SCIENCE

17 June 1977

Volume 196, No. 4296

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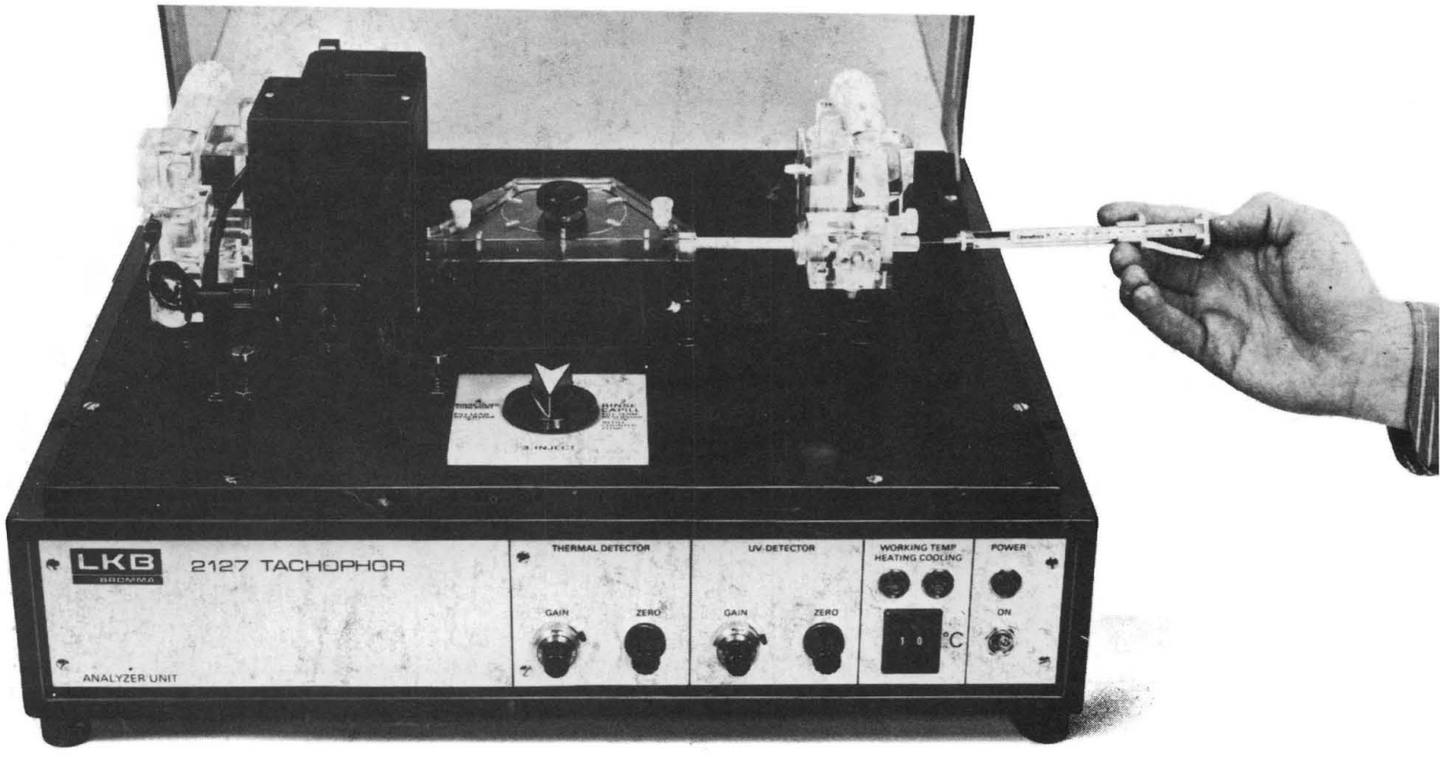

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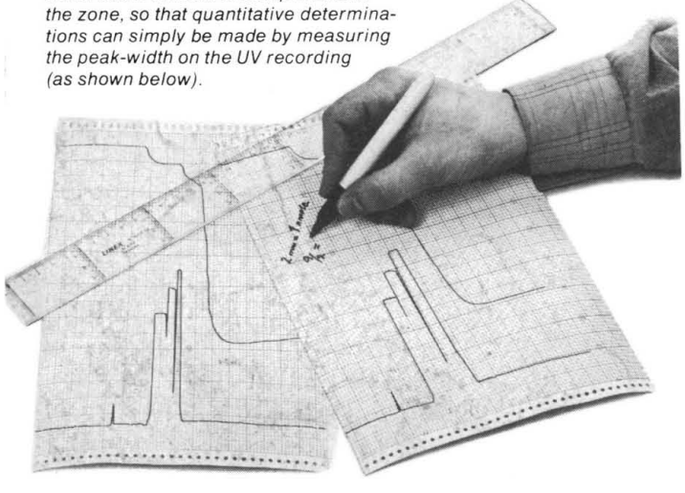

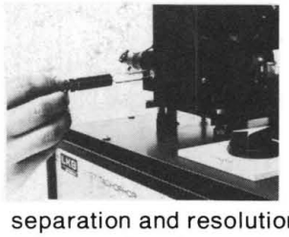

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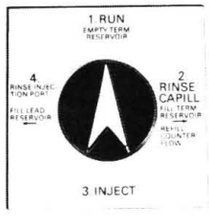

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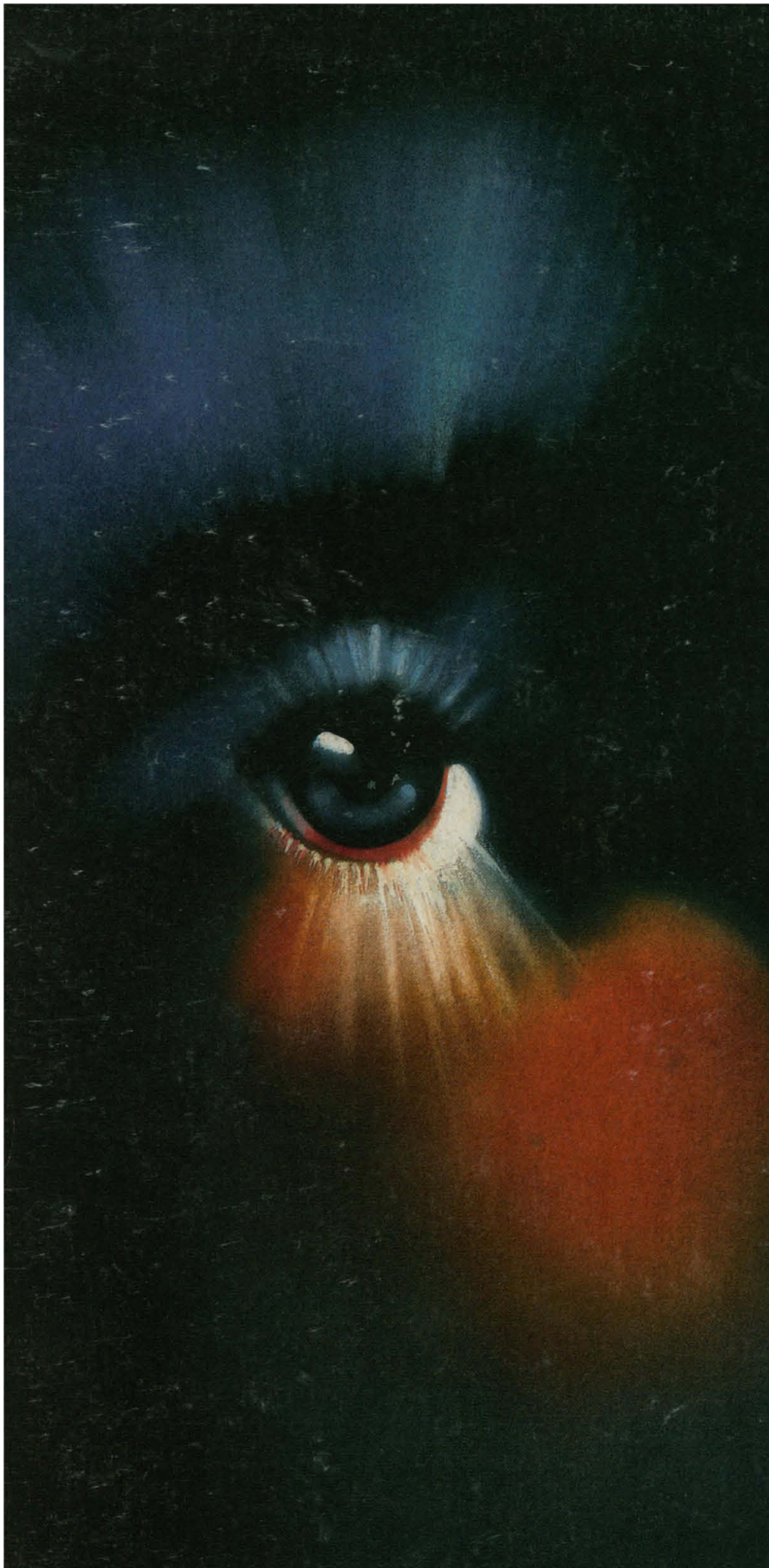

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17 June 1977

Volume 196, No. 4296

SCIENCE

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COVER

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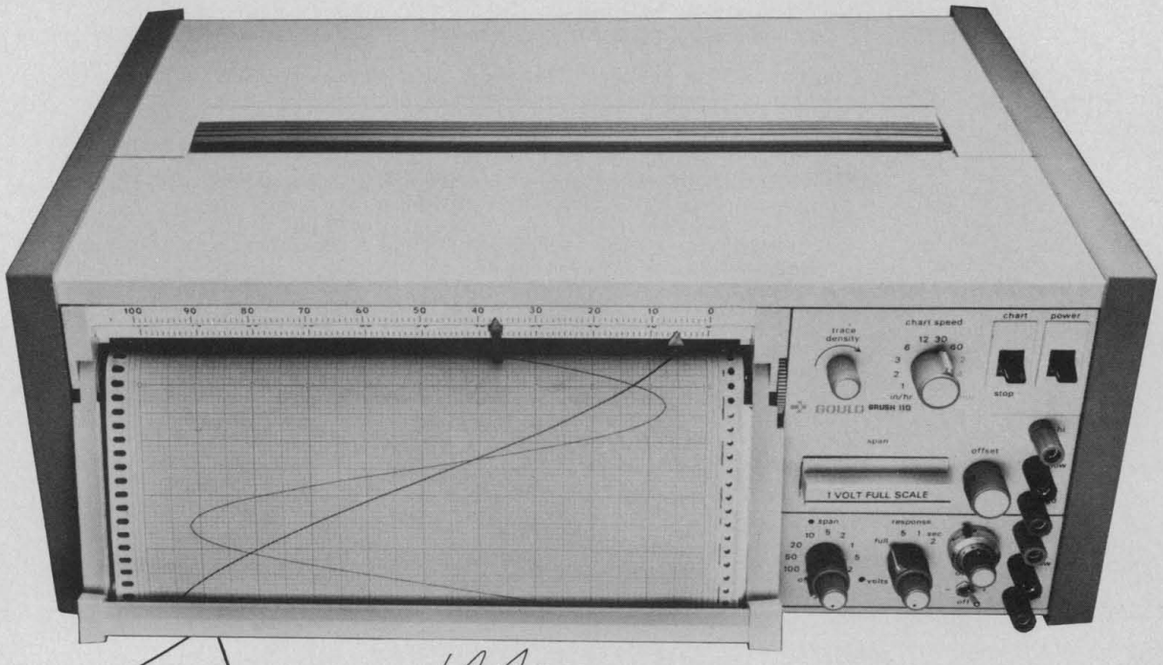

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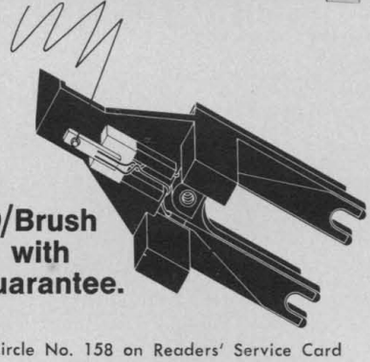

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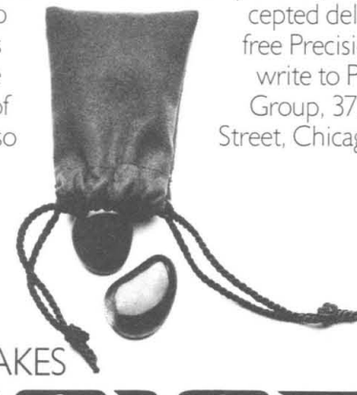

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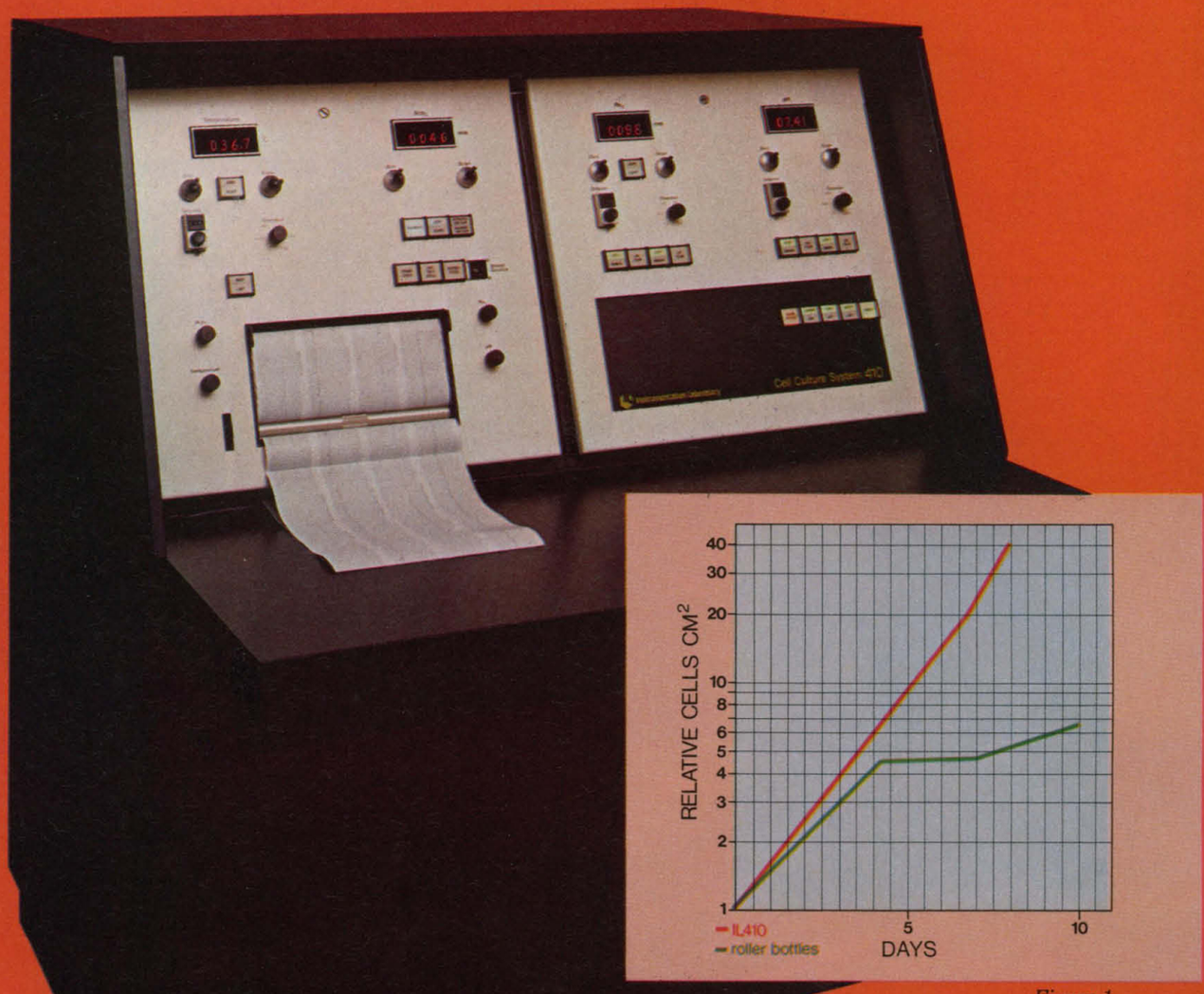

Figure 1

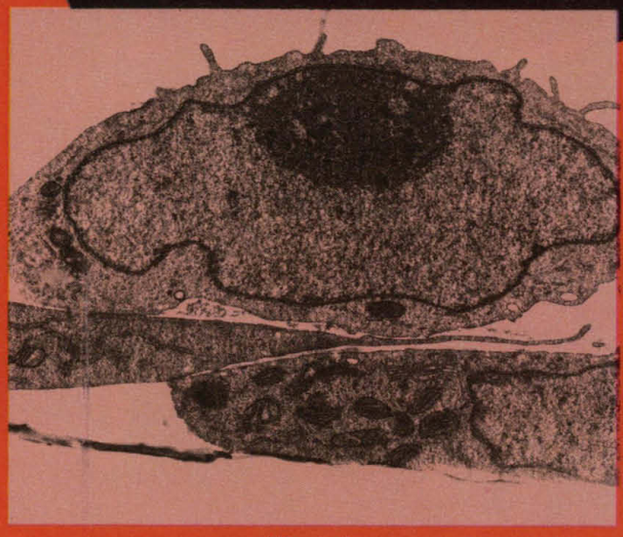

Figure 2

Figure 3

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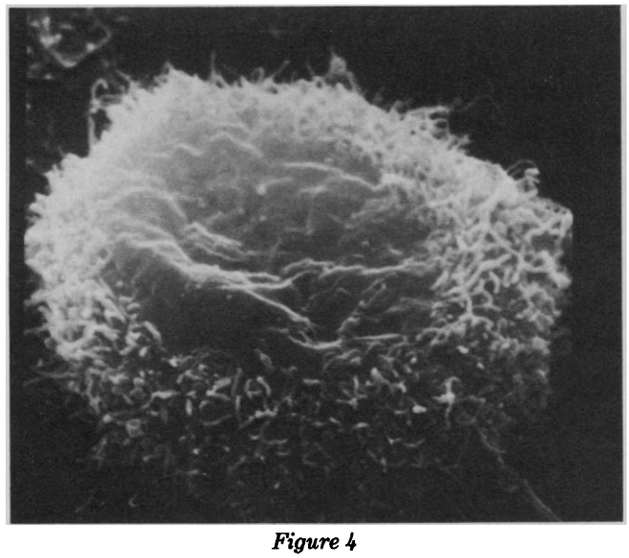

Figure 4

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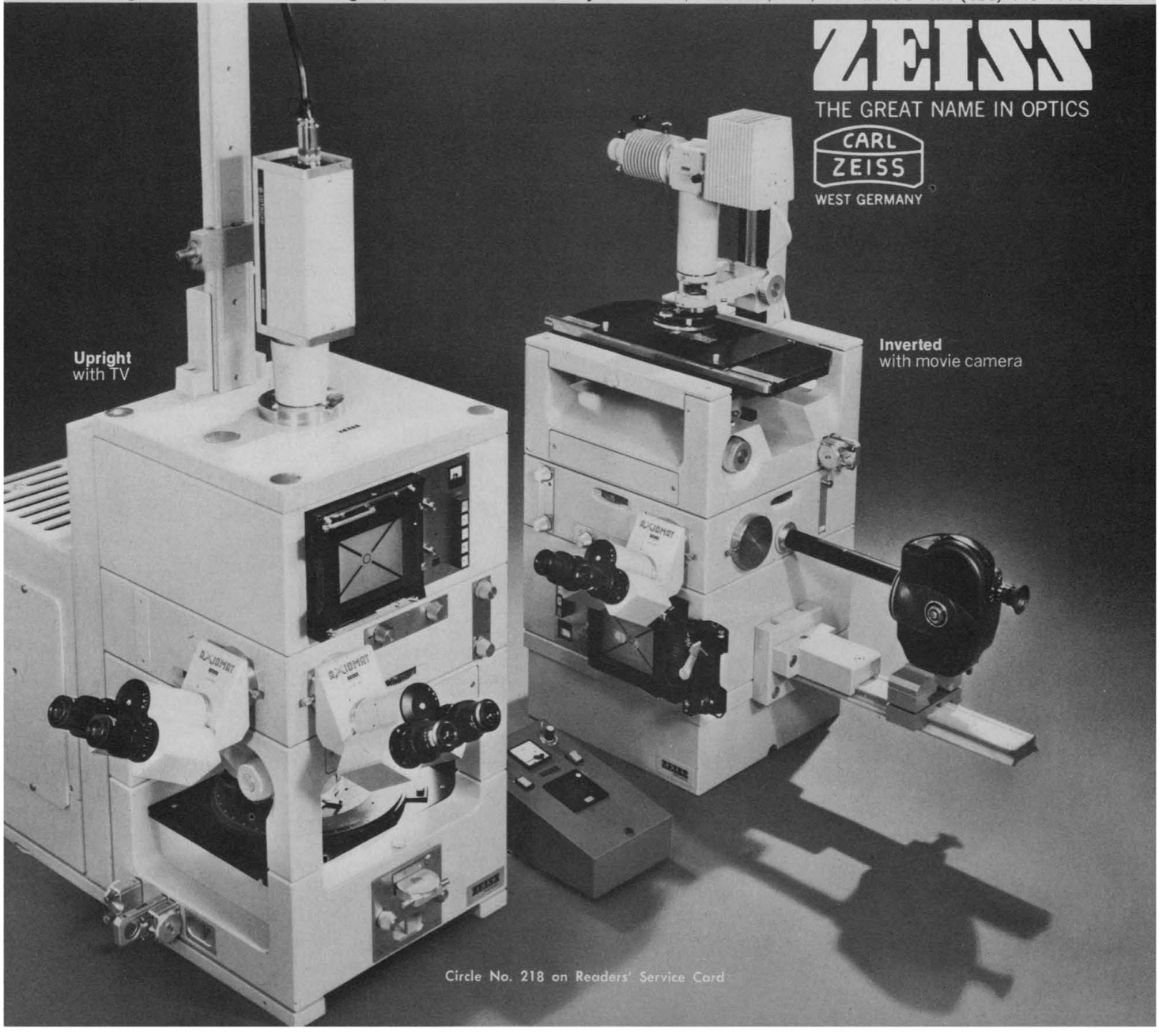

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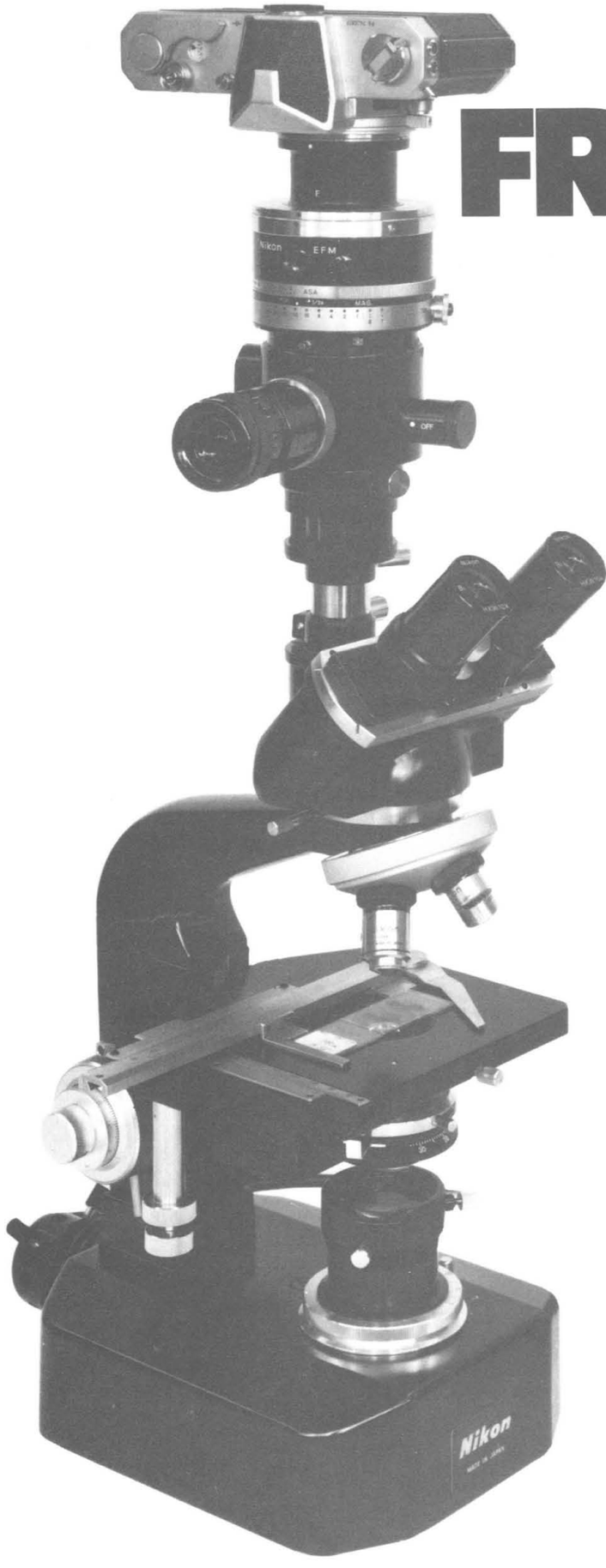

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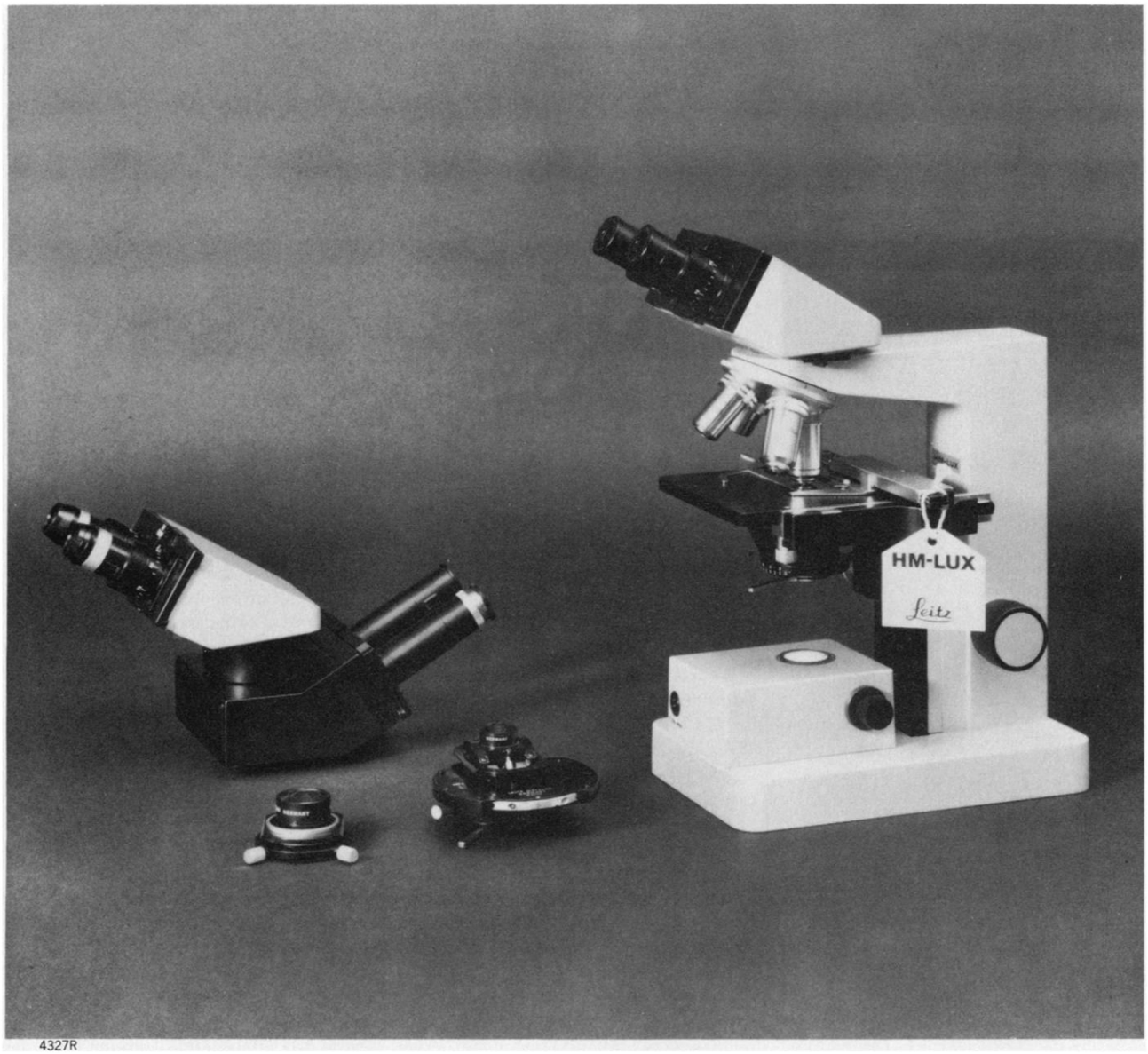

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LETTERS

Catastrophe Theory

Gina Bari Kolata, in her article "Catastrophe theory: The emperor has no clothes" (Research News, 15 April, p. 287), states that Héctor Sussmann, the leading critic of the theory, became "fascinated with the sociology of its growing popularity." I am fascinated by the sense of mission that drives some scientists to denounce heresies and heretics, and by the ease with which others abandon their objectivity and jump on bandwagons. The article provides some insight into how the bandwagon mentality is fostered.

Kolata states that "Zeeman, contacted about Sussmann's harsh criticisms, said he was unfamiliar with the details of the criticisms; when they were described, he gave no direct or specific rebuttals to any of them." She does not say when or how or by whom he was contacted, nor under what circumstances. (One can easily imagine circumstances under which it is wisest not to reply.) She does not say what she means by "direct" or "specific." The impression is left that Zeeman could not answer the criticisms.

Kolata cites charges of sloppiness, exaggeration, irresponsibility, and dishonesty but does not give the details of any of these, nor does she reference either the original papers or the criticisms. The readers are not urged to judge the matter for themselves, but are left instead to rely on the authority of the experts she quotes.

Kolata states that the list of mathematicians opposing catastrophe theory reads like a who's who in American mathematics and cites two names. Even had she cited a hundred, it would hardly constitute proof that catastrophe theory is unsound. The effect of this approach is to make it easy and acceptable for scientists and mathematicians to ridicule catastrophe theory, without having to go to the trouble of studying the details.

Kolata cites Sussmann's contention that the use of "ifs," "coulds," and "maybes" by catastrophe theorists puts the burden of truth on their critics. It could be that this is, instead, an indication of intellectual honesty. If they are aware that their hypotheses are tentative, and put them forth as suggestions, not definitive assertions, then their use of qualifiers is laudable.

Kolata quotes others as saying that catastrophe theorists are reluctant to undertake a real study of the phenomena to which they hope their theories will apply; the reader is left to conclude that, since they don't know what they are doing, they need not be taken seriously. But no evidence whatsoever is given for these charges, nor is it suggested what such a study ought to entail. One should not gloss over the very real difficulties, due to the highly specialized nature of contemporary science, that stand in the way of anyone who seriously tries to understand the problems in another field from the point of view of its practitioners.

MARJORIE SENECHAL
Department of Mathematics,
Smith College,
Northhampton, Massachusetts 01060

Kolata's article concerning criticisms of catastrophe theory was both timely and penetrating. It is true that practical applications of the theory have been either obvious or dubious. It is also true that extravagant claims have been made at a time when the principle statistical technique for the development of a catastrophe surface involves courage and a good eye. Furthermore, social scientists are going to get exceedingly tired of looking at the same three models over and over again (only the fold, the cusp, and the butterfly are based on probability distributions). But before the criticisms grow too loud, it would be wise to distinguish those aimed at Zeeman's work from those aimed at any practical application of the theory. After all, social scientists have not really had a chance to develop the statistical techniques that will be necessary for the creation of believable applications. However, the development of those techniques is under way. For example, I am currently investigating several correlation and regression procedures that can indicate the existence and location of a cusp or butterfly catastrophe. The result is a new model of the psychological changes that occur in crisis situations—a model that makes predictions that are both unexpected and verifiable.

Newton's critics eventually caused him to call science a series of lawsuits. Now Thom's admirers give us a modern Newton and his detractors give us new lawsuits. Let us have neither.

MARC LEWIS
Department of Psychology,
Case Western Reserve University,
Cleveland, Ohio 44106

I was rather sorry to see the recent article on catastrophe theory by Kolata. While it must be granted that a number of immoderate claims in the form of "catastrophe theory can do everything"

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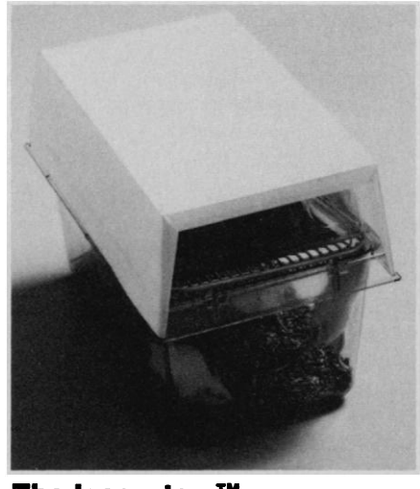

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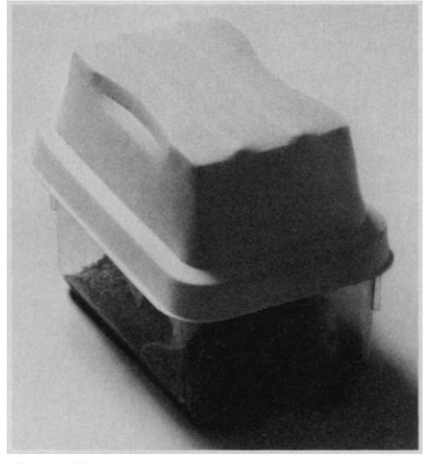

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have been made in the literature, on the basis of too little experience, it doesn't seem that the proper response is an equally immoderate claim that "catastrophe theory can do nothing" on the basis of that same body of experience.

As an interested spectator, with no particular ax to grind one way or the other, I feel it is far too early to tell what the impact of catastrophe theory will be in the sciences. As it stands now, I would say that it is not yet a theory in the scientific sense; rather, it is a series of suggestions on how a theory might be built, based on some suggestive mathematical results. Its utility will depend on the art with which the mathematical formalism can be interpreted in terms of empirical experience. Attempts at such interpretation have barely begun. If some early attempts have been overly hasty, it does not necessarily mean that the formalism is meaningless or inapplicable; or that those responsible are guilty of anything beyond overenthusiasm.

I believe it is true, as imputed in Kolata's article, that the response to catastrophe theory (including the article itself) is a sociological phenomenon, whose roots it would be most instructive to explore. However, it doesn't seem fruitful to reach a decision concerning the applicability of scientific concepts on sociological grounds. In general, if an individual scientist finds such concepts uncongenial, let him not use them. There is no reason why he should take their existence as a personal affront.

The situation regarding catastrophe theory today reminds me of what happened to information theory in the mid-1950's. Then, too, extravagant claims that information theory could provide deep insights into all complex systems, from organisms to societies, were followed by a backlash in which it was asserted that the theory was impotent or fraudulent. Neither was true, and everyone lost from the resultant polarization. I would not like to see this unhappy history repeat itself.

ROBERT ROSEN
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The controversy over catastrophe theory need not itself provide an example of the cusp catastrophe. In other words, there is a possible middle ground between the two extreme points of view.

Thom's theorem of the seven (on which the theory depends) assumes that the system is described adequately and completely by the minimization (or maximization) of a scalar potential function. It is a local theory, not a global one; that is, it applies only in the immediate neighborhood of singular points.

Any claimed application which neglects these presuppositions can be suggestive at best, never definitive. Nevertheless, it should be stressed that the overworked methodology of the correlation coefficient also depends upon local approximation. It follows that much criticism now leveled at catastrophe theory could also be applied to many sociometric and biometric studies.

I have recently been able to find a relatively simple proof of the theorem of the seven (*1*). As such accounts become more widely known, the theory will become more accessible in detail to scientists other than research mathematicians. This will enable more informed critical analysis of claimed applications.

Meanwhile, Thom's theorem provides a possible tool for application. How useful that tool is going to be is a matter that is not yet clear. The settling of this question is not likely to be aided either by irresponsibly extravagant claims on the one hand or by bombastic criticism on the other. What is required is a balanced assessment of the already numerous efforts to apply the theory.

MICHAEL A. B. DEAKIN
Department of Mathematics, Monash University, Clayton, Victoria, Australia 3168

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Carcinogens in Schools

A report appeared last September (*1*) which stated that 418 pounds of carcinogenic chemicals were then being stored or used in schools in California; that there was no evidence that students were being unduly exposed; and that safe, lawful disposal of such materials is available through the state department of health.

The 14 carcinogenic chemicals referred to are regulated by federal and state occupational safety and health acts. We find it difficult to imagine purposes which could justify the storage or use in schools of such quantities of dangerous materials, even though the compounds are distributed among some 200 institutions.

Since there are no permissible exposure levels to chemical carcinogens we

(Continued on page 1358)

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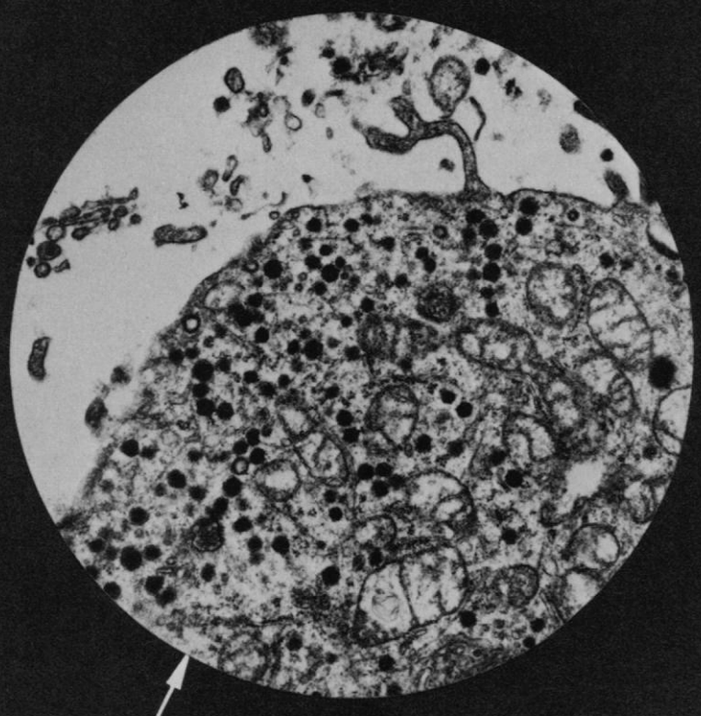

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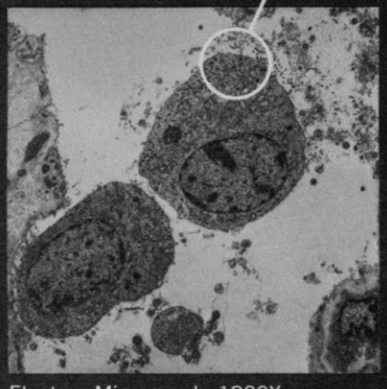
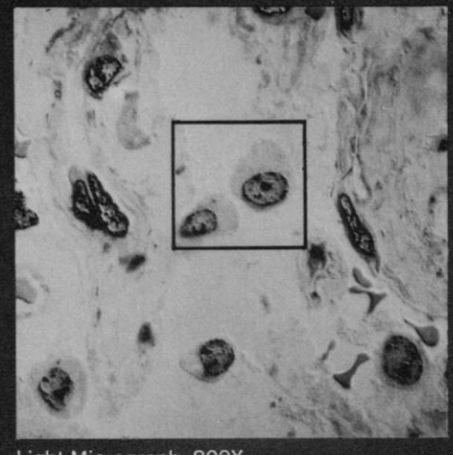


Electron Micrograph, 1800X

Light Micrograph, 1800X



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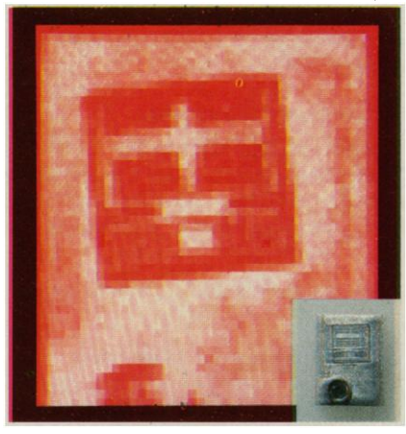

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17 June 1977, Volume 196, Number 4296 **SCIENCE**

Tropical Medicine—New Vigor

There is a new intensity of interest in international health. An initial focus seems to be on biomedical research. New directions are being considered within our government; the Committee on International Health of the Institute of Medicine is preparing a report at the request of Congress; and a number of other groups, such as the Rockefeller Foundation and the Institute of Society, Ethics and the Life Sciences, are looking again at international health as an area ripe for new thinking and initiatives. Beyond this, the World Health Organization has recently launched a Special Programme for Research and Training in Tropical Diseases, and a goodly number of U.S. investigators are already involved in this major scientific assault on diseases primarily found in the tropics.

The idea of blending science and technology into an international initiative is by no means original. However, in this decade biomedical research has not been a major force in American foreign policy. It is true that a number of bilateral and international programs are well under way, but one is impressed that these most often derive from other efforts rather than muster the initiative in their own right. Thus, there remains a challenge to construct an effective and truly integrated alliance with foreign policy.

International health comprises a natural and comfortable combination of science and technology with humanitarian concern. It is a truism that neither health nor illness recognizes national borders, although it is also obvious that much disease has a definable cartographic distribution. But one might say that the somewhat descriptive field of geographic pathology is being broadened into "geographic medicine" through forces of political, social, and economic origin. The diseased and deprived whole human—not just the parasitic granulomas in his liver or the lepromas in his skin—is becoming of paramount concern. The existing scientific base in rapidly moving fields such as immunology, cell biology, and genetics could well serve as the foundation on which to forge a new initiative in tropical medicine. Indeed, the ultimate success of the enterprise may depend on attracting new investigators to work in fields such as tropical medicine—for example, through the creation of new faculty positions at institutions. Such an infusion of new scientific blood from a diversity of disciplines is important to this relatively neglected area of biomedical research.

In terms of support, perhaps what is required is not so much a great gush of money, but rather modest resources coupled with substantially enhanced authority and an opportunity to work in an international setting. This might increasingly take place within the framework of multinational organizations such as the World Health Organization. A "people intensive" approach in international health research may make very good sense not only in terms of biomedical science, but also from a humanitarian perspective. No longer may American scientists who work overseas simply retreat to our shores with a trophy room full of specimens and data; instead, they must construct continuing scientific linkages and mutually productive partnerships.

Although we will need to engage more of our best minds in research in tropical medicine, there is already a growing interest in the immunologic response to parasitic infections, and the study of relationships between cell competence and malnutrition is generating excitement. Indeed, for the newly trained investigator making his way, the scientific rewards are there. With wide scientific opportunity as well as international humanitarian appeal, it should be possible to generate renewed interest among "good people." If we put our minds to it, the next decade may well see research in tropical medicine come to the forefront in the search for new knowledge.—HOWARD A. MINNERS, *Associate Director for International Research, National Institute of Allergy and Infectious Diseases, National Institutes of Health, Bethesda, Maryland 20014*

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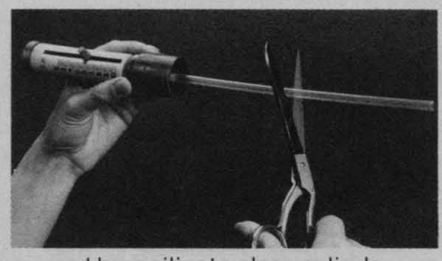

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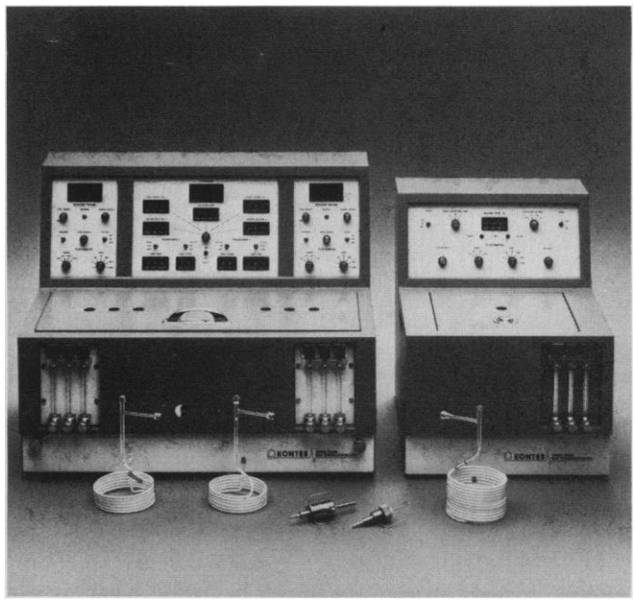

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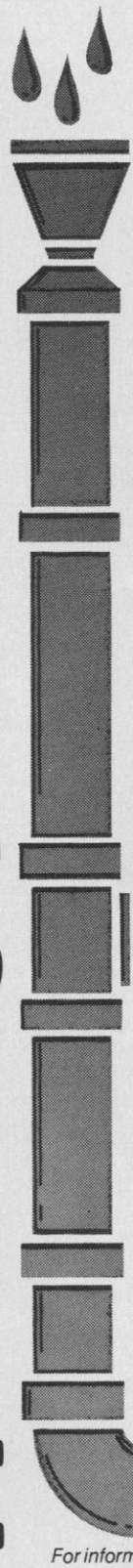

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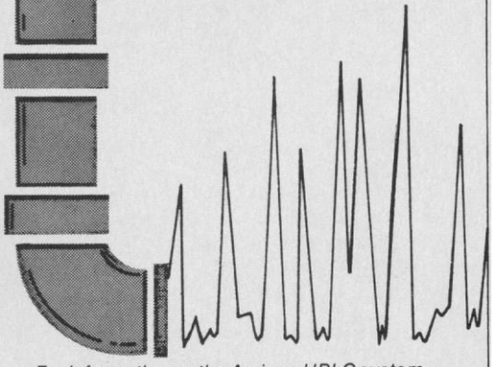

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LETTERS

(Continued from page 1272)

are at a loss as to what "unduly exposed" means. Evidence that students were not being exposed at all would be much more encouraging.

The safe, lawful disposal of carcinogens referred to consists of burial in a class I disposal site (*2*). We believe that disposal of carcinogenic materials by landfill is not suitable. Instead, the carcinogen must be degraded, deactivated, or incinerated.

This report (*1*) suggests that it is likely that very large quantities of chemical carcinogens (and not only those regulated by federal and state occupational safety and health acts) are present in schools (and elsewhere) throughout the country. Moreover, it is certain that these materials are often used without knowledge of their dangerous properties, without establishing safe handling procedures, with no thought given to emergency planning, and with insufficient consideration of methods of disposal.

Although we are encouraged at one state's recognition of the problem and indications of their willingness to grapple with it, we deplore the situation and strongly recommend that a mechanism for informing the users of such materials of the potential hazards associated with them be developed. Appropriate labels and data sheets provided by the manufacturer or supplier might be a worthwhile step in this direction.

E. B. SANSONE
W. LIJINSKY

Frederick Cancer Research Center, Frederick, Maryland 21701

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Origins of an Ecological Theory

In his review of *Biochemical Interaction between Plants and Insects* (*1*) Lawrence Gilbert (28 Jan., p. 387) infers that the highly similar theories of Feeny (*2*) and of Rhoades and Cates (*3*) relating type of chemical defense by plants to the likelihood of discovery of plants or individual plant tissues by enemies were arrived at by induction from the observed patterns in the former case and by deduction from evolutionary ecological theory in the latter. If true this would suggest that ecological theory has now reached a stage at which it can successfully compete with empiricism in describing natural patterns. Undoubtedly ecological and evolutionary ideas contributed importantly to both theories, but before we congratulate ourselves on the predictive power of "selectionist thinking" a note of caution is in order.

At the outset of a study of plant-herbivore interactions in desert ecosystems Orians *et al.* (*4*) made predictions concerning defensive chemistry in plants and the expected grazing patterns of animals that feed on these plants, largely from evolutionary theory and with a limited a priori knowledge of phytochemical patterns. Gilbert suggests that it was testing these predictions that led to the theory presented by Rhoades and Cates, but in fact most were not tested in any depth. A major prediction that was so tested, namely that herbivores that feed on annual and early-successional plants should be more generalized in their diets than those that feed on more predictable plants, such as woody perennials and late-successional species, was confirmed in the desert system, though, as Gilbert points out, this finding may have no general applicability, since most data from other environments run counter to the result. In hindsight, it appears that five or six of the predictions made by Orians *et al.* are probably right and five or six are probably wrong.

The research that was most fruitful with respect to the final theory was a comparison of within-plant distribution of defensive chemicals in the two dominant perennials creosote bush and mesquite, studies that were not initiated from the original predictive scheme. From these studies our theory was developed largely by inductive processes, with a liberal infusion of the ideas of Orians and Schultz. Similarly, Feeny arrived at his conclusions by generalizing from his studies of between-plant distribution of defensive substances, using evolutionary arguments. Thus the difference was not so much in philosophical approach as in type of data collected.

DAVID F. RHOADES

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(Continued from page 1312)

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