was merely the best attained, to meet a prescribed need, has not been asked for obvious reasons. Therein lies an important flaw in the "obvious" inference drawn above; namely, could a better result have been attained had a better scientific base been available? What role was played by a lack of awareness of scientific advances accomplished elsewhere? How much would have been accomplished without the underpinnings that undirected investigations tend to provide? There is no end to such speculation; yet it goes to the very heart of the matter. Fundamental investigations in science are the fertilizers that enrich the soil in which the more utilitarian crops can grow. Without them, the soil would soon become depleted—with familiar consequences. Whether DOD should provide its own foundations or rely on those built by others should be debated on grounds different from those based on percentage estimates of the direct benefit DOD derives from its sponsorship of undirected investigations in science.

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The main emphasis of the Hindsight study was on identifying and analyzing "events" which almost by definition proceed from applied research or development. An advance is not considered an "event" unless it makes a rather direct contribution to the weapons system under study. Essentially no scientific contributions to weapons systems were emphasized which did not have an applied motivation.

To show fully the contributions of scientific research through a Hindsight-type study, one must give major consideration to such factors as defining and analyzing the sources of knowledge available to the principal investigator given credit for the "event." For example, in the case of the titanium-aluminum-vanadium alloy, one should trace the origin of the information concerning the effects of structure on mechanical properties and of composition on corrosion properties, of the data from binary and ternary phase-diagrams, and of all the other information used either implicitly or explicitly by the principal investigator.

Studies such as that being conducted by the Air Force Office of Scientific Research (see "Relating the Accomplishments of AFOSR to the Needs of the Air Force," available from the Clearinghouse for Federal Scientific and Technical Information, Springfield, Virginia 22151) reveal other important parts of the picture. We find that our phenomena-oriented research, for which AFOSR program managers provide the appropriate degree of military orientation, has resulted in many important contributions to weapons systems. Our program has (i) helped pioneer numerous fields of demonstrated utility, such as hypersonics, nonlinear mathematics, and quantum electrodynamics, and has (ii) also provided numerous effective means for coupling a wide variety of scientific research activities (including a great deal not DOD supported) to DOD technological programs. Among contributions accruing from DOD research sponsorship, but not generally identified with Hindsight "events," are the increased scientific knowledge required for attacking difficult practical problems, the education of people essential to technology, and the large consultation activity of scientists who maintain their expertise through research.

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The conclusions Abelson cites are more than a little misleading, and are typical of hardware oriented cost consciousness.

To be sure, direct contributions to systems from recent (post-1945) undirected science have been small, but was it ever really expected to be otherwise? The major contributions expected and abundantly received from the university contracts were and are a tremendous reservoir of trained scientists who would otherwise never have been able to afford graduate school, and large, modern laboratories in a great many universities—laboratories that would never have existed without government contracts. . . . I suggest that as hardware developments have been traced from inception to inventory, so too should the key people who developed these 20 proven weapon systems be traced in terms of their education and training. It would surprise me very much if most of them had not received their graduate school education supported, at least in part, by their employment on a DOD research contract.

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An aspect which seems to have been forgotten by the DOD in its recent foray into the "science of science" is that it was retrospective in a sense additional to the sense intended: the propositi were "weapons systems," followed back to research "events." Thus an apparent conclusion of the report, that mission-oriented research was more productive of weapons systems than non-mission-oriented research, seems to lead to only one of the two proper deductions—that in producing new weapons systems, a greater proportion of support to mission-oriented research would have been more efficient. A different deduction might have been implied if the investigation had also included a "prospective" part, in which significant scientific events of earlier years had been identified, and their results followed forward. The tritest example of this sort of thing is the eventual military application of the pure physics of nuclear fission; but let us note that an unsolicited letter from a nongovernmental scientist direct to the President was the adequate stimulus for the previously unenvisioned development effort which followed. Thus deductions which might be suggested by such a *prospective* study might include, in planning for the ability to identify new and novel weapons systems, greater awareness of many areas of scientific innovation; also, greater institutional flexibility, to re-orient or re-structure development efforts to exploit promising ones, might have been more effective.

Whether or not this would be among the implications of a complementary prospective study, I seriously propose that such a study should be undertaken by the DOD in order to provide a more balanced picture of the relations between research and weapon-making.

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Early Information Evaluation

In a recent editorial (7 Oct., p. 74) I noticed Abelson's reference to a new method, involving data available from the Automatic Subject Citation Alert (ASCA), which may be useful in providing evidence as to the recognition received by a scientist's publications by ascertaining the frequency with which

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,