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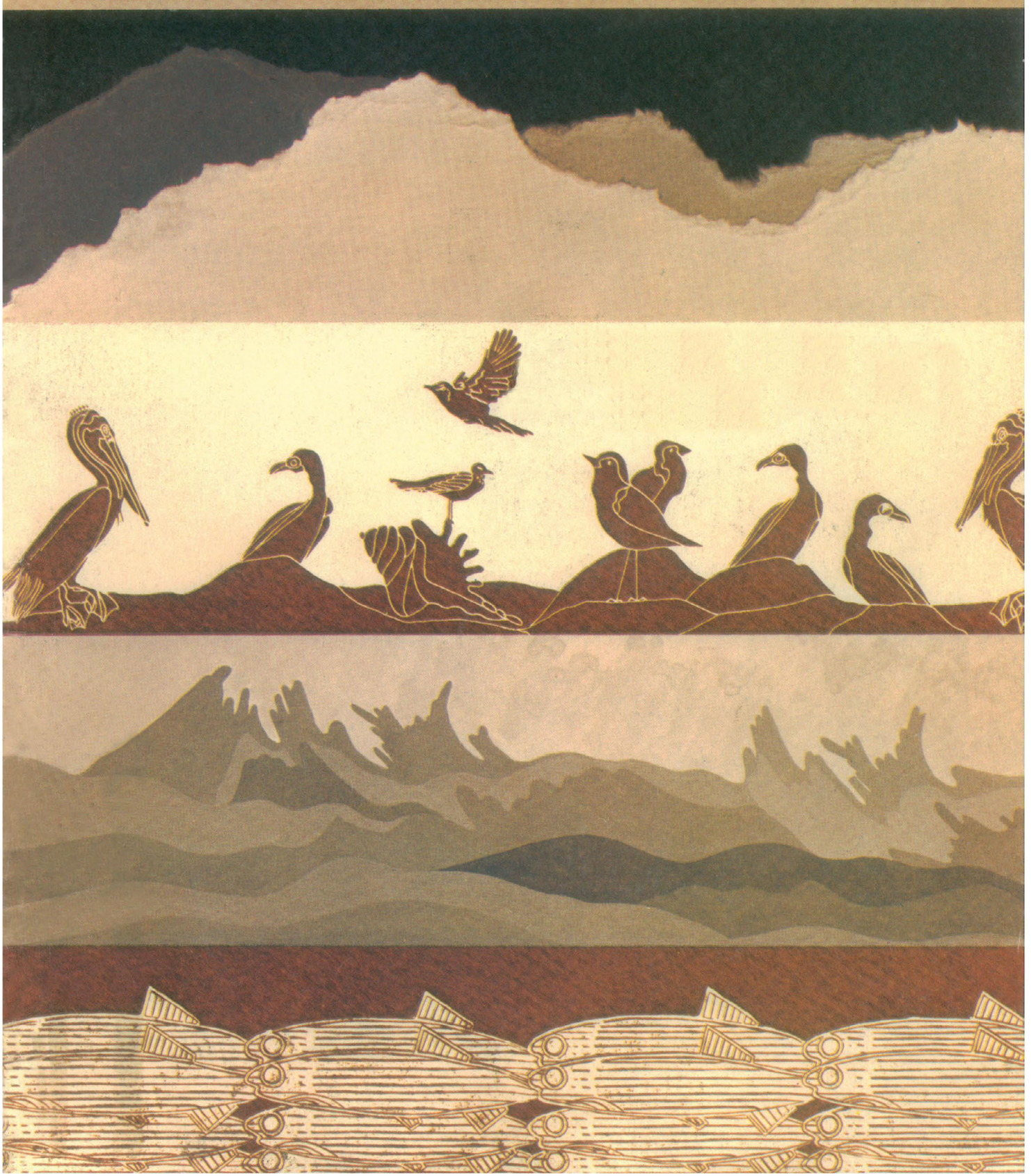

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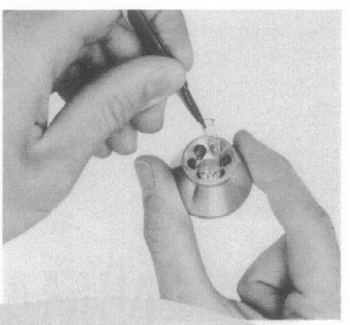

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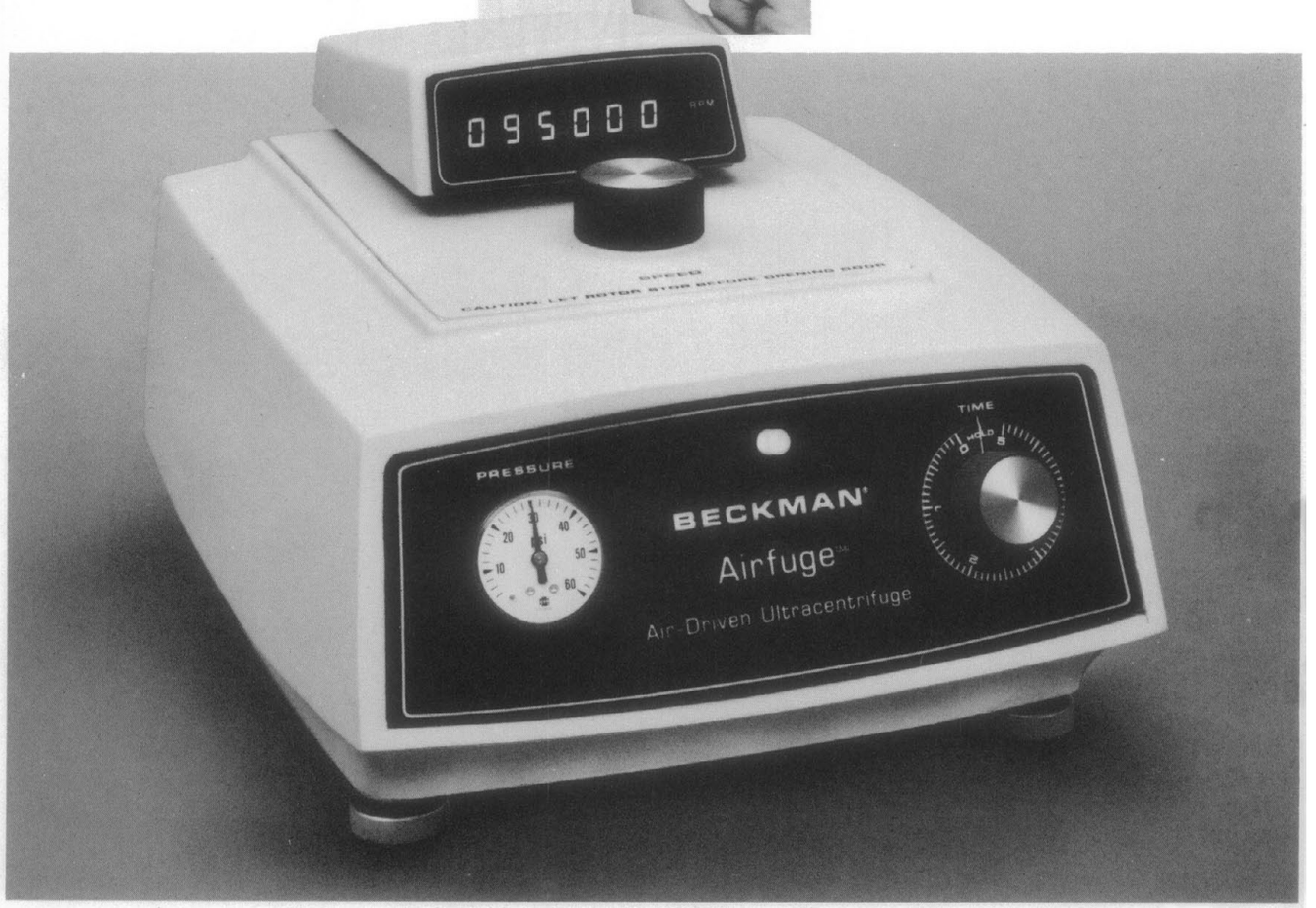

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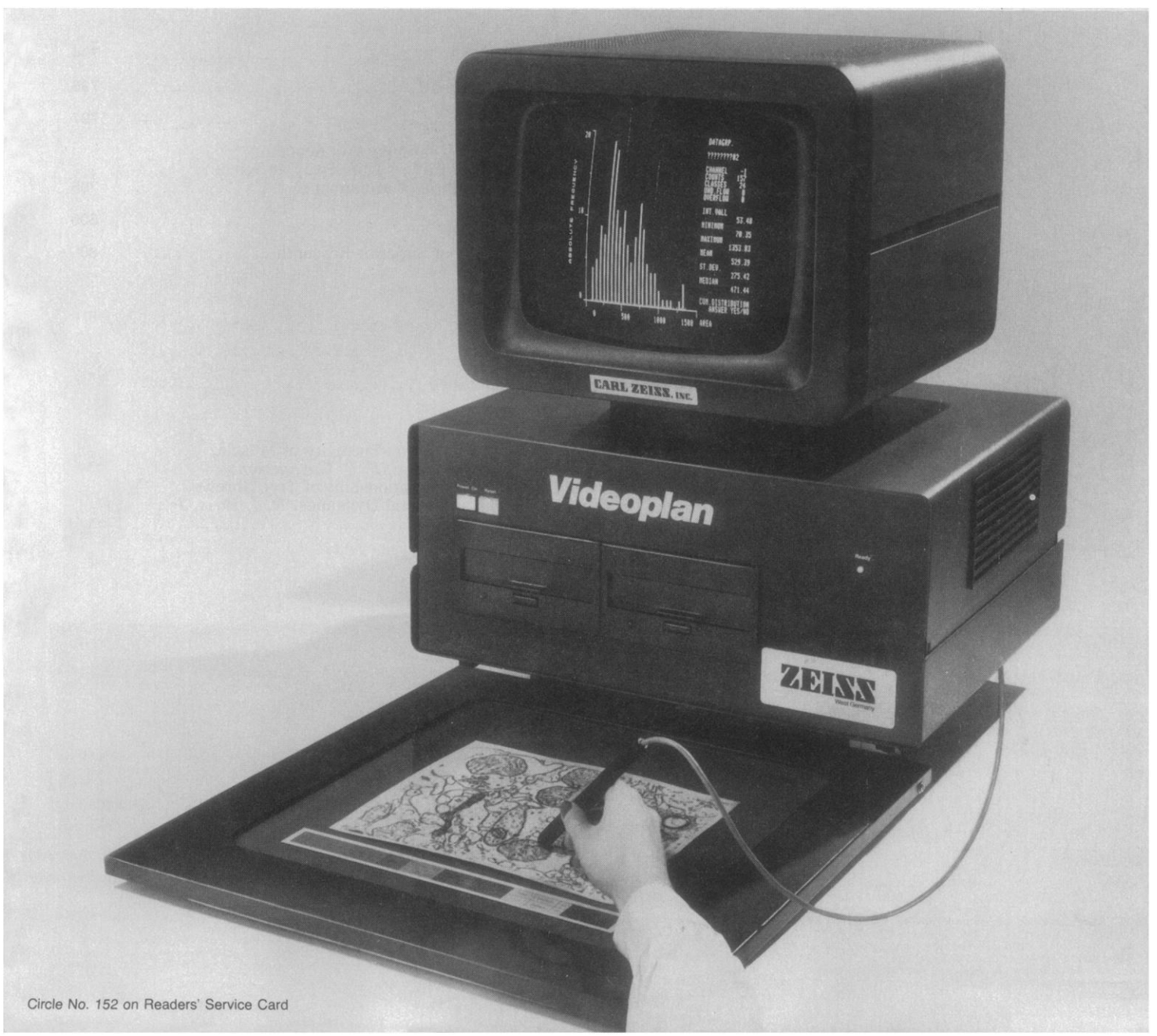

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Graphic representation of large-scale fluctuations in the ocean and atmosphere referred to as El Niño. The event is characterized by excessively warm ocean water (fourth panel) off the coast of Peru (second panel). During El Niño warm water accumulation is excessive, upwelling ceases completely, and large anchovy fishery (bottom panel) almost ceases. Coastal birds (middle panel), which depend on fish for food, die in large numbers. This natural catastrophe is linked to large-scale variations in the tropical atmosphere (upper panel). See *Science*, 2 January, page 22. [Design by W. C. Patzert, Scripps Institution of Oceanography, La Jolla, and Calvin Woo, HUMANIGRAPHIC, San Diego.]

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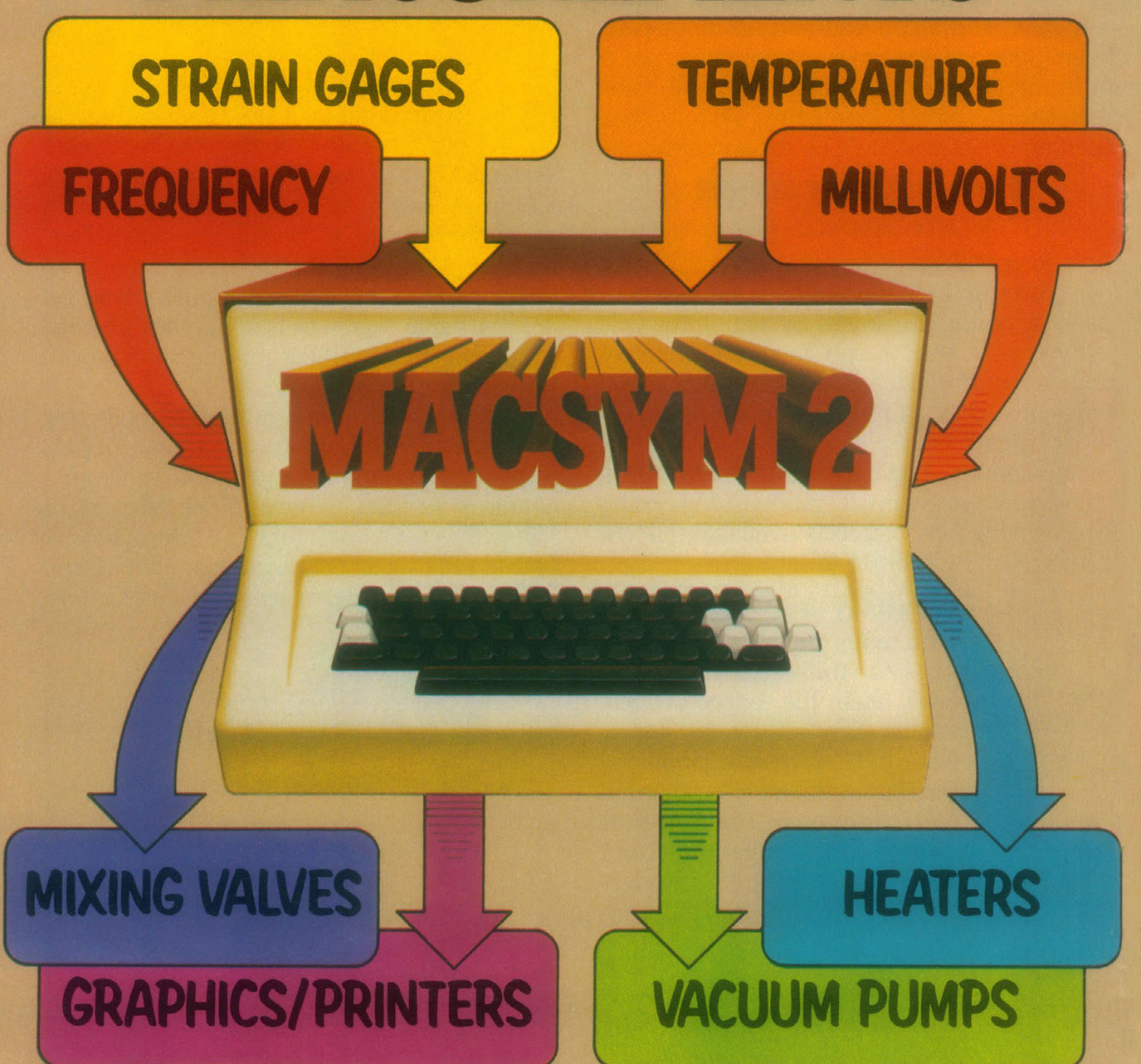

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90 IF X<5 DOT (1,1)=0
100 WAIT.5 GO TO 60
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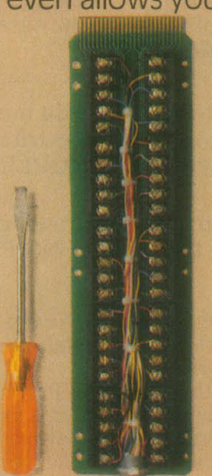

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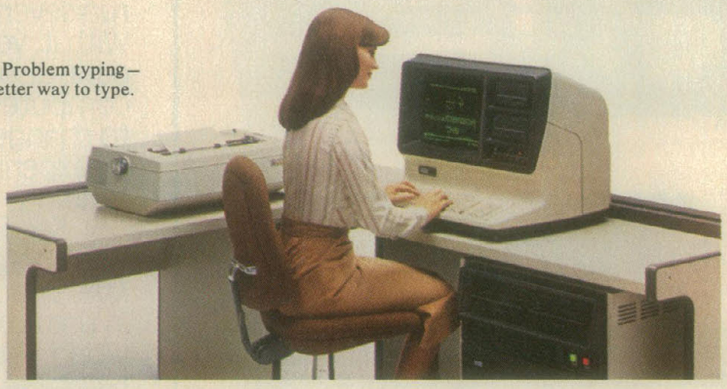

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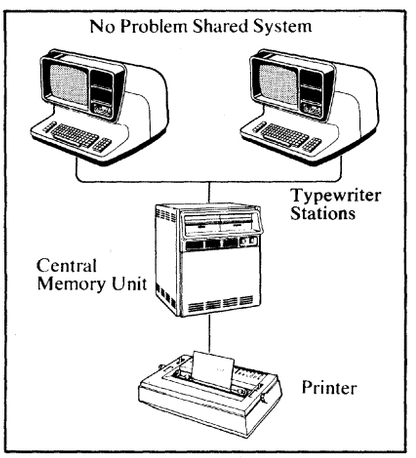

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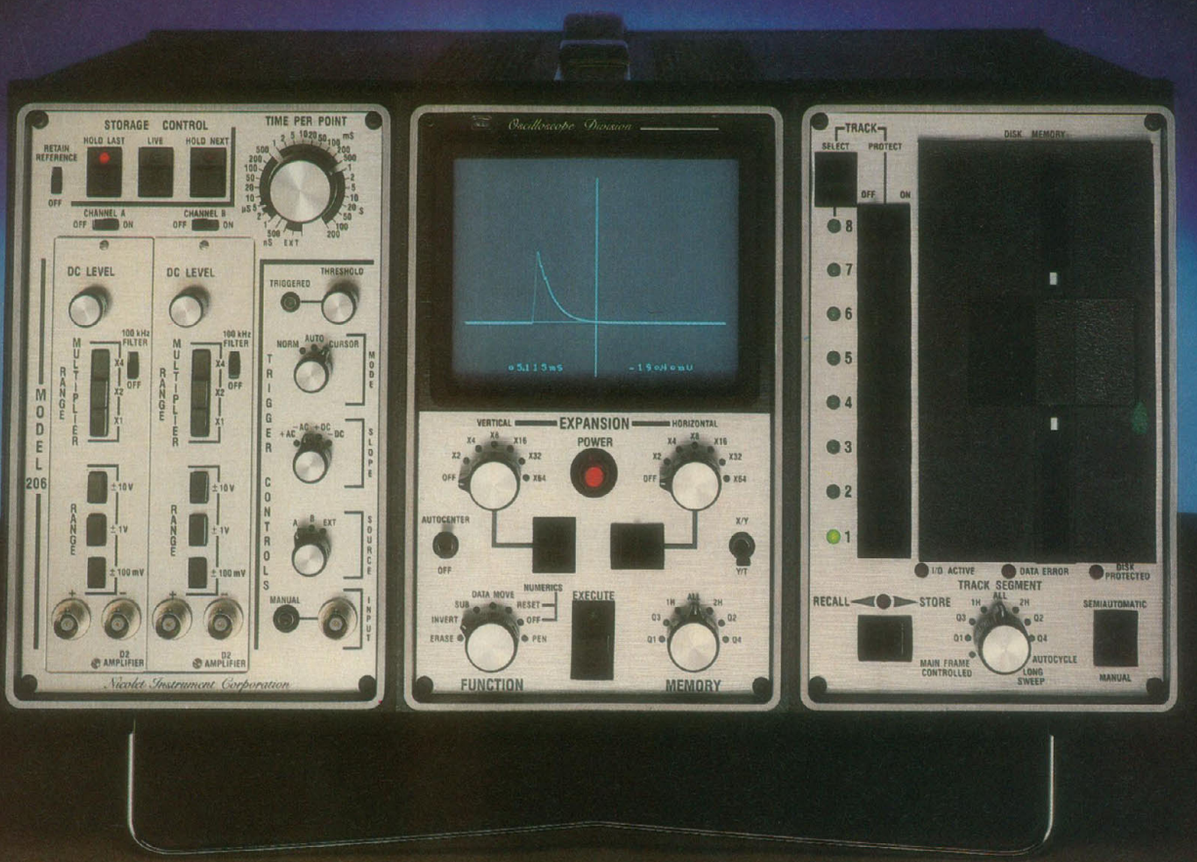

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explanation for adaptive evolution and predicted that adaptive evolution would be characterized by periods of stasis interspersed with episodes of rapid adaptive transition. He also explicitly discussed the macroevolutionary implications of his theories, which are quite incompatible with the stereotyped "Modern Synthesis" presented by Lewin and others.

In summary, the macroevolution meeting at Chicago was not so much an historic challenge to evolutionary theory as it was a challenge to the history of evolutionary theory.

ALAN R. TEMPLETON
L. VAL GIDDINGS
Department of Biology, Washington University, St. Louis, Missouri 63130

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1. C. Darwin, *The Origin of the Species* (Modern Library-Random House, New York, 1936).
2. R. A. Fisher, *The Genetical Theory of Natural Selection* (Dover, New York, 1958), p. 153.
3. J. B. S. Haldane, *Am. Nat.* **71**, 337 (1937).
4. S. Wright, *Proc. Am. Philos. Soc.* **93**, 471 (1949).

Reporters at the Chicago macroevolution conference, including Lewin, apparently missed what was really happening there. The fossil record says eloquently that profuse evolution has indeed occurred over millions of years, but the data just aren't sensitive enough to analyze evolutionary kinetics. This is the province of the evolutionary geneticist who works with descent and change in populations of present-day organisms. Very simply, we have abundantly demonstrated that evolution can be *either* jerky *or* gradual depending on the circumstances and the genes concerned. So what is all the fuss about? Forty years ago, the modern followers of Darwin (Fisher, Haldane, Wright, Dobzhansky, and Mayr) stole the evolutionary spotlight from the paleontologists. This conference saw an attempt by a few fossil zealots who are able to charm reporters to regain attention. Most unfortunately, the ideas they used have neither data base nor innovation.

HAMPTON L. CARSON
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Lewin summarizes a complex series of formal papers and discussions. As he notes, my particular views on the fossil record as a source for data on certain aspects of macroevolution were not to the liking of some of my paleontological colleagues. In the brief comments on this point, however, my position seems to come through as an intonater of the "ancient lament" on the incompleteness of the fossil record. The level of incompleteness of which I spoke can be improperly inferred from the following quoted sentence: "I take a dim view of the fossil record as a source of data" and the reply by John Sepkoski: "I'm tired of hearing about the imperfections of the fossil record." Although tiredness may hardly be a basis for rejection, I believe that both of these remarks, appearing out of context, fail to carry the sense of what was meant and I should like to clarify this.

My presentation was an affirmation of my conviction that evolution must proceed with continuity, involving the derivation of new species from antecedent species. Three hypotheses of the models of the derivation of new species were presented:

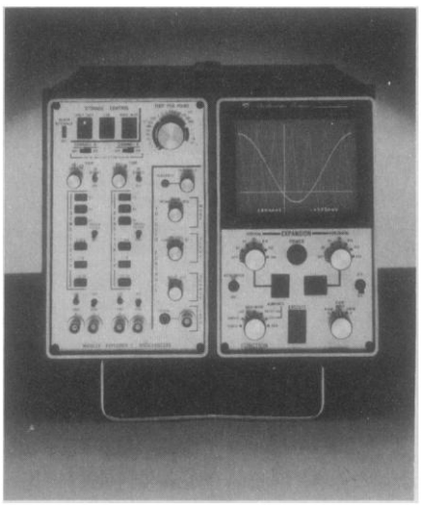
1) Phyletic gradualism, by gradual accumulation of small changes (Darwinian);

2) Punctuated or stepped speciation, with moderate morphological disjunction between antecedent species (as punctuated equilibria); and

3) Speciation with major disjunctions between antecedent and descendant species (mechanism unspecified).

I maintained that by and large the fossil record does not provide data necessary to establish an equivalency between "fossil" species and "living" species. If this is the case, it is difficult and misleading to infer microevolutionary changes from the temporal or geographic sequences in the record at both the infra- or interspecies level. Although both the first and second hypotheses may, and in fact likely do, express modes that exist, the fossil record itself is insufficient to falsify either one. This is the level of incompleteness with which I was concerned, far from denying the value of the fossil record in other aspects of evolutionary investigation. The third hypothesis is supportable in general if data are less than critically analyzed, but can be falsified by many particular instances as long as morphospecies are accepted as a sufficient basis for interpretation, which I consider to be the case at this level. The hypothesis cannot, however, be totally falsified by the contradictory cases alone, both because the record is insufficient for such a generalization and because it is by no means clear that only a single mode of change exists. Biological investigation of existing species can, I believe, provide a more adequate basis for support or falsification of this hypothesis.

Finally, the matter of "species" stasis, a subject of the conference, is sup-



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ported by a great many well-documented cases in the fossil record, if what is meant is the stability of morphospecies over long periods of time. Rarely, however, are the data of the record sufficient for interpretation of microevolutionary changes within these lineages or determination of such consequences as physiological, reproductive, or mechanical changes and similar modifications which may affect functional, populational, and ecological aspects of the morphospecies of concern. It was an exhortation to reasonable caution in these directions that was the thrust of my remarks. I do not, in fact, think that a great gulf exists between me and my colleagues on this matter, nor have I been, as might be inferred from Lewin's article, a diehard proponent of evolutionary gradualism or sufficiency of explanation by synthetic theory.

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Thank you for Lewin's "Evolutionary theory under fire," a fine article that vividly describes the self-correcting manner by which scientific knowledge progresses. The choice of title, while obviously designed to draw attention to the proceedings of an important symposium, is unfortunate because it suggests that evolution is being challenged instead of pointing to the reevaluation of the mechanisms by which organic evolution proceeds. As a result, this article is undoubtedly destined to enter the out-of-context arsenal that has become a mainstay of recent creationist literature.

We are sure the creationists will be delighted to have an opportunity to cite *Science* in apparent support of their cause.

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Lasers in Space

In Nicholas Wade's briefing (News and Comment, 9 Jan., p. 148) about my recent study (with Kosta Tsipis) of laser weapons (*1*), I was identified as an "MIT physicist." Although I was at the Massachusetts Institute of Technology when the study began, since September 1979, I have been at Carnegie-Mellon University.

Although Wade's review accurately summarizes selected points of our report, his comment that a carbon dioxide laser is the Pentagon's leading candidate for a space-based laser weapon deserves clarification. We did not make such a statement. In fact, we did not discuss Department of Defense plans at all; instead, we discussed general constraints applicable to all laser weapons, and we postulated and criticized several weapons and scenarios of our own invention.

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References

1. M. Callaham and K. Tsipis, *High Energy Laser Weapons—a Technical Assessment* (Department of Physics, Massachusetts Institute of Technology, Cambridge, 1980).

Perhaps I am prejudiced because I was employed at Avco Everett Research Laboratory in Massachusetts where a breakthrough to truly high-power lasers was made, but I must take exception to Wade's caustic "A cooler look at laser weapons."

While it is true that atmospheric propagation problems may hinder the use of laser weapons on the battlefield or at sea, their use in space is another matter entirely. Laser or particle beam weapons may be effective defenses against ballistic missiles and may provide a defense against the hydrogen bomb terror that we have faced since such missiles became operational.

There are severe technical difficulties in fielding a network of operational laser-armed antiballistic missile satellites, to be sure. But if current trends are followed, such weapons systems may be undergoing orbital tests before the decade is out. Certainly they will be tested by the Soviet Union, which has shown a continuing interest in developing space-based weaponry, such as their antisatellite systems, and in bringing such weaponry into use. . . .

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Erratum: In the article by C. E. Land (12 Sept. 1980, p. 1197), the labels for the curves P(negative estimate) and Power in the left-hand panel of Fig. 1 (p. 1199) should be reversed; the data sources for table 1 (p. 1200) should have been given as (*19, 25*).

Erratum: Photomicrographs of normal and sickled erythrocytes that were published in the 30 January issue (Research News, p. 469) should have been credited to the laboratory of Patricia Farnsworth, Department of Physiology, New Jersey Medical School, Newark. The photos were taken by graduate student Patricia A. Burke.

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Laboratory Safety

Virtually every teaching and research laboratory conducting studies in the natural sciences does so with some measure of risk to personnel. The principal hazards vary from field to field, but there is a widespread use of chemicals, some of which are toxic. Fortunately, the dangers can be minimized if some simple techniques are followed. The procedures are discussed at length in a new report of the National Research Council (NRC). Highlights of the report are presented (page 777) in this issue of *Science*.

At one time chemists were often exposed to many chemicals. This was particularly true in academic institutions, where the halls of chemistry buildings usually reeked. But times are changing, and chemistry departments in many universities are cleaning up their act. In this effort they are years behind the major chemical companies. I have visited chemical laboratories in five major companies and in none of them could I detect odors of chemicals. The reason was proper ventilation; chemical operations involving volatile substances were conducted in hoods. Besides exposure through the respiratory system, chemicals can enter the body through the skin or the mouth. Use of gloves and protective clothing can minimize entry through the skin. Personal hygiene, avoidance of mouth pipetting, and use of common sense can prevent entry through the mouth.

Among academic chemists, awareness of potential hazards is leading to changes in laboratory practices both in research and in classwork. Use of properly functioning hoods is becoming more widespread. Student experiments are being changed to use smaller amounts of reagents and to avoid use of toxic chemicals. Substances such as benzene and carbon tetrachloride are disappearing from reagent shelves. Safety officers are being designated to monitor practices in laboratories. Lectures on chemical hazards are being given, and examinations are including questions on safety. In other laboratories, where nonchemists are working, there are fewer facilities for coping with chemical hazards. Means of ventilation, and particularly hoods, are sometimes inadequate. There is also often less knowledge about the toxic properties of various substances.

In the current climate of litigiousness, all organizations dealing with chemicals face dangers of costly suits. These can be minimized if prudent practices are implemented. At present, uniform regulations do not exist, but the Occupational Safety and Health Administration (OSHA) is likely to issue regulations to laboratories soon. Federal and state-supported laboratories are exempt from standards mandated by OSHA and will formulate their own, often differing, regulations. However, OSHA standards will doubtless influence others.

In formulating its policies, OSHA has been constructively influenced by the NRC report. In turn, the motivation for preparing the report was what scientists perceived as a threat to scientific research in this country.* Starting in 1977, OSHA had begun to engage in controversial activities and examples of misuse of its power emerged. The quality of information released by OSHA at that time indicated an absence of competence in the agency to deal with chemical matters. Moreover, the top administrator, Eula Bingham, did not respond to offers of assistance from Philip Handler, the president of the National Academy of Sciences. Scientists feared that unrealistic regulations would be promulgated. In particular, there was concern that OSHA would impose on university laboratories the kinds of regulations that would be issued for production plants where workers are exposed to a chemical 40 hours a week, year after year.

An OSHA comment in the *Federal Register* on 22 January 1980 indicates that the agency now recognizes that there may be a difference between university laboratories and production plants. When OSHA publishes its regulations scientists can determine how much the agency has learned during the past 4 years. If it has improved its posture, at least part of the credit will be due to the NRC report.—PHILIP H. ABELSON

*P. H. Abelson, *Science*, 13 October 1978, p. 139.

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