correspondence

In support of catastrophe theory

On 27 October we published an article by R. S. Zahler and H. J. Sussmann (page 759) critical of 'incorrect reasoning, far-fetched assumptions, erroneous consequences and exaggerated claims' in biological and social-science applications of catastrophe theory. Here is a selection of responses to the article

SIR,—Zahler and Sussmann have a basic misunderstanding of catastrophe theory: in their criticism they ignore the fundamental concept of stability. Stability lies at the root of the modern mathematical theories of dynamical systems and singularities, of which catastrophe theory is a part. The concept was introduced by Andronov and Pontryagin in 1937, and it has been greatly developed not only by Thom but also by the Russian school, notably Anosov and Arnol'd, and the American school, notably Whitney, Smale and Mather.

The importance of stability in modelling lies in the fact that if a stable model is perturbed, then its qualitative properties are preserved. Thom acknowledges the mathematical and scientific importance of stability by incorporating it into the title of his book *Structural stability and morphogenesis*, in which he first introduced catastrophe theory. On page 762 of their article Zahler and Sussmann ask the rhetorical question: "So we must ask again: what is special about a model that looks like a cusp?" and the simple answer to their question is that the cusp catastrophe is stable, whereas their figure 4 is not.

For details of their criticism Zahler and Sussmann refer the reader to a longer paper of theirs, which is not yet published. However, in the preprint of this paper, which they have circulated widely, there are major mathematical mistakes underlying their main criticisms; I explained some of these mistakes at length to Sussmann when he visited Warwick University in July this year.

Most of Zahler and Sussmann's scientific criticisms are based on misquotations, misunderstandings, misrepresentations, or quotations out of context. I give a typical example. On page 762 under the heading of 'Careless discussion of evidence' they state a number of so-called "facts", including: "Zeeman's embryology paper (*Lecturers on Maths in the Life Sciences* **7**, 69 (1974)), 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".

In these two sentences Zahler and Sussmann manage to misquote both Crelin and myself. Far from contradicting the metaphor, Crelin's experimental work supports it: Crelin writes (*J. exp. Zool.* **120**, 561 (1952). "The grafts of all the embryos in the rotation series showed a tendency to curl in a matter of seconds after they were severed from the brain. Therefore, if the rotation of the tectum were delayed for some reason such as the graft sticking to the forceps, the graft would curl into the shape of a ball, making it impossible to continue the operation". On page 577 figure 19, Crelin shows a photograph of the graft curled into a ball. Meanwhile on page 127 of my paper (there is no page 27) it is the underlying mesoderm, not the neural tube, that I liken to a roll of stiff paper. The burden of my discussion on that and the preceding pages concerns the forces exerted, *in vivo*, by the underlying mesoderm upon the overlying ectoderm and neural plate, before the latter has rolled up into neural tube, and at a much earlier embryonic stage than Crelin's experiment.

I now give an example of misrepresentation. When sophisticated mathematics is applied to science it is common practice to publish separately both rigorous mathematical proofs and more simplified expositions of the same material: the latter are essential if the work is to be made available to those scientists who are not expert mathematicians. A case in point is my treatment of primary and secondary waves in developmental biology. My initial simplified version, addressed primarily to biologists, is in *Lectures on Maths in the Life Sciences*, while mathematical discussions and rigorous proofs, addressed primarily to mathematicians, are in *Proc. Int. Cong. Math., Vancouver* **2**, 533 (1974) and Wasserman, G. *Acta. Math.* **135**, 57 (1975). The mathematical treatment uses stability with respect to a symmetry-group that is appropriate to developmental biology, namely an extension of the group of diffeomorphisms of space-time preserving the foliation by time-paths. Zahler and Sussmann criticise the simplified version for mathematical naïvety, without acknowledging the existence of the sophisticated version, although the latter has been brought to their attention long ago.

These are two examples: I could point to a hundred others. I have always found that my work has benefited from the constructive criticism of my fellow mathematicians and scientists. However, to argue in print against the determined misrepresentations of Zahler and Sussmann is both tedious for the reader and unproductive, and an adequate answer to this type of criticism is provided by my original papers *Catastrophe theory, Selected papers 1972–1977*. Nevertheless I am prompted to reply to this particular article for three reasons: firstly, their disgraceful omission of any reference to the work of Thom on biology, secondly, their implied dismissal of the fine work of Berry in physics, and thirdly, the potential harm that their article might cause to the work and the careers of other scientists who are using catastrophe theory, particularly the younger ones who have not yet established their reputations.

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SIR,—In response to the criticisms of R. S. Zahler and H. J. Sussmann we wish to point out that contrary to misleading statements they make such as "catastrophe theory is a blind alley", several examples of genuine applications of catastrophe theory already exist in the physical sciences.

For example one of us recently studied the topological behaviour of stagnation points in two dimensional flows where, with the aid of Thom's elementary classification of degenerate critical points, a physical understanding was obtained of the complex behaviour of degenerate and non-degenerate stagnation (critical) points in a particular flow (Berry, M., & Mackley, M. R. *Phil. Trans. Roy. Soc.* **287**, 1–16 (1977)). Further physical applications amenable to direct experimental test exist, some of which were discussed at the Institute of Mathematics and its Applications meeting held at University College London in May 1977.

Concerning the application of catastrophe theory to biology, we agree that some inaccurate or premature claims have been made. In particular, Zeeman's argument for the existence of primary and secondary waves in developing embryos does not have the logical status of a proved theorem since he has not as yet published the full mathematical proof. Rather, his use of catastrophe theory in a description of differentiation gives rise to the hypothesis that such waves occur and, with the additional postulate of a temporal periodicity of state in the tissue, this application suggests how spatially periodic structures such as somites may arise.

These hypotheses have stimulated experimental investigation, which is a major purpose of model-building. Furthermore, Zeeman's treatment of differentiation has the additional virtue of providing a unitary field description of a process which is often erroneously and misleadingly described in terms of separate spatial and temporal mechanisms. In a subject such as developmental biology, which has barely begun to come to grips with its central problem of morphogenesis in terms of models, it is more important to get the correct qualitative treatment than to attempt quantitative precision.

It is far too early to decide whether or not catastrophe theory will be of major value in biology. That it provides useful and accurate descriptions of certain physical processes is now beyond question. More generally, the context for catastrophe theory is topology, and topological thinking has been of immense value in the understanding of many physical phenomena. It seems highly probable that the topological approach will prove invaluable in the study of biological processes as well, but this is an approach that can only be learned slowly, with trial and error. Zahler and Sussman have presented some valid criticisms of applied catastrophe theory, but their over-reaction is unfortunate. It leads them into exaggeration and wholesale rejection of very useful propositions.

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Sir,—The case in favour of catastrophe theory rests not on speculative models in the social sciences, but on successful applications to the physical sciences. In 1975 and 1976 there appeared approximately 42 papers applying catastrophe theory to physics, nine to biology, and 14 others: Sussmann and Zahler's criticisms deal almost entirely with one sociological paper, two on biology, and one model taken from two popular articles and a paragraph in a conference report. They do not hesitate to extend their conclusions to areas they have not studied: "we anticipate that the results of an extended search (covering biology, linguistics, physics, or psychology) will be similar (that is negative)" from (Sussmann, H. J. & Zahler, R. S. *Proceedings of the 1976 biennial meeting of the Philosophy of Science Association, Chicago,* in press). Tim Poston and I have written a book (Poston, T. & Stewart, I. N. *Catastrophe theory and its applications,* Pitman, London, 477 pp.), due in print early in 1978, documenting quantitative applications in the sciences, which casts severe doubt on their conclusions. A major plank in their case—allegation of a repeated mathematical error—is refuted by Poston (*Mathematics Report,* Battelle Geneva (in press)). Their reliability may be judged by their statement: "Stewart repeats the untrue assertion that Zeeman's embryological predictions have been 'recently verified by experiment' ". What I wrote was: ". . . with the prediction that slowing down the chemical reactions of the primary wave would lead to the formation of fewer somites, an effect recently verified by experiment". Which happens to be true.

Similar misinterpretations vitiate many of Sussmann and Zahler's criticisms, rendering them analogous to disproving Pythagoras' theorem by exhibiting a triangle that is not right-angled. With the exception of their discussion of the nerve impulse model, few of their criticisms are conclusive, and some are simply wrong. Others are problems of general mathematical modelling, which can usually be resolved by reference to current scientific practice. Sussmann and Zahler's charges go considerably beyond anything they have correctly substantiated.

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Sir,—It would be a pity if the strong attack by Zahler and Sussman on some biological and sociological models based on catastrophe theory, (27 October, page 759) were to mislead readers into thinking that such new and beautiful mathematics has no useful application in any science. The fact is that in this laboratory catastrophe theory is being employed in the development of new concepts, in the explanation and prediction of phenomena, and in the design of experiments, in two areas of physics.

The first is short wave optics (and quantum mechanics) where Thom's theory classifies the forms of focal surfaces (caustics) and makes it possible to give a precise description of the finest detail in the associated diffraction patterns (Arnol'd, V. I. 'Critical points of smooth functions and their normal forms' *Uspekhi Mat Nauk* (translation: *Russian Mathematical Surveys*) **30,** 1–75 (1975); Berry, M. V. 'Waves and Thom's Theorem' *Adv. in Phys,* **25,** 1–26 (1976); Duistermaat, J. J. 'Oscillatory integrals, Lagrange immersions and unfolding singularities' *Comm Pure App Math* **27,** 207–281 (1974)). The classification describes caustics that are 'structurally stable', that is those whose forms survive perturbation. This makes catastrophe theory particularly suited to the optics of nature rather than artefacts such as microscopes and telescopes whose focussing is dominated by cylindrical symmetry.

We have made progress in understanding the optics of irregular water droplet 'lenses' (Berry, M. V. 'Waves and Thom's Theorem' *Adv. in Phys.* **25,** 1–26 (1976); Nye, J. F. 'Optical caustics in the near field from liquid drops' (submitted to *Proc. Roy. Soc.*), the fine structure of swimming pool caustics (Berry, M. V. & Nye, J. F. 'Fine structure in caustic junctions' *Nature* **267,** 34–6 (1976)), atom scattering by crystal surfaces (Berry, M. V. 'Cusped rainbows and incoherence effects in the rippling-mirror model for particles scattering from surfaces'. *J. Phys. A* **8,** 566–84 (1975)) and the statistics of twinkling starlight (Berry, M. V. 'Focusing and twinkling: critical exponents from catastrophes in non-Gaussian random short waves' (*J. Phys. A,* in press)). This last application (which has proved peculiarly resistant to more conventional forms of analysis) makes essential use of the enormous extension of Thom's classification being developed by Arnol'd (Arnol'd, V. I. 'Critical points of smooth functions and their normal forms' *Uspekhi Mat Nauk* (translation: *Russian Mathematical Surveys*) **30,** 1–75 (1975)) in the Soviet Union.

The other area is fluid mechanics, where the elliptic umbilic suggested the design of the 'sixroll mill' (Berry, M. V. & Mackley, M. R. 'The sixroll mill: unfolding an unstable persistently extensional flow'. *Phil. Trans. Roy. Soc.* (London) **287,** 1–16 (1977)), a device for studying the effects of dissolved long-chain molecules on the flow of Newtonian fluid. The mill produces a sequence of flows with fully describable instabilities, and addition of polymer is dramatically revealed by changes in the topology of the pattern of streamlines. This specialised application has now been generalised (Thorndike, A. S., Cooley, C. R. and Nye, J. F. 'The structure and evolution of vector fields and other flow fields' (submitted to *J. Phys. A*)) into a comprehensive theory of flow patterns, which has already given insight into the structure of the geostrophic wind and the move-