letters

the AAPT was one attempt to alleviate this problem by collecting lists of important references (with annotations) on various subjects of current interest. The AAPT Selected Reprint series is a similar attempt. Recently, A. Fraknoi published a "Subject Index to Astronomy Articles in *Scientific American* Magazine (1960–1976)" in *Mercury*,[2] a publication of the Astronomical Society of the Pacific. Although this contribution was aimed at enlightening lay people and amateur astronomers, it will nevertheless be extremely useful to students and teachers of astronomy as well. Following the lead of Fraknoi we have compiled a subject list to astronomy articles in PHYSICS TODAY.

This index is comprehensive in that it contains references to every astronomy-related (including astrophysics, astronautics and geophysics) feature article published in PHYSICS TODAY during its first thirty years of publication (1948 through 1977). It does not, however, include book reviews, letters, or brief reports in the "Search and Discovery" column. The compilation is extensively cross-referenced and is organized by subject headings that correspond closely to those employed by Franknoi. Thus this index will complement his.

References

1. Anonymous, *Am. J. Phys.*, **38**, 1053 (1970).
2. A. Fraknoi, *Mercury*, **6**, no. 1, 20 (1977).

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Readers may obtain copies of Harney's subject index by writing to PHYSICS TODAY enclosing $2.00 in check or money order for each copy.
The Editor

Opposes MBO

Thomas M. Tobin, in his letter on management by objective (February, page 83), used a phrase in his third paragraph that indicates the real source of trouble in applying MBO in a research laboratory. The phrase is "delegation of responsibility." Any ROTC student learns that one can delegate authority but not responsibility. The two concepts are radically different in meaning. All too often it seems that bench scientists have responsibility for everything and authority over nothing. Accountability is little more than harrassment if the person who is held accountable does not have the discretionary authority and support in securing the resources to accomplish his objectives.

Advocates of MBO presume some sort of idealized environment in which supervisors are visionary and competent, in which organizations are willing to take suggestions from the bench, and in which there is a willingness to take risks and fail. It is also presumed that those who apply MBO know the difference between planning an investigation and predetermining success. (It would not be research if it were possible to program a successful result.) The inclusion of the Peter Principle into the consideration may lead one to question the efficacy of MBO. MBO becomes an instrument by which those who have the least say in what they do (and the most responsibility) become bullied into becoming scapegoats by those who have retained all of the authority.

The real-world misapplication of MBO leads to a decline in innovation. The bench scientist, who is made ultimately responsible, learns early that there is no room for failure. If he manages to keep his job past his first failure, he learns that success or, at least, the *illusion* of success is required. Extend these attitudes to a larger scale and one will see only "safe" projects being taken, risk and failure avoided, and real progress disappearing.

It is a remarkable coincidence that the advocacy of MBO has occurred while many scientists have been and continue to be underemployed and unemployed and, at the same time, the leadership of American science has been evaporating. Maybe it is time to discard useless management systems like MBO and hire people to do *science*.

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Catastrophe theory

Schulman's review (January, page 75) of two "catastrophe theory" books is excellent in the main, but I would take issue with one assertion: that optical caustics as catastrophes "do not represent an application of Thom's theorem because that theorem applies only to finite dimensional 'state' spaces." They do represent such an application, by way of a rigorous, finite-dimensional treatment of oscillatory integrals. (The most complete technical account is in J. J. Duistermaat, *Comm. Pure Appl. Math.* **27**, 207 (1974).) My point in the essay cited was that a version of Thom's theorem valid in infinite dimensions would give a natural way of seeing at the ray-theoretic (or classical-limit) level why caustics "should" be catastrophes, though the physical information or "quantum flesh on the classical bones" available by the existing method would be missing. Moreover such a version would be applicable to a wide range of other problems.

Since "Thom's theorem" actually represents a cluster of results, an infinite-dimensional version naturally appears piecemeal. The main results on truncation of Taylor series and insensitivity to

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.

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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.

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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