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any event the default position for anything except obvious mistakes should be to invite the author to make the change, rather than to make it and invite the author to change it back.

▷ When I recommend as referee that a paper be published, I have enough confidence in the author not to demand that any last-minute stylistic changes be submitted to my scrutiny. Can there be any functioning peer who would maintain that it is peer review bypass (I leave it to the PHYSICS TODAY copy editor to decide where to put in the hyphens) to check proofs against one's personal platonic ideal rather than against the original manuscript?

▷ It is not the one-sentence paragraphs that make it necessary to have a full-time board of experts to interpret the Constitution of the United States. Furthermore, the need for such interpretation of our own professional literature is certainly not minimal; much of what we write is of necessity sketchy, incomplete, conjectural. The habit of careless writing allows an author to introduce far more ambiguity than is inherent in the subject, and by discouraging vivid writing the journals contribute to this problem.

▷ If standards of writing in our scientific journals are aimed at the lowest common denominator of a polyglot international readership and only words with "precise" cognates in all major foreign tongues are acceptable, then there is no hope. Obviously one should avoid obscure colloquialisms, particularly when making points of central importance. But I do not believe that the antipathy of *Physical Review* to "charming monograph" and "Nature herself" has anything to do with the possible puzzlement those phrases might provoke in those who speak English as a second language. It has precisely to do with the stifling (and relatively recent) convention that scientific prose should be mechanical and inhuman.

▷ I stand by my claim that changing "$BD = AF$" to "$d(BD) = d(AF)$ (where d is distance)" is unnecessary and offensive, and I doubt that many readers would disagree.

▷ I am disturbed by the point of view implicit in all these letters, positive and negative, that it is possible to separate the writing of the article from the science the article is attempting to describe. It is a rare article indeed that merely reports simple, clear-cut results. Most articles deal with the motivation underlying the research, they contain conjectural elements, they leave loose ends dangling, they pass judgment on earlier work, and, in short, they reflect the ambiguity, confusion and unavoidable untidiness of science as it is practiced in the real world. Good writing tightens up more than the prose; it tightens up the thinking and the science. Copy-editing practices that discourage and frustrate authors who try to pay serious concern to their prose do not merely make articles less sprightly; they undermine the scientific process.

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12/89

Coming to Terms with Chaotic Systems

The review by James Trefil of *The New Physics* (July 1989, page 67) shows that physicists have been ill served by mathematical education and are somewhat naive about the subject. Trefil writes that when we refer to a system as "unpredictable" in the chaotic sense, we mean that "it is extremely sensitive to initial conditions" so that "the future of the system is, for all practical purposes, unpredictable." But he adds, "We also know that if the initial state is defined with mathematical precision, the system is perfectly predictable."

What is "mathematical precision"? I assume this means zero errors in the initial conditions, the structure of the governing equations and the parameters. What Trefil is getting at is that none of these can be error free in any physical model. In some systems the growth rates of the probable errors of the state "vector" will be exponential or faster; in others they will rise as some fixed power of the time—to consider just some possibilities. So far so good.

But can one have "mathematical precision" in a mathematical model? Trefil assumes this is so, but in fact it is more the exception than the rule. The only calculations that have perfect precision are those restricted to rational numbers. For dynamical models this leaves us difference equations and only the most trivial differential equations. Even the harmonic oscillator, if it has rational coefficients, has an irrational period; so it cannot be extrapolated error free without an infinite computer budget. And if the rational numbers are represented by some floating-point scheme as is used in computer hardware and compilers, the number of perfectly precise models becomes even more circumscribed.

In both theory and fact, rounding

and truncation errors interact with chaotic or random processes in the model equations—often more than errors in initial conditions, because rounding and truncation errors never cease. If Trefil has a numerical integration algorithm that does "predict precise future states" I certainly would like to have a copy of it; I would drop my variable-step-size Runge–Kutta in a minute!

Fortunately there is a messiah whose teachings will lead both mathematicians and physicists away from the sinful practice of the "axiom of omniscience." His name was Errett Bishop, and his gospel may be found in *Constructive Analysis* (Springer-Verlag, New York, 1985), written with Douglas Bridges. Do read this "Good Book" and welcome mathematics back to the sciences after its long infatuation with the ethereal.

FOSTER MORRISON
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7/89

In James Trefil's review of *The New Physics*, edited by Paul Davies, he wondered whether the new frontier of physics called "complexity" is a "garbage classification." "Complexity" is an unfortunately negative name for an exciting frontier of knowledge that some of us call the "kinetics of nonhomogeneous processes." Most things in nature have nonrandom distributions in space and time, and their dynamics are described by nonlinear mathematics. For the general reader, may I recommend chapter 1 of *Kinetics of Nonhomogeneous Processes: A Practical Introduction for Chemists, Biologists, Physicists, and Materials Scientists* (Wiley-Interscience, New York, 1987), edited by myself?

Trefil wrote: "Enormous progress is being made in a large number of related fields, but the fields are not really connected. Perhaps in the future . . . connections will be discovered." This future is already here. The connections are in the *nature* of the dynamics, ranging from cosmology to biology and sociology. One has to consider both deterministic and stochastic models and to understand how they are related.

The first International Conference on Kinetics of Nonhomogeneous Processes was held in Banff, Canada, on 7–13 October 1989. In addition to physics it included biology, chemistry, cosmology, geography, geophysics and materials science. The proceedings will be a special issue of the *Canadian Journal of Physics* in autumn 1990.

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8/89

TREFIL REPLIES: My remarks about imprecise language in the description of chaotic systems were intended as a warning against the very real danger of a misinterpretation of what these systems are by nonscientists, and as an attempt to prevent the descent into mysticism that so often accompanies such misinterpretations. A statement like "Chaotic systems are so sensitive to initial conditions that even the rounding errors that must occur in any real calculation (or the measurement errors that must be made by any real instrument) are enough to make the long-term prediction of the system's behavior impossible" cannot be misinterpreted in this way. It is also, I believe, a more accurate statement of the nature of chaotic systems.

I would argue that Foster Morrison's point falls under the rubric of what I called "unpredictable in practice" as opposed to "unpredictable in principle." While it is true that any conceivable real machine will even-

tually produce different predictions for starting points that are arbitrarily close together, it is also true that you can, in principle, imagine machines whose predictions stay close together for long periods before the inevitable divergence sets in. A machine capable of carrying enough decimal places to keep future predictions within, say, the limits set by the Heisenberg principle for a time longer than the life of the universe would, I think, be considered good enough for most physicists to say that it made "exact" predictions. Even for such a machine, though, we're getting close to arguing about angels on the heads of pins.

I am delighted to hear that the future on the frontier of complexity is already here, but I have to admit that neither the fact that such systems are often nonhomogeneous (as Gordon Freeman suggests) nor that they involve nonlinearities (as I stated in the review) brings me the intellectual satisfaction that accompanies the kind of unification that has recently arisen in elementary-particle physics and cosmology.

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12/89

What Went Unsaid at Physics Chairs Meeting

According to Irwin Goodwin's news story (July 1989, page 62), physics department chairs, meeting in Arlington, Virginia, in May, expressed concern in a letter to designated Presidential science adviser D. Allan Bromley "over the ability of Federal agencies to address adequately [the] important national problem" of "ensuring an adequate supply of doctoral physicists to satisfy our national needs." At the same meeting, Kent Wilson of the NSF's Directorate for Mathematical and Physical Sciences declared: "The overwhelming problem is the physics pipeline.... We have always solved this problem by immigration to this country.... Women and minorities in physics present a more difficult problem. I must admit I don't have a clue as to how to solve the problem. Whatever we've been doing at NSF obviously has not been working."

Judging from Goodwin's listing of conference discussions and resolutions, the physics chairs did not consider the possibility that improving the effectiveness of introductory physics courses might afford major long-term repair of the pipeline that ducts new graduate students into their programs. Perhaps they are lulled or even exulted by the bounteous current outpouring of the pipelines from overseas, which supplied 41% of American physics graduate students[1] in 1988. But foreign pipeline problems were earlier delineated by the Conference Board of Mathematical Sciences[2]:

▷ Many foreign-born students leave the United States after they obtain their degrees.

▷ The increasing number of foreign graduate students has led to language problems in the classroom. We are "hostage to foreign policy" in securing bright students from such places as China.

▷ Severe shortages in American university staffing are anticipated in the 1990s.

The Conference Board committee cautioned that the problem was not too many foreigners, but too few Americans.

Major long-term repair of the pipeline for American science students, including women and minorities,