small perturbations have been proved in a Banach space setting by Magnus (*Battelle Geneva Math. Report* **107,** 1977) and he and I have applied them to plate buckling at a double eigen-value (*ibid,* **109,** 1977). The genericity statement, that "almost every" four-parameter variational bifurcation reduces locally to a Thom catastrophe, apparently holds only subject to an "everywhere Fredholm" condition (which is probably provable *a priori* for a variety of problems), though no one has yet properly set out the details, to my knowledge.

To date it is within the "traditional" physics of scattering theory, non-Newtonian flows, elastic buckling, laser action and so on that catastrophe theory has achieved its most definite successes. (Details of these and other "hard" applications with experimental support are given by T. Poston and I. N. Stewart, *Catastrophe Theory and its Applications,* Pitman-Fearon 1978.) Outside physics, only Zeeman's embryological model has been substantially exposed to experiment: the result is considerable support, though this is not yet "confirmation" [a distinction familiar to physicists, though apparently not to over-excited critics: compare *Nature* **269,** 759 (1977)].

I would like to amplify Schulman's apt comparison of catastrophe theory to "the use or appreciating of . . . the abstract concept 'group'." Symmetry arguments and reasoning "in general" (for example, "more equations than unknowns have no solutions") both date back centuries. The formalization of one is group theory: that of the other is rigorous transversality theory, at the center of "Thom's theorem" and its uses. It is becoming clear that the scientific power of the second formalization compares to that of the first.

TIM POSTON
Battelle Advanced Studies Center
2/14/78 *Geneva, Switzerland*

THE AUTHOR REPLIES: Tim Poston's comment on caustics raises an important problem, which, as far as I know, has been ignored by theorists and philosophers of catastrophe theory, namely the relation between catastrophe theory and approximation. First in specific reply to Poston's letter I'd like to quote the first half of the sentence he quotes from my review: "Caustics in optics are generally acknowledged to be catastrophes, but . . ." Indeed this acknowledgement is the impact of the work of Duistermaat and others. My quibble is that that conclusion is not reached by pure application of catastrophe-theory methods. Rather, by asymptotic analysis of partial differential equations, they found that good approximations to solutions take the form of oscillatory integrals and it is these oscillatory integrals that lend themselves to analysis by catastrophe theory. A *bona fide* "application" of catastrophe theory should, to my mind, take the form of stating some variational principle for Maxwell's equations and then without further analysis invoking a theorem from catastrophe theory to say that whenever (generically) singularities occur their morphology should be that of one of Thom's seven elementary catastrophes.

Now the point about approximations: my demand for a catastrophe theory of caustics that works directly from Maxwell's equations is not just unreasonable, it is impossible. In fact, Maxwell's equations have no caustics on a distance scale small compared to the wavelength of the light. It is only the asymptotic approximation—generalized geometrical optics—that exhibits caustics (hence the mathematical results on caustics and catastrophes are, in a sense, best possible). This situation—the fact that *the exact theory has no catastrophe* but only a certain approximation to it does—is not confined to caustics. For phase transitions mean field theory gives catastrophes, but no one (to my knowledge) has shown the exact theory of any realistic model to have this property. Another example is the transition from spherical to non-spherical nuclei as nucleon number passes about 20. Ady and Yifah Mann and I found (unpublished) that the approximate (Hartree–Fock) theory of this phenomenon exhibits a catastrophe but that presumably an exact theory does not. Yet another example: Even Zeeman's catastrophe machine has its catastrophes smoothed if the kinetic energy of the pointer is taken into account.

So here are two problems for catastrophe theorists: Assimilate this idea of the appearance of catastrophes *only* in an approximate theory, and explain—mathematically—why for so many systems there are in fact good approximations for which catastrophe ideas apply.

L. S. SCHULMAN
Technion
3/20/78 *Haifa, Israel*

APS position of ERA

Why hasn't the Council of The American Physical Society taken a position on the question of holding meetings of the Society in States that have voted down the Equal Rights Amendment or in states where the Amendment has remained bottled up in a legislative committee?

Why hasn't there been any discussion on this question in the "Letters" section of PHYSICS TODAY?

The "object of The American Physical Society is the advancement and diffusion of the knowledge of physics." Physics knowledge is not defined or correlated by sex. The promotion of the goal of equality for women in the physics community

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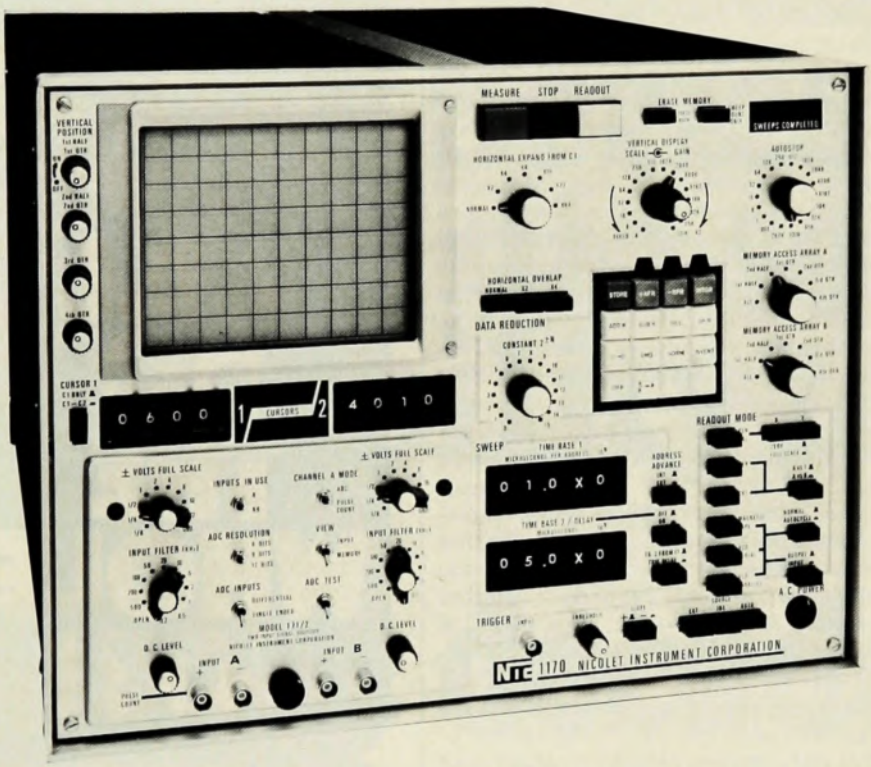

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should not be confined to the internal affairs of The American Physical Society. After all, it is in society at large, which serves as the source of new physicists, where implicit limitations on the career roles and goals of women exist.

The American Physical Society can promote its goals most effectively in society at large, by example and by exerting economic pressure. At the very least, future candidates for election to the Council ought to state their position on this question.

ROBERT J. RUBIN
National Bureau of Standards
Washington, D.C.
4/17/78

Hertzsprung–Russell Diagram

David DeVorkin's article, "Steps toward the Hertzsprung–Russell Diagram" in March (page 32) conveys the principal results of his penetrating examination of spectral classification and stellar evolution at the turn of the century. His researches demonstrate the power of using manuscript material of the sort that is now being widely gathered for more recent astronomy and physics by the Niels Bohr Archives at the American Institute of Physics.

In passing, two small errors in his article should be corrected. Harlow Shapley, the first graduate student of Henry Norris Russell, carried out the 10 000 binary star observations for his thesis at the Princeton Observatory, and went to Mount Wilson only subsequently. Mrs Fleming's system of spectral classification, using the Harvard letters but not the numerical subclasses, was published in the *Draper Catalogue of Stellar Spectra*. Draper's first name is reserved for Annie Cannon's *Henry Draper Catalogue of Stellar Spectra*, whose first volume was issued just sixty years ago in 1918.

The nine main volumes of the HD contain 225 300 stars, with numerical subclasses, arranged in the order OBAFGKMRNS. For several years I have held an annual mnemonic contest in my Natural Science class at Harvard. Among the most memorable entries are "Oh bring a fully grown kangaroo, my recipe needs some" and "Oh brutal and fearless gorilla, kill my roommate next Saturday."

OWEN GINGERICH
Harvard-Smithsonian Center for Astrophysics
Cambridge, Mass
4/6/78

New public interest

As one who is about to make a career change into physics, I have some thoughts to share with the community. These perceptions are gleaned from eight years in the Unitarian Universalist ministry. (I will begin doctoral work in physics at the University of Texas this fall.)

Much of the following of pseudoscience cults results from a sincere desire to understand reality; much of the new found popularity of science fiction stems from a rebirth of wonder. Capra's physico-mystical *The Tao of Physics* is but the latest in a tradition that includes Heisenberg, Einstein and Schrödinger. But this time the readership is *much* larger and broader. Straightforward popularization of science, particularly cosmology, is generating increasing interest from the public.

In short, it appears that society at large is beginning to develop an interest in science apart from that in the "Golden Age," when the goose laid technological marvels daily. As the bases of culture shift, new worldviews form that may or may not incorporate respect for science and its results. Needless to say, a constituency of support among the public would not hurt research funding.

If the physics community responds in an affirming way to this interest, the results might again prove our projections wrong. Pleasantly so.

ADRIAN L. MELOTT
University of Texas
Austin, Texas
4/20/78

Radio program on cosmology

On 7 March there was a benchmark radio program on cosmology broadcast over KGO San Francisco, a major AM radio station owned and operated by the American Broadcasting Companies, Inc. Listeners were able to talk by phone to Grant J. Mathews of the Lawrence Berkeley Laboratory, who was at the station, and to Dietrick Thomsen, Senior Editor of *Science News*, who was on a phone patch from his home in Washington, D.C.

The purpose of this program was to demonstrate to the management of the major electronic media that science is just as interesting to the general public as the other subjects that regularly obtain exposure in that media, a hypothesis I had advanced to some ABC executives one night over cocktails. That hypothesis was not contradicted. There was much interest in the program, which ran longer than the scheduled one hour because of the large public response. The reader might be surprised to learn that this interest included, but was not limited to, the obvious subjects, such as the Jupiter effect and interstellar travel. There was equal public interest, for instance, in the concept of "finite but unbounded" spacetime. Moreover, Mathews and Thomsen did an excellent job of explaining such mathematically sophisticated ideas in lay language, at least to the es-

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