LETTERS

Catastrophe Theory

Gina Bari Kolata, in her article "Catastrophe theory: The emperor has no clothes" (Research News, 15 April, p. 287), states that Héctor Sussmann, the leading critic of the theory, became "fascinated with the sociology of its growing popularity." I am fascinated by the sense of mission that drives some scientists to denounce heresies and heretics, and by the ease with which others abandon their objectivity and jump on bandwagons. The article provides some insight into how the bandwagon mentality is fostered.

Kolata states that "Zeeman, contacted about Sussmann's harsh criticisms, said he was unfamiliar with the details of the criticisms; when they were described, he gave no direct or specific rebuttals to any of them." She does not say when or how or by whom he was contacted, nor under what circumstances. (One can easily imagine circumstances under which it is wisest not to reply.) She does not say what she means by "direct" or "specific." The impression is left that Zeeman could not answer the criticisms.

Kolata cites charges of sloppiness, exaggeration, irresponsibility, and dishonesty but does not give the details of any of these, nor does she reference either the original papers or the criticisms. The readers are not urged to judge the matter for themselves, but are left instead to rely on the authority of the experts she quotes.

Kolata states that the list of mathematicians opposing catastrophe theory reads like a who's who in American mathematics and cites two names. Even had she cited a hundred, it would hardly constitute proof that catastrophe theory is unsound. The effect of this approach is to make it easy and acceptable for scientists and mathematicians to ridicule catastrophe theory, without having to go to the trouble of studying the details.

Kolata cites Sussmann's contention that the use of "ifs," "coulds," and "maybes" by catastrophe theorists puts the burden of truth on their critics. It could be that this is, instead, an indication of intellectual honesty. If they are aware that their hypotheses are tentative, and put them forth as suggestions, not definitive assertions, then their use of qualifiers is laudable.

Kolata quotes others as saying that catastrophe theorists are reluctant to undertake a real study of the phenomena to which they hope their theories will apply; the reader is left to conclude that, since they don't know what they are doing, they need not be taken seriously. But no evidence whatsoever is given for these charges, nor is it suggested what such a study ought to entail. One should not gloss over the very real difficulties, due to the highly specialized nature of contemporary science, that stand in the way of anyone who seriously tries to understand the problems in another field from the point of view of its practitioners.

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Kolata's article concerning criticisms of catastrophe theory was both timely and penetrating. It is true that practical applications of the theory have been either obvious or dubious. It is also true that extravagant claims have been made at a time when the principle statistical technique for the development of a catastrophe surface involves courage and a good eye. Furthermore, social scientists are going to get exceedingly tired of looking at the same three models over and over again (only the fold, the cusp, and the butterfly are based on probability distributions). But before the criticisms grow too loud, it would be wise to distinguish those aimed at Zeeman's work from those aimed at any practical application of the theory. After all, social scientists have not really had a chance to develop the statistical techniques that will be necessary for the creation of believable applications. However, the development of those techniques is under way. For example, I am currently investigating several correlation and regression procedures that can indicate the existence and location of a cusp or butterfly catastrophe. The result is a new model of the psychological changes that occur in crisis situations—a model that makes predictions that are both unexpected and verifiable.

Newton's critics eventually caused him to call science a series of lawsuits. Now Thom's admirers give us a modern Newton and his detractors give us new lawsuits. Let us have neither.

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I was rather sorry to see the recent article on catastrophe theory by Kolata. While it must be granted that a number of immoderate claims in the form of "catastrophe theory can do everything"

have been made in the literature, on the basis of too little experience, it doesn't seem that the proper response is an equally immoderate claim that "catastrophe theory can do nothing" on the basis of that same body of experience.

As an interested spectator, with no particular ax to grind one way or the other, I feel it is far too early to tell what the impact of catastrophe theory will be in the sciences. As it stands now, I would say that it is not yet a theory in the scientific sense; rather, it is a series of suggestions on how a theory might be built, based on some suggestive mathematical results. Its utility will depend on the art with which the mathematical formalism can be interpreted in terms of empirical experience. Attempts at such interpretation have barely begun. If some early attempts have been overly hasty, it does not necessarily mean that the formalism is meaningless or inapplicable; or that those responsible are guilty of anything beyond overenthusiasm.

I believe it is true, as imputed in Kolata's article, that the response to catastrophe theory (including the article itself) is a sociological phenomenon, whose roots it would be most instructive to explore. However, it doesn't seem fruitful to reach a decision concerning the applicability of scientific concepts on sociological grounds. In general, if an individual scientist finds such concepts uncongenial, let him not use them. There is no reason why he should take their existence as a personal affront.

The situation regarding catastrophe theory today reminds me of what happened to information theory in the mid-1950's. Then, too, extravagant claims that information theory could provide deep insights into all complex systems, from organisms to societies, were followed by a backlash in which it was asserted that the theory was impotent or fraudulent. Neither was true, and everyone lost from the resultant polarization. I would not like to see this unhappy history repeat itself.

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The controversy over catastrophe theory need not itself provide an example of the cusp catastrophe. In other words, there is a possible middle ground between the two extreme points of view.

Thom's theorem of the seven (on which the theory depends) assumes that the system is described adequately and completely by the minimization (or maximization) of a scalar potential function. It is a local theory, not a global one; that is, it applies only in the immediate neighborhood of singular points.

Any claimed application which neglects these presuppositions can be suggestive at best, never definitive. Nevertheless, it should be stressed that the overworked methodology of the correlation coefficient also depends upon local approximation. It follows that much criticism now leveled at catastrophe theory could also be applied to many sociometric and biometric studies.

I have recently been able to find a relatively simple proof of the theorem of the seven (*1*). As such accounts become more widely known, the theory will become more accessible in detail to scientists other than research mathematicians. This will enable more informed critical analysis of claimed applications.

Meanwhile, Thom's theorem provides a possible tool for application. How useful that tool is going to be is a matter that is not yet clear. The settling of this question is not likely to be aided either by irresponsibly extravagant claims on the one hand or by bombastic criticism on the other. What is required is a balanced assessment of the already numerous efforts to apply the theory.

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References

1. M. A. B. Deakin, *Bull. Math. Biol.*, in press.

Carcinogens in Schools

A report appeared last September (*1*) which stated that 418 pounds of carcinogenic chemicals were then being stored or used in schools in California; that there was no evidence that students were being unduly exposed; and that safe, lawful disposal of such materials is available through the state department of health.

The 14 carcinogenic chemicals referred to are regulated by federal and state occupational safety and health acts. We find it difficult to imagine purposes which could justify the storage or use in schools of such quantities of dangerous materials, even though the compounds are distributed among some 200 institutions.

Since there are no permissible exposure levels to chemical carcinogens we

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