SCIENCE

15 April 1977
Volume 196, No. 4287
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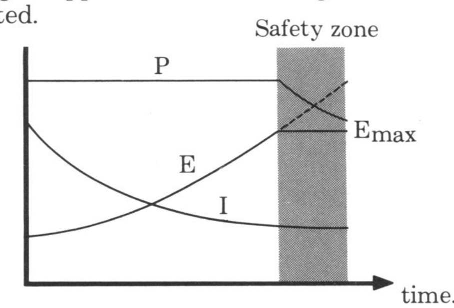

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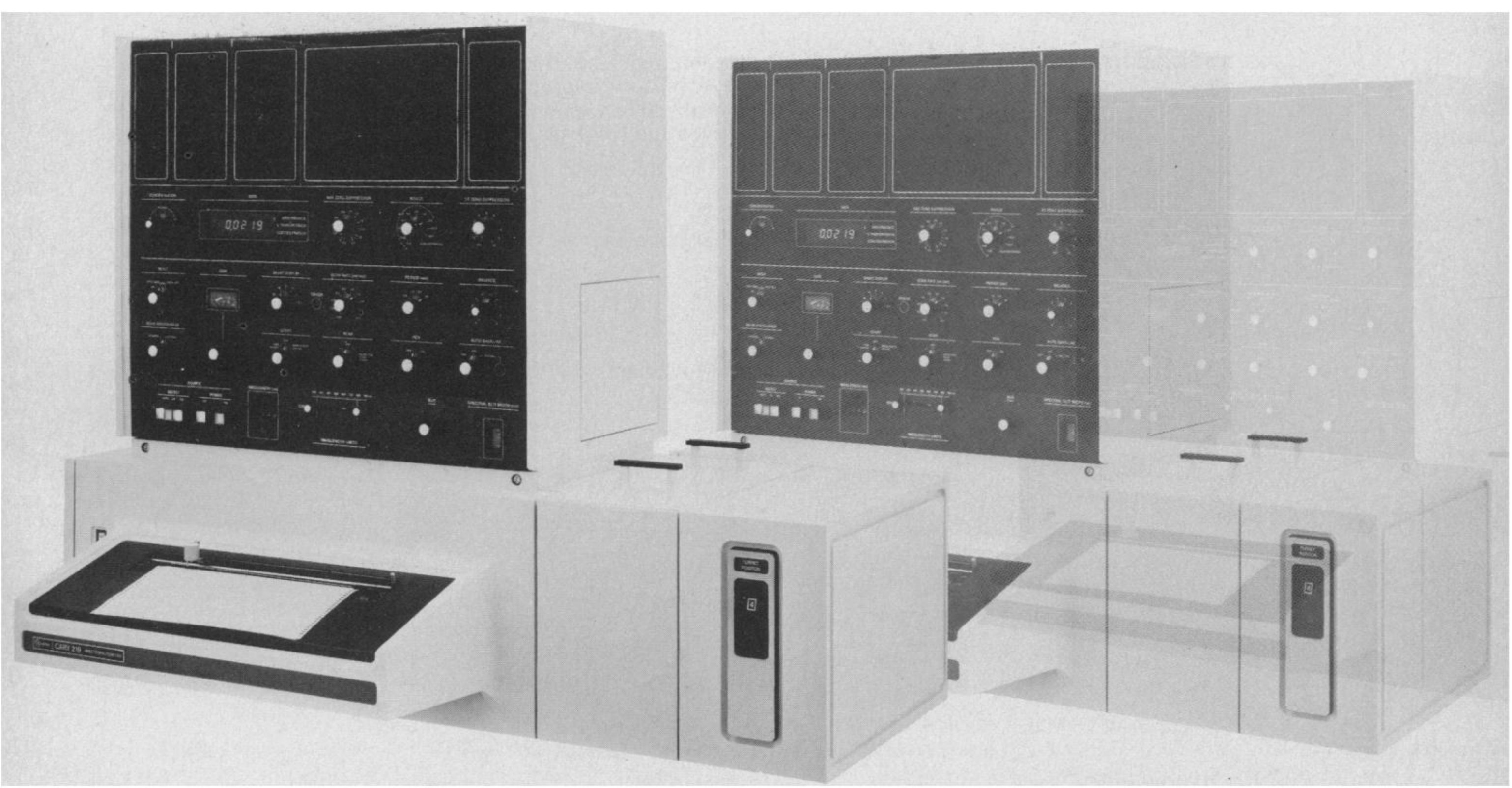

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Volume 196, No. 4287

SCIENCE

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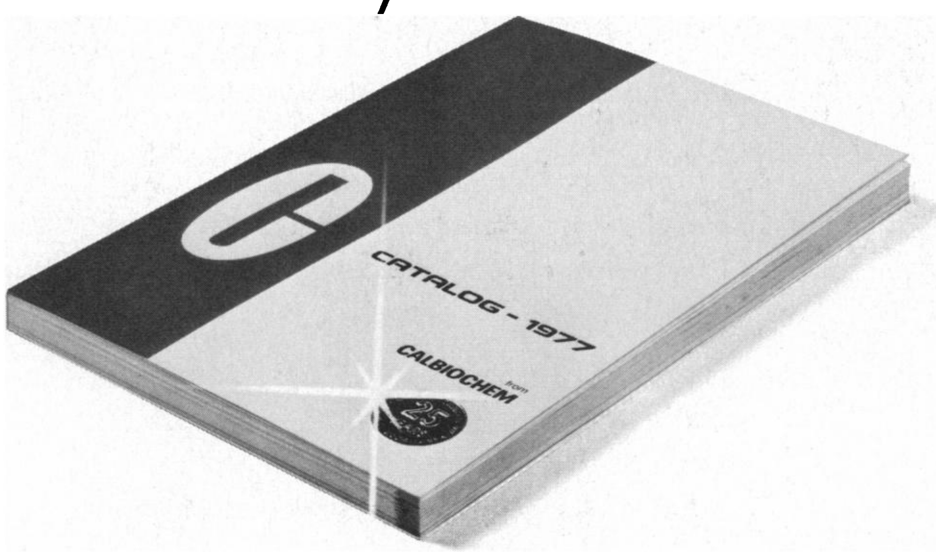
COVER

A male abalone (18 centimeters in length) is shown releasing sperm in response to hydrogen peroxide. Sperm are broadcast in jets of water expelled through respiratory pores in the shell. As many as 10^{12} sperm may be released over a period of 30 minutes in a single spawning. See page 298. [Larry Friesen, Marine Science Institute, University of California, Santa Barbara]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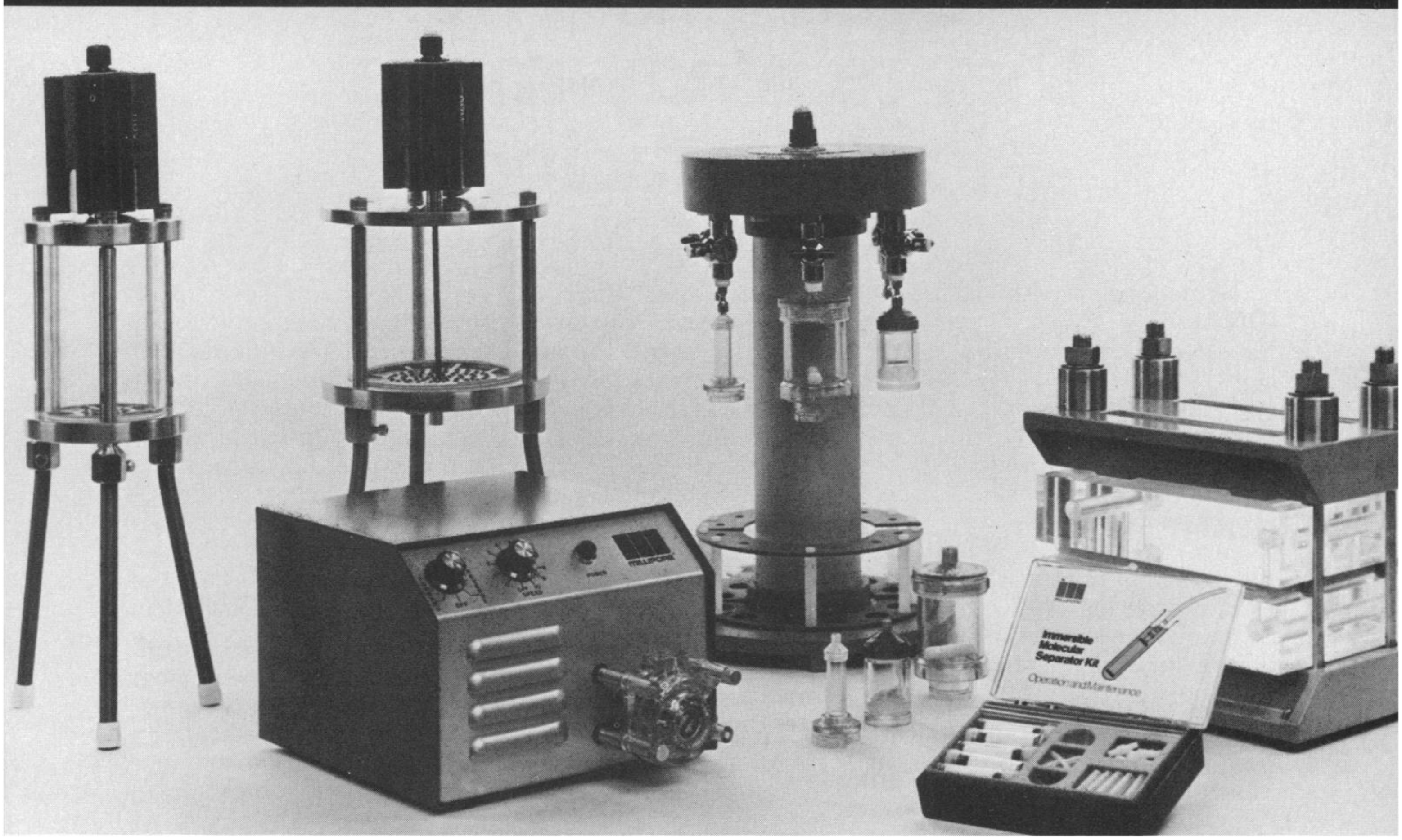

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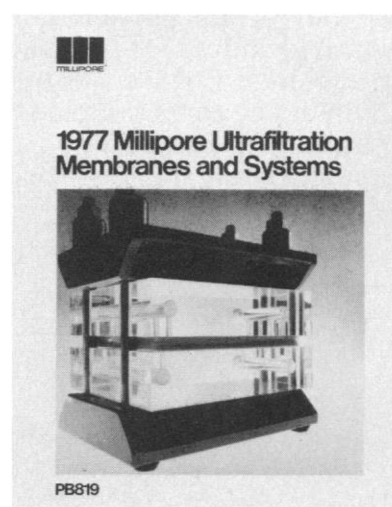

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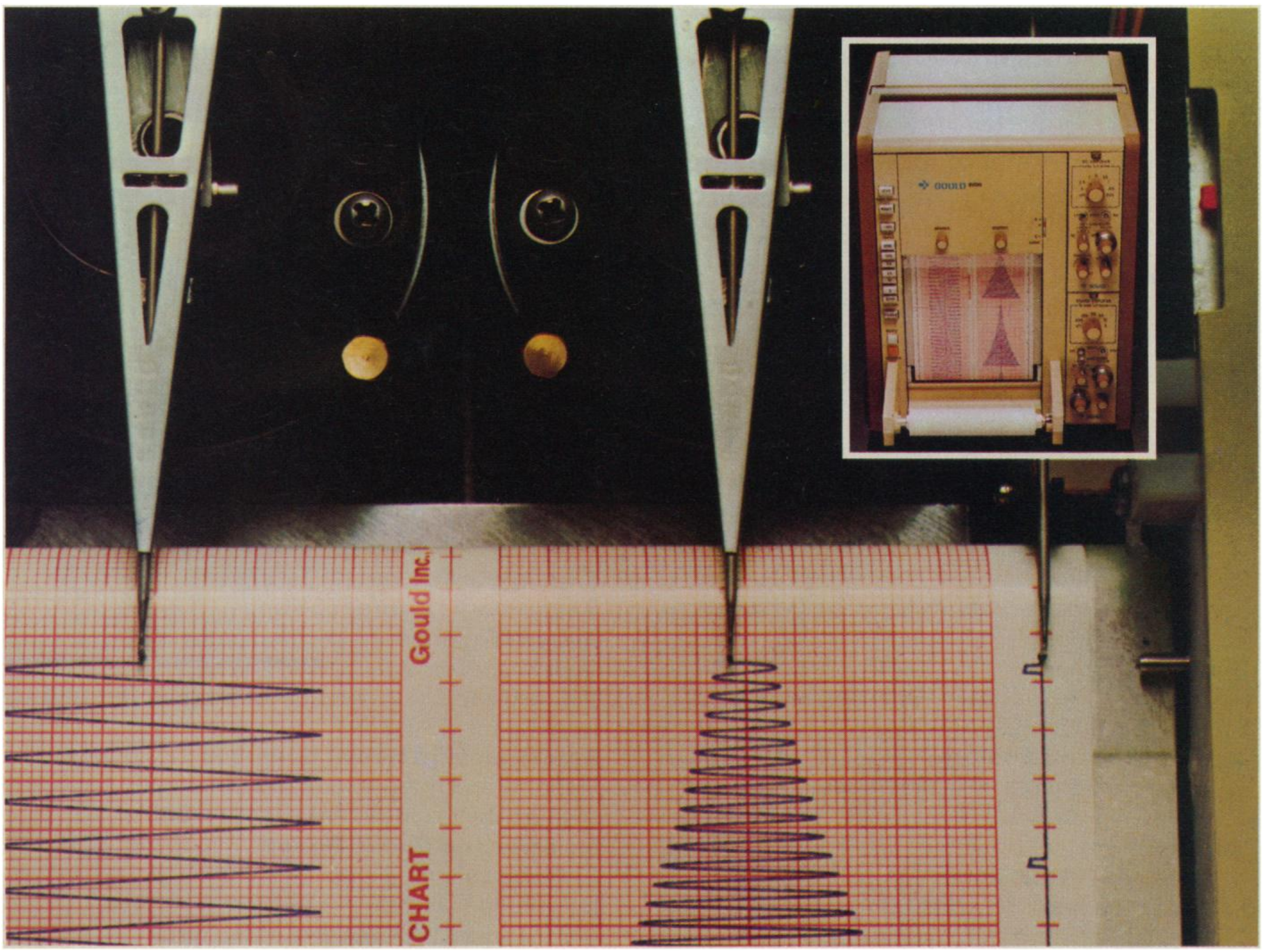

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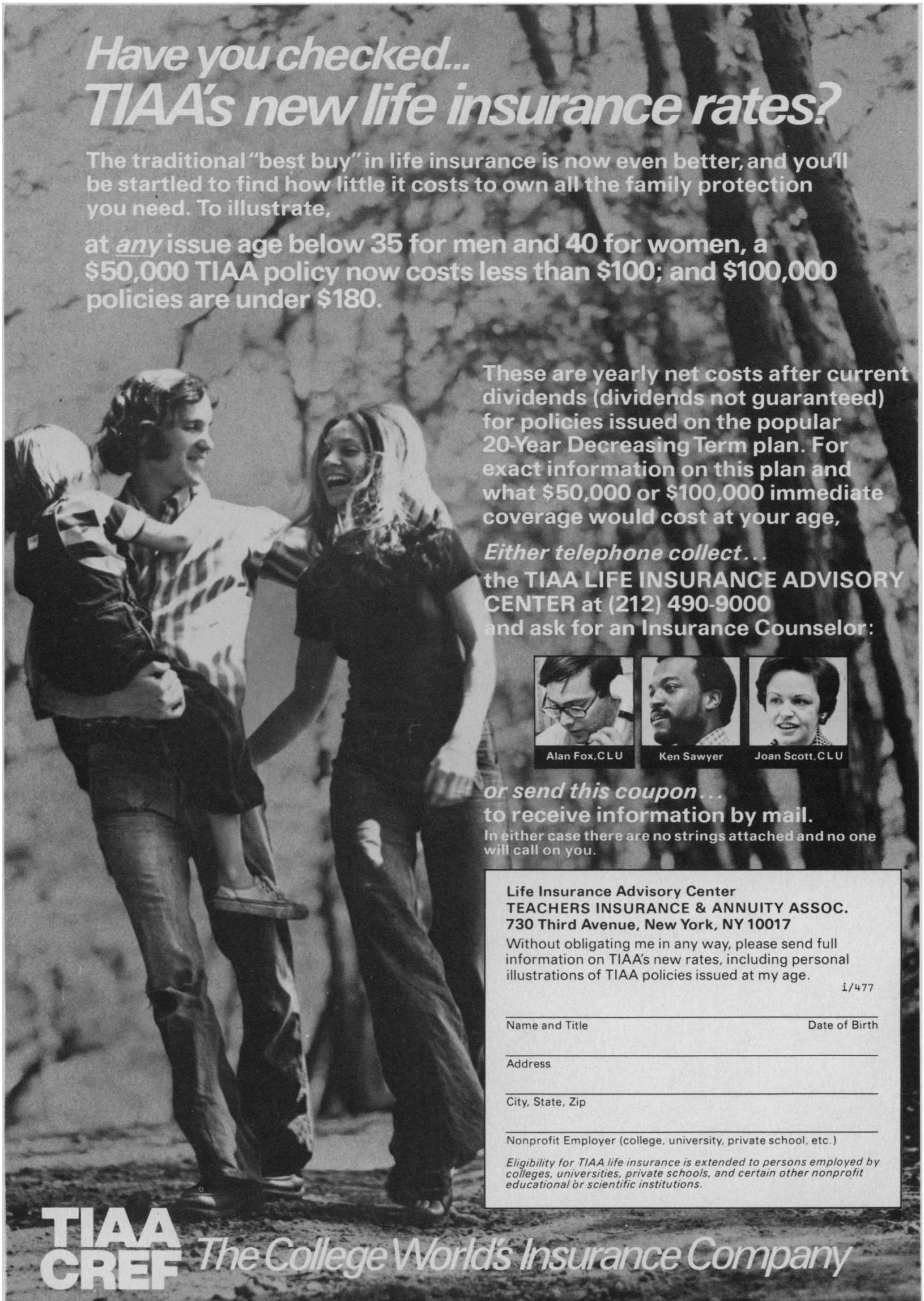
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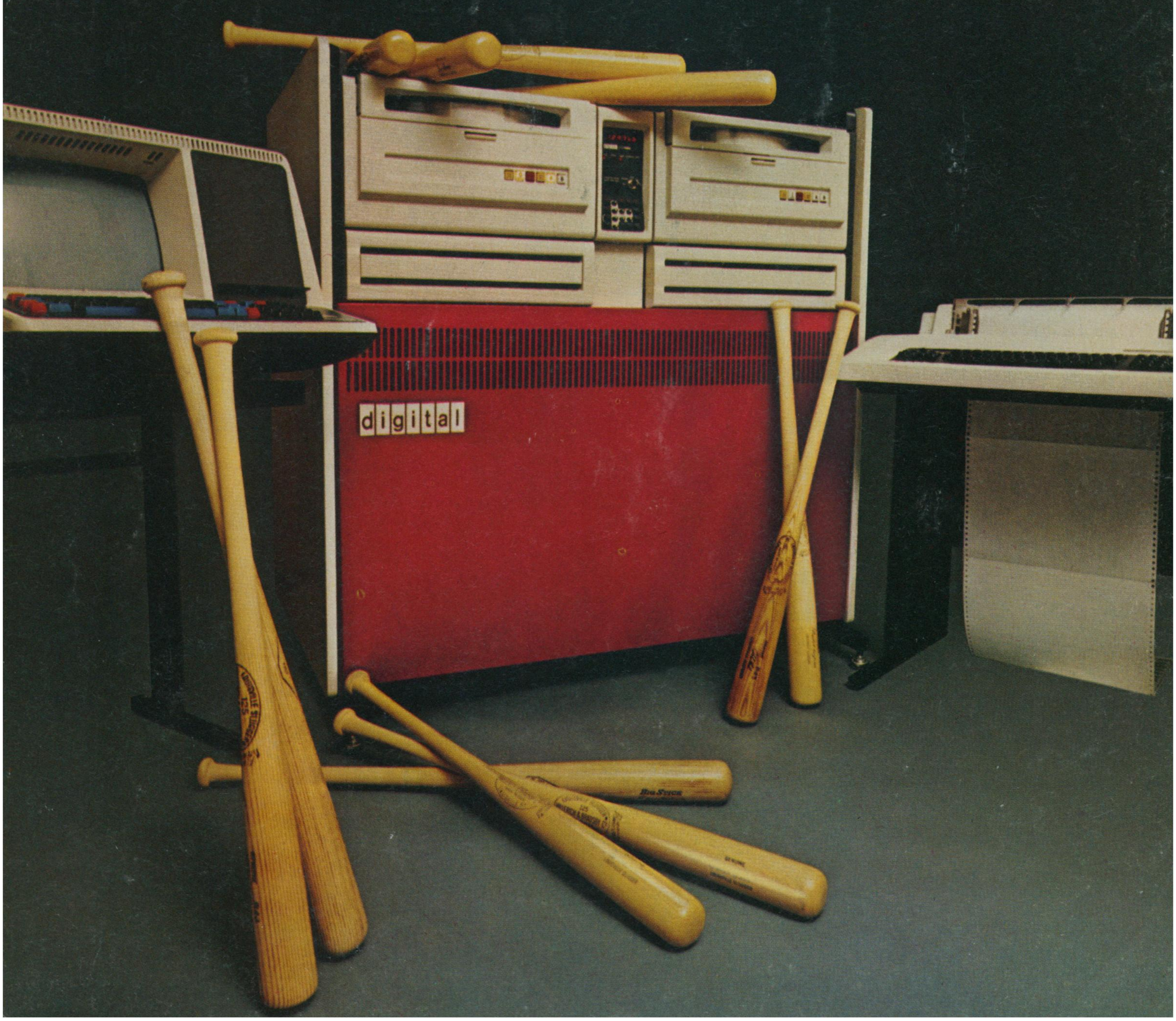

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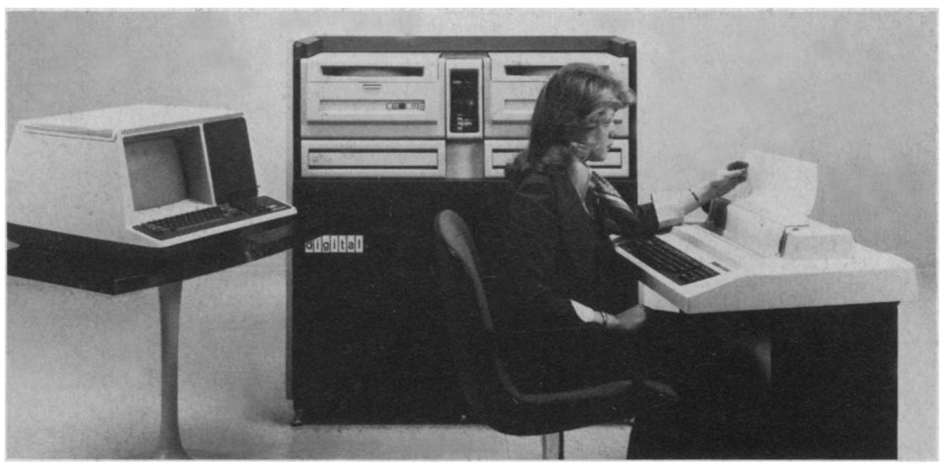

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LETTERS

Environmental Task Force Report

Luther Carter's article (News and Comment, 25 Feb., p. 764), concerning the Rockefeller Brothers Fund task force report "The Unfinished Agenda" may have left an erroneous impression as to the ascribed responsibility for the content and conclusions contained therein. The individuals on the task force were not representing their organizations, only themselves. It was made perfectly clear from the beginning that any attempt to have organizational approval of such a broad range of environmental issues would be an impossible task.

Also, the conclusions reached were adopted by consensus and did not require unanimity of opinion. I personally did not agree with at least one of the conclusions, namely, that atomic power should be phased out over the next 10 years. I am not ready to concede that atomic power should be eliminated at the present time as one of our energy options. The best scientists in this field agree we have at least two very serious problems—how to satisfactorily dispose of radioactive wastes and the security and proliferation implications of a plutonium society. If these problems can be solved, it would be in the national interest to take another look at whether the nuclear option of meeting some of our energy needs would be better than burning an equivalent amount of coal in our effort to meet both energy production and environmental goals.

THOMAS L. KIMBALL

National Wildlife Federation, 1412 16th Street, NW, Washington, D.C. 20036

Antagonisms and Controversies

In the third of the series of articles on multiple sclerosis (Research News, 11 Mar., p. 969), Thomas H. Maugh does a disservice in bringing out the personality conflicts between some individuals in basic protein research. His unfortunate remarks pointing out the antagonisms and controversies in the field do nothing to illuminate the problem and even detract from the scientific value of the article. Every field has its rivals, and the publication of such unattractive sidelights can only add to the bad press the scientific community is already receiving.

MARION E. SMITH

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Energy Analysis

As an energy analyst, I welcome any attempt by an economist to bridge the gap between us and would agree with Huettner (9 Apr. 1976, p. 101) that energy analysis is plagued by many of the problems that confront economists. One of these is the great difficulty each profession has in understanding the other's methods. Huettner does not, for example, differentiate between energy analysis and net energy analysis, and thereby he leads himself into some awkward corners. Net energy analysis does not use "net energy accounting to value inputs and outputs"; it uses gross energy accounting. Nor does Huettner distinguish between the two quite distinct schools of energy analysis: that of Odum, to which Gilliland (*1*) appears to be an adherent, and that which, broadly speaking, supports the conventions from the workshop on energy analysis methodology convened by the International Federation of Institutes for Advanced Study (IFIAS) in Sweden in 1974 (*2*). The former school places an energy value on the sun and on labor. The latter group regards the sun as a free good and argues that, since the objective of the economy is to furnish people with their needs, to count the energy for life support of labor is to double-count. This group treats energy analysis as the "determination of the energy [resource] sequestered in the process of making a good or service within the framework of an agreed set of conventions" (*2*), for example, establishing the amount of energy resource or resources that had to be extracted from the earth in order to deliver one can of beer to Huettner's house on 9 April 1976. The answer is specific with respect to technology, location, and time. The IFIAS workshop method enables numbers to be compared, no more. If I inform an expectant world that the energy resource requirement of single cell protein made in Japan in 1973 is 187 megajoules per kilogram, assuming the use of a particular technology and energy substrate, I can hardly expect resounding cheers. The number by itself is of little value, no more than if I had informed someone who had no knowledge of the rate of exchange between the yen and the dollar that its cost in Japan was 2300 yen per kilogram. The point is that we do not yet completely understand the relation between money (a market measure of all resources) and energy. Energy analysis has made some progress, which can be identified by comments on Huettner's article.

First I take exception to the article's title, "Net energy analysis: An economic assessment." One cannot make an economic assessment of energy analysis, which is a method of study not an input, any more than one can make an energy analysis of economics. But, as Huettner notes, one can compare the conclusions reached by both. Take the case of shale: a 1972 report by the National Petroleum Council (*3*) concluded that shale would be economically viable when the price of crude oil reached $6 per barrel. Since then, the price of crude oil has doubled, and shale is still not "economic." Even the most neutral observer would probably conclude that something was lacking in the methods of economic analysis used. Huettner has alluded to these lacks when he comments, "economists have generally made various adjustments to market values and even estimated market values when no markets exist." An energy analyst offers a more precise alternative. He would compare the net energy of refined oil from crude and shale and conclude that in a free market shale oil would never be economic until oil from crude rises to a gross energy requirement per barrel close to that of shale oil. Such an event may be 30 or more years away. Of course, the government may distort the market by taxes and subsidies and make the production of oil from shale a worthwhile activity in the national interest, but that is the politics of energy independence.

Huettner asserts that energy analysts argue that energy requirements remain fixed, and quite rightly scorns this. In fact, what energy analysts argue is that, given the extent of the data base, knowledge of technology, and the ineluctable fact that any transition requires a minimum energy requirement dictated by the laws of thermodynamics, one can more precisely estimate the future energy requirement to manufacture a good than one can estimate its future price. Price estimation is bedeviled by the uncertainty of what discount rate to apply and how to forecast inflation.

Huettner at one point postulates a parameter α_i, the "competitive, market-determined kilocalories of energy used directly and indirectly to produce one unit of input *i*." I cannot recall a single case of the market determining the energy inputs. These inputs are a reasonably close function of the technology of production and location of plant. Although Huettner goes on to argue that his analysis proves that energy analysts are embracing an energy theory of value, outside the Odum school I know of no one who does so. Perhaps this lopsided view of energy analysis accounts for his conviction that energy content pricing is a logical consequence of energy analysis. The

Energy analysts are currently using a variety of concepts to assess energy problems and rank alternatives. These concepts include techniques which might be loosely labeled as net energy analysis, gross energy analysis, entropy analysis, and even economic analysis. While there are various gradations within each of these categories, it is clear that a fundamental conceptual difference exists between analysts using economic measures, such as market prices, to value inputs and outputs, and analysts using physical measures, such as energy content. While it is clear from the comments of Odum and Slesser that their energy analyses differ, my concern is not with their differences but with their similarities.

My point continues to be that energy analysts employing economic principles will generally reach different conclusions from energy analysts using noneconomic principles and that these differences will remain even if all markets are free or perfect. Claims that one method cuts through confusion or forecasts impending change faster or has more normative content could probably not be proved by proponents of any method, since every discipline is rife with examples of poor research to be exploited by the opposition.

Under these conditions, I believe it makes more sense to examine the basic assumptions and logic of a discipline in an effort to determine where it will take us if we let it guide our decisions. It is on this basis that I argue that energy analysis guided by noneconomic principles constitutes an energy theory of value. While there is general agreement that the Odum "school" embraces an energy theory of value, Slesser maintains that his "school" does not. Yet if one "values" inputs and outputs in energy terms, ranks or compares alternatives in energy terms, and then acts on this information, I believe that an old adage applies, "When in Italy, all roads lead to Rome."

But what is wrong with units of energy and right with dollars? Isn't it true that one can select any good or product in the economy as the numeraire? Couldn't the government issue homogeneous, 1-Btu lumps of coal with George Washington's picture on them instead of printing dollar bills? Wouldn't all inputs and outputs then be valued in Btu's and wouldn't profit or welfare maximization be the same as energy maximization—an energy theory of value? How can the answers to these questions be yes except for the last one?

The question is really one of what or how one determines value. If the energy content of a good is 3 Btu's, would it always trade for three lumps of coal or even tend toward a value of three lumps of coal? Would there be coal inflation and what is the appropriate discount rate in terms of Btu's? Would supply and demand forces determine values or would energy content? Would confusion end, impending change be forecasted faster, and normative content be increased?

DAVID A. HUETTNER
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Origin of Roman "Royal Purple"

I found George D. Ruggieri's article "Drugs from the sea" (29 Oct. 1976, p. 491) most interesting, especially since I have been working with one drug he mentions (tetrodotoxin) for the past 2 years. However, I believe he errs when he states that ". . . Roman ladies . . . bedecked themselves in beautiful gowns dyed purple with a seaweed extract. . . ." The famous "royal purple" of classical times was actually isolated from mollusks (*Purpura* and *Murex*). Had the Roman ladies known explicitly of its origins and manufacture, they would probably have been repelled even more than by the seaweed. It has been suggested (*1*) that the unenviable reputation that the streets of Tyre possessed for being foul-smelling may have come from the decomposing bodies of the mollusks used in the preparation of royal purple.

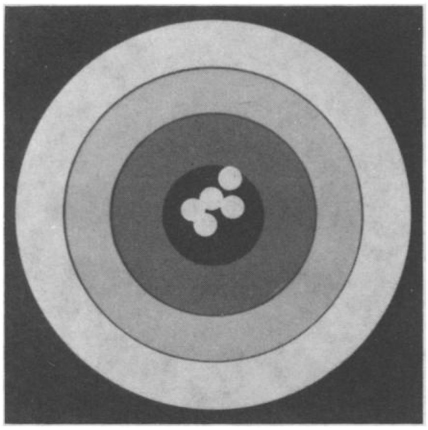
JAMES E. HUHEEY
Department of Chemistry, University of Maryland, College Park 20742

Reference

1. C. Singer, E. J. Holmyard, A. R. Hall, Eds., *A History of Technology* (Oxford Univ. Press, New York, 1954), vol. 1, pp. 247–248.

There is certainly no doubt that the "royal purple" of classical times was of molluscan origin. The original reference, "as wool dyed in seaweed pleases one almost as much as purple" ("ut lana tincta fuco citra purpuras placet"), is from Pliny the Elder's *Natural History*. I'm convinced from the above quote that Pliny, too, was aware of "royal purple" (from Mollusca). But as I indicated elsewhere in my article, some of Pliny's remarks were often fanciful and this perhaps may be another example.

GEORGE D. RUGGIERI
New York Aquarium and *Osborn Laboratories of Marine Sciences, Brooklyn, New York 11224*



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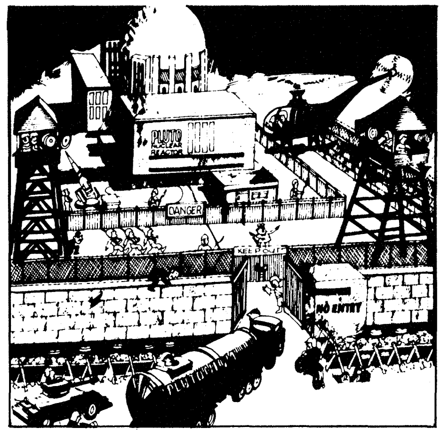

How to live with Plutonium.

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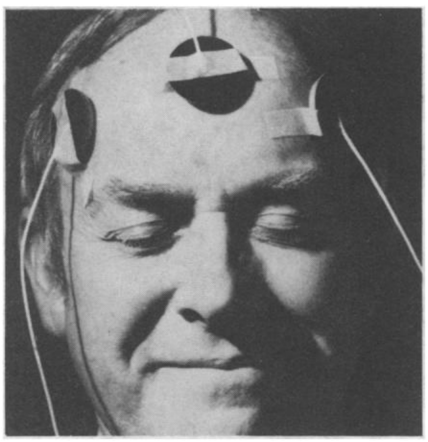

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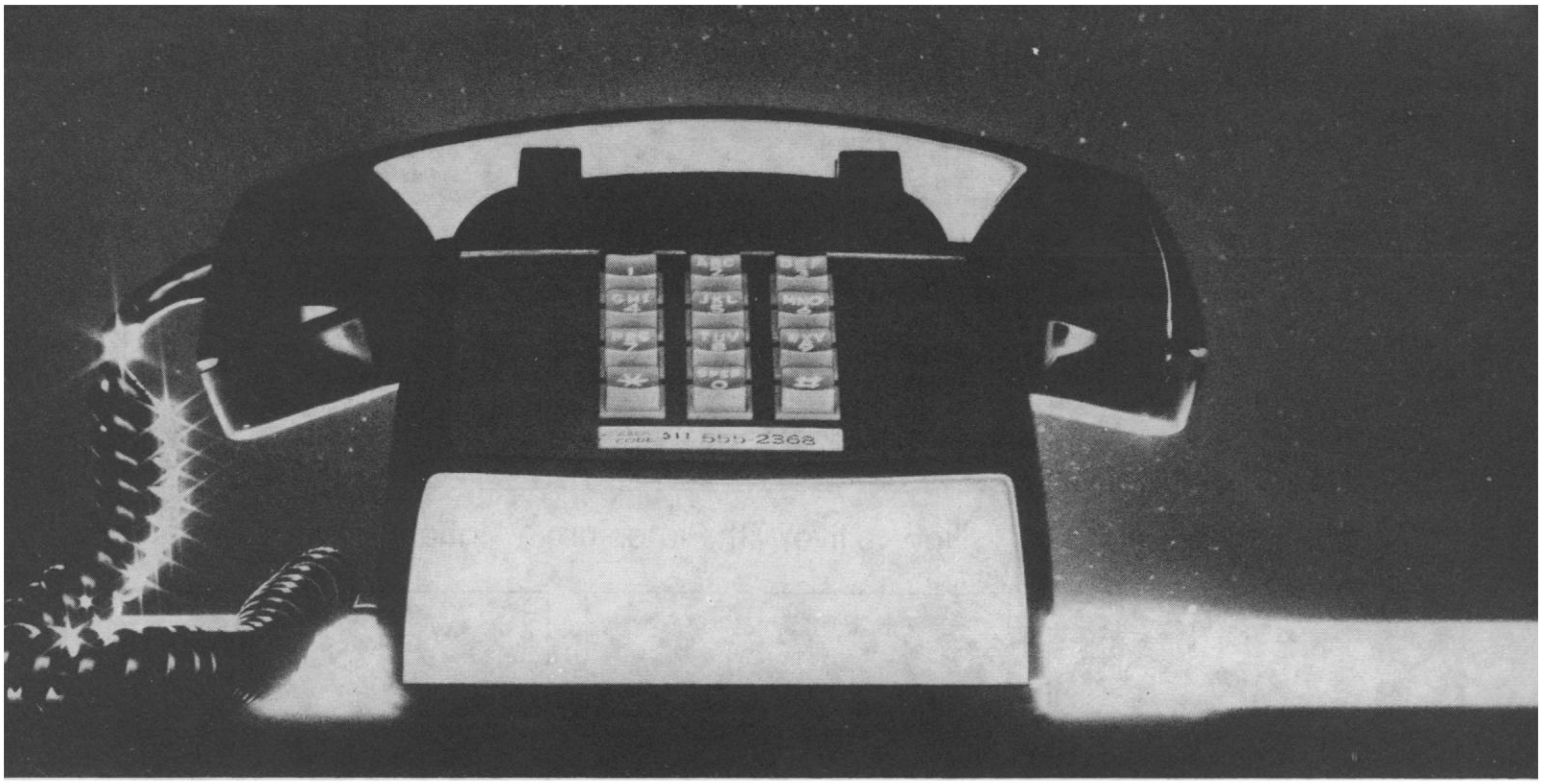

15 April 1977, Volume 196, Number 4287

SCIENCE

The Denver Meeting: Afterthoughts

With the Proceedings Issue of *Science*, the recent Annual Meeting in Denver belongs to the history of the AAAS. At all such meetings, the symposium arrangers and participants, together with the local hosts, control the indices of satisfaction. Judging from the feedback, the Denver meeting seems to have been a thoroughly enjoyable affair, touched in no small degree by the warmth and friendliness of the community. To Maurice Mitchell and John McKinney, who chaired the local committee, and to their volunteer associates the AAAS owes much. As for the symposium arrangers and panelists, on whom the substantive burden of the meeting fell, any expression of thanks falls short of adequacy.

The predicament of Arthur Herschman, who must plan each annual meeting, is awesome. He must make hard choices if the meeting is to be representative of the range and diversity of contemporary science and technology, reflect a sensible balance of subjects and treatment, and convince members that they should attend. Somehow, justice is done and not too many friends alienated, and the suspicion here is that Herschman has found a way to clone Monte Carlo methods with psychology.

The function of an annual meeting is not to be taken for granted. In the case of the AAAS meeting, it has evolved as a process of interdisciplinary communication in science and technology and an instrument for public understanding. But this focus on the horizontal axis can be carried too far, at the expense of straightforward discussion of main directions in scientific discovery. In future meetings, this degree of tilt will be more carefully calibrated.

What might have been just a pleasant and useful week in Denver turned into something else at the midweek meeting of the AAAS Council. A classic AAAS family fight erupted when it was discovered that a controversial psychologist had been recommended for election as a Fellow. What needs to be said, and said firmly, is that scientific disputes are best handled through remedies that exist within the appropriate professional society, rather than by turning the AAAS Council into an inexpert jury. It must be added that the vote to accept one controversial scientist as a Fellow in no way places the imprimatur of the AAAS on theories that many members find unacceptable. Until now, the AAAS has had room for every shade and hue of opinion and has not presumed to judge who is right or wrong. It should beware of moral judgments taken when tempers are hot: they are a smoking gun that science does not need.

Aside from this, the Denver meeting met all expectations. More than 4000 attended, not counting throngs of Colorado citizens who came to the public lectures. Once again, good preparatory work enabled many physically handicapped scientists to participate—a marked change for the better. Women scientists and other minorities including Native Americans left their mark on the meeting. Foreign scientists, young and old, contributed to the proceedings generously, and prominent members of the house of science found time to rap with high school students. The Southwestern and Rocky Mountain Division added a fresh element to the meeting by sponsoring contributed papers and poster sessions.

Time passes. This was the 143rd national meeting of the AAAS, and what we saw was good. Now the work begins so that when we meet in Washington, D.C., the tools of science and technology will fit the work of the nation. By this time next year the Carter Administration will be more of a known quantity and its approaches to national and international initiatives in science, technology, and human affairs should provide a bearing on future trends and issues. If the Denver meeting shed light on "Science and Change: Hopes and Dilemmas," the Washington meeting can be a catalyst for decisions.

—WILLIAM D. CAREY

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theory make criticism difficult. Advocates claim applications to so many fields that potential critics are overwhelmed. And they say things such as, "The method has the potential for describing the evolution of forms in all aspects of nature, and hence it embodies a theory of great generality." People may be dubious of such statements, but the audacity of the claims tends to make them think that something must be there. In truth, says Keller, "there have been zero concrete accomplishments."

The catastrophe theorists also hinder their critics by the way they phrase their claims, Sussmann points out. They deal in "ifs," "coulds," and "maybes." For example, Zeeman writes that "catastrophe theory could thus provide a mathematical language for the hitherto 'inexact' sciences." To counter this claim, the critic must show that catastrophe theory could not provide such a language—a task that is extremely difficult, if not impossible. By such means, catastrophe theorists put the burden of proof on their critics.

Another difficulty in criticizing applications of catastrophe theory, according to Sussmann, is the vagueness of the theorems and proofs. When they do not define terms, do not specifically prove claims, and are unclear about what they are actually doing, catastrophe theorists set up a smoke screen. They can later claim that their critics do not really understand their work.

Guckenheimer fears that the current criticism of catastrophe theory models may force mathematicians to divide into camps and may result in a loss of the content and insight that might be gained from the mathematics behind catastrophe theory. He says that "There is a real possibility that catastrophe theory will blossom into a discipline unto itself under Zeeman's charismatic leadership. Every imagined discontinuity will be fit by a cusp surface with the proper slapdash ad hoc assumptions. If this happens, the important features of catastrophe theory may well get lost."

Keller has a somewhat different opinion of the possible effects of the current criticism. He feels that Sussmann is doing a service to society by pointing out the problems with these models since "overblown claims about the possibility of doing something with mathematics have a whiplash effect. Afterwards people think mathematics cannot be usefully applied." Although it is too late to nip the claims for catastrophe theory in the bud, Keller and others hope that the catastrophe theory craze will now began to wane.—GINA BARI KOLATA

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(*Continued from page 290*)

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