LETTERS

Catastrophe Theory: A Skeptic

I am prompted to write this letter by one gap in Gina Bari Kolata's otherwise excellent article on catastrophe theory (Research News, 15 April, p. 287) and by some letters from readers (17 June, p. 1270) in which my name is mentioned.

Although Kolata says clearly that I have given lectures in many leading American universities and that the criticisms Zahler and I have raised against catastrophe theory are widely known by the mathematical community, she does not state that one person who should surely know about them is E. C. Zeeman. I sent a copy of my first critical article on the subject (*1*) to Zeeman in October 1976; he acknowledged receipt and wrote that he was "looking forward to reading it." Preprints of a second article (*2*), written with R. S. Zahler, were sent to a long list of people, including Zeeman, and to everyone else who asked for one.

Senechal (Letters, 17 June, p. 1270) objects to the fact that Kolata's article did not provide the evidence on which we base our criticisms, thereby seeming to imply that perhaps there is no evidence. Kolata simply tells the story and identifies the people that are part of it; anybody who wishes to pursue the matter can do so. Senechal could have written to us and requested a preprint, as many other readers have done, rather than blame Kolata for not writing a specialized technical article.

If, in March 1977, Zeeman said he was not familiar with a paper he had received in October 1976 then, maybe, it was because he had not read it. Is it legitimate to use Zeeman's ignorance in his own defense, as Senechal seems to do? If her arguments were valid, it would be very easy for anybody to be guaranteed immunity from criticism. Just ignore the critics!

Senechal's letter provides a good illustration of why I am fascinated by the sociology of catastrophe theory. Senechal writes that the catastrophists, when they use "ifs" and "maybes," may be doing so out of intellectual honesty. But when Zeeman writes that catastrophe theory "has the potential for describing the evolution of forms in all aspects of nature" (*3*) and that the "theory could provide a mathematical language for the hitherto 'inexact' sciences" (*3*), what would a reader be likely to notice first, the big claim or the small qualifiers?

Senechal also appears to make a free speech issue out of a scientific disagreement. This is a free country, and

we all believe in free speech; nobody is trying to follow Lysenko's footsteps and impose a ban on "heretical" scientific views. This being clear, it should also be clear that *all* kinds of speech should be free, including critical speech, and that scientific theories must be subject to critical scrutiny.

Lewis, Rosen, and Deakin, in their letters (17 June, pp. 1270–1272), seem to argue for a "middle of the road" view. The catastrophists have overdone it, they say, but so have the critics, and the truth must lie somewhere in the middle. Do they believe in the general postulate that, given any disagreement, the truth must lie in the middle? Such a claim seems clearly fallacious. If you believe in Nazism and I believe in democracy, how many will argue that the truth must lie halfway between us?

Lewis, Rosen, and Deakin should provide reasons why, in this case, the rule applies. Deakin gives none. He has found a simpler proof of Thom's theorem, but what is being discussed is the applicability, not the truth, of the theorem. By labeling the criticism as "bombastic," without saying why, Deakin is taking an easy way out. Is it the critics' fault if the theories they are presented with are such that little criticism can fail to be "bombastic"? My experience from lecturing indicates that many people who are unfavorably predisposed toward the critics have not read the catastrophists' papers. In my talks I show the audiences what is really there and discuss it. That usually suffices. Sometimes someone says I am being "too harsh," but nobody has so far come up with a refutation of any of the many concrete, specific points that Zahler and I have raised.

Rosen's point is similar to Deakin's. He says it's immoderate to claim that the theory can do nothing on the basis of the experience so far. But it certainly is not immoderate to claim, on the basis of what has been attempted so far, that little has been achieved so far. And that is all we claim. What the future may bring, nobody knows. Maybe catastrophe theory will have great successes. Maybe the philosopher's stone will be found. But, until it is, skepticism seems an appropriate attitude.

HÉCTOR J. SUSSMANN
Department of Mathematics,
Rutgers University,
New Brunswick, New Jersey 08903

References

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2. ——— and R. S. Zahler, *Synthèse*, in press.
3. E. C. Zeeman, *Sci. Am.* **234**, 65 (April 1976).

Modern Agrarianism

In these days of rapidly changing energy prices, it is difficult to predict the future economic and social structure of the United States. But if energy consumption is used as the independent variable, an interesting and relevant calculation is possible.

For example, in 1974, the total nonsolar energy expended on food consumption and production in the United States amounted to about 16.5 percent of all energy use (*1*). This figure is the sum of all energy consumed from ground to table. In that year we spent about 7.1 units of nonsolar energy to consume 1 unit of food energy (*2*). About 38 percent of the food calories came from meat and animal products (*2*).

Suppose that somehow U.S. animal feed grain programs were suspended (with the exception of exported feed grains). The only animal products consumed would then be (i) imported meat and animal products and (ii) meat and other products from animals raised on hay, silage, and grazing ground. At current rates of import and production, this change would result in a reduction in the consumption of meat and animal products by approximately one-third (*2, 3*). The decrease in protein consumption need not be reconciled, as Americans apparently consume on the average about 45 percent more protein than required (*4*). However, the protein change could be made up through consumption of vegetable protein grown on a fraction (0.5 percent) of the cropland now used for the same amount of animal protein (*5*). Since a unit of beef protein requires about six times the total energy of an equivalent unit of soybean protein (*5*), such a change would reduce U.S. energy use by about 1 to 2 percent (*2, 6*).

With these assumptions, about 80 million acres of land previously used to raise prime feed grain could be considered released from production. If biomass-electricity production (the growing of crops such as sunflowers, sugarcane, or hybrid trees and their combustion under steam boilers for the production of electricity) took place on this land, we could install about 255 1000-megawatt electric plants (*7*). This is equivalent to approximately half of the electricity generating capacity installed as of 1974 in the United States (*8*). The biomass energy input to the electric plants would be equal to approximately 20 to 25 percent of all U.S. energy use in 1974 (*8*). Biomass cultivation energy use would approximately equal that used in producing the feed grains.

About one-third of all electricity in