his research papers are referenced by others.

This approach was tried experimentally some 20 years ago by John A. Hinckley, a chemical engineer then on the staff of the Office of Naval Research in its Chicago branch office under Harry Kelly, as chief scientist. Hinckley's study covered a period of 5 years or so in the field of chemistry. We in ONR were interested in several aspects of it: as a simple device to find who were currently in the forefront of research, or currently known; as a possible simple way of bringing quality as well as quantity into evaluation of research productivity; and as a start in ascertaining how and to what extent active and successful scientists were receiving support for their research, that is, from private or public sources, from within or without their institutions.

To the best of my recollection the major results were somewhat as follows: quotations of recent data or papers strongly predominated, with a rapid falling off of references to papers published more than a few years previously. There was not a particularly high correlation between the most prolific research contributors and those most quoted, and there were wide variations in several fields, with maxima of quotations in currently active or controversial fields, as might be expected. At the time, many of those whose work was most quoted were receiving sole support from their own institutions.

Anyone interested in evaluative studies of research productivity might also find it profitable to consult chapter 3 of *America's Psychologists*, K. E. Clark, Ed. (American Psychological Assoc., Washington, D.C., 1957), pp. 26–61.

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Propylene Oxide and Air

Liquid propylene oxide and ethylene oxide are sometimes used for non-destructive sterilization of culture media, biological materials and plastics.

The labels on bottles of propylene oxide caution the user that it is flammable and should be kept from sparks or flame. However, I believe that it is not commonly realized by biologists that propylene oxide and air can form explosive mixtures which present a potential hazard in the laboratory. Perhaps the use of liquid propylene oxide or ethylene oxide for sterilization should be discontinued. Instead, commercially available nonflammable mixtures of ethylene oxide plus carbon dioxide or freon could be used. However, manufacturers warn that these mixtures, although nonexplosive, can act as asphyxiants and vesicants.

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On Procuring Russian Literature

As a self-styled specialist in and translator of Russian scientific literature, I was most interested in Manheim's review "Soviet books in oceanography" (25 Nov., p. 995). It might be worthwhile to add some information on how to obtain Russian books for those interested in keeping abreast of Soviet developments in some particular discipline.

Books should be ordered well in advance of the publication date and from an authorized dealer in the United States such as the Four Continent Book Corp., 156 Fifth Ave., New York 10010, or the Victor Kamkin Bookshop, 1410 Columbia Rd., Washington, D.C. 20009. (Znanie Bookstore, 5237 Geary Blvd., San Francisco, Calif. 94109 is an outlet for Victor Kamkin imports.) From my experience, V/O Mezhdunarodnaya Kniga deals only with the aforementioned *authorized* book dealers and not with individuals or institutions. Sometimes the latter succeed in entering into exchange arrangements with their Soviet Union counterparts, or with Soviet libraries.

It is important to acquire the weekly publication *Novye Knigi SSSR* (New Books USSR) for a listing, by disciplines, of books to be published according to the plan of the editorial board of particular publishing houses for that year. This listing includes author, title, publisher, approximate size, and approximate date of publication (in Russian, to be sure). Books may be ordered by forwarding the catalog number, that is, the number of the weekly issue and the item number of the book, to the aforementioned dealers, who can then purchase the desired quantity of any item from Mezhdunarodnaya Kniga. This method of purchasing Russian publications, including the nonperiodicals, is nearly infallible.

The Soviet bibliographic works such as the *Knizhnaya Letopis* and the abstract journal *Referativnyi Zhurnal* are extraordinarily useful, but not for the purchase of books since the books listed have already been published and are customarily out-of-print upon publication (a planned economy, you know). However, they could very well prove useful to individuals and institutions engaged in exchanging publications. It is often possible to find bibliographic listings of local publications in the *Knizhnaya Letopis*, which would not appear in the *Novye Knigi*, and may therefore only be acquired via exchange.

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A Critical Size for Research?

Marshak's interesting article ("Basic research in the university and industrial laboratory," 23 Dec., p. 1521) refers to a "research director" as though such a person exists and is commonly found at universities in the United States. Maybe I've been visiting the wrong universities, but I have yet to meet anyone who has the authority to do all the things Marshak attributes to this sterling fellow. Possibly this director is in a state of resonance and may appear sometimes as the president, graduate dean, department chairman, or a professor. But often the little man isn't there at all. Planning at the level of the entire institution is still uncommon, and growth, or at least change, occurs too often as a result of chance. Many good things come by chance, but when opportunities arise by way of new federal programs, and a university responds directly to the jangle of money, the university often finds itself on a road it would have avoided, on looking backward, if it had had a plan.

Unlike Marshak, I don't think there is any need for critical size in order to do first-class research in general: what was the critical size of the group working with Darwin or Gibbs, or more recently, Bridgman? Is it possible that critical size is needed today where it wasn't before World War II? Is critical size important for big science but not for little science? Is it really necessary to have critical size by field,

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.