

FIG. 225

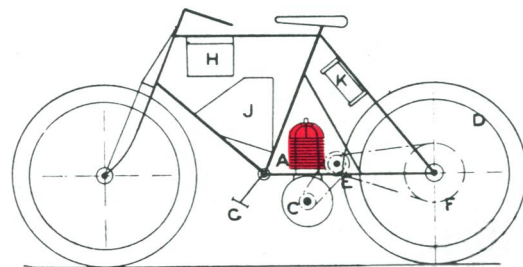


FIG. 226

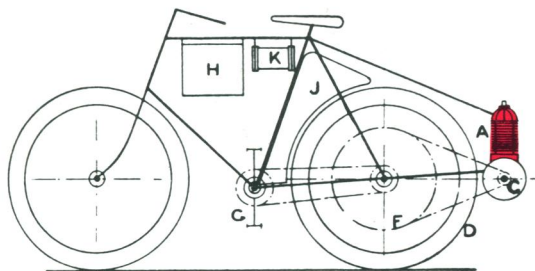


FIG. 227

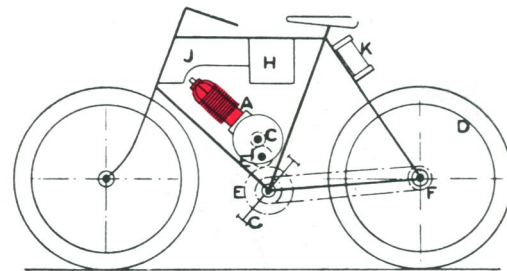


FIG. 228

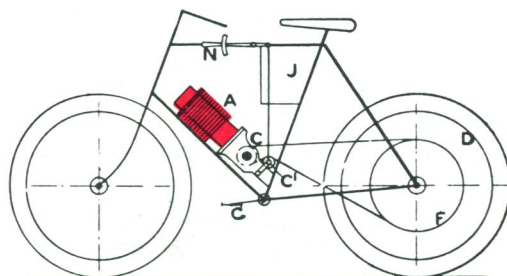


FIG. 229

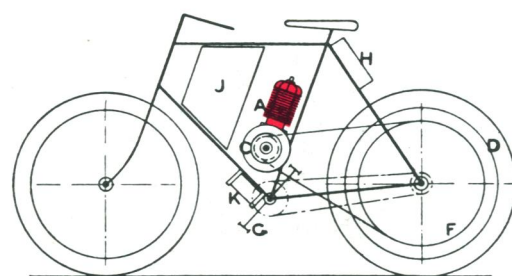


FIG. 230

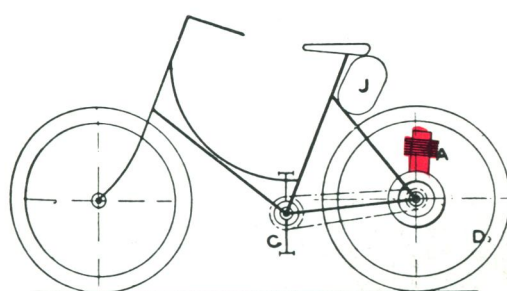


FIG. 231

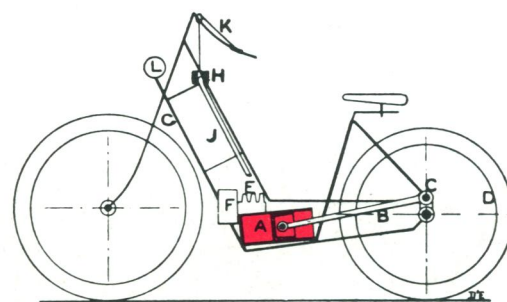


FIG. 224

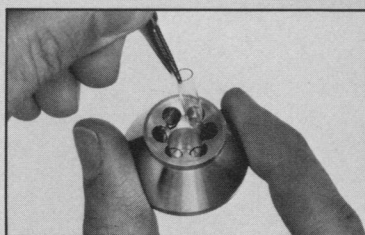
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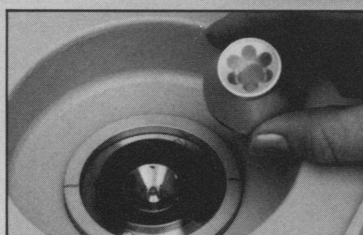
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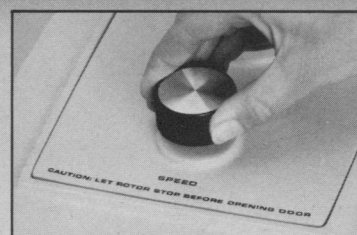
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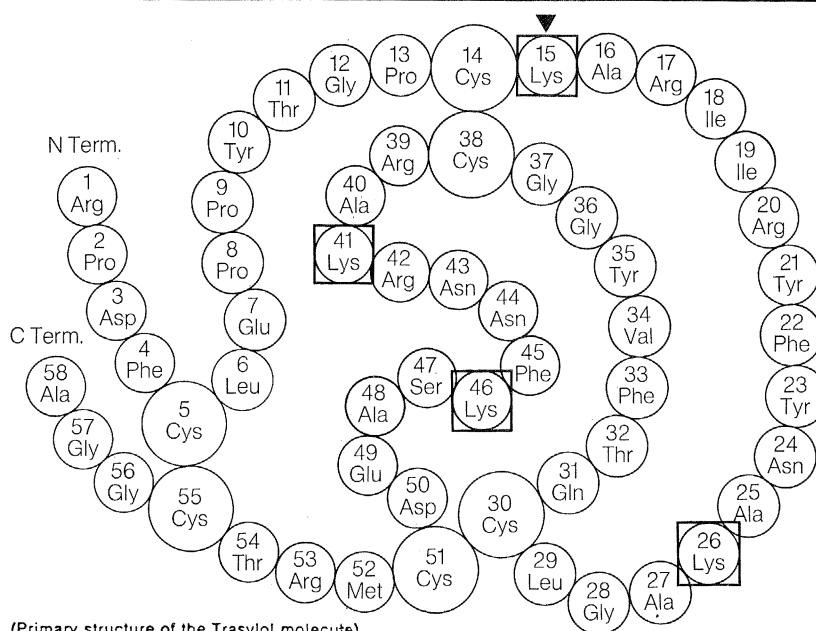
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2) A. M. Eisentraut, N. Whissen and R. H. Unger: Incubation Damage in the Radioimmunoassay for Human Plasma Glucagon and its Prevention with "Trasylol". Amer. J. med. Sci. 255, 137-142 (1968).

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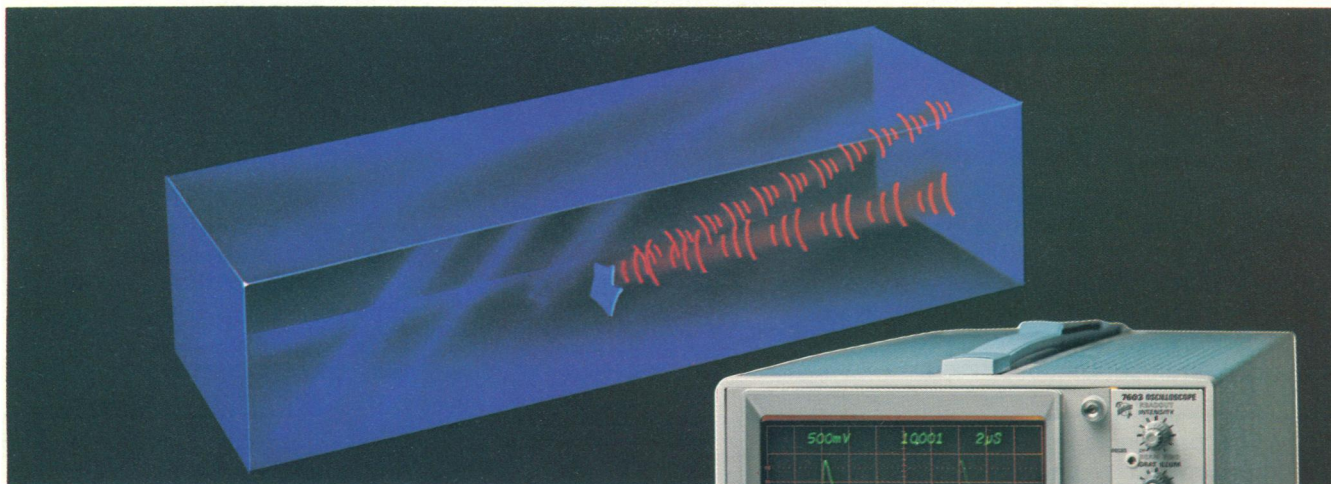
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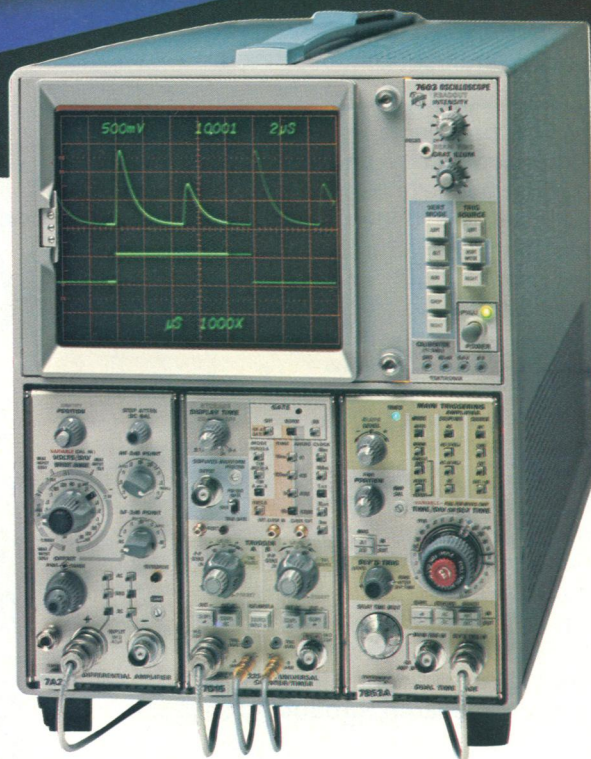
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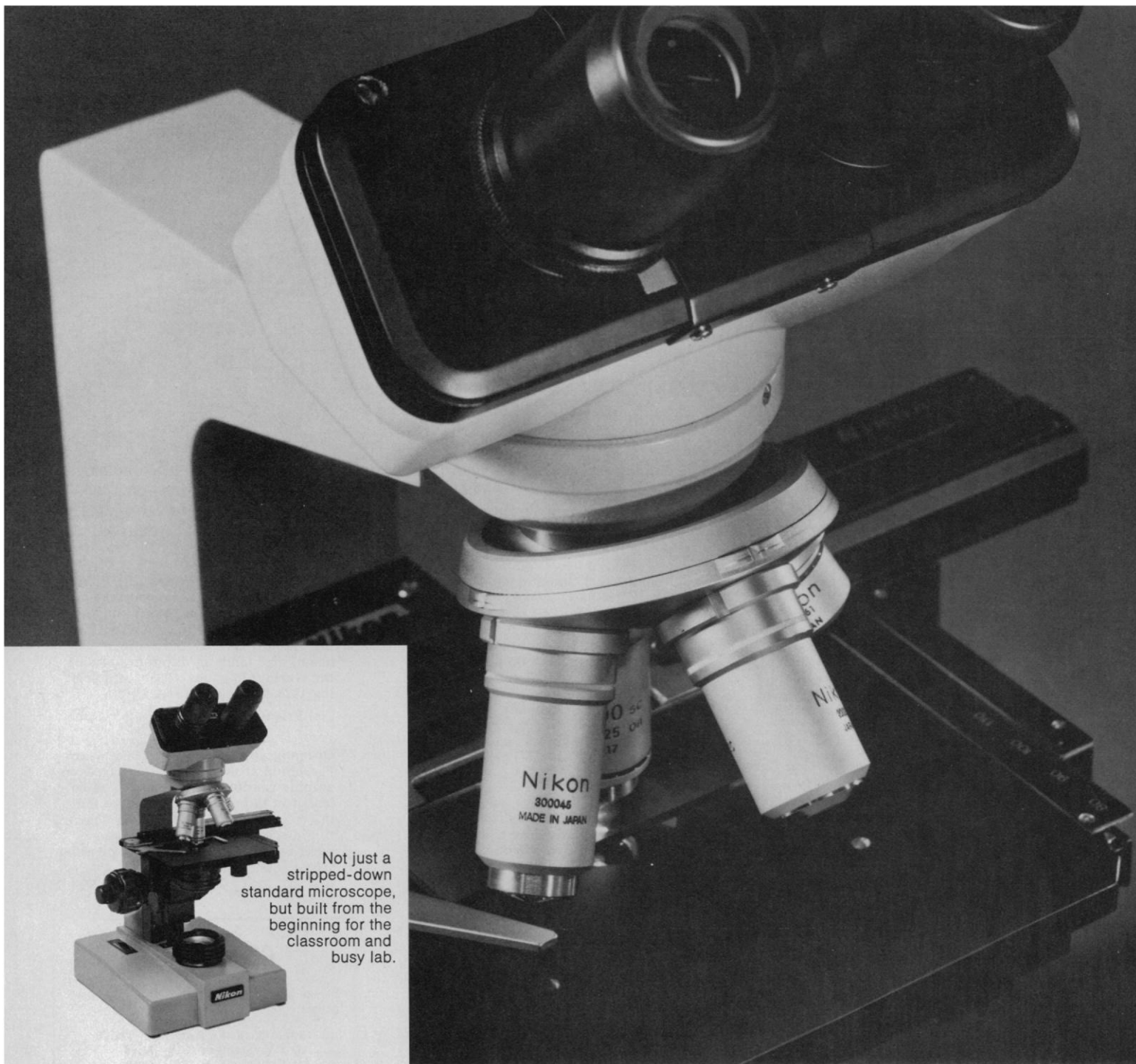
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LETTERS

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.

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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 non-solar 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).

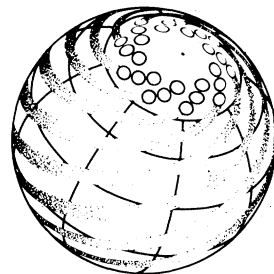
Suppose that somehow U.S. animal feed grain programs were suspended (with the exception of exported feed grains). The only animal products consumed would then be (i) imported meat and animal products and (ii) meat and other products from animals raised on hay, silage, and grazing ground. At current rates 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.

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How Fares Basic Science?

When the elders gather to assess the spirit and substance of science, apprehension is invariably expressed about the state of basic research. Thus, it was only normal for the recent summer meeting of the Committee of Scientific Society Presidents to vote that "basic science is in trouble."

This is a useful, and suitably gloomy, battle cry to raise on the eve of the annual jousting exercise of budget-making. Unfortunately, the quality of the supporting evidence leaves much to be desired. Is basic science in grave trouble, in significant trouble, or in *some* trouble? Is it equally in trouble in government and in industry—or is there a great difference: a tale of two cities? What are the properties of the trouble—financial, political, institutional, attitudinal, or managerial? How much of the trouble is self-inflicted? It would be helpful to have answers. Lamentation is not enough, and basic science is not homogeneous.

As has been said before on this page, basic science long ago drifted amiably into the arms of government at the price of those checks and balances which go with pluralistic support. It should not come as a surprise to learn that the marginal industrial and foundation dollar has been driven out of the picture. It has also been noted that although government has been a very good friend of basic science, everything considered, it provides an erratic and uncertain environment for long-term research because it has not yet come round to treating basic science as *investment*, in contrast to year-to-year expense. This library of familiar music is likely to play for some time.

What becomes important now is the question of productivity in basic science. Instead of measuring "trouble" strictly in terms of rising, falling, or steady-state budgets, we need to ask different questions and apply different tests. It may very well be that built-in inefficiencies and distractions are sapping the vitality of the research process and that the dollars allocated to basic science no longer tell us much about the true levels of research effort.

Which factors operate to devalue the basic science dollar? The indirect cost surcharge on research grants is a familiar kind of burden, but not the only one. Countless man-months are subtracted from research effort in order to satisfy the routines of renewal application, accounting, reporting, and compliance with the rising tide of governmental and institutional regulations. Obsolescence of instruments and equipment, together with queuing delays, works to drop the blood count of research. The torrent of what passes for scientific and technical information presents obstacles through which investigators must blast or tunnel their way. This is the enervating dimension of basic science as it must be practiced now, and little of it meets the unwary eye. Would it overreach by too much to say that, compared with the environment of a decade ago, the research dollar has been devalued, in terms of productivity, by one-third?

With zero-based budgeting coming on strong, and a balanced federal budget being scripted for 1981, prospects for growth of support for basic science are problematic. The normal revenue growth under present tax laws will be claimed by defense, welfare reform, energy development, and perhaps a national health insurance program. Three-quarters of the government's budget is already relatively uncontrollable, leaving a very narrow area for discretionary expenditure, and it is in this cramped and bitterly competitive corner that basic science is to be found. Given all this, there are strong incentives for isolating those influences which undercut the productivity of basic research at existing support levels.

Basic science has known lean times. But a combination of austerity, industrial disinclination, continuing inflation, and falling productivity could prove to be too much. If the science adviser, the National Science Foundation, and the scientific community would take a close look at the issue of productivity, a brighter light might be shed on the sources of "trouble" in basic science.—WILLIAM D. CAREY

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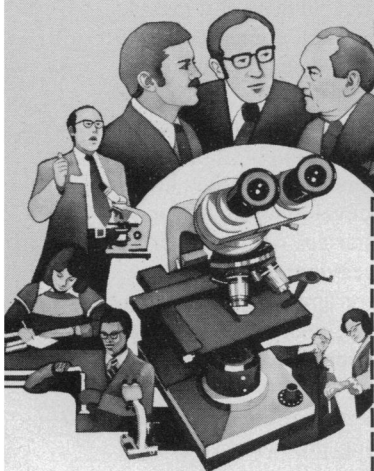
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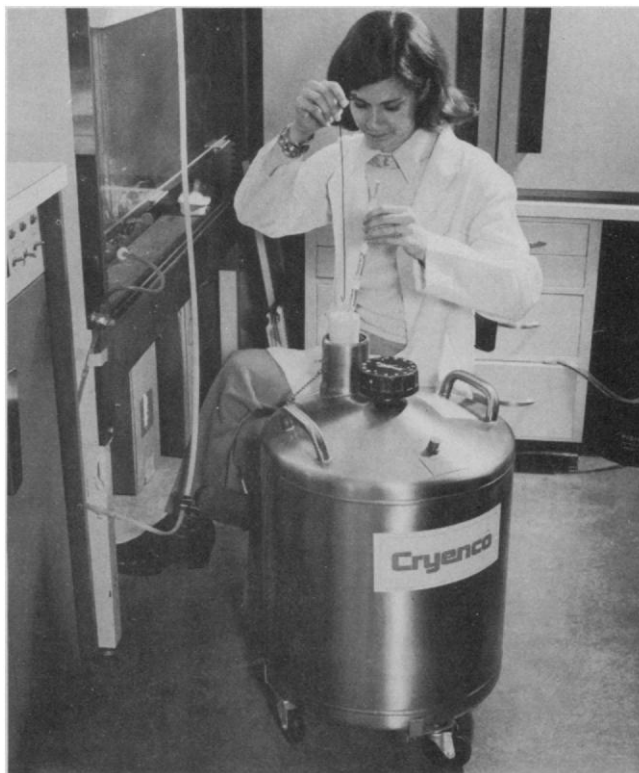


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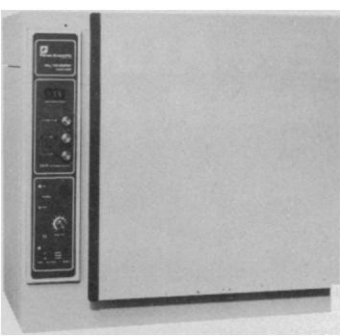
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