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 movement of ice in the Arctic ocean.

These are genuine applications of catastrophe theory; they have led to advances in our understanding of the physical systems concerned. It is important to distinguish them from illustrations of the theory, where the mathematics is employed correctly (that is to systems satisfying its axioms) but in more sophisticated derivations of results already known; elastic buckling and the mean field theory of phase transitions fall into this category. The applications should also be distinguished from what I shall call invocations of the theory, where it is employed because of the suggestiveness of its images in the hope that its axioms might eventually be shown to apply; perhaps it is towards this area that Zahler's and Sussmann's criticisms are really directed.

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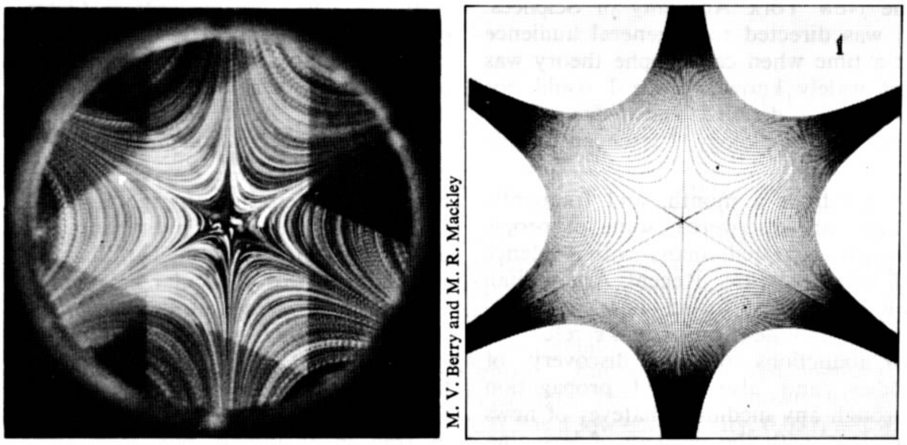

The six roll mill: experimental observations and computer simulations of the patterns illustrating elliptic umbilic critical point.

SIR,—While I do not wish to perpetuate unwarranted enthusiasm for the ability of catastrophe theory to transform the natural and social sciences, I believe that Zahler and Sussmann (27 October, page 759) have overstepped the bounds of decency in their vehement attack. Let me point out a few specific instances in which they seriously mislead the reader.

A fundamental error that they make is in the statement, "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". I can only conclude from this statement that Zahler and Sussman have not read Thom's work. (There is no reference to Thom in the paper apart from his theorem). Thom describes the catastrophes which Zahler and Sussmann discuss as "elementary catastrophes" but repeatedly makes it clear that there are other kinds of catastrophe as well.

The authors' confusion on this point creates a straw man which they repeatedly flail in their article. For example, juxtapose the quote in the first paragraph of their paper with Thom's general definition of catastrophe and with their restricted one. The quote refers to a general approach to studying questions rather than the repeated use of a specific mathematical theorem, and makes much more sense in the context which was intended. The section on 'Better alternatives' accuses catastrophe theory of ignoring the study of shock waves and bifurcation theory even though these are explicitly discussed in Thom's book, *Structural Stability and Morphogenesis* (Addison-Wesley, 1972), in which he lays out his theory in detail for the first time. I might add that one of the most successful applications of catastrophe theory has been to the study of shocks in a single convex conservation law (Schaeffer, D. 'Regularity theorm for conservation laws' *Advances in Mathematics II* 368–386 (1973) and Golubitsky, M. & Schaeffer, D. 'Stabiliyt of shock waves for single conservation law', *Advances in Mathematics* **16,** 65–71 (1975)). In their portrayal of the scope and content of catastrophe theory, Zahler and Sussmann are simply wrong.

Let me turn to a second point. In a section entitled 'Careless discussion of evidence', Zahler and Sussmann quote Zeeman's statement: "Recent experiments by J. Cooke and T. Elsdale appear to confirm some of my *predictions*" (my italics). They then refute this statement with a quotation from T. Elsdale *et al.*, "we do not yet conclude that the observations here presented have confirmed Cooke and Zeeman's *model* to the exclusion of others" (my italics). The paper of T. Elsdale *et al.*, does indeed confirm some of the *predictions* of the Zeeman–Cooke model. It does not confirm the *model,* but then Zeeman did not assert that it did. Zahler and Sussman never say that these two statements contradict one another, but they clearly imply that Zeeman has made unsupported claims in his statement. This is false, and they try to mislead us into believing it.

At another point in their discussion, Zahler and Sussmann misrepresent the work of Kozak and Benham on denaturation of proteins. They assert that an essential feature of the work of Kozak and Benham is the 'delay rule' which predicts that there will be hysteresis in the denaturation-renaturation phenomenon. Yet Kozak and Benham do not rely upon this 'delay rule', and indeed use the 'Maxwell convention' throughout the second part of their three part work ('Denaturation: an example of Catastrophe II. Two-state transitions', *J. Theoretical Biology* **63,** 125–149 (1976)). Zahler and Sussmann also criticise this model for predicting that the temperature denaturation curves have vertical slopes while the enthalpy change limits their steepness. Their criticism ignores the statistical discussion of Kozak and Benham which addresses the fact.

The thrust of this last criticism is also misplaced. A similar criticism could be made to the study of shocks in the solution of hyperbolic conservation laws, a phenomenon which is better understood mathematically. Real gases have viscosity, and viscosity prevents a truly discontinuous change in the velocity of the gas. Nonetheless, abrupt changes do occur. They can be modelled well by hyperbolic conservation laws which do allow discontinuous solutions. The addition of a term representing viscosity to the equations smooths out this discontinuity, and the behaviour of solutions has been studied as this viscosity term tends to zero. The conservation laws work well in giving approximations to real fluid flow. It is even the case, as we noted above, that catastrophe theory has described the shocks for the simplest (but only the simplest) conservation laws. The confusion of "the intuitive notion of 'jump' as a rapid change with the precise mathematical notion of a jump discontinuity" is not inherent to catastrophe theory, but is a common and useful approximation in many mathematical models.

I prefer to make a few remarks on the paper as a whole rather than continue to belabour specific failings. As a 'review article', Zahler and Sussmann review a single paper—their own. Their review is a summary and not a review. There are others who are more optimistic about the potential applications (and the past successes) of catastrophe theory than Zahler and