expect* that the empirical morphology will take such and such a form. If reality does not obey the theorem—that may happen—this proves that some unexpected constraints cause some lack of [genericity], which makes the situation all the more interesting."

The concordance between the genericity view and that proceeding from potential functions is exact for the simpler catastrophes, but breaks down when the codimension (the number of control variables) exceeds five. This means that the genericity arguments used to produce catastrophes in the simple cases entail the existence of locally valid potential functions which are infinitely differentiable. This does imply some restriction on the validity of the enterprise.

An excellent discussion of the methodology and its underlying principles has recently appeared in the book by Woodcock and Davis (1978).

7. *Aside on Physical Applications.* Although there are physical situations in which the catastrophe theoretical formalism is known to be as exact as any other mathematical account, there are others to which the formalism does not apply. If

$$y = V(\boldsymbol{x}, \boldsymbol{a}) \tag{8}$$

is taken to be the governing potential, involving the (vector) state variable $\boldsymbol{x}$ and the (vector) control variable $\boldsymbol{a}$, we are restricted to those V values which possess Taylor expansions about the point under consideration.

It is a sobering thought, however, that many potentials in common use do not satisfy these requirements. This includes the standard $1/r$ potential of gravitation and electrostatics.

Change of state phenomena have been investigated in catastrophe theoretic terms. The cusp catastrophe formalism produces a version already familiar as the Landau theory. The case of the Van der Waals' gas law has been discussed by Fowler (1972), Lavis and Bell (1977) and others. Lavis and Bell point out that thermodynamic potentials obtained by Fowler's approach do not produce, in any natural way, those familiar to thermodynamicists [see, however, Poston and Stewart (1978) for a fuller discussion of this matter].

In more complicated cases, phenomenological theories such as the Van der Waals equation break down. In one such case, a version of the two-dimensional Ising model for a ferromagnet, an exact potential was calculated by Onsager (1944); this potential has no Taylor expansion about the critical point. (I am indebted to Mr. S. Carnie for this remark.) Hence, we are not able, even in the relatively simple physical cases, to infer universally the application of standard catastrophe theoretic models.

These known limitations should be kept in mind as one assesses the more speculative models in the biological and social sciences.

8. Zeeman's Heartbeat Model. Zeeman's paper on the heartbeat and the nerve impulse (Zeeman, 1972a) remains his most impressive and most complete study using the "metaphysical way". Even H. J. Sussman, Zeeman's sternest critic, has called this a "beautiful paper" (Sussman, 1975). It gives *phenomenological* accounts of the systems studied, and suggests itself as archetypal for biological studies over a wide range of other situations. "The novelty of the approach lies in modelling the *dynamics* (which is relatively simple) rather than the *biochemistry* (which is relatively complicated). This approach might be useful for a large variety of phenomena in biology, whenever there is a trigger mechanism leading to some specific action."

Three qualities are displayed by heart muscle fibres, which Zeeman lists as: (1) stable equilibrium; (2) threshold, for triggering an action; (3) "jump" return to equilibrium.

He considers the surface as given by (2) but in a slightly different form, achieved by scaling the parameters. Set

$$f(x, a, b) \equiv x^3 + ax + b = 0. \tag{9}$$

The surface represented by (9) is still, in essence, that depicted in Fig. 1. This surface is achieved by the extremisation of a potential

$$\phi = 1/4x^4 + 1/2x^2 + bx, \tag{10}$$

a form very similar to that of (1).

Now consider the response of a system which is governed by the extremisation of a potential such as ϕ. If such a description is to be useful in a dynamic and not merely a static sense, the response of the particle representing the system must be rapid to alterations that remove it from the surface and relatively slow to displacements on the surface. The problem involves two time-scales, as we see in many physical situations [e.g. the Euler arch (Zeeman, 1976b) and the Zeeman Catstrophe Machine (Zeeman, 1972b) among others].

To a first approximation, the dynamics are given by

$$\dot{x} \propto \frac{d\phi}{dx}, \tag{11}$$

but this approximation does not allow for variation of a, b with time. This effect must be supplied by giving equations for $\dot{a}$, $\dot{b}$ or both. As these will be slower, the initial description (11) is modified by the inclusion of a small constant ε,

$$\varepsilon\dot{x} = -f(x, a, b). \tag{12}$$

(The minus sign is chosen for notational convenience.) Equation (12) is said to give the *dynamic* of the system. The slower response of the equations for $\dot{a}$, $\dot{b}$ are said to give the *feedback flow* on the surface

$$x_a^3 + ax_a + b = 0. \tag{13}$$

This surface is referred to as the *slow manifold*, while the trajectories of the system that return the reference particle to this surface are called, collectively, the *fast foliation*. Equation (13), by genericity, is a general one for a two-parameter system.

Zeeman now takes x for a particular heart muscle fibre to be its length, $-a$ to be its tension, and b to be the value of some chemical control. The simplest control system for b makes $\dot{b}$ proportional to (in suitable units equal to) the deviation of x from its equilibrium value x_a:

$$\dot{b} = x - x_a. \tag{14}$$

No equation for $\dot{a}$ is given, it being supposed that a alters in response to external forces, so that the value of a is taken to be, in the ideal case, a given constant.

The heartbeat takes the form of a cycle of rhythmic contractions and relaxations of the fibres. The contraction (systole) alternates with the relaxation (diastole). Zeeman supposes an equilibrium value of b, b_0 for the diastolic or relaxed, fibre, but a trigger, or switch, adjusts the value to a new value b_1 for the systolic, or contracted, state. Fit with the subsequent data leads him to suppose $b_0 < 0 < b_1$, a hypothesis he believes could be tested experimentally. (He tentatively identifies b with membrane potential.)

His model may now be summarized as in Fig. 4, with governing equations (12), (13), (14). We may now envisage four cross-sections for particular values of a.

Firstly, consider a value of a large enough to ensure a unique value of x_a for all b. Here the distinction between systolic and diastolic states is inoperative, but we may still envisage the switching mechanism between the two values b_0, b_1. Suppose the system initially to be given by $b=b_0$.

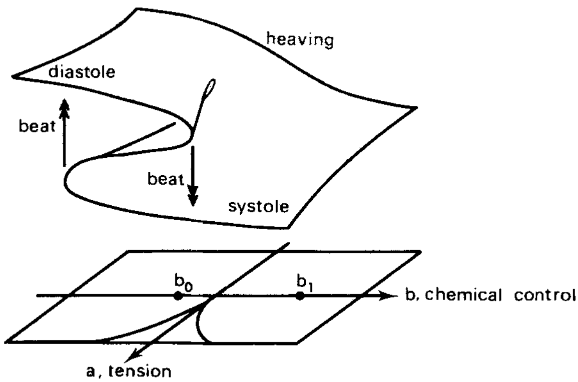

Figure 4. The geometry of Zeeman's model of the heartbeat. Redrawn after Zeeman (1972a)

Then the switch adjusts the value to b_1, so that the system responds, by (14), to the appropriate new value of x_a. When this is attained, the equilibrium value of b reverts to b_0, and again the system responds. No beat, properly called, is produced, but the smooth changes in x give a series of contractions and relaxations, known as "heaving". Heaving is observed in the surgically bypassed heart and in an experimental situation first described by Rybak and Béchet (1961).

Secondly, consider a value of a for which there is a small section of two-valued surface lying between b_0 and b_1. As the system responds to the switching mechanism, it includes in that response a small beat in both parts of the cycle, i.e. the trajectory of the system includes a part due to the fast foliation, given by (12). This Zeeman takes to be a description of the weak atrial beat.

Thirdly, take an increase in tension, so that for the case $|b_0|<|b_1|$, which Zeeman implicitly assumes, x_a is single-valued at b_1, but multivalued at b_0. The beat is now larger, and overshoots b_0 on the return, as the equilibrium lies on the upper, rather than the lower, surface. That the strength of the beat increases with the applied tension is a classical observation, referred to as "Starling's Law of the Heart". Zeeman takes this third case to be a description of the strong ventricular beat.

Fourth, consider a value of a such that the surface is multivalued for both $b=b_0$ and b_1. In this case, the trajectory no longer includes a portion of the fast foliations and the fibre remains in diastole. This is regarded as a description of the response of an overstretched fibre. The four cases are summarised in Fig. 5.

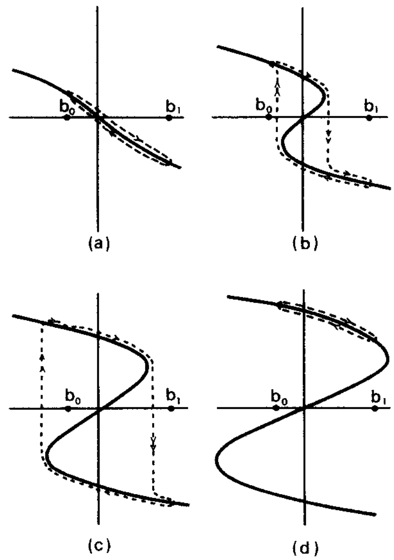

Figure 5. A summary of the four types of heartbeat allowed by the Zeeman model: (a) bypassed heart, (b) artrial fibre, (c) ventricular fibre, (d) overstretched fibre. Redrawn after Zeeman (1972a)

The above describes the response of an individual fibre. Zeeman's analysis continues beyond this to present a global description of the heart's response to a pacemaker wave of stimulation—an account which seems to accord with observation, at least in its major features.

9. *Zeeman's Nerve Impulse Model.* In the case of the nerve impulse, Zeeman's model postulates a different feedback flow, allowing for slow, rather than "jump" returns to equilibrium. This theory competes with several others, notably that for which Hodgkin and Huxley (1952) won a Nobel Prize. Zahler (Sussman and Zahler, 1978) has compared the two theories in detail to the detriment of Zeeman's. It should be pointed out, however, that much of this criticism is incorrect in detail, and some points in fact apply equally well to the Hodgkin–Huxley theory.

Other defects in the Zeeman account have been noted by Stewart (1975 and personal communication), who with Woodcock, has prepared a full account of the matter (Stewart and Woodcock, in press). The present paper will thus not consider the nerve impulse theories in any detail.

It is apposite to remark, however, that the Hodgkin–Huxley theory is tested in considerable detail at the *biochemical* level. My own view is that such theories will and should tend to be preferred to purely phenomenological ones. It is probably not unfair in the present case to say that the balance of the evidence is in favour of the Hodgkin–Huxley account.

10. Embryological Theories. The most ambitious of Zeeman's biological work is concerned with a number of embryological questions. In this, Zeeman follows Thom, whose early development on catastrophe theory was much influenced by embryology. Zeeman's papers in the field are the most informed by experimental data and there is now some evidence (Elsdale, Pearson and Whitehead, 1976) in their support. The three relevant papers (Zeeman, 1974a, 1976c; Cooke and Zeeman, 1976) are ambitious and technical, although rather more speculative than the models so far discussed. They can only be briefly summarised here.

We begin by considering a mass of tissue E as it develops in an interval of time T. Zeeman sees four aspects as fundamental to an account of the development.

(a) Homoeostasis—each cell is in a state of equilibrium, which may change with time.
(b) Continuity—the conditions in different cells are to be represented by functions smooth over E.
(c) Differentiation—cells of one type are to give rise to cells of two distinctly different types.
(d) Repeatability—the qualitative outcome of the process is to remain the same if the initial conditions are slightly perturbed.

From these conditions, Zeeman deduces the existence of a "wave"—a frontier that forms in E, moves and deepens, slows up and stabilises, and finally deepens further. I take him to mean by the word "deepen" that the distinction between the two types of cell on either side of the frontier becomes more pronounced.

The wave in question is, in Zeeman's terminology, a primary wave, i.e. one whose passage produces a (possibly hidden) determination of cell types. This primary wave may or may not be accompanied by a secondary wave whose passage marks the appearance of observable differences.

The proof of the existence of the primary wave proceeds via a translation of the four desiderata into mathematical form. The translation is plausible, but not forced upon us, so that the word "proof" is perhaps a little strong. Nevertheless, at least a powerful heuristic argument results.

From this argument a version of the cusp catastrophe emerges. The spatial organisation of E is seen as one-dimensional and Fig. 5 emerges from the analysis. Note that the alignment of the cusp has altered, which allows the existence of the point c_2. Figure 7 shows how cells at different

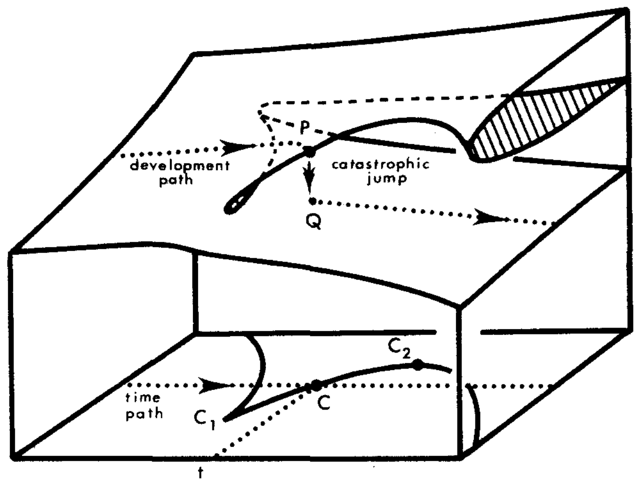

Figure 6. The model underlying Zeeman's main embryological theorem. Redrawn after Zeeman (1974a)

positions ultimately differentiate and stabilise. The heuristic or proof of the primary wave result is essentially an argument that Figs. 6 and 7 (which produce the stated behaviour) are generic.

Some semi-quantitative aspects of the process are analysed, giving local approximations to the speed of the wave, and other details.

This describes the major theoretical work of Zeeman's (1974a) first and longest paper on the subject. The remainder is taken up with plausible and reasonably detailed applications to quite specific situations. Some twenty-two experiments, some of them capable of testing aspects of the theory, are proposed. I am unable to comment in detail on the plausibility or the significance of the resulting descriptions and predictions, and suspect that most developmental biologists would be put off by the mathematics of the paper. Its significance may thus remain hard to assess for some time to come.

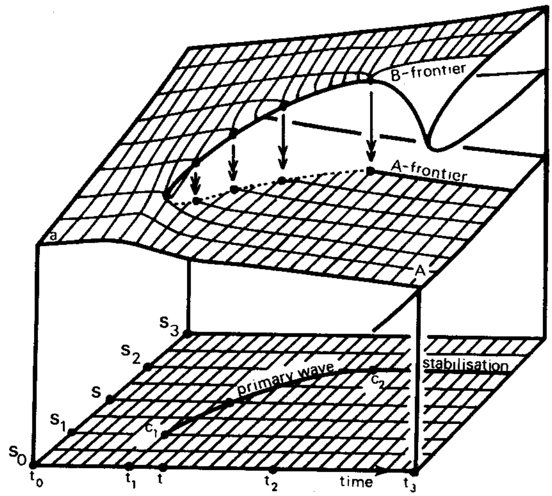

Figure 7. The stabilisation of the frontier in Zeeman's embryological model. Redrawn after Zeeman (1974a)

The other two papers are shorter and more specialised, but depend on the basic ideas adumbrated above. They concern the formation of somites (segments) in amphibian and bird embryos. Aspects of this work have been confirmed in that some of its specialised predictions have been found to hold in experiments and that some researchers in the area find it a useful conceptual framework.

11. The Prison Riot Model. The attempt by Zeeman *et al.* (1976) to describe prison riots in catastrophe theoretic terms has generated much interest and attention. Apart from the morbid fascination of the subject matter, which is also more amenable to popular exposition than those so far treated, the study is important for its quantitative character, its inclusion of a stochastic element, and for the methodological questions it raises.

Figure 8 shows two aspects of the model adopted. Hypothesis one of the major paper postulates the applicability of the cusp catastrophe. The reasoning is, in essence, that previously applied to the heartbeat.

We are interested in a variable which measures the state of disorder in a prison and we plausibly seek to calculate this from two others, representing, to a first approximation, the actions of the guards and the state of mind of the prisoners. These variables are called "alienation" and "tension" respectively. We now suppose that disorder, in general, is a smooth function of tension and alienation, but that if alienation is

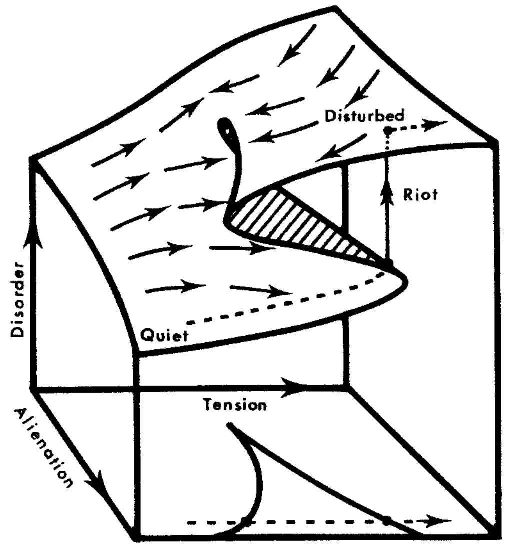

Figure 8. The cusp surface and feedback flow for the catastrophe theoretic prison riot model. Redrawn after Zeeman *et al.* (1976)

sufficiently high, small triggering increases in tension can cause sudden increases in disorders, i.e. riots can occur. (It is important to note in this regard that riots are sudden increases in disorder in response to trivial initiating incidents.) Genericity now ensures a cusp description and simplicity restricts us to a single cusp. This argument makes the hypothesis plausible, although again it does not compel our acceptance.

Hypothesis two, "[there] is a tendency for an institution as a whole to avoid the extremes of 'quiet' and 'disturbance'", is used to provide the feedback flow shown diagrammatically in Fig. 8.

The third hypothesis is a new one. It allows for the incorporation of a certain stochastic noise component, as shown in Fig. 9. The dotted component of the sectioned surface separates two regions of the fast foliation. Above it, the dynamic returns the system to the upper surface; below it to the lower.

Elaborate indices for tension and alienation were constructed and an attempt was made to measure disorder. A tentative cusp was fitted to the data. The results are not clean mathematically in that two cusps were drawn, and it was hypothesised that the cusp had moved.

Since the initial study, a monitoring system has been in force at Gartree, the prison first analysed. (It does not involve the hidden cameras mentioned by Panati (1976), but continues retrospective analyses of

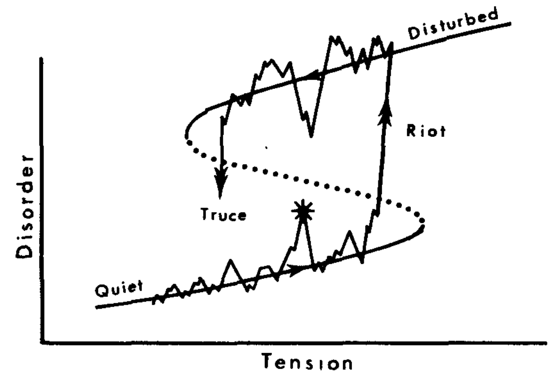

Figure 9. The addition of stochastic noise to the prison riot model. Redrawn after Zeeman *et al.* (1976)

tension, alienation and disorder.) I am privately informed that the model has had a predictive success in that the one riot occurring during the follow-up study corresponded with the single time at which tension and alienation had values such as to lead to its prediction.

12. Stock Exchange Crashes. Of Zeeman's elaborated models in the social and biological sciences, that discussing the behaviour of share indices (Zeeman, 1974b) is the least developed and the least impressive. Once again, a cusp catastrophe is plausibly, but not compellingly, pressed into service. The state variable, J, is the rate of change of a share index (such as the well-known Dow-Jones); the control variables C, F represent respectively the proportion of money held by "chartists" (investors whose policy is based on trend analyses) and the excess demand for stock by "fundamentalists" (those whose policy is based on more traditional economic indicators of soundness).

The paper proceeds by an elaboration of hypotheses (seven in all) of varying plausibility. The result is the model of Fig. 10 with a feedback flow analogous to that of the nerve impulse model. There is virtually no validation (despite the enormous literature the subject has generated), but a few tentative predictions are offered.

13. Other Models. Thom (1972a) has advanced a number of models, mostly of a highly speculative character, and often more for the purpose of illustrating the methodology of Section 6 than for discussing the situation being modelled. The elaborated models are mostly due to Zeeman. Besides those already discussed, we find discussions on:

(d) the embryological models;
(e) aspects of the prison riot study;
(f) aspects of the stock exchange analysis.

In an earlier paper (Deakin, 1977), I wrote of the dog that occurs in several of Zeeman's papers "This example is effective didactically both for the apprehension of the cusp catastrophe and for the understanding of the ethological question. Whether it possesses any significance beyond this is doubtful." This is still my position on the matter. Zeeman, in my view, overworks the dog a little, but Sussman and Zahler (1978) devote a fifth of their paper to this trivial and unelaborated model. It is not worth this amount of effort. The same may be said of the war model. Ten per cent of their paper attacks this brief and relatively simpleminded analysis of war and public opinion. Sussman (1979) uses the war model as the main target in a condemnatory review of Zeeman (1977b).

Neither of these models deserves to be treated as a serious scientific account of its subject. Lorenz's original discussion of the dog (Lorenz, 1966) says everything that Zeeman tries to say and does it more simply. The war model seems to add nothing to the vast literature of non-mathematical work on the subject. While there is some evidence that Zeeman overstates his case, these simple models do not deserve the attention Sussman and Zahler give them.

The other three cases deserve more serious consideration and will be dealt with in separate sections. The heartbeat model is not criticised.

16. Objections to the Embryological Models. The major criticism brought against Zeeman's embryological papers concerns the "Main Theorem" of Zeeman's (1974a) paper. Apart from minor quibbles, Sussman and Zahler (1978) criticise the proof of the "theorem" and produce diagrams alleged to depict counter-examples.

Zeeman's procedure in his "proof" is to model the situation by a smooth (i.e. infinitely differentiable) potential. This is probably a dubious assumption, although Sussman and Zahler do not seriously challenge it. This potential must be such that initially it possesses a single minimum and later this bifurcates. The simplest available such potential is that modelled by the cusp catastrophe. On these grounds, Zeeman chooses it. Sussman and Zahler object to this, but it is a reasonable step, although perhaps unusual in what is presented as a "proof".

The major criticism is directed at what follows, which strikes me as unexceptionable (apart from the caveats I raise below). The next point of Zeeman's proof concerns the alignment of the cusp surface. The axis of symmetry of the cusp is typically (generically) not parallel to the time axis

These studies will not be discussed in detail here. They are mentioned to give the reader an idea of the scope of such work. Several are of very poor quality.

Woodcock and Davis (1978) give a number of other minor models, which they refer to as "invocations". As distinct from true *applications* of the theory, or even *illustrations* (production of known results in a new way), invocations are tentative but employ catastrophe theory on account of the suggestiveness of its images. The terminology is due to Michael Berry.

14. The Reaction. The widespread use of catastrophe theory in the "metaphysical" mode has generated a reaction, centering particularly in the criticisms of Sussman and Zahler (Sussman, 1976; Sussman and Zahler, 1977; Zahler and Sussman, 1977; Sussman and Zahler, 1978; Sussman, 1979). These authors have publicly and vociferously criticised applied catastrophe theory, particularly when applied according to the "metaphysical way", and most particularly when applied by Zeeman.

Their approach is haphazard, their style somewhat self-righteous, but their work contains the germs of some sound criticism. One might also say that Zeeman's style has a tendency to flamboyant advertisement that, to say the least, is not publicly admired in scientific writing.

The most ambitious of the critical papers is the lengthy attack on some of Zeeman's models published in the journal *Synthèse* (Sussman and Zahler, 1978). This article makes a number of general points and enters into detailed criticism of a number of models. Its most detailed criticism, to which 20% of the text is devoted, concerns the nerve model, and thus lies outside the scope of this discussion. The remainder, and the criticisms of their other papers, will be dealt with, although not always in detail.

The uneven quality and carping tone of much of the Sussman–Zahler critique is unfortunate. Criticism of several of the catastrophe theoretical models is certainly called for, and the methodology of the "metaphysical way" is a new departure that needs to be carefully evaluated. An article on Thom's work is promised by Sussman and Zahler (1978) and their article does indicate the source of some of their unease—however, their major criticisms are directed at Zeeman.

15. Specific Criticisms. Sussman and Zahler (1978) advance a number of criticisms against specific studies. In particular, they attack:

(a) the account of the nerve impulse;
(b) the discussion of canine behaviour;
(c) the Isnard–Zeeman war model;

(d) the embryological models;
(e) aspects of the prison riot study;
(f) aspects of the stock exchange analysis.

In an earlier paper (Deakin, 1977), I wrote of the dog that occurs in several of Zeeman's papers "This example is effective didactically both for the apprehension of the cusp catastrophe and for the understanding of the ethological question. Whether it possesses any significance beyond this is doubtful." This is still my position on the matter. Zeeman, in my view, overworks the dog a little, but Sussman and Zahler (1978) devote a fifth of their paper to this trivial and unelaborated model. It is not worth this amount of effort. The same may be said of the war model. Ten per cent of their paper attacks this brief and relatively simpleminded analysis of war and public opinion. Sussman (1979) uses the war model as the main target in a condemnatory review of Zeeman (1977b).

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The major criticism is directed at what follows, which strikes me as unexceptionable (apart from the caveats I raise below). The next point of Zeeman's proof concerns the alignment of the cusp surface. The axis of symmetry of the cusp is typically (generically) not parallel to the time axis (and not perpendicular to it, either). This again is an assumption, but a reasonable one. Its justification is as given in Section 6.

Once we incline the two axes, the existence of a unique point c_2 is an immediate consequence, as may readily be verified from (3). The existence of a stabilised frontier that deepens follows also from the geometry of the surface described by (2).

One difficulty, however, remains. The point c_2 may, in fact, be a long way from the apex (c_1) of the cusp (arbitrarily far away), and as Thom's theorem refers to the *local* geometry of the surface, the point c_2 may not exist within the neighbourhood of validity of the cusp representation. The same objection applies to the deepening after the stabilisation. It may be that this is what Sussman and Zahler have in mind in their criticism of these conclusions.

Omitted from consideration here are petty details on the more specific applications. Some general objections will be considered in Sections 21 and 22.

17. Critiques of the Prison Riot Study. Zahler and Sussman (1977) criticise the prison riot model as also does Rosenhead (1976). Both critiques emphasize inadequacies in the statistics and the data analysis. Rosenhead goes beyond this to criticise the endeavour itself as repressive.

Zeeman *et al.* (1976) list weekly values of alienation and tension over a period of a year for Gartree prison (their Table 1). In Table 2 of their paper, the more serious incidents are listed and assessments measuring disorder are given. Thus, disorder (measured by its symptomatic incidents) is not assessed weekly as are the control variables. The incidents listed fall into three categories:

(a) incidents involving nearly all the inmates in some new mode of protest;
(b) incidents assessed at seriousness greater than 5 not listed in the first category;
(c) other incidents listed in their Table 2.

There were 5 incidents in category (a), 10 in category (b), and 5 in category (c).

In order to fit the cusp surface to this data, Zeeman *et al.* consider first a cross-section of the surface postulated (Fig. 8 of this paper). Incidents in category (a) are presumed to constitute riots and to correspond to observed positions of the point R_2. Now let a represent the alienation, t the tension, and d the disorder. The locus of the point R_2 is now given (subject to suitable placement of the origin) by the surface analogous to (13)

$$d^3 = t + ad, \tag{15}$$

with the further condition

$$27t^2 = 4a^3. \tag{16}$$

Eliminating t from (15) and (16), we find

$$(3d^2 - 4a)(3d^2 - a)^2 = 0,$$

so that the locus appropriate to the point R_2 is

$$3d^2 = a, \tag{17}$$

a parabola connecting a and d.

Curve fitting begins by fitting a parabola to the incidents corresponding to riots. It should be possible to fit a parabola to the five category (a) incidents, although the small number of sample points is a problem. Zeeman *et al.* do not, however, take this road. They plot all incidents on an a–d plane, notice that the minimum observed disorder (for a category (c) point) is 2.3, hypothesise a vertex for the parabola at a disorder level of 1, and *draw* a parabola whose whole vertex lies on the line $d = 1$, "paying particular attention to the solid circles" [i.e. category (a) incidents]. There is no attempt to do anything more rigorous than this, nor are confidence limits discussed.

Clearly this methodology is less than ideal. Rosenhead's (1976) stricture—"The vertex of the cusp is only located with the aid of a breath-taking piece of parabolic extrapolation through a cluster of points which support almost any curve fitting exercise—or none"—clearly carries some weight. Figure 4 of Zeeman *et al.* (to which Rosenhead is referring) would look more impressive, however, if category (b) and (c) points were omitted—especially the minor incident used (in small part) to locate the vertex (this incident—"Inmate found in possession of counterfeit £5 note"—is hardly riotous).

The above exercise, which Zeeman *et al.* "emphasize is largely guesswork", yields an alienation value at the cusp point (i.e. the unique point of type (c), in the sense of Section 5) of 92 ± 10. (The error attributed would seem also to be the result of guesswork.) This argument is now held to justify their Fig. 3, which plots the year's events in the t–a plane, and finds most to lie within a curve somewhat reminiscent of a cusp.

In fact, two such curves are drawn. One encompasses events for the first 16 weeks, the other those for the next 32. The second is drawn with a vertex at an alienation value of 92 (despite the fact that one category (a) incident and two category (b) incidents occurred during the first 16 weeks, fitted by a curve with vertex at an alienation value of 97). In other words, the parabolic fitting which established the value of 92 involves one point out of five (incidentally, that lying closest to the curve) for which the fit is discarded. (I am indebted to Mr. P. McIntosh for this remark.)

There are two other disturbing features of Zeeman *et al.*'s Fig. 3. In the first instance the axes of symmetry of the "cusped" curves are not parallel to the alienation axis. This need not be a criticism (see Section 18), but in the present context it is, as (17) and the subsequent parabolic fit involved estimations of R_2. Thus category (a) points should lie near the right edge of the fitted cusp (see Fig. 8 of this paper). Figure 11 (redrawn from Zeeman *et al.*'s Fig. 3) shows how far this condition is from being satisfied.

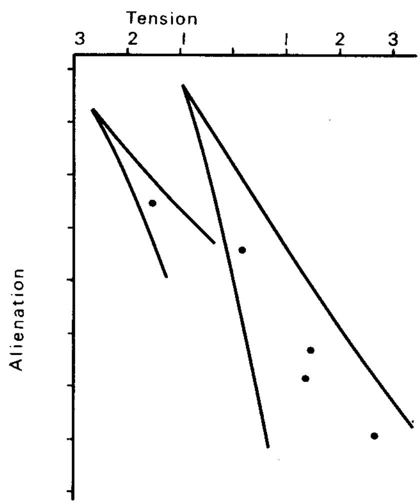

Figure 11. Cusps and riot incidents in the prison riot study. Redrawn after Zeeman *et al.* (1976)

The prison riot model is criticized by Sussman and Zahler (1977) and by Zahler and Sussman (1977). I would agree with these critics that the fitting is most unsatisfactory and would also agree that it is desirable in such studies to produce a statistic saying how good one's fit is. Zeeman *et al.* do not. I would not however join Sussman and Zahler (1977) in their contention that "other possibilities fit the data at least as well as Zeeman's cusps. (A pair of parallel lines, for instance, does a very good job.)". This begs the question of how we test the claim. Furthermore Sussman and Zahler offer no underlying model for this theory at all.

Nor would I agree with Rosenhead (1976) that this model constitutes a repressive misuse of mathematics. I see no evidence of repressive intent in the paper under discussion, and furthermore hold that repressive instincts do not require catastrophe theory. An earlier analysis of prison riots, due to Fox (1971) includes the following analogy: "The way to make a bomb is to build a strong perimeter and generate pressure inside." We cannot press this too far, or we shall find difficulties of detail. Nonetheless, we do not regard it as a misuse of engineering theory; it strikes me as anti-repressive.

18. Mathematical Problems of Catastrophe Fitting. The question of deriving accurate fits for cusp (and other) catastrophes is a difficult and largely unsolved one. It arises critically in the case of the prison riot model. To simplify discussion, suppose the terminology of that model, but with a more satisfactory measure of disorder. (The use of reportable *incidents* can leave us without a measure, or greatly bias the measure—there is no "truce incident" reported by Zeeman *et al.* Nor could their scale measure one.)

Let us now distinguish alienation, tension and disorder *as we happen to measure them* from the same terms as ideal quantities satisfying (15). Continue to use lower case symbols for the ideal quantities of the model and use A, T and D to represent respectively alienation, tension and disorder as measured.

Assuming, as is reasonable, that we can distinguish clearly between a control variable and a state variable, we must still allow for coordinate transformations

$$a=\theta(A, T) \tag{18}$$

$$t=\phi(A, T) \tag{19}$$

$$d=\psi(D), \tag{20}$$

before we can apply (15) or some canonical variant.

Here

$$\theta(A, T)=\theta_{00}+\theta_{10}A+\theta_{01}T+\text{higher order terms}, \tag{21}$$

and corresponding equations apply for ϕ, ψ. The zeroth order terms serve to fix the origin. (This is the only point of what *quantitative* work Zeeman *et al.* undertake.) The linear terms of θ, ϕ serve to rotate the cusp in the alienation–tension plane. (For present purposes, assume the matrix

$$\begin{pmatrix} \theta_{10} & \theta_{01} \\ \phi_{10} & \phi_{01} \end{pmatrix} \tag{22}$$

to be regular—the case of singular matrices is discussed in Section 23.)

The surface-fitting problem may now be stated as:

Given n experimental points (A_i, T_i, D_i), *find functions* θ, ϕ, ψ *in such a way that the transformations* (18)–(20) *yield, in some sense, the best fitting surface* (15).

One could, of course, arbitrarily truncate the series (21), etc. at terms of some order and thus reduce the problem to a standard one of (in general) non-linear regression. This is, at best, a partial solution. The general problem remains unsolved and urgently in need of solution.

Lewis (1977) claimed some initial success in this area, and Cobb (1978, in press a, b) has made some progress. On a related question—testing for the appropriateness of the catastrophe model—Cobb (personal communication) reports partial success in that he has a relevant existence theorem.

Prior to the prison riot model, it was fashionable to introduce either a diagonal matrix (22) (described by the disjunction between the "normal factor" t and the "splitting factor" a) or the composition of this with a 45° rotation [producing the "competing factors" of (e.g.) the dog's behaviour]. These two situations are [as Zeeman himself (1974a) pointed out] non-generic. The cusp is likely to be inclined at a different angle in the A–T plane.

The urgency of the problem is highlighted, even if one merely takes the minimalist view suggested by Poston (1978). "Much numerical work in sociology, psychology, etc. consists of fitting data to a few standard forms, such as linear relationship and bell-shaped curves.... Addition of the elementary catastrophes to the forms used is pure gain." Without adequate techniques for the fitting of the catastrophe surfaces, much of the point of this, otherwise admirable, remark is lost.

19. Stock Exchanges Revisited. The main criticism to be raised against the stock market model begins with the observation that stock markets and their behaviour have been extensively studied. The prediction of crashes in the market is a large industry. Previous crashes have been subjected to much retrospective analysis. Zeeman's analysis refers to none of this. It is quite unclear how his catastrophe theoretic account accords with existing wisdom; virtually none of the enormous bank of accumulated data is quoted in support of his model.

Surprisingly, this is not Sussmann and Zahler's (1978) main criticism. They confine their remarks to the relatively specific objection that the model involves an unrealistic feature. They claim that, on Zeeman's model, a purely speculative market will never crash. This claim is based on an interpretation of Fig. 10. If C is increased for zero F, the state of the market can be represented by a point on (in some cases) the upper surface. If these constraints are imposed, the market will not crash. However, this argument neglects the feedback flow on the catastrophe surface which decreases F at such points, so that purely speculative markets are very transitory phenomena preceding crashes. This feedback flow also prevents the "anti-crash" mentioned by Deakin (1977) in criticism of the model.

Nonetheless, the model appears unconvincing. A crash is a drop in the rate of change of the index, rather than a drop in the value of the index. This usage strikes some readers as unusual. The restriction to two control variables also seems less assured than in the prison riot model.

20. Protein Denaturation. One model, not by Zeeman, is criticised at some length by Zahler and Sussman (1977). This is an account of protein denaturation by Kozak and Benham (1974) (since extended by those authors—Benham and Kozak, 1976, 1977, 1978). This uses the cusp catastrophe to discuss what is essentially a change of state phenomenon. In some cases, the cusp appears in a distorted form, i.e. inverses of (18)–(20) are used to present a modified cusp.

In view of catastrophe theory's limitations in describing change of state phenomena (see Section 7 above), this theory may well not be correct in detail. Dodgson (1977) adduces further reasons for scepticism here.

Nonetheless, Zahler and Sussman's critique seems to depend upon a misreading of Kozak and Benham's (1974) paper. They regard the transformed cusp as no longer generic, which is wrong. The misreading is apparent from the subtle differences between Kozak and Benham's Fig. 2 and Zahler and Sussmann's redrawing of it (their Fig. 2).

The usefulness of this model awaits expert evaluation.

21. More General Criticisms. Inherently more important than criticisms of specific studies are those objections to the methodology on which such studies rest. Sussmann and Zahler (1978) raise a number of trenchant points in opposition to the philosophy outlined in Sections 4, 6 of this paper. Thus, they find in some of Thom's writing "an attitude of contempt by the pure thinker towards those who busy themselves with almost meaningless tasks, such as deciphering the genetic code" instead of adumbrating grand syntheses. "Thom has a vision to offer, that of the mathematician's Platonic dreamworld, a world of pure ideas uncorrupted by the intrusion of treacherous facts."

There is a certain justice in some of these complaints—yet they are also somewhat exaggerated. Thom does have a vision to offer, but visions are notiriously difficult to assess, as Guckenheimer (1978) noted in the present case. The core of that vision would seem to be a view of a science as being (ultimately) successful when it is absorbed into Pure Mathematics, as has happened with geometry (see Thom, 1975). Thom's philosophy of science is novel and controversial. A full review of it lies outside the scope of this paper, but see Deakin (to appear). The passage quoted in Section 6 above, however, shows that it is oversimplifying the matter to speak of Thom's methodology as a claim "that the world can be deduced by pure thought" (Zahler and Sussman, 1977).

Two excellent accounts by Poston (1978, in press) investigate the mathematical basis of Thom's genericity assumption. Its novelty is likely to cause reaction and scepticism, but it should be recognised that it is not without an intellectual ancestry. The axiom outlawing phenomena which depend upon the satisfaction of a precise equality among parameters has been termed by Hardin (1960) the *axiom of inequality*. This is widely used in biomathematical work. Thom's genericity assumption is no more than an extension of this.

Sussman and Zahler (1978) attack in trenchant terms Zeeman's use of the genericity assumption, particularly in reference to his embryological theories. Their sub-heading here is "How to Prove, by Pure Thought, that Everything Moves". One section of Zeeman's main theorem (see Section 10) is parodied as "If nothing exceptional happens, then the frontier moves". Zeeman's proof is restated: "If it did not move, that would be exceptional. By hypothesis, exceptional things do not happen. Hence it moves." A further paraphrase reads: "everything moves (except for those exceptional objects which, at exceptional times, do not)".

The principle of genericity does need to be used with some care, and can never lead to secure results. This is not, however, to say that it is useless. Once this is realised, it is perfectly possible to accept that Zeeman's result has essentially the status ascribed to it by Sussmann and Zahler, without regarding this (as they appear to do) as a *reductio ad absurdum* of the whole enterprise.

The status of the principle may be appreciated in terms of another Sussmann and Zahler criticism (Sussmann and Zahler, 1978); "... take any event at all, such as the fact that yesterday I got up at 8.00 a.m. Let T be the time when I got up yesterday. Appealing to genericity, we can justify assuming that T is not 8:00 a.m., since the property '$T \neq 8{:}00$ a.m.' is generic. In fact, we can justify assuming that $T \neq \bar{T}$, no matter what $\bar{T}$ is, since, for any particular $\bar{T}$, the property '$T \neq \bar{T}$' is generic. So actually, we can justify assuming that I did not get up at all!"

The conclusion is, of course, fallacious. What is true is that, if a theory depended upon the statement, or led inexorably to the statement "$T = 8{:}00$ a.m.", it would be most unlikely to be true. The statement

"$T = 8{:}00$ a.m." is a strong one. It is perhaps relevant to note that Popper (1959) regards it as the hallmark of a good scientific theory that it makes strong statements such as this. Thom (1975 and elsewhere) disputes this criterion. In practice, the problem is solved by interpreting the statement "$T = 8{:}00$ a.m." as (for example)

$$T \in (7{:}59.5\,\text{a.m.},\ 8{:}00.5\,\text{a.m.}),$$

which is generic.

There remains the problem first raised by Berlinski (1975) as to why so many well attested physical laws do not obey the genericity requirement. Thom's (1977) answer that this expresses the sociological constraint that several different observers can communicate is an interesting one, which I hope to discuss in a later paper.

22. Smoothness and Genericity. One telling criticism of the genericity assumption is raised by Sussmann and Zahler (1978) against the use of smooth functions. As they point out, "within the class of continuous functions, the class of nowhere smooth functions is generic, and the class of smooth ones is not". The use of Thom's theorem, according to the "metaphysical way" is only justified if we *first* assume our functions to be smooth.

That this apparently harmless assumption can lead to difficulties has already been noted (see, e.g. Section 7 above). The restriction to C^∞ functions is a severe limitation of the theory and one which, to my mind at least, deserves wider prominence as a criticism. It should always be borne in mind when the applicability of some elementary catastrophe is *postulated.* Catastrophe theoretic accounts of phenomena require experimental test like any other, not only because genericity may be violated, but because smoothness may not apply.

23. Scale Changes. Zahler and Sussmann (1977) point out that (4) gives a surface different from that of Fig. 1 and state that in consequence of this, the postulation of a cusp is unjustified. This may be answered by considering b^3 to be replaced by the generic cubic $b^3 + \varepsilon b$, which reinstates the cusp, but this does not entirely meet the objection.

It is assumed that however we measure the control variables, a cusp results. This is perhaps an optimistic expectation, for the geometry is altered by any transformation of the form

$$b \to b^\alpha \tag{23}$$

for $\alpha \neq 1$. We are by no means assured that the particular system of measurement we use corresponds to $\alpha = 1$. What is experimentally natural may not be most convenient mathematically.

On the other hand, the precise cusp geometry is not always required. The curve fitting exercise for the prison riot model is so poor that this discussion can hardly be said to depend on it—much would remain valid if a parabola replaced the cusp. In the case of the "main theorem" of the embryological discussion, however, significant differences would appear in the account.

Again the question of the local nature of the theory is at issue. The parabolic cross-section produced by Fig. 2, becomes, if we alter b^3 to $b^3 + \varepsilon b$, a cusped curve such as that shown in Fig. 3. Looked at in very fine detail, this presents a cusp geometry. However if looked at on a larger scale, it might well be seen, in an experimental situation, as a parabola.

The question of axis scaling has, in fact, been raised by Thom (1977) as an objection to attempts to quantify the "metaphysical way". He sees it as *a priori* implausible that we could arbitrarily multiply all the control variables by some scale factor and keep the basic geometry intact, as happens with, e.g., (3). Hence the cusp situation so represented requires specially chosen coordinates and the choice of a cusp appears arbitrary. As Thom sees catastrophe theory as a development in the structuralist tradition of "reduction of arbitrariness" (see Deakin, to appear) such use does not appeal to him.

24. Mathematical Metaphors. When Thom's (1972) book first appeared many reviewers looked on its applications as being not so much applications in the usual sense of that word, but rather "mathematical metaphors". Thom does not dispute this. Indeed, he writes (Thom, 1975): "When narrow-minded scientists object to [catastrophe theory] that it gives no more than analogies or metaphors, they do not realise that they are stating the proper aim of [catastrophe theory] which is to classify all possible types of analogous situations."

Such views are likely to be regarded as most controversial and are unlikely to be assessed fully in the near future. In another paper (Deakin, in press) I attempt a more thorough description of these and related positions.

Nonetheless, metaphor should not be seen as necessarily unscientific. Fox's (1971) statement, quoted in Section 17 above, is clearly metaphor, but not on that account unhelpful, imprecise or incomprehensible. Zeeman *et al.* (1976) produce a model that may also be viewed as metaphor. The real test will be found in whether or not this metaphor turns out to be useful to people.

25. Conclusion. Catastrophe theory, applied according to the "metaphysical way" leads to speculative or often qualitative models. While none of these compel our acceptance, some are elegant, suggestive and seemingly useful. Some have been validly criticised in recent papers but other criticisms advanced are incorrect or overstated.

Catastrophe theoretic models need evaluation like any others, particularly in the social and biological sciences. This evaluation process takes time and is unlikely to yield results of either a wholly positive or a wholly negative character. This paper is a contribution to that process.

The above discussion has been greatly helped by correspondence with L. Cobb, T. Poston, I. Stewart, H. Sussmann, R. Zahler and E. C. Zeeman, who do not necessarily agree with the ideas expressed. Discussions with S. Carnie, P. McIntosh and D. Paterson have assisted me in understanding aspects of the published analyses.

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