say solid-state physics or microbial genetics, rather than in the general area of physics or biology?

The danger of transferring the concept of critical size from the number of atoms of uranium-235 to the number of scientists in a given discipline or subdiscipline is that quality is left out. Even though Rochester and Harvard and Caltech may apply this concept to recruitment, I am very skeptical about smaller colleges and universities that claim to be subcritical, and think that if only the number of faculty could be increased, everything would be jolly. What they may end up with are large third-class departments.

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Compliments

Bragg's very helpful advice on "The art of talking about science" (30 Dec., p. 1613) was conveyed most forcefully in the address by Eyring at the AAAS meeting and by the article adapted from that talk (30 Dec., p. 1609). Reading the article, prepared with Dr. Eyring's usual clarity and expertise, certainly educates the reader and provides him with a "storehouse of information" (Bragg). However, the more rewarding experience, by far, was attending the brilliant, live presentation; Dr. Eyring's amusing explanatory gesticulations and adventures in anthropomorphism will not be easily forgotten. And more important, his principal message—the "leaky" mechanism of membrane processes—was as firmly ingrained as any message could be.

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Illness prevented Sir Lawrence Bragg from delivering the address upon which the article "The art of talking about science" was based. Unfortunately, there was not time to correct the footnote statement that the lecture was delivered on 28 December 1966.—ED.

Diversity and Hindsight

Diversity, indeed! (J. Platt, 2 Dec., p. 1132). What a refreshing, stimulating prospect, and what a contrast to Hindsight! (News and Comment, 18 Nov., p. 872).

Just open to the latter reference and look at it for a moment. Don't bother to read it.

I think the best applicable comment for this is Werner Heisenberg's (*1*):

> The task of pure science at any given time is to clear and prepare the ground for the growth of technical development. Since this ground is quickly taken over, it is important that it should be continually extended, and in this theoretical research plays its part. The interaction between technical development and science is in the last resort based on the fact that both spring from the same sources. A neglect of pure science would be a symptom of the exhaustion of the forces which condition both technical progress and science.

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Reference

1. *Naturwissenschaften* **40**, 669 (5 Oct. 1934).

Platt's excellent and interesting article on "Diversity" (2 Dec., p. 1132) provides much food for thought. Diversify and then make a great discovery in a new field sounds great, but it is easier said than done. Pioneer work is not that easy. Take Platt's own example, the laser. In Maiman's first laser, the output looks very much like noise, and in fact, his paper was rejected by *Physical Review Letters*. It took Bardeen's genius, the expert assistance of Cooper and Schrieffer, and more than 20 years of work to solve the superconductivity mystery. The searchers for quarks and magnetic monopoles have not yet succeeded despite intensive efforts.

The BCS superconductivity theory is indeed worth 20 years of labor, but a lesser man can work for 20 years on the problem without getting anywhere. After winning his Nobel Prize, Purcell can afford to work on a long-shot experiment such as the magnetic monopole, but a starving graduate student struggling for his Ph.D. (or a young assistant professor hoping for his tenure) cannot afford to do so. Here failure means practically lifetime banishment from the scientific community.

When a lion makes a kill, the vultures gather. Let us be honest, few scientists are acting like lions. Many of us are facing a dilemma; we do not want to act like vultures, but we are also afraid of being banished.

Hence, to "diversification" may I add, according to Confucius, "moderation." I believe in an indirect form of diversification. When a professor becomes reasonably well established, he should not take on too many graduate students. With fewer graduate students, he can work on the long-shot (and perhaps more important) problems himself. Similarly, a research scientist should refrain from building his own empire and having too many assistants; otherwise he will spend all his time thinking up routine work to keep his technicians busy.

In educational curricula, we should allow the brightest students to diversify, but the average and mediocre students profit most from conventional and tested educational techniques. Diversification for its own sake can go too far!

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Dimethyl Sulfoxide Toxicity

Sack's report (28 Oct., p. 543) and recent challenge to those of us who have reported toxic effects of DMSO to animal eyes (New York Academy of Sciences conference on dimethyl sulfoxide, March 1966) is certainly justified. High doses of most any effective pharmaceutical will produce some form of toxicity in small animals.

We have just completed a study in rabbits (2nd International Symposium on DMSO, Vienna, 8 and 9 Nov.) using amounts comparable to those commonly recommended for human therapy. Topically applied DMSO, of a quality recommended for human use and at a dosage of 0.1, 0.5, and 1.0 g/kg per day, failed to produce any retinoscopic changes after 11 weeks of therapy. Weekly biomicroscopic examinations revealed no lenticular change at any concentration after 8 weeks therapy and only the slightest suggestion of any early DMSO effect after 11 weeks when the dosage was 1.0 g/kg per day, which is approximately five times the usual dosage for studies on humans. Oral consumption of the same low dosages were essentially the same as those described above, except that the biomicroscopic changes were observed a few days earlier.

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UFO's: Dimensions and Speed

Seaman's report of a UFO sighting (Letters, 2 Dec.) is typical of most I have seen on this topic—his conclusions do not follow from his observations. In particular, the diameter of the UFO could not have been estimated, in feet or in meters, without further data. Nor could the UFO's speed, whether phenomenal or not, be determined.

It is fair to assume that stereoscopic vision is of no use beyond 20 or 30 meters, and that no radar or sonar was used to determine the distance to Seaman's UFO. The only clues in this sighting are, therefore, the angle subtended by the object and the angular rate of motion at the observer's position. This information alone does not determine the linear dimension or the linear speed of the object.

There remain, so far as I can see, only two kinds of dimensional information. One is stadiometric: if the size of the UFO is known, as, for instance if UFO's were known a priori to be of a certain diameter, or if the P-38 pilot had sighted a P-38 of dimensions known to him, then the distance to the object could be determined. The other is ranging: if the distance to the UFO were known within limits set by occlusion of other objects at known distances, then the diameter of the UFO could be determined from its subtended angle. However, Seaman's UFO was "on the horizon about a mile away." Had the UFO occluded an object at a known distance, it would have been possible to set an upper limit to the UFO's distance, and hence to its diameter. Or, had an object at a known distance occluded the UFO, it would have been possible to set a lower limit to its size. Even the deceptive "occlusion" by haze is ruled out in this report for it was seen in the "crystal-clear afternoon."

Lacking any information whatsoever about a linear dimension, no other linear dimension or linear speed can be deduced from visual clues. Since a slowly-moving object traverses the visual speed at an unlimited angular velocity if close to the observer, I am at a loss to interpret a "phenomenal" angular rate. Nor do I read here any support for Seaman's reference to "machines."

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