SCIENCE

14 November 1975

Volume 190, No. 4215

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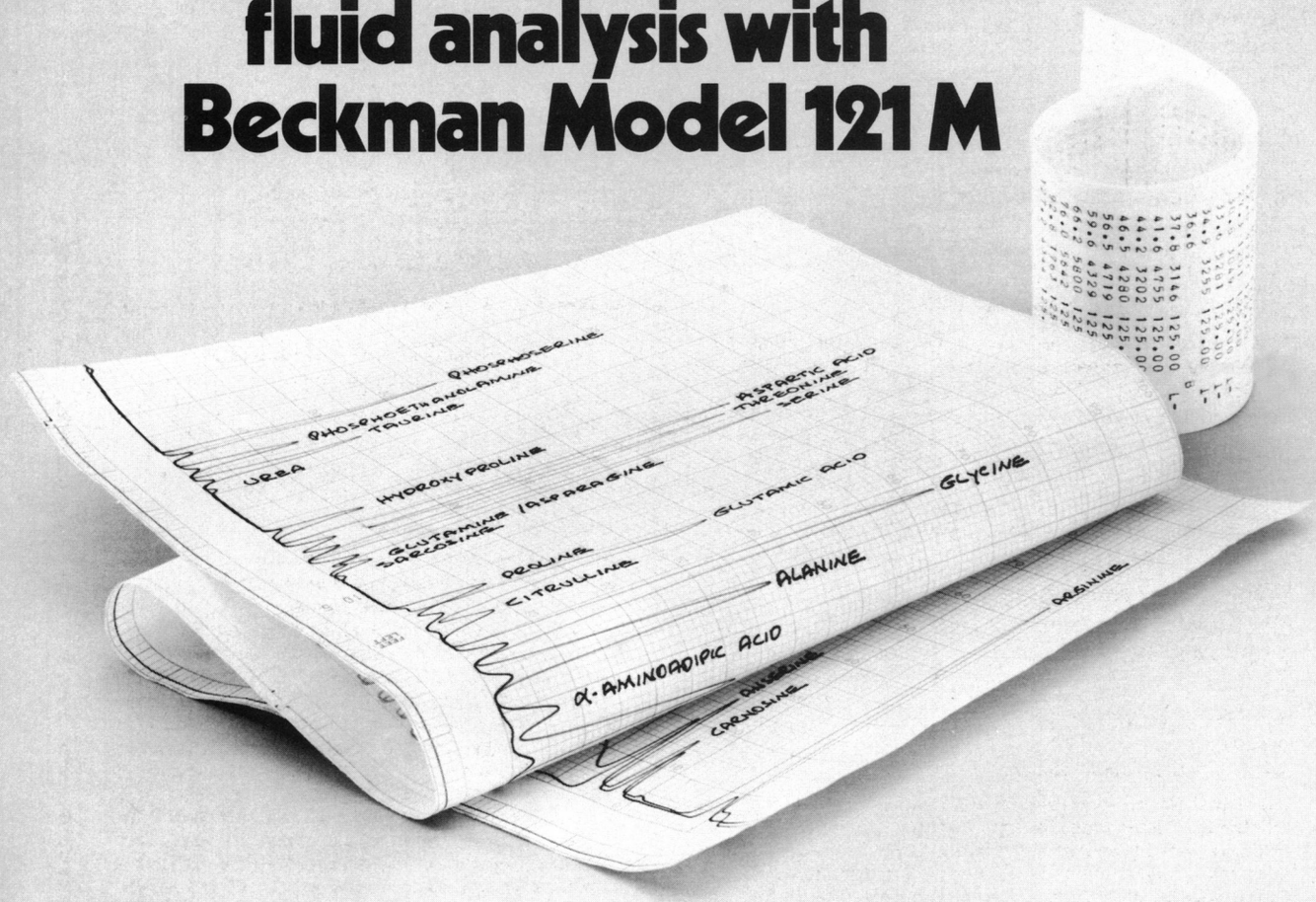

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COVER

Terminal phase male of the bluehead wrasse (*Thalassoma bifasciatum*). Males of this coloration are often the result of sex changes in previously female fish. See page 633. [D. R. Robertson, Smithsonian Tropical Research Institute]

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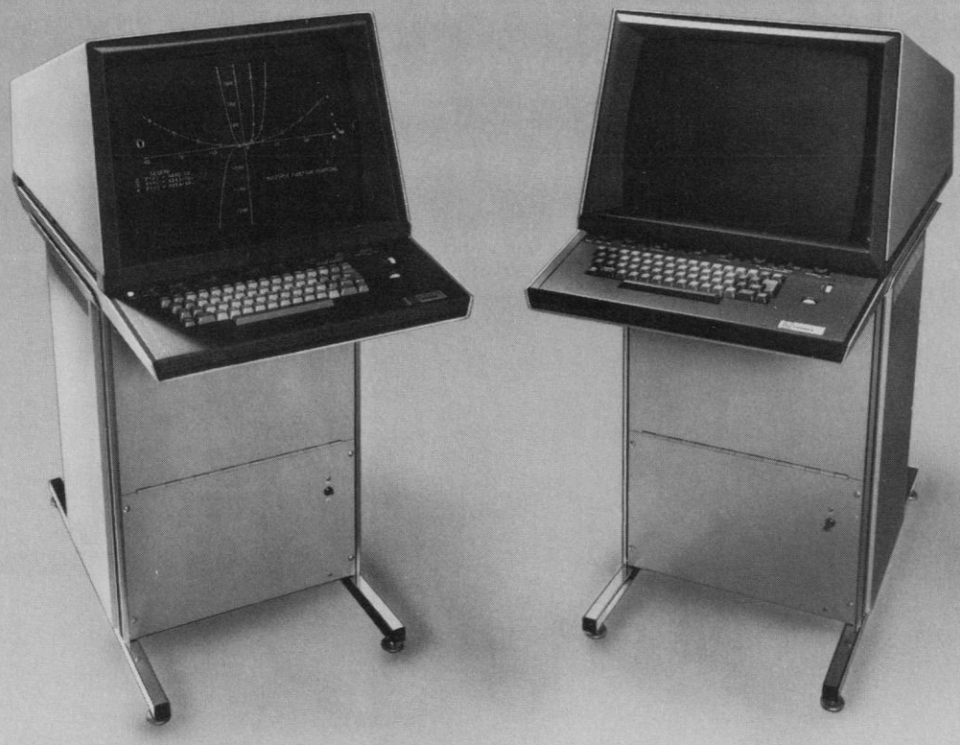

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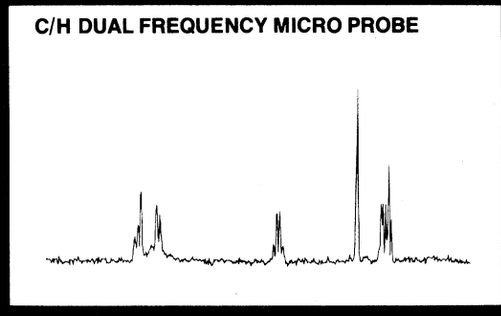
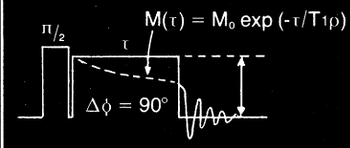
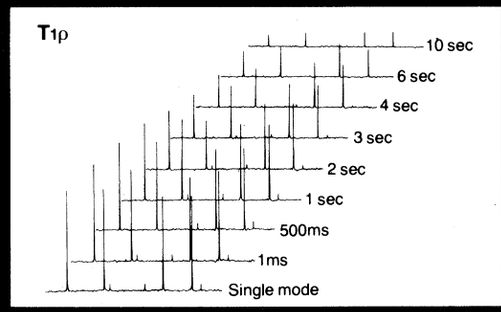


$T_1\rho$ is the transverse relaxation time in the rotating frame and is useful for studies of chemical dynamics in liquids.



Sample shown is chlorobutane.



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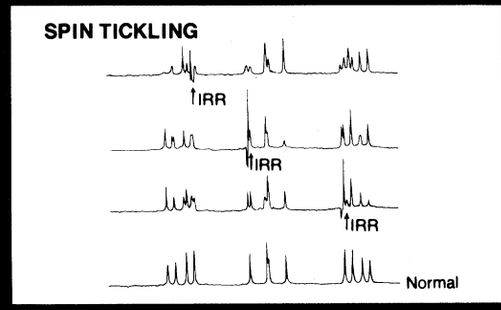
Sample shown is dibromopropionic acid.



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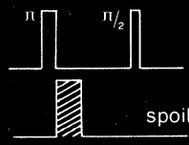
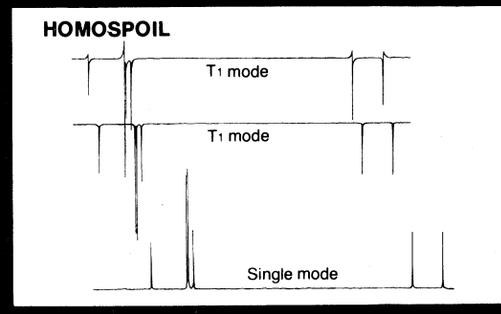


Sample shown is ethylbenzene (distortions are magnified to illustrate the technique).



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Age at Issue	MALE / FEMALE	25 / 30	**30 / 35**	35 / 40	40 / 45	45 / 50
Annual Premium (Payable only 18 years)		$187	**$235**	$328	$487	$747
Cash Dividend End of First Year*		97	**113**	142	188	265
First Year Net Payment		$ 90	**$122**	$186	$299	$482

**Subsequent yearly dividends will be in the same amount, according to TIAA's current dividend scale which is not guaranteed.*

Decreasing Term policies provide their largest amount of protection initially, reducing by schedule over the years to recognize diminishing insurance needs and increasing savings, retirement benefits, etc. TIAA issues such policies for 15, 20, 25 and 30 year periods, depending upon age. Decreasing Term insurance is available in amounts of $20,000 or more to persons under age 56.

To use a different illustration,

$100,000 of 5-Year Renewable Term coverage costs only $180 for a 30 year old man or for a 35 year old woman. Here are the cost figures for this policy issued at various ages.

Age at Issue	MALE / FEMALE	25 / 30	**30 / 35**	35 / 40	40 / 45	45 / 50
Annual Premium		$258	**$288**	$373	$530	$774
Cash Dividend End of First Year*		101	**108**	140	185	254
First Year Net Payment		$157	**$180**	$233	$345	$520

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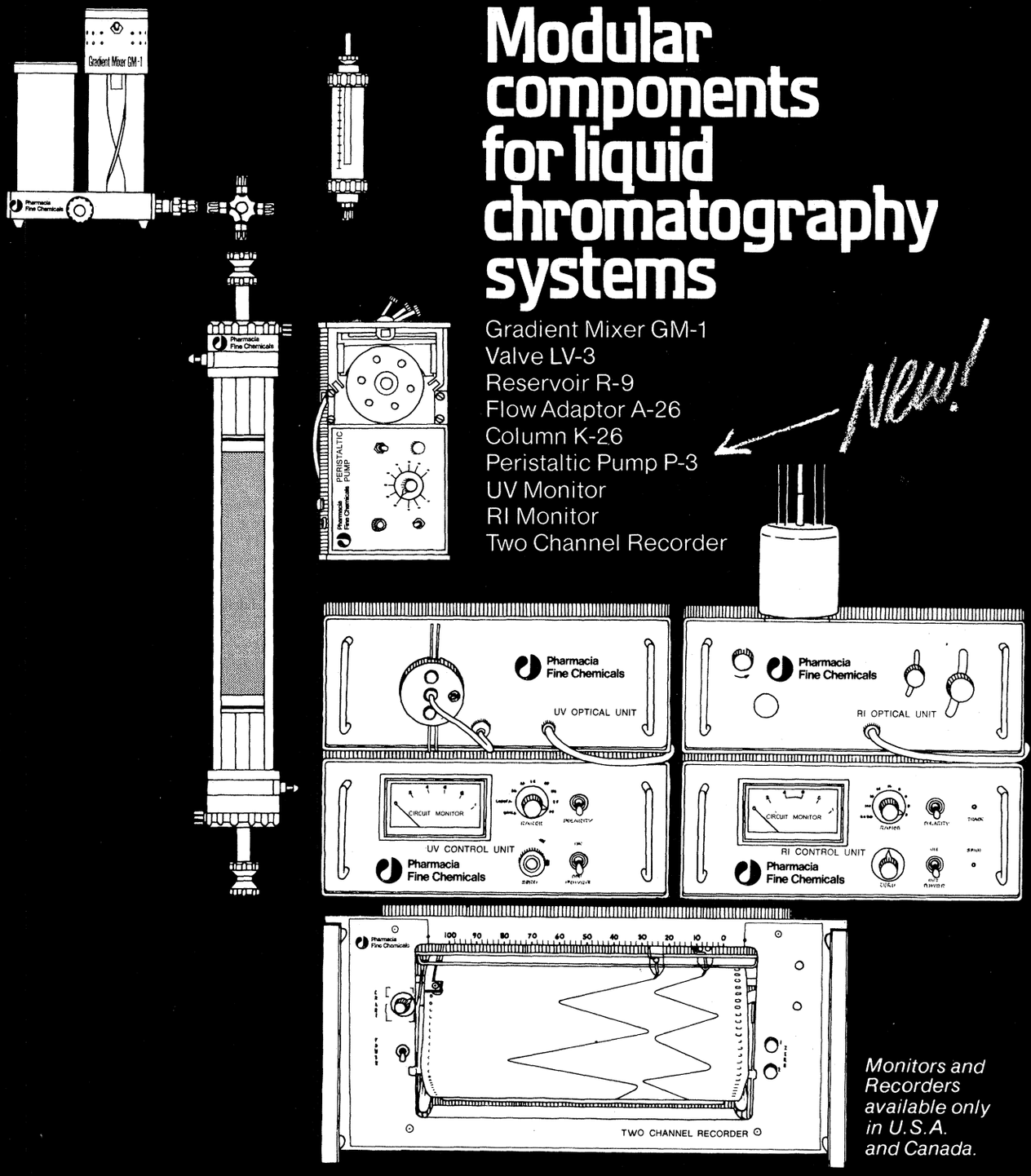

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LETTERS

Nomina Generica

In a report by S. Remillard *et al.* (19 Sept., p. 1002), we read that a potent tumor inhibitor, maytansine, inhibits mitosis. From where does this remarkable substance come? We learn only that it was isolated from "various *Maytenus* species." Is it a mold, beast, or tree? I finally learned (*1*) that the substance can be extracted from the fruit, stem, and wood of *Maytenus*. Aha! Evidently it must be a plant. Aware of my botanical ignorance, I inquired of four botanists the nature of *Maytenus*; none could tell me anything. I eventually learned that it is a member of a group of flowering shrubs or shrub-trees.

No doubt it is essential to have a specific Latin name for each organism, but since more than 1.5×10^6 species of organisms have been identified, a generic designation is often not sufficient, particularly when these names have frequently undergone substantial change. The importance of retaining common names was made clear by George Wald (*2*) when he commented on a table of data on the precipitin test prepared by Nuttall in 1904, which showed that rabbit antiserum against human serum, when mixed with nonhuman serums, caused less and less precipitate the more distantly related was the species of animal providing the serum. Wald wrote:

> In the original version of this table, Nuttall mentions *Cynocephalus mormon* and *sphinx*, omitting their common names. I have learned since that one is the mandrill, the other the guinea baboon. Since Nuttall wrote in 1904, these names have undergone the following vagaries. *Cynocephalus mormon* became *Papio mormon*, otherwise *Papio maimon*, which turned to *Papio sphinx*. This might well have been confused with *Cynocephalus*, now become *Papio, sphinx*, had not the latter meanwhile been turned into *Papio papio*. This danger averted, *Papio sphinx* now became *Mandrillus sphinx*, while *Papio papio* became *Papio comatus*. All I can say to this is, thank heavens one is called the mandrill, the other the guinea baboon.

AUSTEN RIGGS

Department of Zoology, University of Texas, Austin 78712

References

1. S. M. Kupchan, Y. Komoda, W. A. Court, G. J. Thomas, R. M. Smith, A. Karim, C. J. Gilmore, R. C. Haltiwanger, R. F. Bryan, *J. Am. Chem. Soc.* **94**, 1354 (1972). (This reference is incorrectly cited in Remillard *et al.*)
2. G. Wald, in *Modern Trends in Physiology and Biochemistry*, E. S. G. Barron, Ed. (Academic Press, New York, 1952), p. 339.

We have been informed by R. E. Perdue, Jr., the botanist primarily responsible for the *Maytenus* collections in Africa, that the only common name he has found used for any species of *Maytenus* is "ack-ack," reflecting the sound made when the plant is burned as firewood. We appreciate Riggs calling our attention to a typographical error in the citation of a most important article.

S. MORRIS KUPCHAN

Department of Chemistry, University of Virginia, Charlottesville 22901

A New Characteristic of Life?

One of the major problems with the insight that Darwin and Wallace brought to our knowledge of the origin and diversity of life is its simplicity (*1*). The essence of natural selection lies in the notion that adaptiveness is a result of the differential reproduction of individuals of different genotypes. However, many people find the concept of natural selection too straightforward to explain the past and present array of living forms and their manifold means of adaptation. The article by J. M. Burgers (18 July, p. 194) is an example. He, with others, feels that the principle of causal relationship which forms the basis of scientific inquiry is not enough to account for the living world. Or, more simply put, surely life cannot be reduced to a set of interactions among atoms and molecules. It is indeed a distasteful realization, but likely true. Burgers argues that the human "notion of being alive" cannot as yet be explained by modern physical principles. From this he jumps to the statement that "we must give attention to the idea of freedom, as being an essential aspect of life. . . ." Life he defines as a game in which the players maneuver to maintain the ability to choose, to retain "some measure of freedom." Surely we are dealing here with as yet poorly understood psychological aspects of the human mind. Burgers is correct—neither physics nor biology can explain the "notion of being alive," but just because it happens to be a part of human psychic makeup does not render it a basic aspect of all life. There is always the hope that science will eventually lead us to an understanding of even such sacred complexities as our own minds.

In an attempt to bolster his argument for a new conceptual approach to the living world, Burgers appears to have reached some misunderstandings. He feels that, while molecular biology has revealed the structures and operation of living systems, it has added nothing to our understanding of the origin and evolution of these structures. Nothing could be further from reality. Much of what we know of the evolutionary history of many groups of organisms has been gained from a comparative analysis of structures across systematic boundaries. The results of molecular biology research have allowed similar comparisons at a finer resolution. Consider, for

A Photomicrograph of a Newt Triturus Viridescens taken with the new Olympus Series BH System Microscope and automatic camera, Model PM-10-A.

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example, the origin and evolution of cytoplasmic organelles in plants and animals. The current theories (*2*) are based largely on the findings of molecular biology and biochemistry. What about the analyses of the evolution of enzymes and other proteins? A newly emerging field of molecular evolution is based almost entirely on the techniques and data of molecular biology.

Burgers also seems to misunderstand the concept of natural selection. While he is correct in stating that reproduction generates more possibilities for life through the process of genetic recombination and selection, it cannot be considered "a secondary feature in the evolution of life." As I have noted above, differential reproduction is the essence of natural selection and it need not, as Burgers envisions, involve death. Moreover, contrary to Burgers' assertion, the idea of natural selection can be invoked to explain some molecular reactions, such as those involved in the replication of nucleic acids (*3*).

Burgers is insistent that life must possess something more fundamental than the ability to reproduce. He sees a "purposiveness" to the adaptations of organisms that suggests to him that the "evolution of forms of life is dependent on an interplay of traditions and initiatives as is the evolution of human societies." This belief is a more radical statement of the notion of orthogenesis first postulated by Eimer in 1897. Orthogenesis stipulates the gradual unfolding of preformed or rudimentary structures through evolutionary time.

Burgers' basic dislike of the notion of reproduction as an essential characteristic of life is seen in his concluding paragraph. He finds it impossible to imagine that what we call mentality could ever arise from a "chemical structure." The beauty of the process of natural selection is that it is creative; it proceeds in much the same way a work of art is generated—by trial and error and constant remolding. The marvels of the living world are indeed a compelling fascination, and man's infatuation with his own mind has become almost hypnotic. Yet natural selection as a process can still account for all these wonders if we will just give it the opportunity.

ROLLIN C. RICHMOND
Department of Zoology, Indiana University, Bloomington 47401

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It appears that Richmond and I interpret the concept of natural selection differently. Like others, Richmond seems to attach to it an exaggerated reverence, as if it were a hallowed creed. In the last paragraph of his letter he writes: "The beauty of the process of natural selection is that it is creative; it proceeds in much the same way a work of art is generated—by trial and error and constant remolding." In my opinion the words "it is creative" are misplaced here, and the comparison is inappropriate. The trial and remolding which characterize the work of an artist are instances of what I call conceptual activity: the artist tests and remolds his creation so that it is adequate to express a concept formed in his mind. Natural selection, on the other hand, is an effect of the differential permissivity of the environment; no new forms are created, but some are eliminated, in passive secondary reactions to mutations. The creative effect is in the generation of the mutations. Much controversy can be resolved if the term "differential permissivity of the natural environment" is substituted for "natural selection"; this takes away the impression that natural selection can be regarded as an active creative factor.

Natural selection does not in itself produce new chemical compounds; it can only admit them or lead to their rejection. It can do this because of differences in the viability of the living organisms carrying them. I believe that natural selection can operate only in chemical systems where living beings already exist.

In my article I attempted to sort out processes in which conceptual activity and creativity are involved from those depending on causal relations (among which we may reckon death from predators). Perhaps Richmond and I are using different languages. I hope that my remarks may bring some clarification. I add that I do not dislike the notion of reproduction; I only suggest that it has evolved at a later stage in the history of life.

Albert Claude says in his Nobel lecture "The coming of age of the cell" published in *Science* (8 Aug., p. 433): "Life, this anti-entropy, ceaselessly reloaded with energy, is a climbing force, toward order amidst chaos, toward light among the darkness of the indefinite, toward the mystic dream of love, between the fire which devours itself and the silence of the cold." In a poetical form, this expresses some of the ideas that I tried to discuss in my article.

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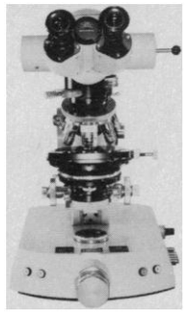

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14 November 1975, Volume 190, Number 4215 **SCIENCE**

Peer Review and the Structure of Science

In recent months, the process of peer review of scientific projects has been much in the public eye. All sorts of questions have come up: Is peer review fair? Does it provide for the support of the best science? Can it recognize potential breakthroughs? Are the reviewers chosen well? Do they respond objectively? Such questions as these do not have one simple answer because they really refer not just to the immediate issue of how peer review works, but to the structure and nature of science in general and in particular. On closer examination, there are many different versions of peer review, each adjusted to apply to the science at issue. All of these versions have one purpose: To help decide how the limited funds available for the support of science can best be spent to advance both science itself and the national purposes to which it contributes. Science is a complex and multifaceted attack on the unknown. Guesses as to how it will work can most accurately be made by people who have themselves succeeded in such attacks on the unknown. These people are the peers.

Which peers? In a recent hearing it was proposed that some peer reviewers be picked "at random" from some list. This would tend to produce some reviewers who are not knowledgeable about the proposed projects. For many scientific problems there are only a few peers—perhaps only one or two—who really understand the technicalities at issue and the possible outcomes. Thus, reviewers must be chosen by knowledgeable people. Because of the wide variety of expertise needed—often much more than can be found on any one panel of reviewers—many branches of science require written reviews. This way is more likely to obtain a perceptive, well-informed review for each project.

What breakthroughs? At recent congressional hearings a number of witnesses expressed concern that the present peer review system might be biased against really innovative proposals, those which go against the general opinion in the field and which could lead to major conceptual changes. Now, it is clear that scientists, and in particular peer reviewers, should be alert to recognize potential breakthroughs. This is by no means easy. It is also clear that some ideas claimed to be potential breakthroughs are nothing of the sort. The hearings brought out no hard evidence that any really decisive breakthrough ideas had been ignored. They did bring out various complaints. This is hardly surprising at a time of shrinking budgets, when it is certain that some solid proposals will be passed over.

Which panels? Some commentators have urged that most peer review be done by reviewing panels in regular meetings. The panel device is one that works well in certain fields, such as in some of the biological sciences, where it has long been the practice for the National Institutes of Health to make extensive use of study groups. In big science, where the choice is between several large projects, panels may be necessary because of the size and complexity of the financial decision to be made. However in other fields panels just do not work well, in part because many different and intricate specialties are involved. With many small proposals, each of which requires particular expertise, there is really no way to assemble a panel of reasonable size with the requisite experts to cover everything. For that matter, it is by no means clear that the most incisive judges of the merits of a particular new idea are identical with those who are willing to come to Washington for a panel meeting.

Ultimately, peer review has the task of picking which scientific projects to support. Getting the right projects is not (as sometimes claimed) a question of openness, nor should it be one of responsiveness to political issues. It is fundamentally a choice between projects on the basis of promised quality. Comparable choices arise for scientific journals. There the knowledgeable editors pick the appropriate expert (and usually anonymous) referees to judge the merits of manuscripts. That method of choosing referees has played an essential and effective role in the development of science. For referees, as for peer reviewers, the system depends on individual judgments of quality.

—SAUNDERS MACLANE, *Department of Mathematics, University of Chicago, Chicago, Illinois 60637*

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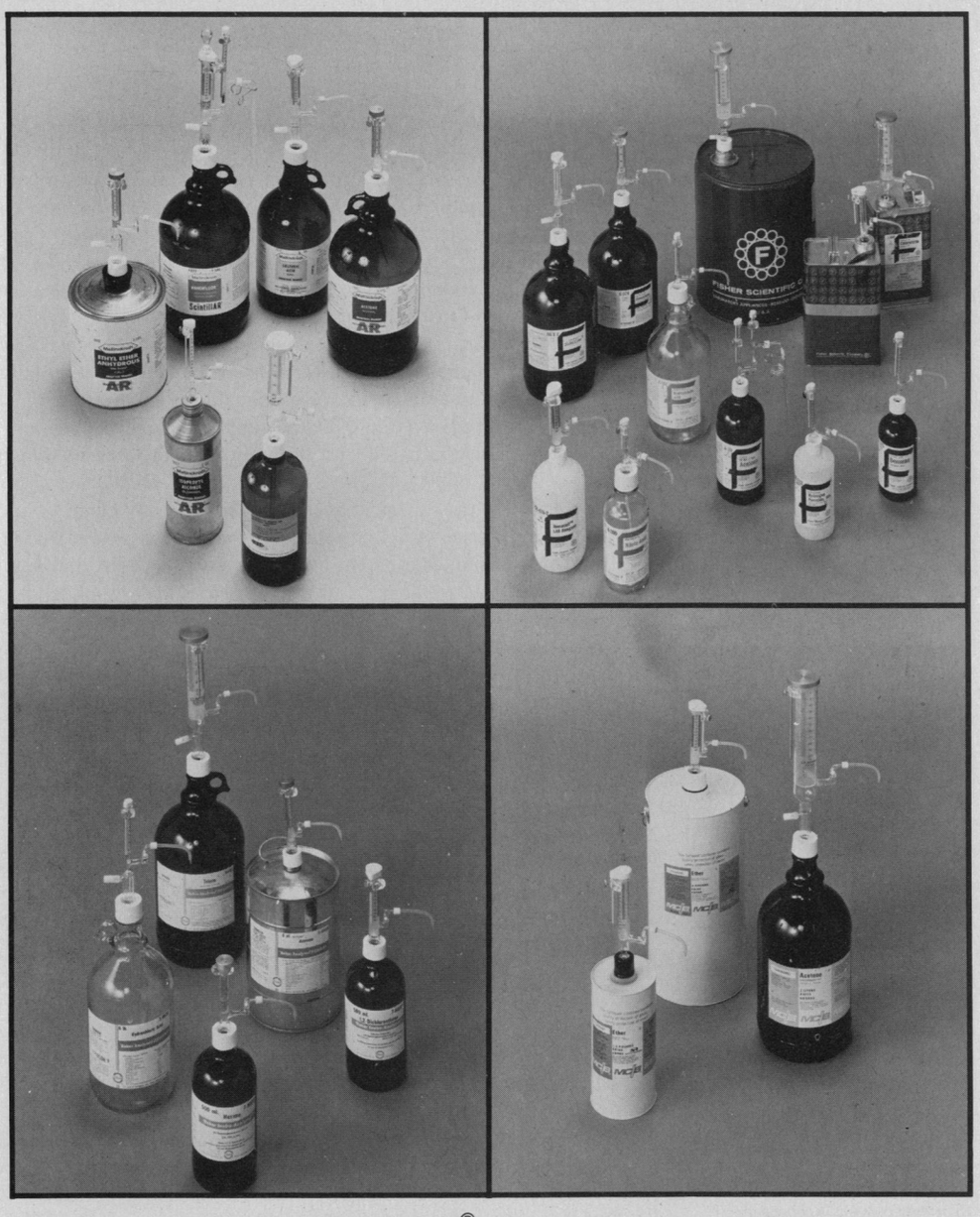

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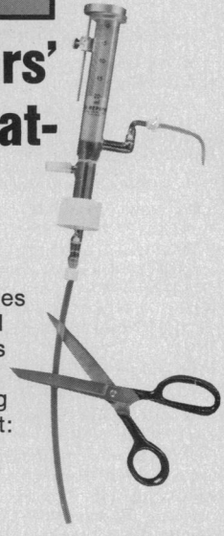

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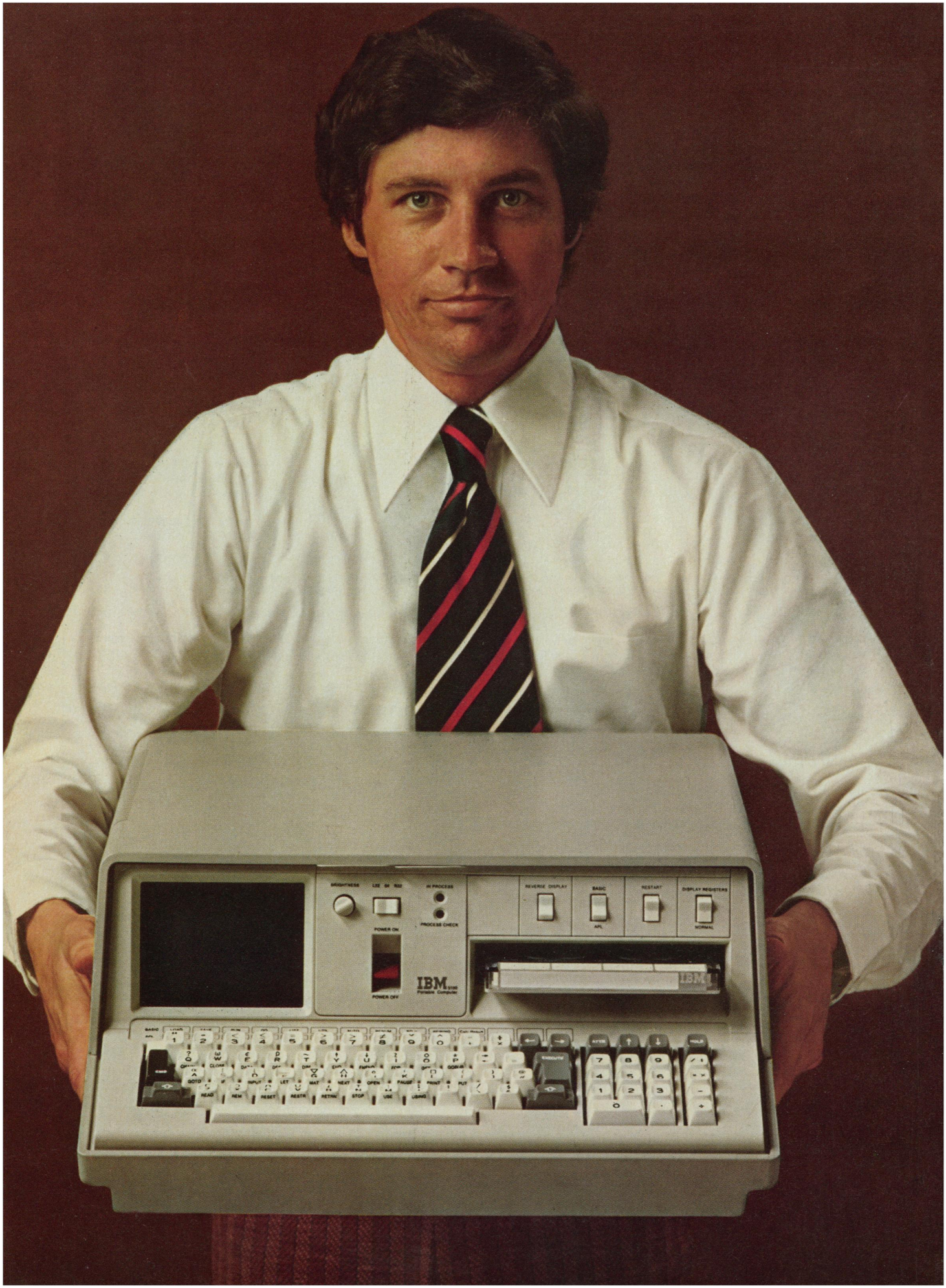
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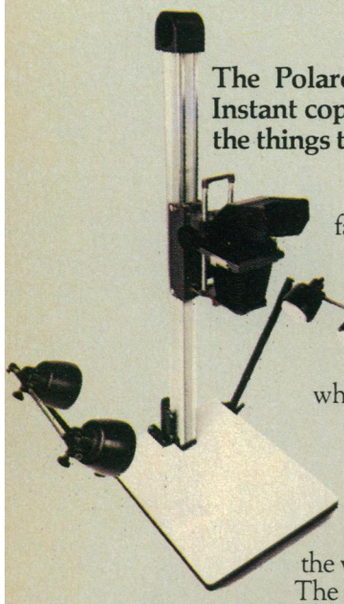

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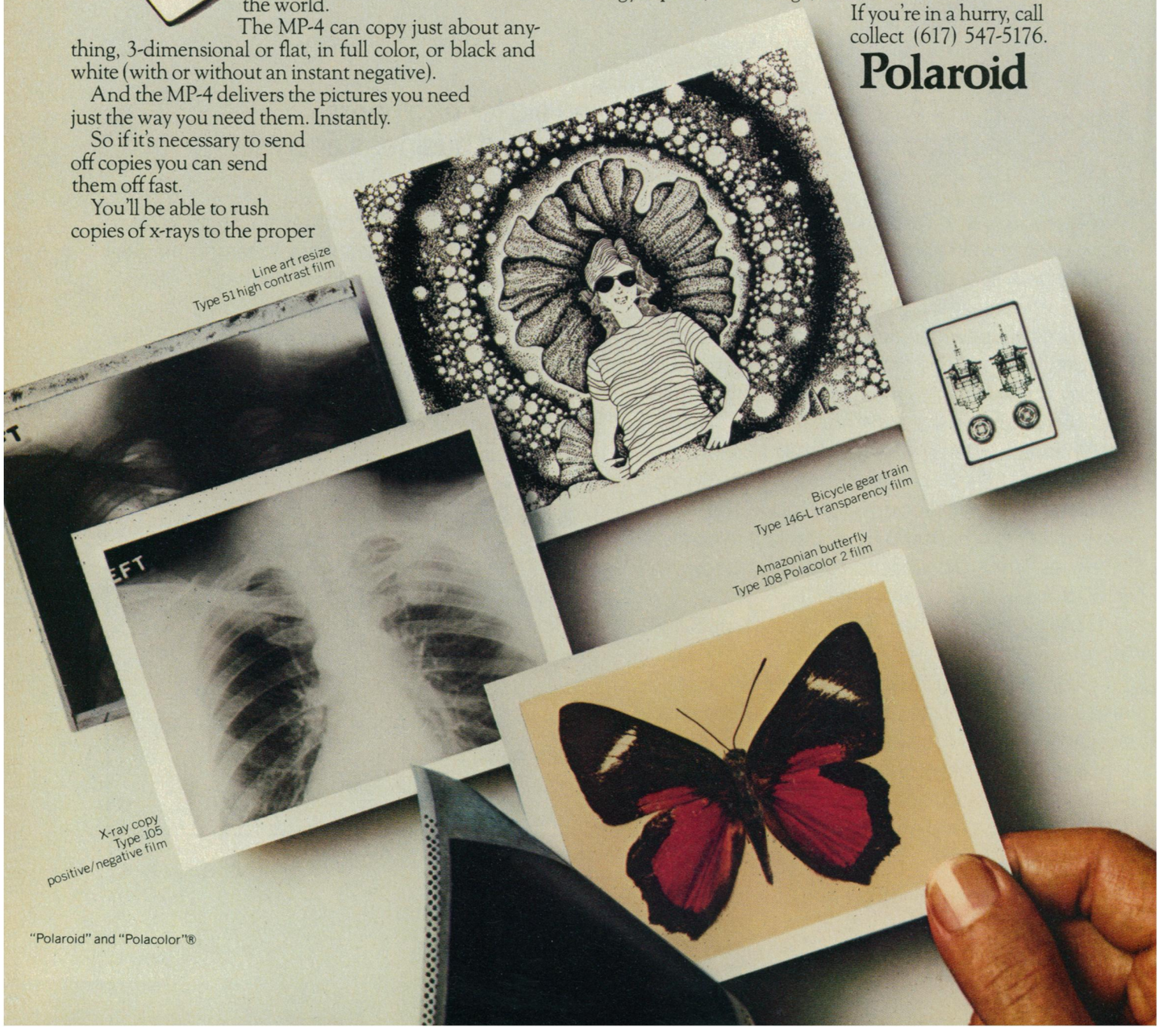

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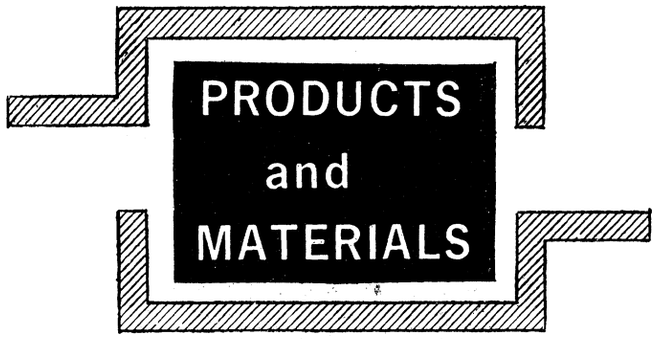

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Temperature Measurement and Recording Instruments lists specifications, applications, and design features of a line of thermometers and recorders. William Wahl. Circle 791.

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(Continued from page 668)

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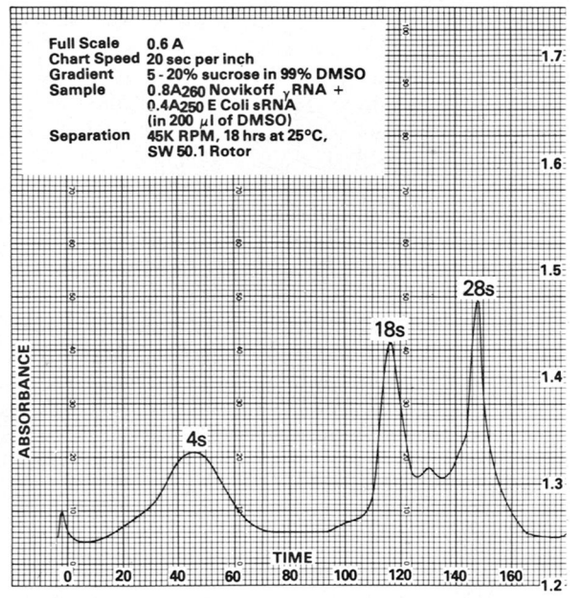

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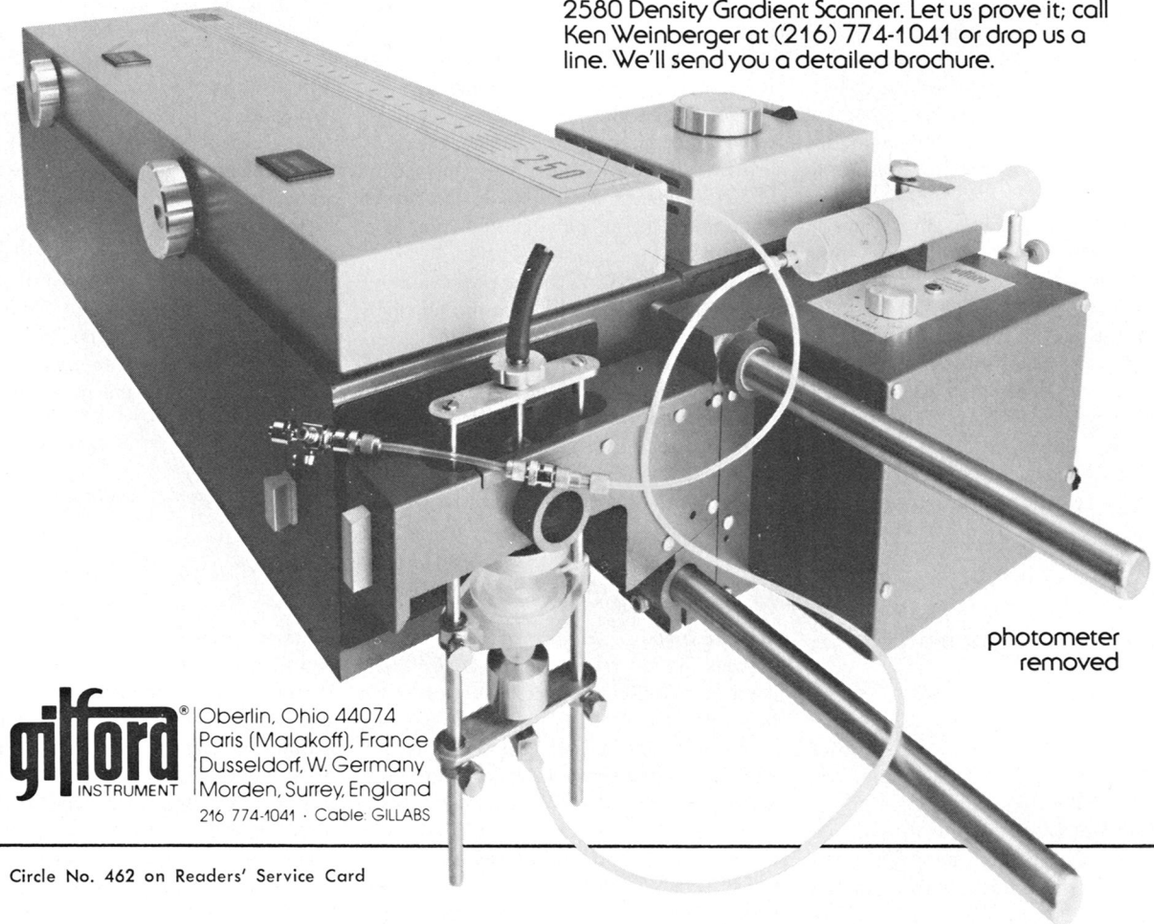

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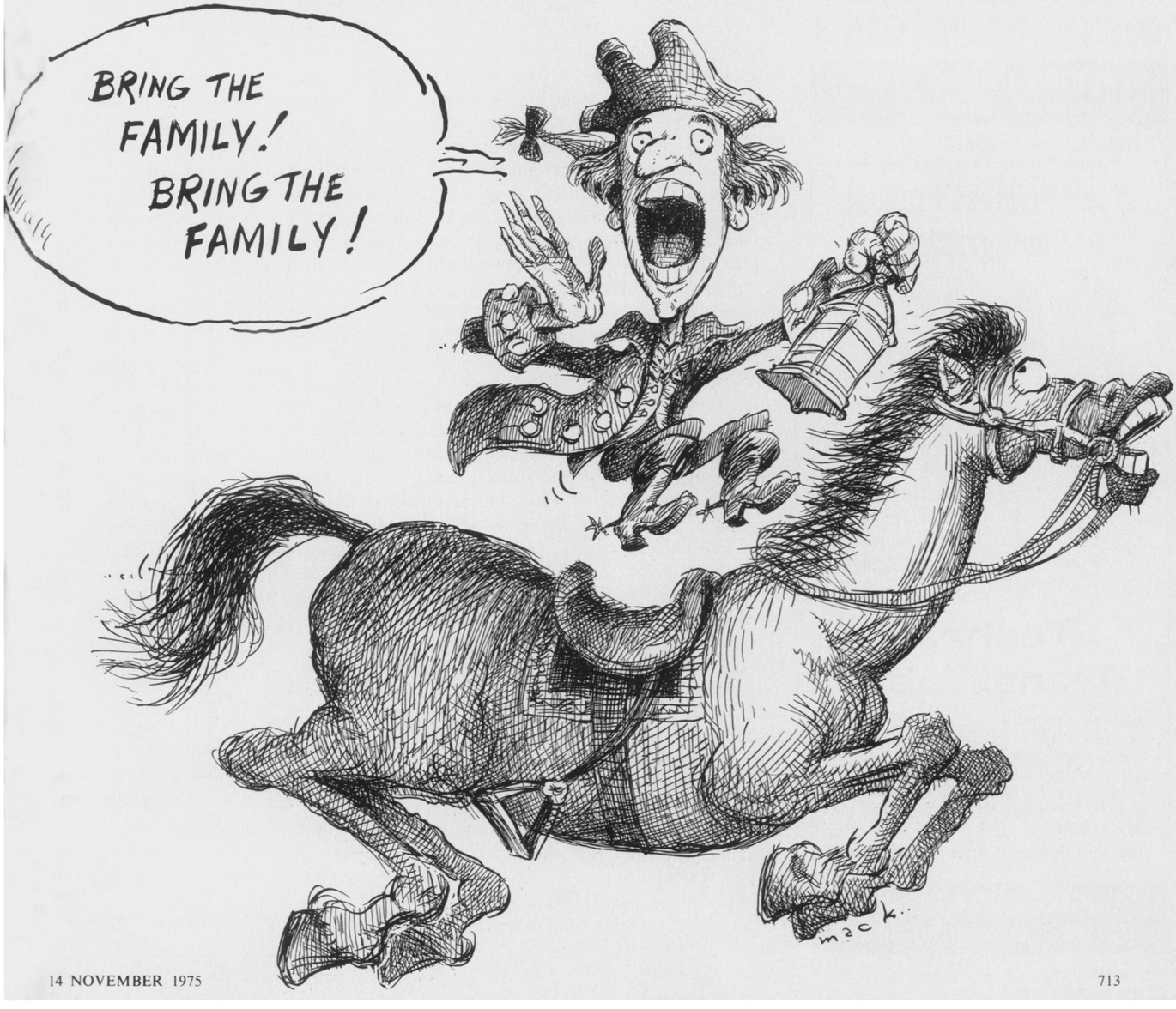

sis, would block RSV infection, even though the genome of RSV was a single-stranded RNA. To account for these observations, Temin proposed in 1964 that a DNA intermediate is involved in RSV infection. This "provirus hypothesis" envisioned that after infection a DNA provirus was synthesized which contained all of the genetic information of the RNA viral genome. Progeny viral RNA would be synthesized from the provirus, thereby accounting for the sensitivity of infection to inhibitors of both DNA synthesis and DNA-dependent RNA synthesis. If the provirus were integrated into the cellular genome, the stable inheritance of RSV transformation could be explained.

The provirus hypothesis did not achieve instant popularity. The evidence for the idea was indirect, and much of it relied on studies in which inhibitors, which were widely suspected of producing artifacts, were used. Moreover, the hypothesis conflicted with the scheme that had become known as the central dogma of molecular biology. The dogma specified that the information of DNA was first transcribed into RNA and then into proteins. People had come to expect that information flow would not occur in the reverse direction, that is, from RNA to DNA. However, Temin and John Bader independently accumulated additional evidence over the next 5 years implicating DNA synthesis in RSV infection, but made few inroads into the skepticism of their colleagues. Then, in 1970, Temin and Satoshi Mizutani did an experiment that changed the climate of opinion dramatically. They showed that the virions of RSV contain an enzyme that can transcribe the single-stranded virion RNA into DNA. This evidence, obtained simultaneously by Baltimore, using Rauscher murine leukemia virus and RSV, provided a mechanism for the formation of a DNA intermediate during RNA tumor virus infection and transformation, and clothed Temin's original hypothesis in respectability.

David Baltimore was trained as an animal virologist, working with Richard Franklin and James Darnell at the Rockefeller Institute and at the Massachusetts Institute of Technology. In 1965 he moved to La Jolla to join the virology group at the Salk Institute with Dulbecco. There he studied the mechanism of replication of poliovirus, making rapid progress in characterizing the replication of the viral RNA and the synthesis of the viral proteins. In 1967 Alice Huang joined the laboratory as a postdoctoral fellow. She had studied with Robert Wagner, doing much of the pioneering work on vesicular stomatitis virus (VSV) and its defective interfering particles.

In 1968 Baltimore moved to MIT, and Huang went with him to Boston, where they were married. Baltimore continued his work with poliovirus, and Huang resumed her work with VSV. They were led to consider the problems facing an RNA virus whose genome is complementary to its messenger RNA. In order to replicate, such a virus would have to use a cellular RNA-dependent RNA polymerase (for which there was no evidence) or would have to contain a virus-coded polymerase in its virion. Baltimore, Huang, and M. Stampfer showed that VSV particles did indeed contain an RNA-dependent RNA polymerase. Baltimore, with his broad interests in the biology of animal viruses, immediately decided to test for the presence of a polymerase in the virions of RNA tumor viruses. Because his laboratory was not equipped to grow large quantities of RNA tumor viruses, he used a sample of Rauscher murine leukemia virus provided by the National Cancer Institute and demonstrated the presence of an RNA-dependent DNA polymerase in these virions and in RSV.

The RNA-dependent DNA polymerases of RNA tumor viruses have quickly become standard tools for cell biologists. They are used to prepare radioactive DNA probes to detect viral genetic information in normal and malignant cells. They are used to prepare DNA copies of messenger RNA's which can be isolated and purified. Whether they play a role in cellular differentiation or development is not clear, but their role in the life cycle of RNA tumor viruses has been firmly established.

The awarding of the Nobel Prize to Dulbecco, Temin, and Baltimore marks the convergence of lines of research which at one time were thought to be quite separate. It now seems clear that the DNA tumor viruses and the RNA tumor viruses operate, at least in part, by a common pathway. They become part of the genetic material of the cells they transform. To what degree they resemble each other in the mechanisms by which they affect cell growth regulation is a fascinating question for the future.

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Articles on the Nobel Prize for Chemistry and the Nobel Prize for Physics will appear in subsequent issues of Science.