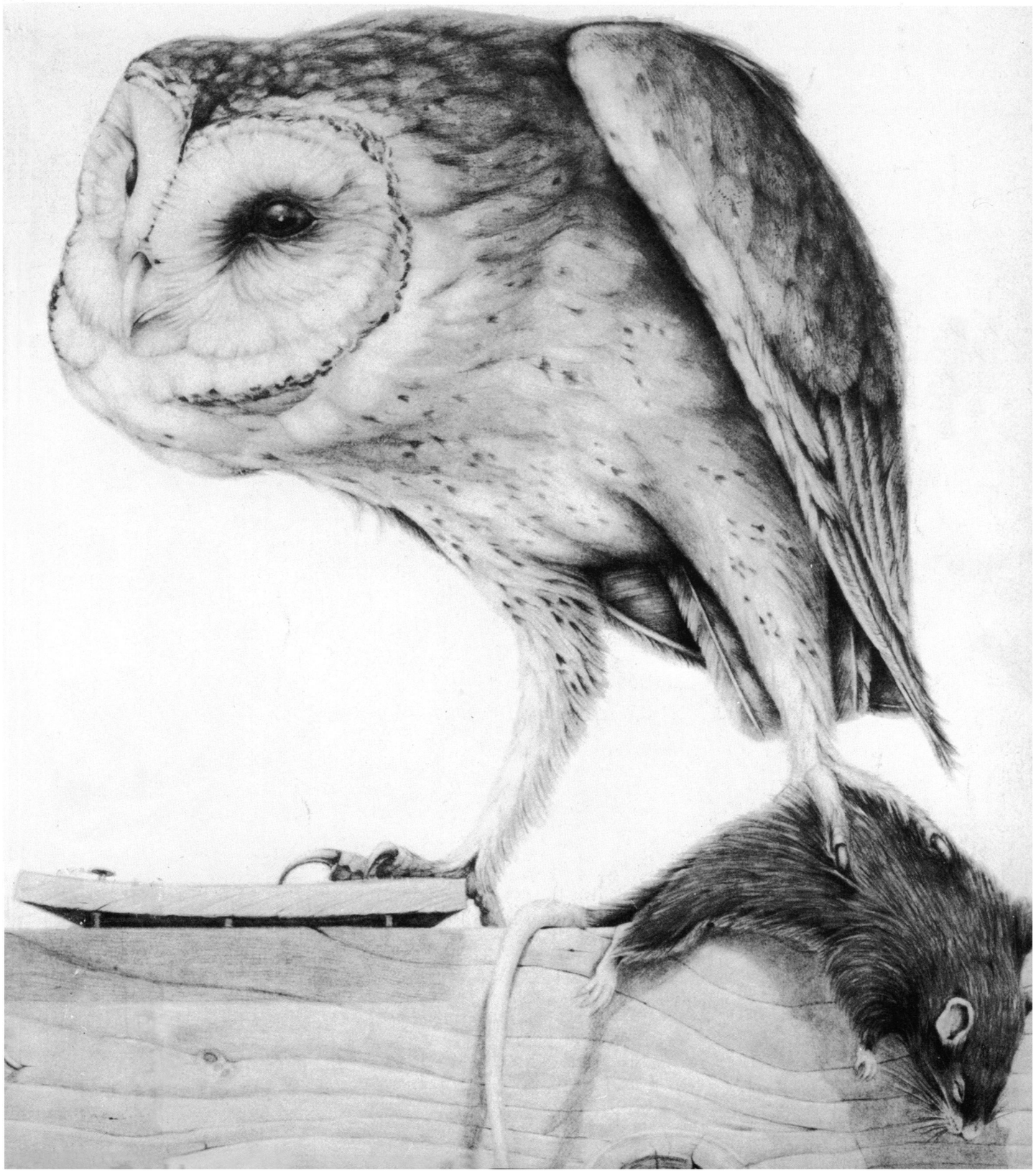
SCIENCE

20 August 1976

Volume 193, No. 4254

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE



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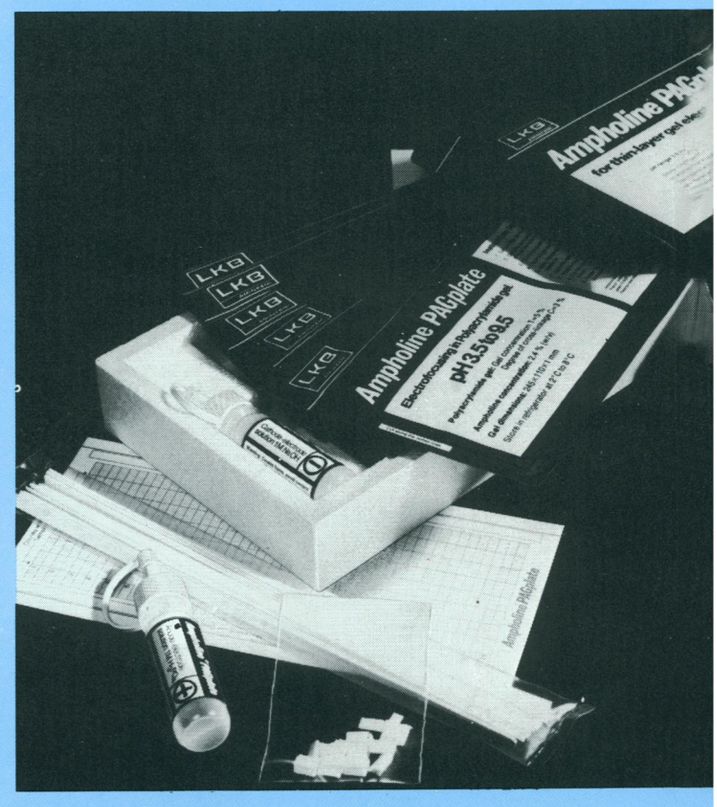

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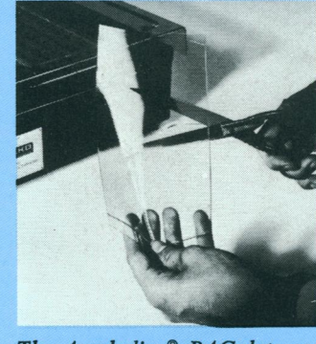

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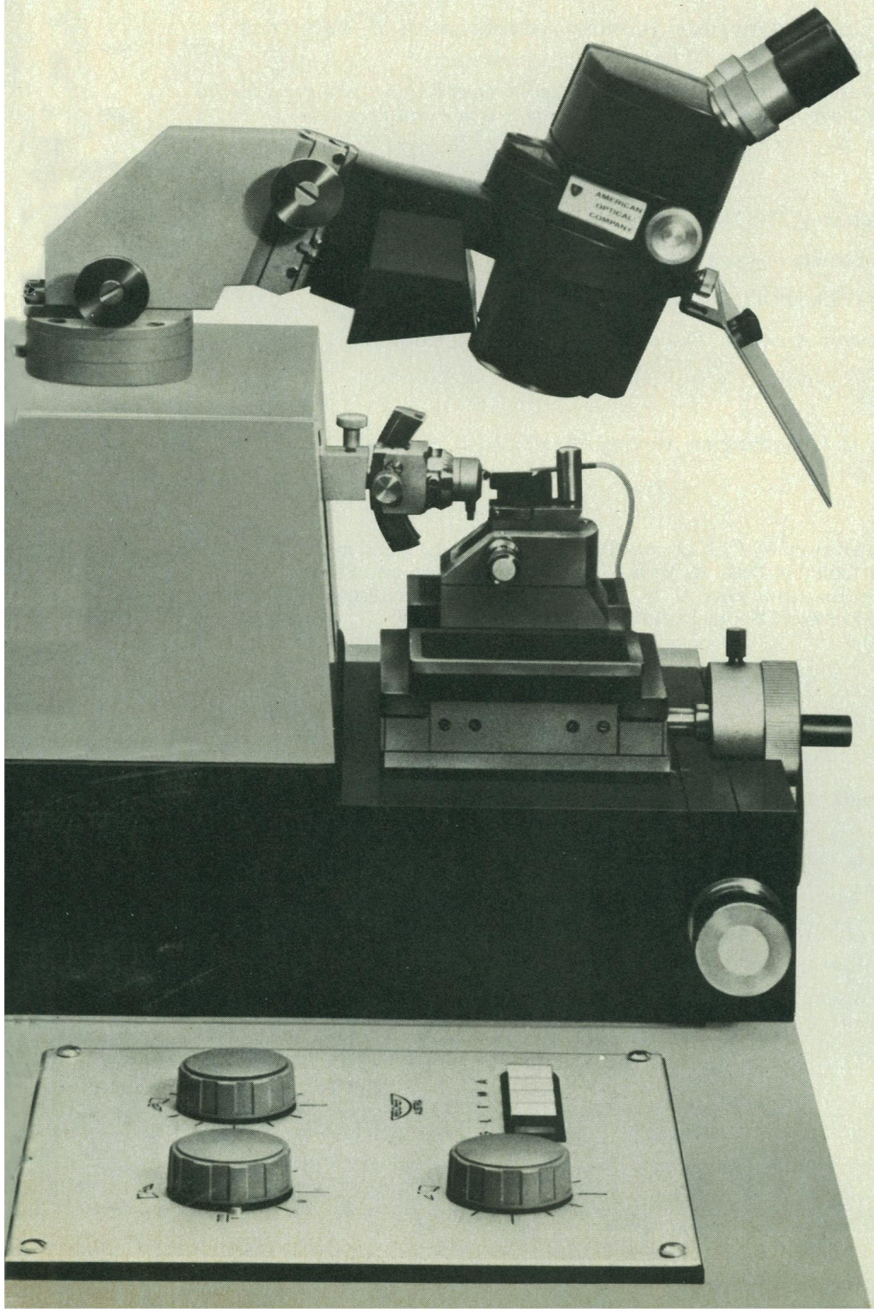

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COVER

Barn Owl (*Tyto alba*). The frontal eyes and prey-catching skills of the owl suggest that the bird achieves stereopsis, the highly precise binocular depth sense. See page 675. [Drawing by Dean Rocky Barrick,© Seattle, Washington]

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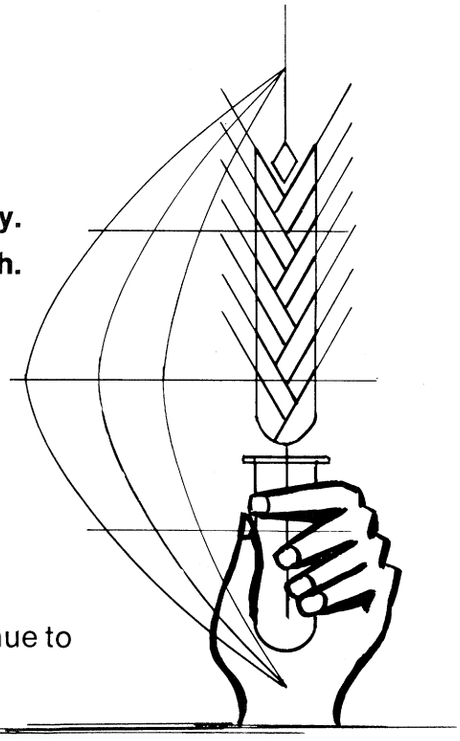

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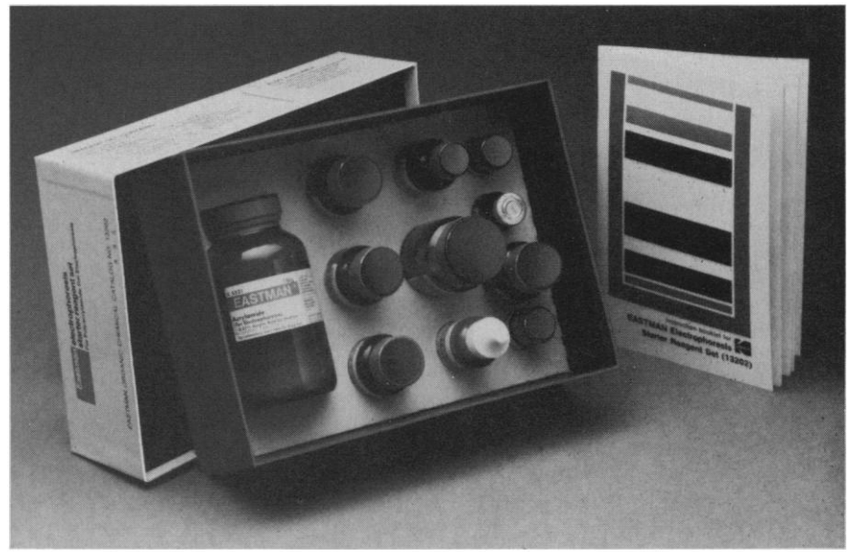

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in the long term. The details behind these numbers can be found in the UWMAK-III report (*3*, chapter xiii). Even the "unoptimized" early conceptual fusion designs show that they could be competitive with future fast breeder reactors, high-efficiency coal stations, geothermal stations, and fuel cells. All systems seem substantially less expensive than central station solar units. Thus, to suggest the cost of electricity from fusion will be several times that from advanced fission reactors is simply not consistent with our work. Any final choice among the available options (if such a choice must be made) will be made for a *combination* of reasons, including the long-term environmental impact (resource availability, storage of radioactive materials, land despoilment, and so forth) and the short-term societal impact (diversion of important materials, vulnerability to terrorist attacks, and so forth). We have found that fusion shows great promise in some of these areas, and that conclusion, combined with "reasonable" costs, encourages us to push on with fusion research to arrive at the final answers.

Undertaking the first generation of fusion reactor studies, conceptual though they are, has been a very healthy aspect of the fusion program. An effort is being made to understand the entire system and not to leave key aspects for 10 or 20 years hence or to spring surprises at that late date. We have discovered that certain engineering design approaches once thought most favorable are not particularly attractive. On the other hand, more optimized designs are being developed to take their place. We can now more clearly see the directions to go and we have a detailed basis to guide our analysis.

In his article, Metz notes that several people have called for a program that puts more emphasis on innovation and discovery, particularly referring to plasma confinement schemes. We would argue that this first generation of reactor studies has in fact laid the foundation for us to proceed ahead with innovation and imagination in the engineering design of these reactors to produce a more highly optimized system. Five or six years is only enough time to scratch the surface of an area like fusion reactor technology, and it should be clear that our most promising engineering discoveries still lie in the future.

ROBERT W. CONN
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2. ———, *UWMAK-II, A Conceptual Tokamak Power Design* (Nuclear Engineering Department Report No. UWFDM-112, University of Wisconsin, Madison, 1974).
3. ———, *UWMAK-III, A Noncircular Tokamak Power Reactor Design* (Nuclear Engineering Department Report No. UWFDM-150, University of Wisconsin, Madison, 1976).

The Wisconsin reactor studies indeed show a progression in which the third design is slightly more compact than the first. But the reactor containment building for UWMAK-III would nevertheless be 65 meters high and 75 meters in diameter, with heat exchanger and turbine buildings almost as large. Even though the Wisconsin team has succeeded in boosting the power density of a fusion reactor from 0.7 to 2 Mw/m^3, there is a very long way to go before achieving the 100 Mw/m^3 power density that would allow a fusion reactor core to be as small as that of a light water fission reactor.

—W.D.M.

International Decade of Climatology

We were pleased to see from Willard F. Libby's letter of 28 May (p. 843) that an International Decade of Isotope Climatology Study is proposed, but we are concerned that only isotopic studies have been singled out. The research referred to by Libby is primarily marine in nature; the program, therefore, suffers from the exclusion of the great majority of the terrestrial record. Moreover, much of the marine sedimentary record cannot be interpreted with the degree of high resolution obtainable in the record on land where more rapid sedimentation allows annual or seasonal events to be resolved. This information is surely vital to questions of rapidity of past (and future) climatic change. In addition the marine record may be more climatically buffered than the land record.

Libby's reference to isotopic studies of the Greenland ice core raises doubts about the sole reliance on that approach for the polar regions, since alternative interpretations of the chronology of the glacial oxygen-18 record have been proposed and since complexities in correlations between ice cores have been noted (*1*). The ice core records do not provide adequate summer paleoclimatic data, and the tree-ring analyses are not applicable to the tundra regions. Thus very large arctic and antarctic areas which are sensitive to climatic change and were probably the first regions to experience

the onset of glaciation and "abortive glaciations" would not feature at all in the proposed studies.

As recorded, the Holocene period is of highest priority in terms of extreme conditions and the frequency characteristics of climatic fluctuations. In the present formative state of paleoclimatic research, it would seem prudent not to put all our eggs into one (isotopic) basket, but to take this opportunity to involve the entire range of paleoclimatic expertise in a program of mutual data testing and evaluation. Would not an International Decade of Climatology be a preferable research framework?

HARVEY NICHOLS
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1. W. Dansgaard, S. J. Johnsen, H. B. Clausen, N. Gundestrup, *Medd. Groenl.* **197** (No. 2), 53 (1973); W.S.B. Paterson, paper presented at the Fourth Biennial American Quaternary Association Meeting, Tempe, Ariz., 1976.

Statistics and Rare Phenomena

Walzer and Gerald (Reports, 19 Dec. 1975, p. 1228) correctly report (*1*) that, in a study in which they obtained karyotypes of about 10,000 newborn males, the difference between the children of upper-class fathers and those of lower-class fathers in frequency of occurrence of the XYY (and also the XXY) karyotype was not statistically significant (*2*). The authors interpreted their results as showing "no significant effect of social class upon the frequency of XYY, and of XXY, among newborn males." An Associated Press report of the study stated flatly that XYY "occurs as frequently in the higher socioeconomic classes as in the lower stratums" (*3*).

The frequencies are not stated in Walzer and Gerald's report but can be calculated from the data in their table 2. For XYY they are .00195 and .00107 in the lower-class and upper-class groups respectively (*4*). This is a difference of 81 percent and would be noteworthy if it were found repeatedly; but because of the rarity of the karyotype it would not reach statistical significance except in much larger samples.

With their sample sizes and overall frequency of the karyotype, the frequency in the lower-class sample would have to be more than 3½ times that in the upper-class sample (*5*) to show statistical significance. For the difference between the observed frequencies to be statistically significant at the .05 level, the sample size would have to be increased fourfold (*6*). In short, the usual statistician's caveat against interpreting lack of statistical significance as counterevidence must be observed with special scrupulousness when dealing with rare phenomena such as XYY.

DOUGLAS H. BOUCHER

Division of Biological Sciences, University of Michigan, Ann Arbor 48109

References and Notes

1. The statistical test used in the study was not mentioned; presumably it was a chi-square, maximum likelihood, z-, or similar test.
2. Following a common practice, the authors use the term "significant," not "statistically significant."
3. "Chromosome abnormality hits all classes," Ann Arbor *News*, 17 December 1975, p. 15.
4. These frequencies were calculated from Walzer and Gerald's table with the procedures described in their text: non-Caucasians were excluded from the analysis, maternal age was disregarded, and classes IV and V were pooled as lower-class and I, II, and III as upper-class. The corresponding frequencies of the XXY karyotype are .00117 in the lower-class sample and .00092 in the upper-class sample, a 27 percent difference.
5. A chi-square value of 5.365, for which $P = .021$, is given by frequencies of .00273 and .00077 for lower-class and upper-class infants respectively. These correspond to 5 XYY's among 6512 infants for the upper-class sample and 7 in 2564 for the lower-class sample, a 3.56-fold difference.
4. A fourfold increase of Walzer and Gerald's sample corresponds to 20 XYY's in 10,256 cases for the lower-class group and 28 in 26,048 for the upper-class group. These results give a chi-square statistic of 3.63, for which $P = .057$, and a maximum-likelihood (G) statistic of 3.96, for which $P = .047$.

The analysis described by Boucher was likewise performed by us during the initial examination of our data. If the frequency of individuals with the XYY karyotype in social classes IV and V is indeed as great as .00195, it would still be insufficient to account for the 10- to 20-fold excess of XYY individuals in certain institutional populations. This was the basis for our concluding that whatever effect social and economic factors might have on the institutionalization of XYY individuals, the effect was not *primarily* due to any influence on nondisjunction.

It is still reasonable to believe that the frequency of the XYY karyotype in newborns is not affected by the social class of their fathers. It should be noted that, while an excess of XYY individuals was present in social classes IV and V in the Boston study, a *deficiency* in these two classes occurred in the Edinburgh study (*1*).

PARK S. GERALD
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1. S. G. Ratcliffe and H. J. Evans, *Lancet* **1975-I**, 1144 (1975).

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20 August 1976, Volume 193, Number 4254 **SCIENCE**

An R & D Delivery System

The federal government has funded and consumed a fabulous amount of research for its own needs—radar, inertial guidance, proximity fuses, ballistic missiles, antiballistic missiles, "beating the Russians to the moon," and on and on and on. The emphasis has been on federal use. The National Aeronautics and Space Administration speaks of agency users, not citizen users.

At the same time the citizens are saying, "If we can put a man on the moon, we can do this or that." They will not be put off by the alibi, "That is a different subject." We must solve their problems as well.

In his editorial*, Bisplinghoff makes a potent point that federal research should be and is now aiming more at citizens' needs, and he goes on to say, "It is not at all clear that funneling federal R & D funds through existing federal institutions will come within a country mile of accomplishing the intended purpose."

I agree wholeheartedly—it will not. Fortunately, there is a family of state institutions with a federal connection that do come within that "country mile of accomplishing the intended purpose" of research for citizens' needs. They do so because they are not federal. I refer, of course, to the Agricultural Experiment Station system.

The first experiment stations were set up a century ago by states to solve problems of farmers, the largest group of citizens at the time. Soon the hoped-for solutions began to come—better varieties, better fertilizers, control of disease, and better nutrition. Soon city people found that the stations helped to feed them. The economic return on the investment in research was high.

By the time the first 13 state experiment stations were in operation, Congress decided that here was a useful system through which to funnel federal R & D funds with the aim of solving citizens' problems. The Hatch Act was passed, and there is now an Agricultural Experiment Station in every state.

By now the stations have solved reasonably well the problem of cooperating with the industries that produce our food. And they have a delivery system that has functioned for nearly a century. Being spread over the nation and directed from the grass roots, stations know citizens' needs and how they vary over the country. Therefore a federal agency does not direct them, but Congress funds them and thus spreads its scientific bets across the nation. Since no Washington committee can possibly think of all the angles or numbers that might win, the decisions must be and are made locally.

A basic policy is steady money. Congress funds the stations on a regular basis, not on the feast-or-famine basis of individual grants. That means that if a fire flares up it can be quenched before people go hungry, as the corn blight epidemic of 1970 plainly showed. In cooperation with industry, the system put out that fire in 2 years. Had the system depended on contracts and grants, the red tape would hardly have been unrolled in that time.

While doing work with practical applications, the stations can do profound research too, as several Nobel prizes and memberships in the National Academy of Sciences prove. Vitamins and hybrid corn testify to the value of the stations. So, too, do dicumarol, streptomycin, and biological control of pests.

Bisplinghoff is correct: the stakes are large, they are getting larger, and the hour is late. Fortunately, in its research for our groceries the country has developed a system that can make discoveries, can translate them into solutions for the public and private sectors, and can deliver the results.

Perhaps the Agricultural Experiment Station system is a first approximation to examine as we seek greater economic return on our federal R & D investment.—JAMES G. HORSFALL, *Director Emeritus, Connecticut Agricultural Experiment Station, New Haven 06504*

*R. L. Bisplinghoff, "Federal R & D—Outmoded Management Policies," *Science*, 12 December 1975.

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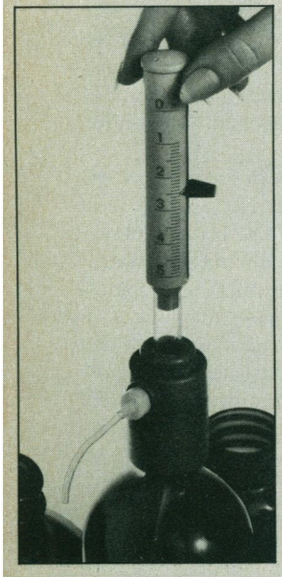

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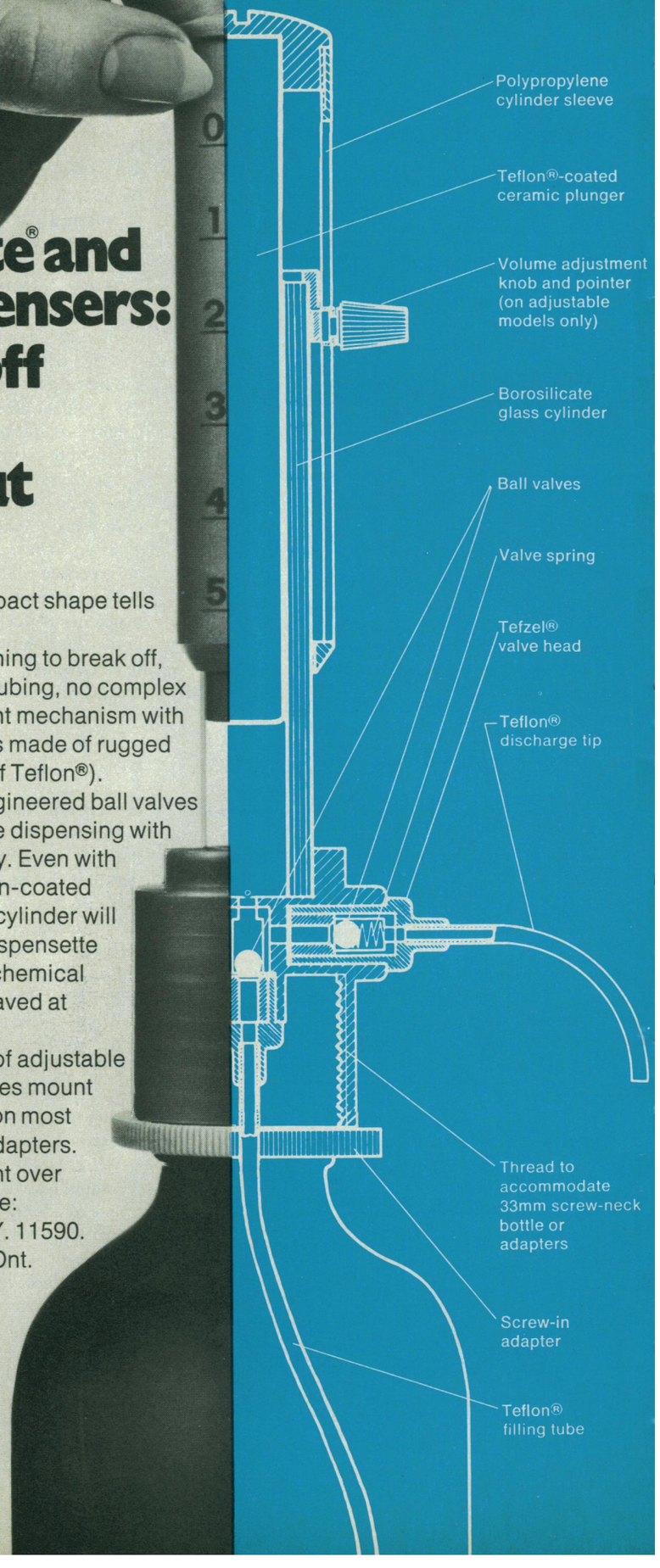

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ning electron micrographs of etched samples to account for the calcite reported by Houck *et al.* We did not find any evidence in the etched samples of calcite outside the microborings. Houck *et al.* (*1*) also reported "angular crystals" presumed to be calcite which are too large to be accommodated into the microborings. Our scanning electron stereomicrographs of freshly fractured coral surfaces exposing "angular crystals" reveal well-developed cleavage in coarse crystals, both along microborings (Fig. 1D) and across wider areas where they branch or coalesce.

If Feigl's solution (*6*) is applied to the etched surface, the skeletal aragonite is stained black, leaving the protruding calcite filaments comparatively unaffected. Resolution limitations of our electron microprobe prevented us from obtaining completely isolated analyses of the material in microborings; however, filamentous areas show lower strontium/calcium ratios and higher magnesium/calcium ratios than the skeletal aragonites—a feature consistent with the calcite mineralogy.

Houck *et al.* (*1*) reported that other samples of *Porites* from the same area at Waikiki consisted entirely of pure aragonite. This finding detracts from their contention that the calcite is a biogenic deposit of the coral itself. This atypical occurrence of calcite may represent a unique example in modern corals of "low-magnesium" calcite precipitation in association with boring activity of endolithic plants.

The origin of the calcite within the microborings remains unknown. Nevertheless, the data clearly indicate that the calcite is not the primary biogenic skeletal deposit of the coral.

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7. We thank W. T. Boykins for assistance with the photography and W. R. Brown who operated the scanning electron microscope. We thank R. W. Buddemeier for making available to us study specimens. We appreciate the complete cooperation of J. E. Houck, R. W. Buddemeier, and K. E. Chave with this interesting problem.

29 December 1975

The findings of Macintyre and Towe are certainly relevant to our observations (*1*) and will help to focus further research on this interesting problem. Although we accept the validity of their observations, some comments on points of interpretation seem in order.

First, the differential etching and staining experiments could be interpreted as showing the effects on carbonate reaction rates of the refractory organic coatings rather than of differences in mineralogy. Although persuasive, these results do not prove that the calcite occurs only in the microborings or that the contents of the borings are exclusively low-magnesium calcite. Second, we have investigated further a number of presumed calcite crystals with dimensions of several tens of a micrometer in at least two directions. The tubules shown by Macintyre and Towe do not appear capable of containing crystals of these dimensions, and we remain somewhat skeptical that all of the large calcite blocks are situated in junctions of 3-μm tubules. Finally, neither group of workers has carried out a quantitative correlation between microscopic observations and the composition as determined by bulk analysis, and so it is still not possible to show conclusively that microborings account for all of the calcite present.

We certainly agree that the additional data suggest that it is less likely that the calcite is "primary" skeletal deposition by the coral, but we do not agree that the observation necessarily rules out the possibility that it is biogenic deposition by the coral. A secondary calcification in which the coral deposits calcite or makes possible its precipitation in the borings left by another organism seems to us a real possibility. Deposition by endolithic plants, either primary boring organisms or their successors, is also possible. In either case the frequent occurrence of borings of micrometer size (*2*) without calcite and the fact that the calcite in question has a low magnesium content complicate any simple explanation. It is this sporadic occurrence of low-magnesium calcite in scleractinian corals which remains the central phenomenon to be explained.

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1. J. E. Houck, R. W. Buddemeier, K. E. Chave, *Science* **189**, 997 (1975).
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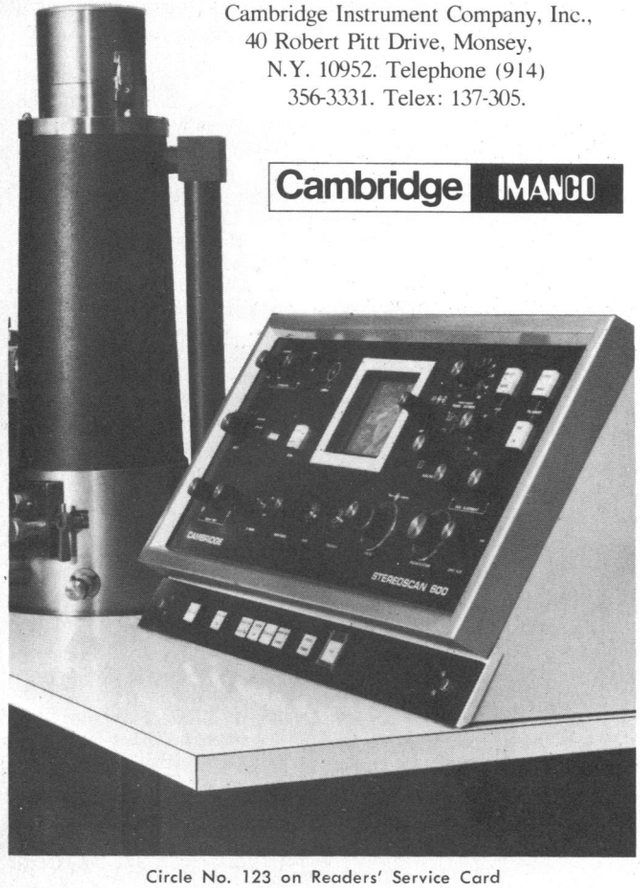

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