REPORT

ON THE

CHEMISTRY PROGRAM OF THE AIR FORCE OFFICE OF SCIENTIFIC RESEARCH

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BY THE

AD HOC ADVISORY COMMITTEE ON THE CHEMISTRY PROGRAM OF AFOSR

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CHEMISTRY PROGRAM OF AFOSR

Introduction

The Air Force Office of Scientific Research (AFOSR) was established in 1955, succeeding the former Office of Scientific Research. As a field agency in the Office of Aerospace Research, it is the primary Air Force agency for the support of extramural research. Its program is carried on through grants and contracts awarded mainly to universities, but also to other non-profit organizations and industrial laboratories, in response to unsolicited proposals. The program is evidence that the Air Force recognizes its dependence on basic science from which the technology of tomorrow will flow.

AFOSR is organized in seven directorates. The Directorate of Chemical Sciences, with a program of about $3.5 million (10% of the AFOSR budget), concerns us here. The mission of the Directorate of Chemical Sciences is "to encourage and support fundamental research designed to increase understanding of the science of chemistry, to stimulate the recognition of new chemical concepts, and to provide for early military exploitation of their military implications."[1] The Committee for the Survey of Chemistry of the National Academy of Sciences computed the total annual direct support of chemical research in Ph.D.-granting chemistry departments at universities alone in the United States as in excess of $60 million in fiscal 1964[2]. In view of the small fraction of this total contributed by AFOSR, the Director of Chemical Sciences, Dr. Amos G. Horney, has requested advice concerning the most effective way to utilize the funds at his disposal.

In a letter of 23 November 1966 to Professor Paul J. Flory, Chairman of the Division of Chemistry and Chemical Technology of the National Research Council, National Academy of Sciences, Dr. Horney put the question as follows: "In the light of the AFOSR Air Force mission and recognizing that the Directorate of Chemical Sciences AFOSR supports only a very small percent of the basic research in chemistry and therefore cannot meaningfully support all areas: What specific areas of chemistry should receive primary emphasis in the AFOSR sponsored research program, conversely what areas should be left to other resources?"

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[1] 12th Annual Chemistry Program Review, Air Force Office of Scientific Research, Publication AFOSR 66-1854, Arlington, Va., December 1966, p. 1.

[2] "Chemistry: Opportunities and Needs," NAS-NRC Publication 1292, National Academy of Sciences-National Research Council, Washington, D.C., November 1965, p. 170.

With the approval of the Governing Board of the Research Council, a representative committee of research chemists was convened to study the question and to try to formulate an answer. The membership, appointed with the approval of Dr. Frederick Seitz, President of the National Academy of Sciences, was as follows: Professor L. B. Rogers, Purdue University,(Chairman); Professor Jerome A. Berson, University of Wisconsin; Dr. Jacob Bigeleisen, Brookhaven National Laboratory; Professor R. A. Bonham, Indiana University; Dr. N. Bruce Hannay, Bell Telephone Laboratories, Inc.; Dr.R. K. Iler, E. I. du Pont de Nemours & Company, Inc. Professor Ervin R. Van Artsdalen, University of Virginia sat with the Committee in an ex officio capacity as Chairman of the Chemistry Research Evaluation Panel of AFOSR. Dr. Martin A. Paul, Executive Secretary of the Division of Chemistry and Chemical Technology, served as NRC staff representative. The Committee met for a full day on March 30, 1967 and again on June 7, 1967. Several members of the AFOSR staff attended part of these sessions by invitation. Informal discussions were held among members of the Committee at other times. This report presents a consensus of the Committee's deliberations and conclusions.

Characteristics of Basic Research

To understand the recommendations made later, it is important to recognize some of the characteristics of basic research and its discoveries. In a study by the Materials Advisory Board of the National Academy of Sciences-National Research Council[3] it was found that:

1) The time at which a "breakthrough" will occur, or even whether one will occur at all, cannot be predicted with any certainty. Such a characteristic merely reflects the incompleteness of our knowledge.

2) The basic discovery is often found to be valuable in areas only vaguely related to the problem under study at the time of the discovery.

3) There is a long period between the basic discovery and the time when it is widely appreciated and applied by scientists and engineers in developmental research. A period of five years appears to be minimal, while ten years is common, and periods of twenty to forty years are known.[4]

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[3]Report of the Ad Hoc Committee on Principles of Research-Engineering Interaction, Materials Advisory Board, Publication MAB-22-M, National Academy of Sciences-National Research Council, Washington, D.C., July 1966.

[4]Ibid.

These factors, especially the last one, pose great difficulties if one wishes to base the desirability of supporting a particular proposal for a basic study on its apparent relevance to a current pressing problem in developmental research or engineering. Even if basic research funds were concentrated in a current problem area, not only might the relevant important discoveries be slow in coming but, more importantly, the pressing developmental problems of ten years from now might well be in some of the areas that were neglected. In contrast, increased efforts in developmental research, which systematically seek an optimum solution within narrower limits of knowledge, may be expected to yield useful results in a much shorter period of time, but only if a broad foundation of basic information is there to be tapped.

It is also important to realize that advances in basic research can be made in two different ways. One approach is to proceed from known principles, exploring their implications in a systematic way. The other approach is empirical, and is based on sketchy observations or incomplete reasoning that may even contradict existing concepts. The fact that many important discoveries have been initiated in the latter way, some by sheer accident as in the case of Pyroceram® brand glass-ceramics[5], argues in favor of supporting both types of research.

Chemistry and the Air Force

Chemistry involves the study of changes or reactions of materials at the atomic and molecular levels, and includes the characterization of products. Therefore, it is directly concerned with the synthesis of new materials and with the effects of chemical composition and structure on their physical and chemical properties.

There is no need to spell out in detail the heavy dependence of the Air Force upon chemistry. Clearly, the Air Force draws directly upon advances in chemistry for new materials (high-strength glasses and composite materials; high-temperature organic and inorganic polymers; heat-resistant and cold-resistant synthetic fabrics; special lubricants; chemical protective coatings; chemical ablatives; insulators, semiconductors, and magnetic and optical electronic devices; chemicals for control of fungi, insects, and disease) and new reactions as sources of power (explosives, propellants, jet and rocket fuels, fuel cells and batteries). Because of the tremendous breadth of the involvement of the Air Force with chemistry, it is virtually impossible fo find an area of study within chemistry that is not of potential value to the Air Force and, hence, relevant to its needs.

It is important to note that significant discoveries in basic research of direct consequence to the Air Force have come from the program sponsored by

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[5]Ibid.

the Directorate of Chemical Sciences AFOSR. Among such discoveries have been those of Professor W. F. Libby on the detection and measurement of low-level radioactivity, leading to development by the Air Force of the means of reconnaissance for detecting and analyzing the nature of distant nuclear explosions. Another discovery, the applications of which are still in their infancy, is the first truly chemical laser - a laser in which the radiation is derived from energy of a chemical reaction; this discovery by Professor George C. Pimentel in 1964 was an outcome of research on matrix isolation of transient molecules which he started eleven years earlier and which lead to his development of a rapid-scanning infrared spectrophotometer used in surveying chemical reactions for laser potentialities. Characteristically, the uses to which these discoveries could be put were not foreseen in the basic research programs originally conceived. Nevertheless, it is clear in retrospect that the discoveries themselves were dependent on a setting of basic research conducted by imaginative investigators.

Guidelines for Directing Support

The broad expanse of chemical areas that are of obvious interest to the Air Force and the wide applicability of many basic research findings together indicate that an equally broad, well-balanced program would best serve the mission of the Directorate of Chemical Sciences. Nevertheless, the Committee has explored different reasons for limiting the amount of Air Force support in certain areas or increasing it in others. Several important conclusions were reached.

First, it seems unwise for the Air Force to devote a major fraction of its budget to an area that falls within the primary mission of another agency and is well supported by that agency. For example, although the Air Force has an obvious interest in all aspects of health, the primary responsibility for major research efforts in that direction appears logically to rest with the National Institutes of Health.

On the other hand, the fact that the Air Force shares an interest in an area with one or more other agencies does not appear to be a valid reason for withholding support. To cite one example, new instruments and improved techniques for determining structure or composition of materials are of as much interest to the Air Force as to any other agency. Similarly, it seems appropriate for the Directorate of Chemical Sciences to stimulate research thinking in areas of chemistry that are of long-range interest to the Air Force but appear to be relatively neglected. Such stimulation might take place through sponsorship of informal discussions or formal conferences, with the goal of obtaining more and better proposals in that area over the long term. However, it is in the nature of basic research that easily recognizable contributions to the solution of current developmental problems will be small. Rather, a foundation will be laid for attacking developmental programs of the future.

The Committee made an effort to isolate those aspects of Air Force operations that were unusual and might, therefore, serve to distinguish its long-range needs from those of other agencies. Considering that Air Force operations encompass the globe and extend into outer space, it seems highly probable that continuing heavy demands will be made for new materials and new reactions that will perform satisfactorily over tremendous ranges of temperature, pressure, humidity, mechanical stress, and radiation. Furthermore, the materials must often withstand a rapid rate of change from one extreme to another. Thus, the discovery of new kinds of materials, the design of instruments and techniques for investigating their behavior under extreme conditions, the theory and experimental study of small-molecule chemistry and ionization phenomena, especially at low pressures, all promise to be enduring aspects of Air Force interest in chemistry. These areas might, therefore, be considered as a broad definition of a chemical mission of the Air Force.

At one of its meetings, the Committee started to prepare a detailed list covering the theoretical and experimental aspects of topics of unusual interest to the Air Force, including such areas as: high-temperature polymers, systematic exploration of new inorganic ternary systems, spectroscopy related to energy-loss problems, surface chemistry, solid-state chemistry (defects, crystal growth), chemistry of glass, photographic chemistry, and improved methods of analysis. However, the Committee found themselves forced to make decisions about specific topics within those areas to emphasize the fact that certain aspects, only, of those topics might deserve support. Because the acceptability of many of the specific topics depended on the state of information in the recent literature, the value of such a list would decrease rapidly with time. Moreover, it seems likely that such a list would soon become a check-list for determining the suitability of support and might lead to pressures to accept proposals that satisfied best such a superficial criterion. More important is the danger that even an up-to-date list would presuppose that the persons who made it up had all of the worthwhile ideas. It seems inevitable that the crucial components, program flexibility and timeliness of the study in relation to other basic studies, would rapidly decrease.

For those reasons, the Committee abandoned as undesirable the idea of preparing a list of specific research areas particularly worthy of AFOSR support. Instead, the Committee believe that a wiser course is to rely upon the collective judgment of members of the Chemistry Advisory Panel and the staff of the Directorate of Chemical Sciences to select, from among proposals freely submitted, those that represent the best chemistry in the broadly defined area of long-range interest to the Air Force.

Cost Sharing

The Directorate should explore ways of sharing direct costs as a means of increasing the effect of its program. The Committee first considered cost-sharing as a means of providing for the purchase of major items of research equipment which, though very expensive, are a necessity for modern chemical research. Sharing the costs with other agencies or with the institution appears to be especially attractive when more than one principal investigator would be able to use the apparatus. The concept of assisting one or more departments in an institution to acquire a vital instrument would make it possible for new staff members, as well as those with intermediate experience, to undertake improved research programs.

In principle, sharing of direct costs might be considered also for unusually large research programs aside from instruments. However, the danger must be recognized of rendering ineffective an otherwise sound program, if matching funds from other sources prove to be unavailable.

Another Aspect of the Funding Problem

The Committee believe that their suggestions for restricting the scope of Air Force interests in basic chemistry and for stretching the funds through cost sharing will still leave a gap that will pose a difficult problem. At the Committee's request, Dr. Horney provided the information contained in the accompanying tables. According to Table I, both the number and the percentage of all new proposals received that could be supported in the Chemistry Program of AFOSR have decreased drastically since FY 1965. According to Table II, the percentage of new high-quality proposals that could be supported has dropped below 20%. Such a level of support is unrealistically low in relation to the number and variety of good ideas calling to be tested. The Committee predicts that, if the level of support continues to be so low, the competent investigators, on whom the success of a program depends, will be discouraged from submitting proposals. In order to ensure a continued flow of high-quality proposals, we believe that a substantial increase in funds for basic chemistry will be required.

Conclusions

1) Because of the extremely wide range of applications of chemistry by the Air Force, it is virtually impossible to single out any aspect of chemistry that is not of potential value to the Air Force. However, a broad area in which the Air Force does have a primary long-range interest is the synthesis,

characterization, and theoretical understanding of new materials and new chemical reactions. The Air Force must have materials that not only perform well under extreme conditions but also are capable of withstanding rapid changes from one extreme to another.

2) Specification in further detail of research areas considered to be most deserving of support within the broad area of primary interest is undesirable, especially if the topics are selected mainly on the basis of their relevance to current developmental problems. Instead, reliance upon the scientific and technical judgment of the Chemistry Research Evaluation Panel and the staff of the Directorate of Chemical Sciences is recommended for the selection of appropriate proposals, freely conceived and submitted.

3) As general guidelines for selecting proposals, the Committee recommend that emphasis be placed on novelty of concept, with special attention to neglected areas or new, unexplored areas of chemistry, and that the AFOSR not concentrate a significant fraction of its funds in areas clearly falling within the province of another mission-oriented funding agency.

4) The possibility of stretching available funds by sharing costs for major research instruments with institutions or with other funding agencies should be explored.

5) The Directorate of Chemical Sciences, AFOSR is to be commended for having assembled and maintained a basic research program of high quality. The sharp down-trend in percentage support of high quality proposals received during the last two years is disturbing and may soon lead to a significant decrease in the number of such proposals received. With additional funding, the Directorate would be in a better position, both now and in the future, to provide the Air Force with an enlarged program of basic research more commensurate with its needs.

Table I

Data Relating to All Proposals Received

During the Specified Fiscal Year

	FY 65	FY 66	FY 67
New Proposals from Investigators Not Previously Supported:			
Number Received	238	287	298
Number Funded	55	18	6[a]
Percent	23%	6%	2%
New Proposals from Investigators Previously Supported:[b]			
Number Received	33	35	29
Number Funded	3	13	7[a]
Percent	9%	37%	24%
Total New Proposals:			
Number Received	271	322	327
Number Funded	58	31	13[a]
Percent	21%	10%	4%
Renewal Proposals:[c]	24	48	53
Percentages of All Proposals Received:			
Funded	28%	21%	17%
Not Funded	72%	79%	74%
Awaiting Action	0	0	9%[a]

a) On August 9, 1967, 33 new proposals (9% of all the proposals received) were awaiting action.

b) Most of the research support from the Directorate of Chemical Sciences is initiated as four-year efforts, so proposals for continuing or completing the four-year efforts are called renewals. If an investigator applies for another four-year effort, his proposal is classified as a new proposal, reviewed in competition with proposals from investigators not previously receiving support.

c) All the renewal proposals considered during the three fiscal years covered by the table were funded.

Table II

Data Relating to New Proposals

Judged to be of High Quality[a]

	FY 66	FY 67	FY68
Number Received	110	100	75
Number Funded	37	13	12
Percent	34%	13%	16%
Dollars Requested	$3,061,000	$2,975,400	$2,667,000
Dollars Funded	$1,207,557	$ 580,669	$ 615,631
Percent	39%	20%	23%

a) Deemed to be worthy of support had funds been available. The numbers in this table refer to high-quality proposals on which action was taken during the specified fiscal year; some of these proposals were received during the previous year.

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13. ABSTRACT

With the approval of the Governing Board of the National Research Council, National Academy of Sciences, the authors of this report were selected as a representative committee of research chemists to advise on what specific areas of chemistry should receive primary emphasis in the AFOSR sponsored research program. This report presents a consensus of the committee's deliberations and conclusions.

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