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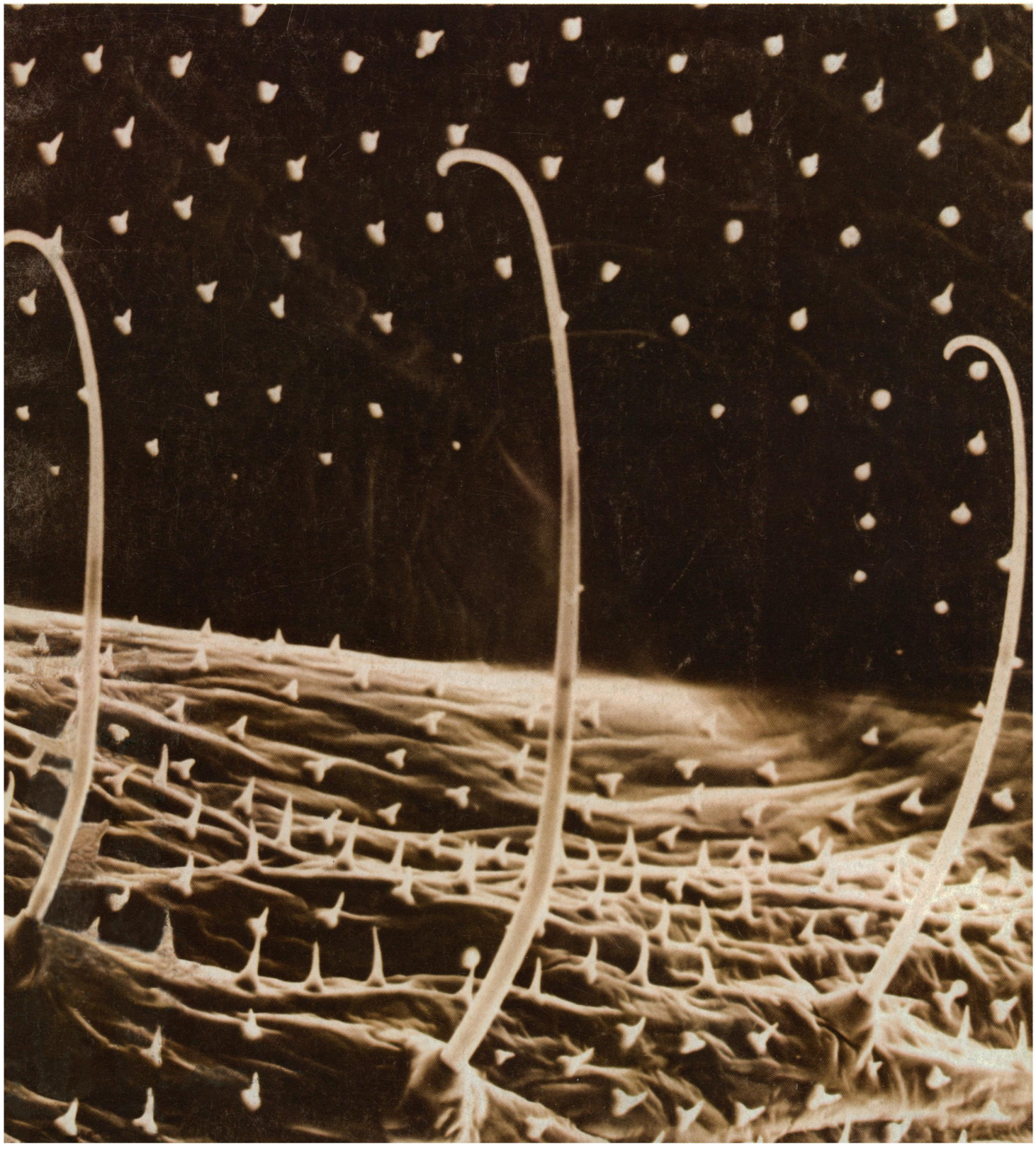

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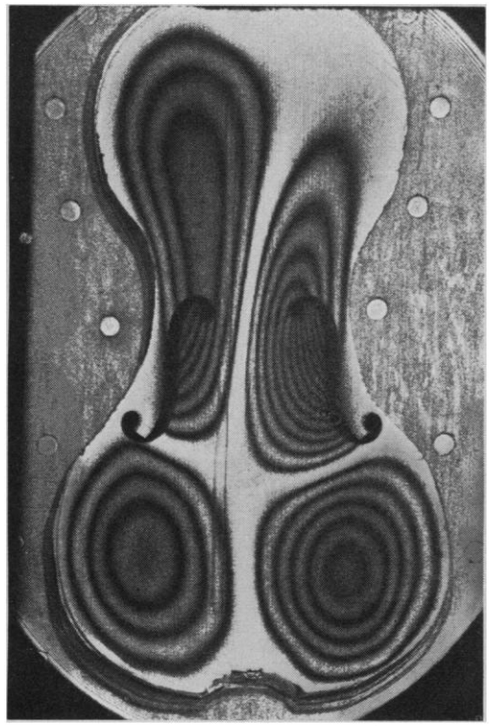
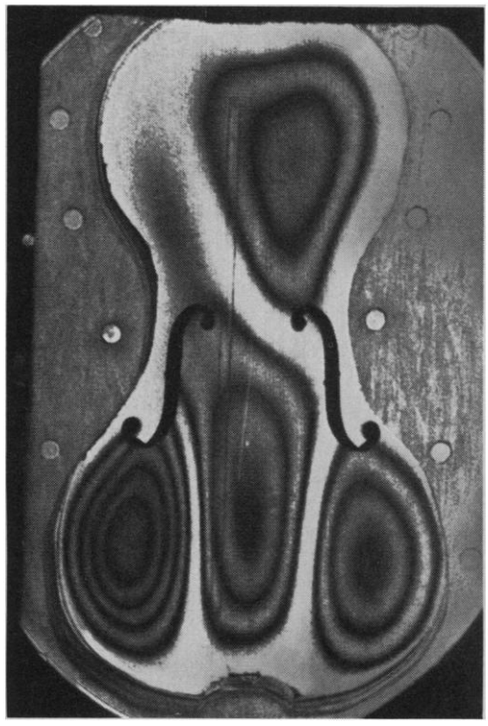
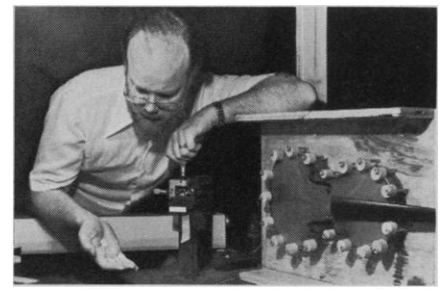




These are hologram reconstructions, photographs made with a camera receiving images originally recorded as time-average interference holograms. The dark fringes connect all points of common amplitude on the inside of the vibrating top of an experimental viola. It's part of an effort to improve the loudness, dynamic range, and playing ease of the viola.

Time-average interference holography is widely used by engineers to analyze vibration in lots of things besides violas. One of the two people who devised it is Karl A. Stetson (*J. Opt. Soc. Amer.* 55:1593 [1965]). These images are Dr. Stetson's work.



For both the holograms and the reconstructions he used He-Ne laser light, here seen impinging on his palm. He made the holograms on KODAK High Speed Holographic Film SO-253 and the reconstructions on KODAK Technical Pan Film (ESTAR-AH Base) SO-115.

Stetson gives two reasons for using SO-115 in this application: 1) high attainable contrast* to bring out the higher-order Bessel fringes for counting; 2) hardened emulsion permitting superproportional reduction of the negative with ammonium persulfate to accentuate the fringes by suppressing detail in the high-density areas that represent absence of vibration.

Other technical users may have quite different reasons for liking SO-115. With KODAK WRATTEN Filter No. 58 for photomicrographic contrast enhancement in phase contrast or Normarski illumination, as in chromosomal studies, it is about 2/3 stop faster than KODAK Photomicrography Monochrome Film SO-410 and KODAK Solar Flare Patrol Film SO-392, which are now discontinued. Addition of a dyed-gel backing against halation and curling has slightly reduced red sensitivity but not enough to bother those who need it to study the sun or the night skies. Red sensitivity still takes its plunge only around 690 nm. Extremely high resolving power. Extremely fine grain. Good latent-image stability. Stocked in 36-exposure magazines, 35 mm x 150 ft, 4 x 5 in. Ask Scientific and Technical Photography, Kodak, Rochester, N.Y. 14650 about other formats and about "POTA" developer for pictorial quality with this film rather than high contrast.

The standout characteristic of SO-115 is its extremely wide range of contrasts. Elaine Stetson, writer on early Americana and director/curator of the Noah Webster Foundation of West Hartford, Conn. likes the way the low-contrast end of its performance range and the extended red sensitivity bring out detail in antique furniture. The Stetsons are here shown photographing the traveling trunk used extensively by the young author of the famous American speller, who was also to become arbiter of the American language. Both pictures of the Stetsons on this page were taken for us by photographer Frances L. Funk at E. I. 25 on the very same SO-115 film. Karl processed them for 5 minutes in POTA developer, made up of 1.5 g of KODAK Balancing Developing Agent BD-84 and 30 g of sodium sulfite per liter of deionized water.

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*Like gamma 4 with 5 minutes in KODAK Developer D-19 at 20°C, for which exposure index is about 100.

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SCIENCE

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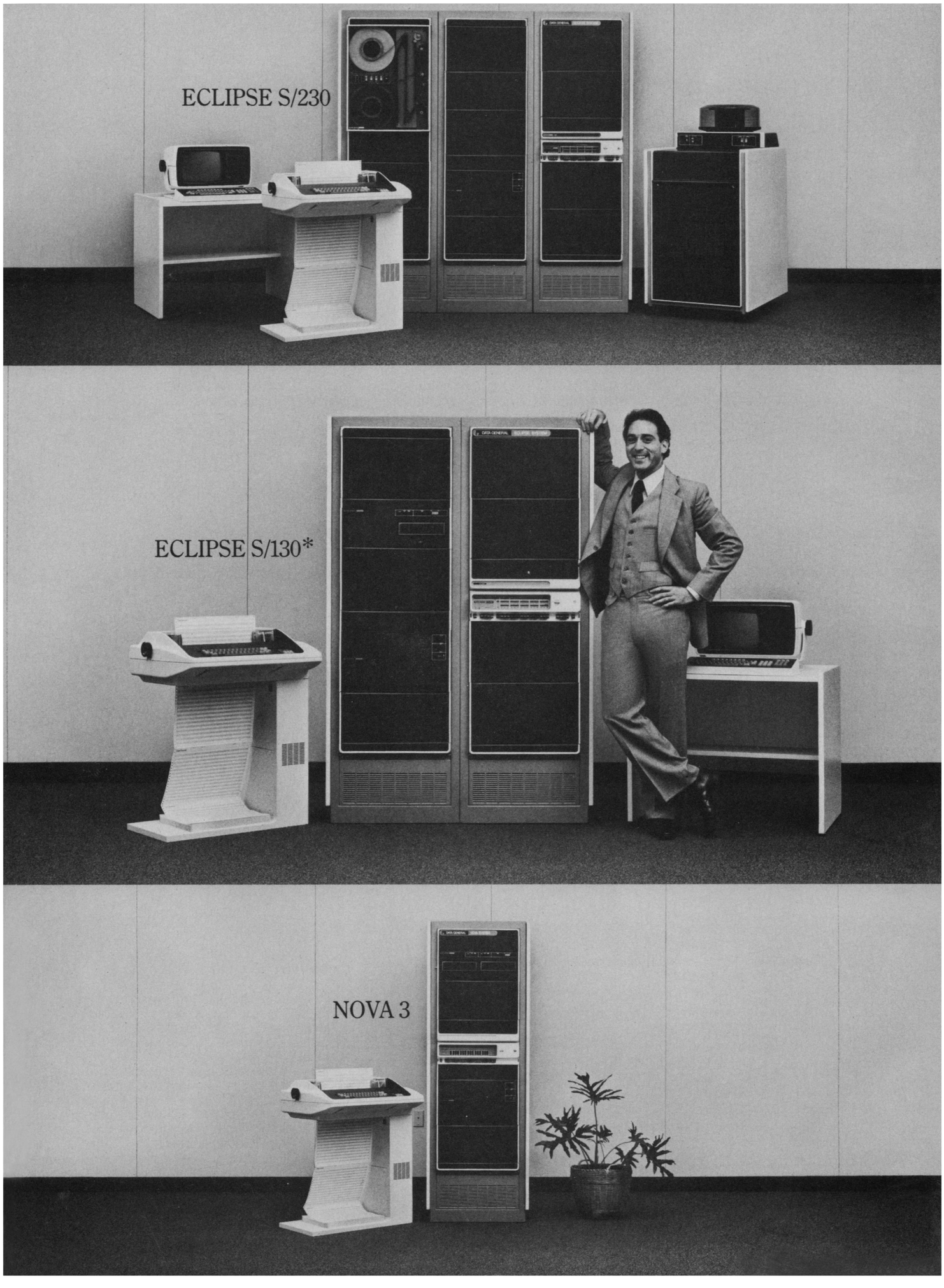
COVER

"Grappling hooks" on dorsal surface of an insect larva (*Chrysopa slossonae*). The hooks enable the larva to fasten tufts of wax to its back which it plucks from the bodies of its aphid prey. Ants that guard the aphids are "fooled" by the acquired investiture of the larva, which they mistake as an aphid. See page 790. [Thomas Eisner, Cornell University, Ithaca, New York; Akira Kabaya, JEOL, Inc.]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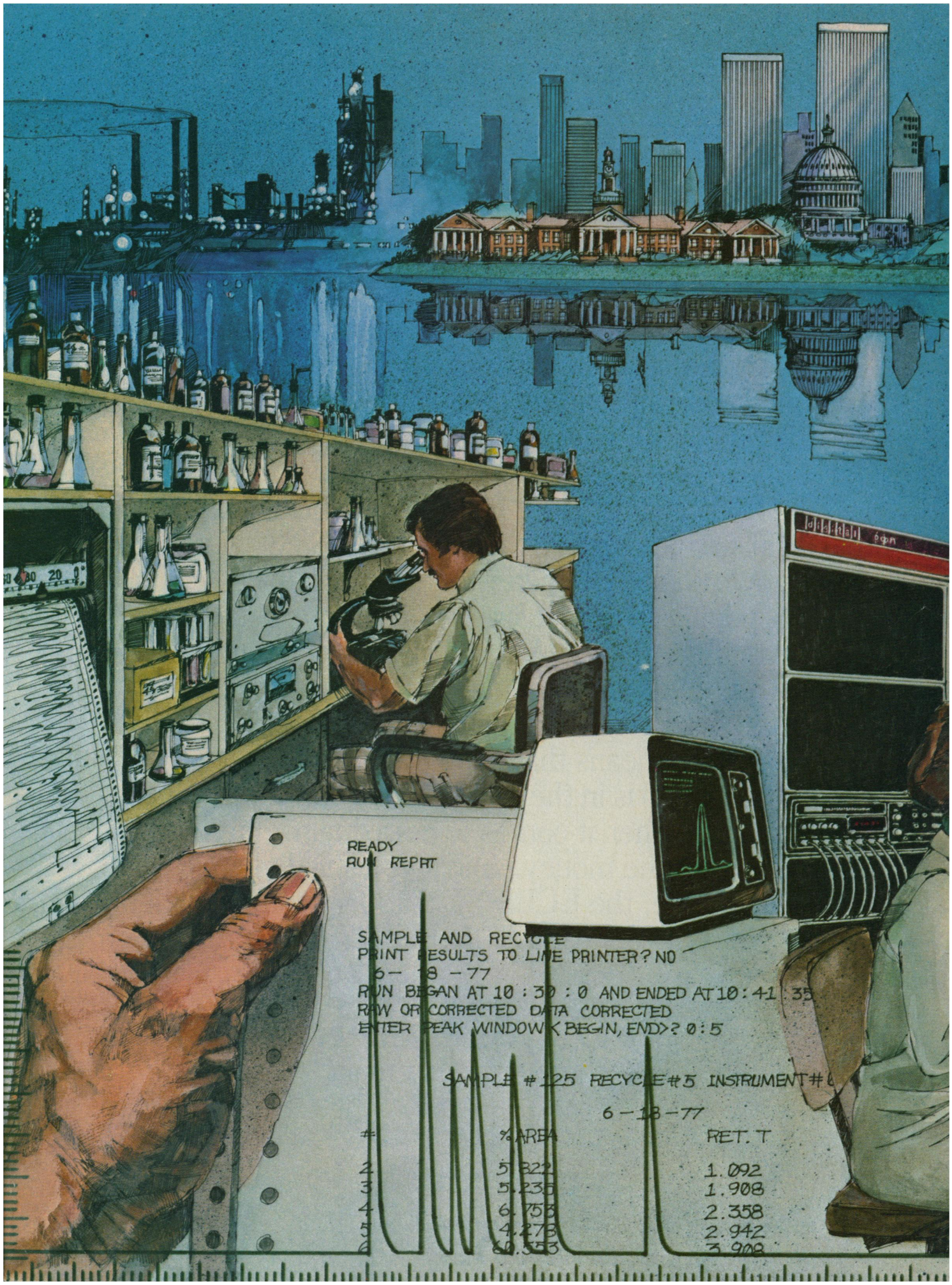
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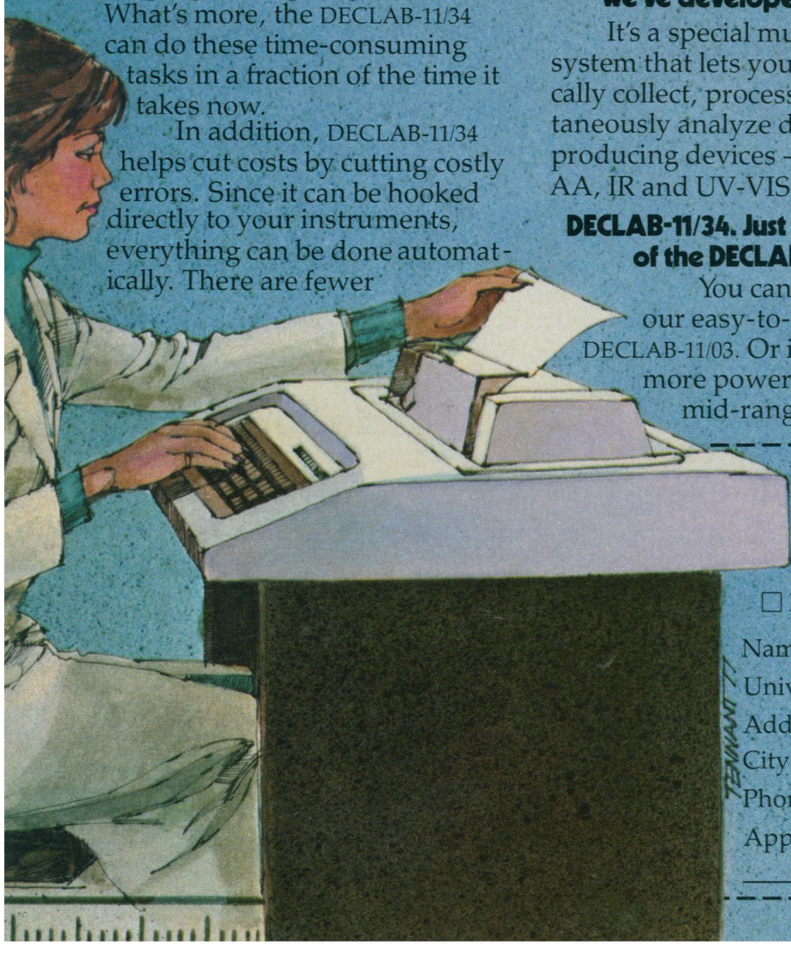

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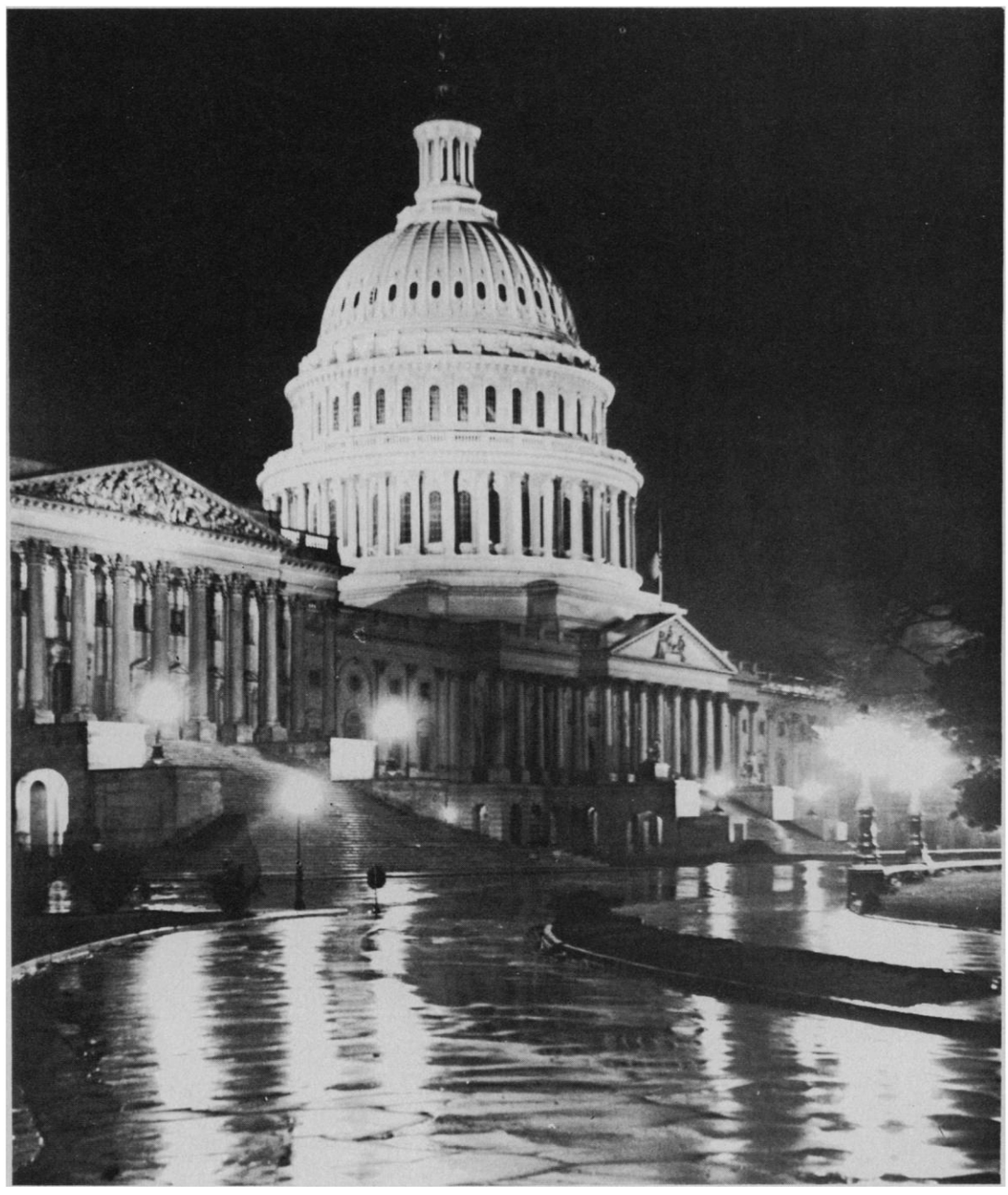

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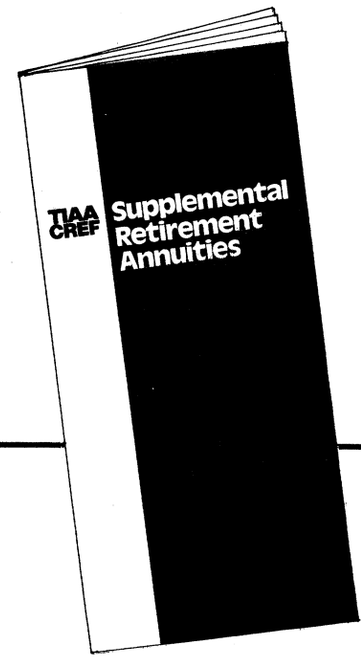

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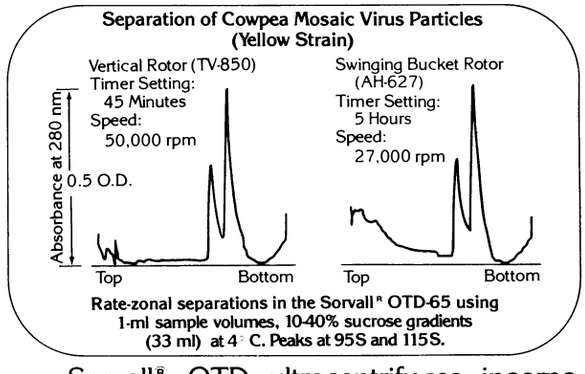

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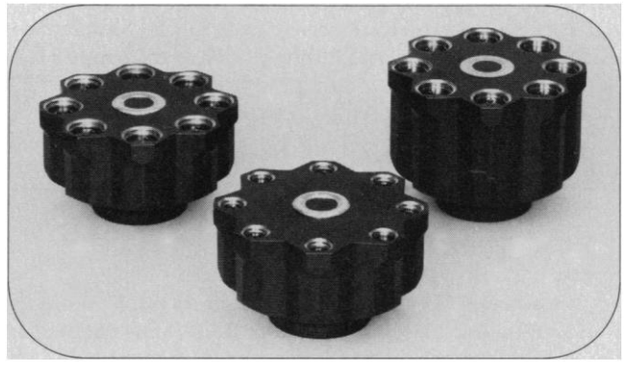

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LETTERS

Japan's Nuclear Bomb Project

Deborah Shapley's article on Japanese nuclear research during World War II (News and Comment, 13 Jan., p. 152) contains several misleading interpretations not supported by the historical evidence available at this time. Shapley writes of the "curtain of silence which the Japanese themselves seem to have pulled over the subject." Yet much of her story is based on translations of historical accounts published in Japan in 1953 and in the early 1970's, which I made available to *Science* at her request. These accounts were made available to me by Japanese historians and physicists, who also provided personal recollections and archival documents. They did so voluntarily in response to my 1974 article (*1*) on the development of nuclear physics in Japan in the 1930's and the destruction of the Japanese cyclotrons by occupation forces after the war. Until then, I was unaware of the Japanese wartime nuclear research projects; indeed, outside of Japan the subject was virtually absent from all of the published studies of the period. At that time, Yagi and Price told me about their 1962 letter (*2*) published in the United States which had asked for more information concerning a reference to the Japanese fission project published in Japan in 1951. That letter did not elicit any responses. I have just learned that in 1959 Arnold Kramish (*3*) cited a 1952 Japanese publication on the subject.

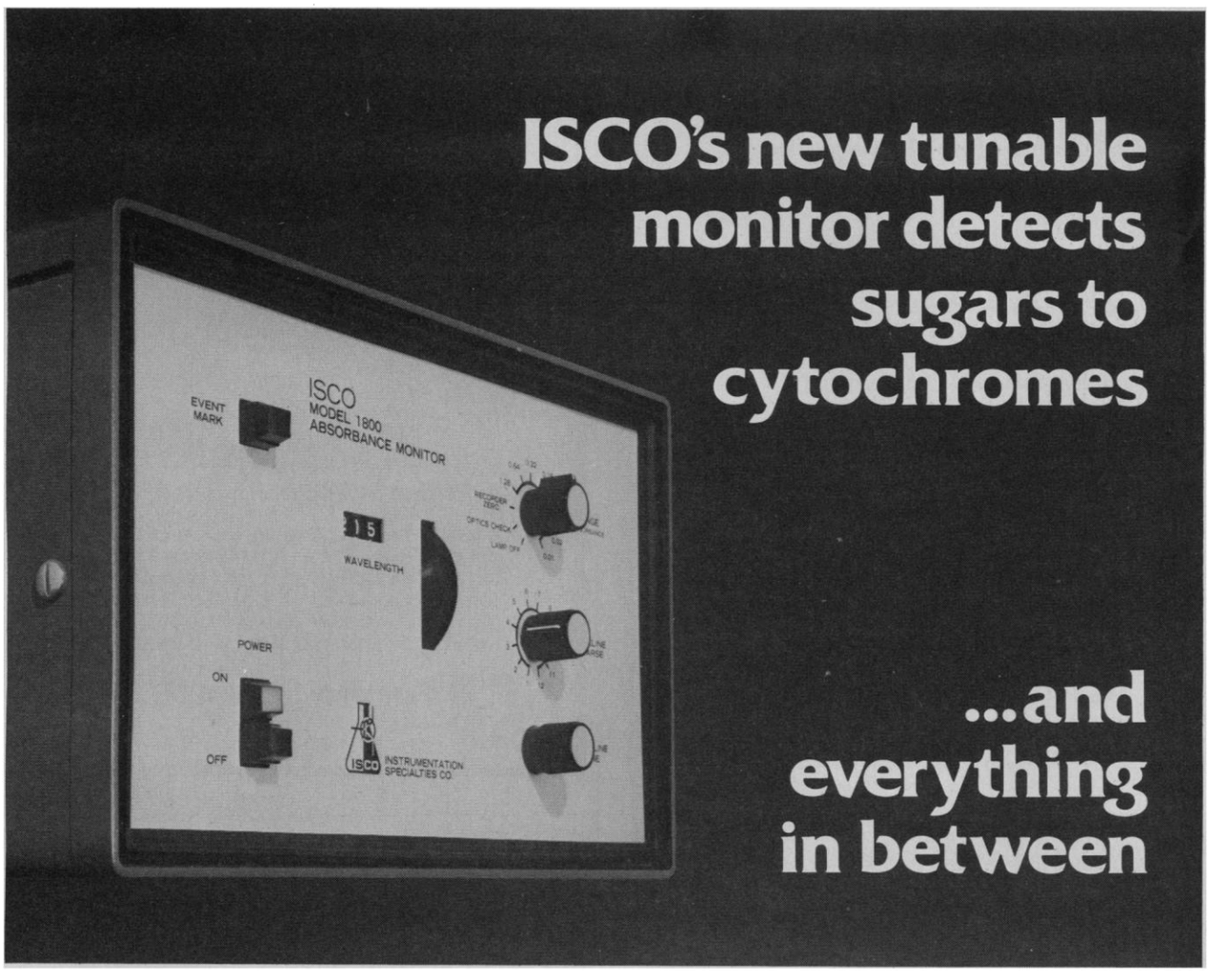
My subsequent study of the issue, which is now being prepared for publication, shows that, during the war, the United States investigated the possibility that Japan might produce a nuclear weapon. The finding was that it would not be possible. The same conclusion was reached by the Japanese physicists themselves. Japan never posed a nuclear threat against the United States, nor did the officials responsible for the decision to drop the bombs on Hiroshima and Nagasaki ever think that was the case. The military and political motives that were the basis of that fateful decision have been well documented by historians, especially by Martin Sherwin in his recent definitive study (*4*) based on previously unavailable documents. The criticism of that decision on humanitarian and ethical grounds should not (and, on the basis of the historical evidence, *cannot*) be dismissed by "arms race" or "technological imperative" explanations as quoted in Shapley's article or her conjecture that if the other side had one they "would not have hesitated to use the bomb against the United States." The consequences of such rationales should be apparent in this age of Mutually Assured Destruction (MAD) and the neutron bomb.

Shapley reports that the U.S. scientists who arrived in Japan just after the surrender found no evidence of a Japanese atomic bomb project, and that such information was neither revealed by Nishina nor vigorously sought by the Americans. However, the official U.S. scientific intelligence mission headed by Karl T. Compton did report that the Japanese had explored the applications of nuclear fission during the war and had not gotten very far. His recommendation that the Japanese be permitted to use their cyclotrons for projects in biology and medicine was approved by the occupation officials, and this policy was in effect when an order to destroy the cyclotrons was received from Washington in November 1945 (*5*). Karl Compton, Ernest Lawrence, Lee DuBridge, and Vannevar Bush, along with atomic scientists' organizations throughout the United States, vigorously protested the destruction. They correctly pointed out that cyclotrons were basic scientific research instruments and could not be used to make atomic bombs, that Japan did not have the necessary uranium, and that in any event all such research was forbidden and was under strict control by the occupation authorities. The historical evidence now available does not change this assessment, nor does it justify the order to destroy the cyclotrons, which Secretary of War Patterson acknowledged in December 1945 was a mistake (*6*).

CHARLES WEINER
Technology Studies Program, Massachusetts Institute of Technology, Cambridge 02139

References and Notes

1. C. Weiner, in *Proceedings of the XIVth International Congress of the History of Science, 1974, No. 2* (Science Council of Japan, Tokyo, 1975), pp. 353–365.
2. E. Yagi Shizume and D. J. de Solla Price, *Bull. At. Sci.* **18**, 29 (1962).
3. A. Kramish, *Atomic Energy in the Soviet Union* (Stanford Univ. Press, Stanford, Calif., 1959).
4. M. J. Sherwin, *A World Destroyed: The Atomic Bomb and the Grand Alliance* (Knopf, New York, 1975; Vintage, New York, 1977). For documentation of the decision to drop the bombs on Hiroshima and Nagasaki, see p. 209 of the Knopf edition, and, for additional new data, p. 209 of the Vintage edition.
5. Nishina's published account of the event, written shortly after it occurred, is in basic agreement with other documentation from U.S. sources. See Y. Nishina, *Bull. At. Sci.* **3**, 145 (1947).
6. A. K. Smith, *A Peril and a Hope: The Scientist's Movement in America 1945–47* (Univ. of Chicago Press, Chicago, 1965), pp. 352–356; (*1*).



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Pollution in Maryland Valley

The article "Pollution: Chemical company's effort to sue its accuser fails" by Thomas H. Maugh II (News and Comment, 13 Jan., p. 157) reads as though the writer was not present at the trial and did not read the transcripts. The jury's duty was not to decide a scientific question, namely, what had caused the increased incidence of cancer in Little Elk Valley, Maryland. Their duty was to decide if I had acted responsibly or irresponsibly in publicizing my observations.

The Galaxy Chemical Company's solvent recycling plant operation in the Little Elk Valley in 1961. Since 1969, there has been an increase in the occurrence of lymphomas: seven within about 1 kilometer from the plant and two more within 3 or 4 kilometers. Increase of other malignancies has also been reported.

In my unpublished paper, I did not make conclusions about the cause or causes of this increase, but I cannot subscribe to the theory of the witness hired by the prosecution, William P. Radford, who attributes the high lymphoma rate to the old paper mill that previously operated on the site of the Galaxy plant. I have been unable to find lymphomas in other former paper-mill workers in the area, except for the valley group. Further, although the paper mill was in operation from the 1880's until 1948, the area did not experience an increased death rate due to lymphomas from 1963 to 1968, according to records of the Maryland State Health Department and from 1940 to 1960, according to my own inquiries. With regard to the incubation period required for the development of lymphomas, their occurrence has been reported in individuals from 1 year to about 25 years after those individuals began receiving therapy with diphenylhydantoin (*1*). Consequently other possibilities discussed at the trial by defense witnesses B. Friedlander and Samuel S. Epstein are more logical to me.

My principal objective has been to stimulate governmental health agencies to take a more active role in protecting the public from the effects of industrial pollution. At least in part, the Galaxy affair and its resulting publicity has promoted this objective, as illustrated by the establishment of a Tumor Registry in Maryland.

PIETRO U. CAPURRO
Union Hospital of Cecil County, Elkton, Maryland 21921

References

1. M. P. Fried and Y. Sunwoo, *Laryngoscope* **85**, 1770 (1975); F. P. Li, D. R. Willard, R. Goodman, G. Vawter, *Cancer* **36**, 1359 (1975).

We wish that were the case... that raising a car's miles per gallon lowers its exhaust pollutant parts per million. But mpg and ppm don't seesaw. They tend to move up or down together, unfortunately.

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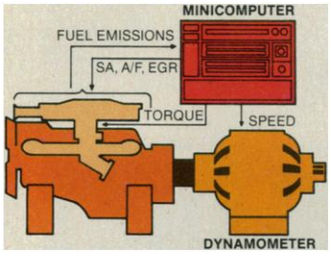

Electronics engineers here at the General Motors Research Laboratories are helping close in on the answer. They're doing it through an understanding of the dynamic interaction between spark advance (SA), air-fuel ratio (A/F), and exhaust gas recirculation (EGR) and how this affects fuel economy and emissions. Their goals have been to determine the optimum control strategies for the three variables and devise systems to implement these strategies.

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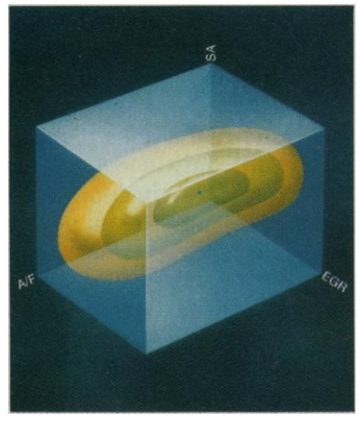

- Modified this system so that the computer makes the engine think it's in a vehicle, then drives the "vehicle" through federal schedules.
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A Policy-Oriented R & D Budget

Human nature being what it is, the annual appearance of the President's budget tends to rivet attention on the bottom line. That is fair enough but hardly good enough. What counts is understanding the reasoning behind the numbers for what can be gleaned about the quality of the decision-making that led to the results.

Mr. Carter's budget for fiscal 1979 shows his striking willingness to get on with shaping public policies for government's role in scientific research and development. Federal commitments to R & D will go up in 1979. Congress willing, but not just because more is better, nor simply to pump up the country's relative share of the gross national product assigned to R & D. Those arguments have never had much force, and it is well to lay them aside.

It is not often that White House speech writers will let a President use the State of the Union Message to single out science and technology as strategic goals of national policy. Such words do not excite the needles on the applause meters. The President had to mean it when he went out of his way to place priority on strengthening the nation's research centers and encouraging a "new surge of technological innovation by American industry." The implicit judgment behind the words is that not all is going well with the vitality and enterprise of our scientific and technological effort, and that the Administration has come to see value in a convergence of science policy with economic growth policy. This is no trivial breakthrough.

No less striking, in terms of reorienting science and technology policy, is the appearance of the term "investments" in the jargon of the budget documents explaining R & D decisions. This has been a long time in coming and we hope it is here to stay. The implication is that government outlays for R & D now are to be viewed not merely as year-to-year expenses but instead as allocating resources to produce long-term returns. The purchasing philosophy that has for so long dominated the government's R & D funding will, one hopes, be overtaken by an investment mentality in which scientific discovery and development are recognized (and evaluated on the proper scale) as growth enterprises requiring long perspectives, confidence, and stability. If this is to be the new departure in federally funded R & D, new funding methods may need to be devised and tested as replacements for the "procurement" approach in supporting R & D.

The Carter budget for R & D also helps to reveal the workings of zero-based budgeting for resource allocation under tight constraints. Zero-based budgeting has not turned out to be a blunt instrument. If anything, it appears to have brought something to the clarification of government's views toward support of R & D in the civilian sector. Public policy has been ambivalent about where the line is to be drawn between government and the private sector in research, development, and demonstration. The decisions in the 1979 budget may go a long way toward settling that question. The appropriate role of government, we are told, is to emphasize longer-term research for the future and new technology options, rather than major commercial scale demonstrations. It is a logical position, assuming that government also understands that the market economy's abilities to supply risk capital will require both incentives and the removal of barriers whose continued presence can reduce such a sensible proposition to ideology without substance.

The 1979 budget for R & D will take close study and inspection before we can arrive at judgments about the merits of particular choices and decisions. This is one of the uses of the annual AAAS analysis of the R & D budget and the June Science Policy Colloquium. But while the Budget Message is still fresh there is value in searching it for signs of new directions in national policy for science and technology. Viewed from here, it rates excellent marks.—WILLIAM D. CAREY

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