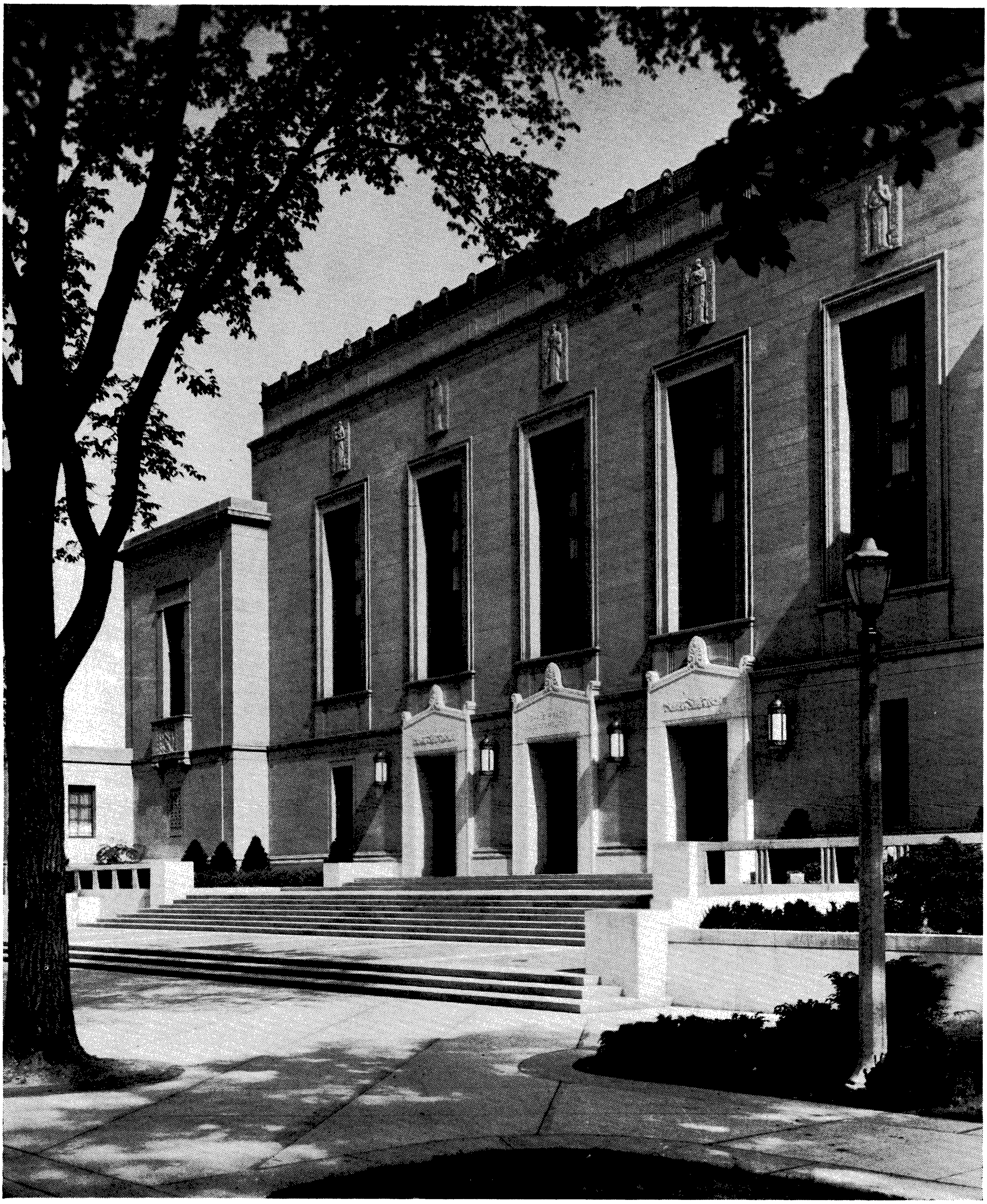


The Horace H. Rackham School of Graduate Studies
University of Michigan

PROCEEDINGS

of the

FOURTH ANNUAL CONFERENCE ON THE ADMINISTRATION OF RESEARCH

UNIVERSITY OF MICHIGAN

September 11-13, 1950

ENGINEERING RESEARCH INSTITUTE - UNIVERSITY OF MICHIGAN

1951

FOREWORD

The first Conference on Administration of Research was held at The Pennsylvania State College School of Engineering, State College, Pennsylvania, on October 6 and 7, 1947. It was initiated by a number of men who, not previously accustomed to administering large organized research groups, had been placed in charge of new research laboratories established during World War II. Some of these men, in talking with others in a similar situation, conceived the idea that an exchange of information regarding the procedures and practices which they had followed in the administration of their laboratories might be of benefit to all concerned.

The conference proved to be such a success that it was decided to hold another the following year. This, in turn, led to the idea of an annual conference. The second and third conferences were also held at The Pennsylvania State College.

The organizing group, which was made up of representatives from industrial, governmental, and educational institutions, formed the Advisory Committee, the only formal organization of the conference. Participants in the conference were invited on the basis of experience in the administration of research. This resulted in a profitable exchange of facts and ideas.

The Advisory Committee felt that the conferences would be of even greater service if they were held at different institutions. However, in order to safeguard against any unforeseeable adverse circumstances during the infancy of the project, it was proposed to hold both the fourth and fifth conferences at the same place. The choice fell upon the University of Michigan for the conferences of 1950 and 1951, whereafter it is expected they will be at a different institution each year. The 1951 conference will be held September 24, 25, and 26.

For carrying out the actual arrangements, preparing the program, etc., an executive committee composed of members of the Advisory Committee is chosen each year. To have someone in charge of local arrangements, a representative of the host school is appointed to the Executive Committee.

The proceedings of the fourth conference, edited and published by the host institution (as were those of the first three conferences), are herewith presented as a record of the official meetings.

The Engineering Research Institute of the University of Michigan, under whose sponsorship these proceedings were prepared, has had plentiful evidence that the fourth conference was a success. This success was due not only to the excellent program arranged by Dr. Harold K. Work, Chairman of the College of Engineering, New York University, and to the facilities which the University of Michigan has for holding such conferences, but also to the spirit of those who attended and, above all, to the excellent cooperation of everyone who contributed a paper and took part in the discussions.

Those who attended the Invitational Luncheon on the first day of the conference heard the welcome extended by President Ruthven. This welcome expressed the thoughts of all the University people cooperating. We hope that the pleasure of those who attended the meetings equals ours in making and carrying out the necessary arrangements. May next year's conference be even more successful!

January, 1951

C. W. Good
Assistant Director
Engineering Research Institute
University of Michigan

CONTENTS

FIRST SESSION, Monday, September 11, 9:00 A.M.—H. W. Work, presiding.

CALCULATED RISK—Its Place in the Selection, Control, and Termination of Research Projects 1

I. T. H. Vaughn 1
Discussion 9

II. E. D. Reeves 10
Discussion 13

III. D. H. Loughridge 14
Discussion 18

SECOND SESSION, Monday, September 11, 2:30 P.M.—H. M. O'Bryan, presiding.

MEASURING THE RETURN FROM RESEARCH 22

I. Allen Abrams 22

II. W. S. Parsons 24

III. C. G. Suits 27
Discussion 31

DINNER MEETING, Monday, September 11, 6:45 P.M.—J. P. Adams, presiding.

ENGINEERING RESEARCH IN MODERN CORPORATIONS, J. C. Zeder 37

THIRD SESSION, Tuesday, September 12, 9:30 A.M.—M. A. Williamson, presiding 41

WHAT IS NEEDED IN A RESEARCH EXECUTIVE

I. J. C. Flanagan 41
Discussion 45

II. A. E. Lombard, Jr. 48
Discussion 51

III. R. D. Stevens 53
Discussion 56

FOURTH SESSION, Tuesday, September 12, 2:00 P.M.—A. E. White, presiding 58

OVERHEAD—As a Factor in Sponsored Research, W. K. Pierpont 58
Discussion 63

FIFTH SESSION, Wednesday, September 13, 9:30 A.M.—J. I. Mattill, presiding

NEW GOVERNMENT SERVICES TO RESEARCH 70

I. National Science Foundation, T. J. Killian 70
Discussion 74

II. Building Research, R. U. Ratcliff 74
Discussion 78

III. Research for Small Industry, J. C. Green 81

SUMMARY OF CONFERENCE

E. A. Walker 86

ROUND-TABLE DISCUSSIONS 20, 34, 57, 69

COMMITTEES

EXECUTIVE COMMITTEE

I. C. Crawford, *Chairman*

C. W. GOOD
GEORGE L. HALLER
H. P. HAMMOND
MAURICE HOLLAND
O. C. MAIER
H. J. MASSON
C. G. WORTHINGTON
G. H. YOUNG

ADVISORY COMMITTEE

C. G. Worthington, *Chairman*

S. L. BASS
W. R. BRODE
D. B. CHAMBERS
I. C. CRAWFORD
F. M. DAWSON
P. D. FOOTE
GEORGE L. HALLER
H. P. HAMMOND
K. L. HOLDERMAN
MAURICE HOLLAND
D. B. LANGMUIR
D. H. LOUGHRIDGE
O. C. MAIER
H. J. MASSON
H. M. O'BRYAN
D. L. PUTT
A. M. ROTHROCK
H. A. SCHADE
N. A. SHEPARD
M. H. TRYTTEN
E. A. WALKER
A. S. WATERMAN
G. H. YOUNG

PROGRAM COMMITTEE

H. K. Work, *Chairman*

C. W. GOOD
H. M. O'BRYAN
FRED OLSEN
M. A. WILLIAMSON

REGISTRANTS

Abrams, Allen	Marathon Corp., Rothschild, Wis.
Adams, J. P.	Provost, University of Michigan
Adams, R. R.	Battelle Mem. Inst., Columbus, Ohio
Anthony, R. N.	Bus. Adm. Graduate School, Harvard Univ., Boston 63, Mass.
Asbury, W. C.	Standard Oil Development Co., New York, N. Y.
Barker, E. F.	Dept. of Physics, University of Michigan
Bartell, F. E.	Dept. of Chemistry, University of Michigan
Bartholomew, Earl	Res. Labs., Ethyl Corp., 8 Mile Rd., Detroit 20, Mich.
Beall, P. R.	Research and Development Board, Washington, D.C.
Beals, R. P.	Burroughs Adding Machine Co., Detroit 32, Mich.
Begun, S. J.	Brush Development Co., 3311 Perkins Ave., Cleveland, Ohio
Bennett, W. I.	Dean, Coll. of Arch. & Design, Univ. of Michigan
Binkerd, E. F.	Armour and Co., Union Stock Yards, Chicago 9, Ill.
Bittenbender, W. A.	Merck and Co., Inc., Rahway, N. J.
Blake, W. T.	Pillsbury Mills, Minneapolis, Minn.
Blanc, L. A.	Caterpillar Tractor Co., Peoria, Ill.
Boston, O. W.	Metal Processing Dept., Univ. of Michigan
Boyce, E. F.	Civil Engineering Dept., Univ. of Michigan
Boyce, Joseph	Argonne National Labs., Box 5207, Chicago 80, Ill.
Boyer, Raymond	Dow Chemical Co., Midland, Mich.
Briggs, R. P.	Vice President, University of Michigan
Brothers, L. A.	Hdqrs., USAF, 5D-866 Pentagon, Washington, D.C.
Brown, G. G.	Chem. and Met. Eng. Dept., University of Michigan
Callender, Arch. B.	Glenn L. Martin Co., Baltimore 3, Md.
Carpenter, W. H., Jr.	Babson Inst. of Bus. Adm., Babson Park, Mass.
Carr, A. R.	Dean, College of Eng., Wayne University, Detroit, Mich.
Champlain, W. P.	AF Cambridge Res. Labs., Cambridge, Mass.
Cole, R. I.	Watson Labs., AMC, Red Bank, N.J.
Coles, H. L.	Michigan College of Mining and Tech., Houghton, Mich.
Cooper, A. H.	Bucknell University, Lewisburg, Pa.
Crane, N. D.	Research and Development Board, Washington, D.C.
Crawford, I. C.	Dean, College of Eng., University of Michigan
Crowe, J. M.	Exec. Ed., American Chemical Soc., 1155 16th St., N.W., Washington, D.C.
Curry, R. B.	Applied Physics Lab., Johns Hopkins University, Silver Spring, Md.
Daniels, C. E.	Dev. Eng. Div., E. I. DuPont de Nemours and Co., Wilmington, Del.
Dannemann, H. F.	AF Cambridge Res. Labs., Cambridge, Mass.
Davis, Thomas E.	Ohio State University Res. Found., Columbus, Ohio
Dawson, F. M.	Dean, College of Eng., University of Iowa, Iowa City, Iowa
Doan, R. L.	Phillips Petroleum Co., Bartlesville, Okla.
Donaldson, H. C., Jr.	Cluett, Peabody and Co., Inc., Troy, N.Y.
Dow, Douglas	Detroit Testing Lab., 554 Bagley Ave., Detroit 26, Mich.
Dow, W. G.	Electrical Engineering Department, University of Michigan
Drysdale, Taylor	USAF, 719 Henry St., Ann Arbor, Mich.
Dunn, G. W.	Defense Research Board, Ottawa, Ontario, Canada
Dyke, H. Gordon	RCA Laboratories, Princeton, N.J.

Farrar, P. H.	Engineering Research Institute, University of Michigan
Fisher, F. R.	Res. and Dev., Sinclair Refining Co., Harvey, Ill.
Fisher, Lyman C.	Underwater Ord. Dept., U. S. Naval Ordnance Lab., White Oak, Md.
Flanagan, John C.	American Inst. for Res., University of Pittsburgh, Pittsburgh, Pa.
Fleming, John G.	Res. and Dev. Div., Bristol Co., Waterbury 20, Conn.
Fontaine, A. P.	Director, Willow Run Research Center, University of Michigan
Foote, Paul D.	Gulf Research and Development Co., Pittsburgh 21, Pa.
Furnas, C. C.	Cornell Aeronautical Lab., 4455 Genesee St., Buffalo 21, N.Y.
Furth, F.	Capt., USN, Naval Research Lab., Washington 20, D.C.
Gibson, R. E.	Applied Physics Lab., Johns Hopkins University, Silver Spring, Md.
Good, C. W.	Assistant Dir., Engineering Research Inst., University of Michigan
Graham, R. B.	Bendix Aviation Corporation, Fisher Bldg., Detroit, Mich.
Green, J. C.	Office of Technical Services, Dept. of Commerce, Washington, D.C.
Green, T. S., Jr.	Norton Co., Worcester 6, Mass.
Gunness, R. C.	Standard Oil Co. (Ind), Chicago 80, Ill.
Haller, G. L.	Dean, School of Chem. and Physics, Pennsylvania State College, State College, Pa.
Hambraeus, Gunnar	Tech. Adv., Swedish Embassy, 630 Fifth Ave., New York, N.Y.
Hammer, C. F.	Eng. Mgr., Westinghouse Air Brake Co., Wilmerding, Pa.
Hartley, J. C.	Winchester Repeating Arms Co. Div., Olin Indust., Inc., New Haven, Conn.
Hawkins, G. A.	Eng. Exp. Station, Purdue University, Lafayette, Ind.
Hickox, G. H.	Eng. Exp. Station, University of Tennessee, Knoxville, Tenn.
Holcomb, W. F.	Res. Dept., Parke, Davis and Co., Detroit 32, Mich.
Holland, Maurice	Ind. Res. Advisor, 40 W. 40th St., New York, N.Y.
Holmes, L. C.	Res. Dept., Stromberg-Carlson Co., Rochester 3, N.Y.
Huebner, G. J., Jr.	Res. Dept., Chrysler Corp., Box 1919, Detroit 31, Mich.
Hughes, E. C.	Standard Oil Co. (Ohio), 2127 Cornell Rd., Cleveland, Ohio
Hyde, J. W.	Coll. of Arch. & Design, University of Michigan
Ikehara, Shikao	Tokyo Inst. of Technology, Meguroku, Tokyo, Japan
Jacobsen, J. M.	Office Govt. Spon. Res., University of Texas, Austin, Texas
Jacobsen, Mrs. J. M.	Office Govt. Spon. Res., University of Texas, Austin, Texas
Jakkula, A. A.	Res. Found., A. and M. College of Texas, College Station, Texas
Jones, W. N.	College of Eng. & Science, Carnegie Inst. of Technology, Pittsburgh 13, Pa.
Jordan, Louis	Div. Eng. & Ind. Res., Nat'l Res. Council, 2101 Constitution Ave., Wash., D.C.
Keirn, D. J.	Col. USA, Res. & Dev. Command, Dept. Air Force, Washington, D.C.
Kelly, H. C.	Sci. Sec., Office of Naval Research, Chicago, Ill.
Keniston, Hayward	Dean, College of Lit., Sci., & the Arts, University of Michigan
Kenyon, R. L.	Assoc. Ed., Chem. & Eng. News, 25 E. Jackson Blvd., Chicago, Ill.
Killian, T. J.	Office of Naval Research, Washington, D.C.
Kimball, C. N.	Midwest Res. Inst., 4049 Pennsylvania Ave., Kansas City 2, Mo.
Klopsteg, P. E.	Technological Inst., Northwestern University, Evanston, Ill.
Knapp, T. E.	Res. Dept., Champion Paper & Fibre Co., Hamilton, Ohio
Kolstad, G. C.	Res. Div., U.S. Atomic Energy Comm., 1901 Constitution Ave., Washington, D.C.
Kurt, O. E.	Res. Labs., Ethyl Corp., 8 Mile Rd., Detroit, Mich.
Larson, C. T.	College of Architecture & Design, University of Michigan
Lawrence, F. I. L.	Res. and Dev., Kendall Refining Co., Bradford, Pa.
Lehman, Paige	Pillsbury Mills, Inc., Minneapolis 2, Minn.
Lombard, A. E., Jr.	Hdqrs., USAF, Directorate of Res. & Dev., 4E-322 Pentagon, Washington, D.C.
Loughridge, D. H.	U. S. Army, 3E-616 Pentagon, Washington, D.C.

Lowance, F. E.	Naval C. E. Lab., Pt. Hueneme, Cal.
Lyman, C. H.	Capt. USA, Bur. Ord., Navy Dept., Washington, D. C.
McKean, W. B.	Col. MC, Naval Science, University of Michigan
Mahin, W. E.	Armour Res. Found., Ill. Inst. Tech., Chicago 18, Ill.
Maier, O. C.	Res. Div., Pullman Standard Car Mfg. Co., Hammond, Ind.
Marchetti, John	AF Cambridge Res. Lab., Cambridge, Mass.
Marlowe, D. E.	Naval Ordnance Lab., White Oak, Md.
Mattill, J. I.	Eng. Coll. Res. Council, Mass. Inst. Tech., Cambridge, Mass.
Maxfield, F. A.	Bur. Ord., Navy Dept., Washington, D.C.
Meid, G. D.	National Academy of Sciences, 2201 Constitution Ave., Washington, D.C.
Miller, F. L.	Esso Laboratories, Elizabeth 13, N.J.
Montgomery, W. P.	Booz, Allen & Hamilton, 135 S. LaSalle St., Chicago 3, Ill.
Morgen, R. A.	Florida Eng. & Ind. Exp. Sta., University of Florida, Gainesville, Fla.
Morse, R. S.	National Research Corp., Cambridge, Mass.
Nelles, Maurice	Eng. Exp. Sta., Pennsylvania State College, State College, Pa.
Northrup, D. L.	Hdqrs., USAF, Off. for Atomic Energy, DCF/O, Att: AFOAT-1, Washington, D.C.
O'Brien, R. A.	Amer. Soc. of Mech. Engrs., 29 W. 39th St., New York, N.Y.
O'Bryan, H. M.	Research and Development Board, Washington, D.C.
O'Roke, E. C.	School of Nat. Resources, University of Michigan
Owens, J. S.	Ohio State University Res. Found., Columbus 10, Ohio
Parsons, W. S.	Rear Adm. USN, Weapons Sys. Eval. Group, Off. of Sec. of Def., Washington, D.C.
Pennington, J. V.	Drilling Res., Inc., 1320 City Nat. Bank Bldg., Houston 2, Texas
Perkins, J. A.	Asst. Provost, University of Michigan
Pettee, G. S.	Ops. Research Off., ORD, Ft. McNair, Washington, D.C.
Pierpont, W. K.	Controller, University of Michigan
Poehle, H. F.	Engineering Research Institute, University of Michigan
Quinby, E. J.	Electronic Research & Dev., Monroe Caculating Machine Co., Orange, N. J.
Quinsey, W. E.	Engineering Research Institute, University of Michigan
Ratcliff, R. U.	Housing and Home Finance Agency, Washington, D.C.
Reeves, E. D.	Standard Oil Dev. Co., 15 W. 51st St., New York, N.Y.
Reichl, E. H.	Res. & Dev. Div., Pittsburgh Cons. Coal Co., Library, Pa.
Robertson, R. M.	Phys. Sci. Div., Off. of Naval Research., Washington, D. C.
Rosselot, D. A.	Eng. Exp. Sta., Georgia Inst. of Tech., Atlanta, Ga.
Ruthven, A. G.	President, University of Michigan
Sawyer, R. A.	Dean, Graduate School, University of Michigan
Schade, H. A.	Dept. of Mech. Eng., University of California, Berkeley, Cal.
Scheumann, W. W.	Cities Service Res. & Dev. Co., 70 Pine St., New York, N.Y.
Schuh, A. E.	Res. & Dev., U. S. Pipe & Foundry Co., Burlington, N.J.
Seeger, R. J.	Aeroballistic Res. Dept., Naval Ord. Lab., White Oak, Md.
Seiler, F. J.	Off. of Air Res., AMC, Wright-Patterson AF Base, Dayton, Ohio
Selvidge, Harner	Spec. Prod. Dev., Bendix Aviation Corp., Detroit, Mich.
Seski, E. J.	Cook Res. Labs., 1457 W. Diversey Pkwy., Chicago, Ill.
Shepard, N. A.	American Cyanamid Co., 30 Rockefeller Plaza, New York, N.Y.
Shinn, C. E.	Electronics Dept., Burroughs Add. Mach. Co., 511 N. Broad St., Philadelphia, Pa.
Simon, L. E.	Brig. Gen. USA, Ord. Dept., Aberdeen Proving Ground, Md.
Smith, R. L.	Sinclair Refining Co., Harvey, Ill.
Smith, T. R.	Res. and Dev., Maytag Co., Newton, Iowa
Solberg, A. N.	Res. Found., University of Toledo, Toledo 6, Ohio
Spencer, R. G.	Washington University Res. Found., Clayton 5, Mo.
Stauffer, R. A.	National Res. Corp., 70 Memorial Dr., Cambridge 42, Mass.

Steding, H. R.	Chrysler Corp., Detroit 31, Mich.
Steinle, J. V.	Res. & Dev., S. C. Johnson & Son, Inc., Racine, Wis.
Stern, Benjamin	Col. USA, Signal Corps Eng. Labs., Fort Monmouth, N.J.
Stevens, Raymond	A. D. Little Co., Cambridge, Mass.
Stevenson, R. A.	Dean, School of Bus. Administration, University of Michigan
Stewart, D. B.	Res. Center, B. F. Goodrich Co., Brecksville, Ohio
Stewart, Ross	Cook Res. Labs., 1457 Diversey Pkwy., Chicago 14, Ill.
Suits, C. G.	General Electric Res. Labs., Schenectady, N.Y.
Suter, C. M.	Sterling-Winthrop Res. Inst., Rensselaer, N.Y.
Tailman, O.	Watson Labs., AMC, Red Bank, N.J.
Thompson, A. P.	Eagle-Picher Co., Box 290, Joplin, Mo.
Tour, Sam	Sam Tour and Co., 44 Trinity Pl., New York 6, N.Y.
Townsend, J. C.	Burroughs Adding Machine Co., 511 N. Broad St., Philadelphia, Pa.
Travis, Irven	Burroughs Adding Machine Co., 511 N. Broad St., Philadelphia, Pa.
Trichel, G. W.	Chrysler Corporation, Detroit, Mich.
Upton, H. H.	Double A Products Co., Manchester, Mich.
Vaughan, H. F.	School of Public Health, University of Michigan
Vaughn, T. H.	Wyandotte Chemicals Corp., Wyandotte, Mich.
Vincent, H. B.	American Structural Products Co., Toledo 1, Ohio
Walker, E. A.	Research and Development Board, Washington, D.C.
Walker, H. S.	Detroit Edison Co., Detroit 26, Mich.
Waterman, A. T.	Office of Naval Research, Washington, D.C.
Way, Gordon T.	Res. Div., Smith, Kline & French Labs., 1530 Spring Garden St., Philadelphia, Pa.
Wendt, K. F.	Eng. Exp. Sta., University of Wisconsin, Madison, Wis.
Wernette, J. P.	Dir., Bur. of Bus. Res., University of Michigan
White, A. E.	Dir., Engineering Research Institute, University of Michigan
White, R. H.	Maj. USA, Res. and Dev., Detroit Arsenal, Centerline, Mich.
Williamson, M. A.	Pullman Standard Car Mfg. Co., Hammond, Ind.
Woods, Hubert	Res. Dept., Portland Cement Association, 33 W. Grand Ave., Chicago 10, Ill.
Woolrich, W. R.	Dean, College of Eng., University of Texas, Austin 12, Texas
Work, H. K.	Chairman, College of Eng., New York University, New York 53, N.Y.
Worthington, C. G.	Ind. Res. Institute, 60 E. 42nd St., New York, N.Y.
Young, G. H.	Mellon Inst. of Ind. Res., Pittsburgh, Pa.
Zarem, A. M.	Stanford Res. Inst., Los Angeles 17, Cal.
Zeder, J. C.	Chrysler Corporation, Box 1919, Detroit 31, Mich.

First Session

Harold K. Work, *presiding*
Director of Research Division
College of Engineering, New York University

CALCULATED RISK
Its Place in the Selection, Control, and Termination of Research Projects

— I —

by
THOMAS H. VAUGHN
Vice President — Research and Development
Wyandotte Chemicals Corporation

THERE ARE MANY FACTORS of risk which enter into the selection, control, and termination of a research project. Although many of these factors are common to all industrial research, there are a few which are peculiar to the type of research practiced by the chemical industry. Being in a chemical organization and associated with other speakers, who are primarily concerned with the type of research practiced in the petroleum industry and our military establishments, I shall confine myself to considerations of risk with respect to research in the chemical industry.

It is somewhat difficult to come to grips with a topic of this type in so short a time because there are many variations in the types of risk factors and in the degrees of their application at various stages in the life of a research project. Thus, certain factors which may be dominant and controlling in connection with the selection of the project may have little bearing on the control of the work but may again become important in connection with the termination of the project.

Accordingly, I wish to concentrate on one phase of risk calculation which is of overriding importance in all phases of a research project from its inception to its final disposal, and that is the calculation or estimation of the effect of the project on the health of the organization supporting the work. I use the word "health" primarily in a financial sense but also as including other factors, as will be apparent.

All executive groups in industrial organizations who come in contact with the product of research divisions are increasingly interested in measuring the return from research. The time to start thinking about the returns from a program of research is at the beginning, when you start the various projects which comprise the program.

While research and development divisions in most chemical organizations have many responsibilities which cannot properly be defined as research and development, all, or certainly most, are primarily charged with responsibility for the development of the future products of the corporation and, in most instances, are responsible for the maintenance of quality and superiority, under competitive conditions, of the products in which the corporation is presently interested. In other words, research has a broad responsibility for the future growth and success of the corporation and in some instances has laid down for it by the president or the board of directors the areas into which its work must be channeled in order to be of maximum value. This places the executive in charge of research in a position where he has some general background for the evaluation of ideas which may be brought up for inclusion in the research program.

There are many general criteria by which the suitability of a project can be judged. These range from purely technical evaluations or guesses as to the possibility of completing the research satisfactorily from a technical point of view to very broad business questions having to do with the future health of the child of research, should it pass through adolescence and reach maturity. Such general business questions should be of great concern to the man responsible for planning and conducting the research program. A few simple questions of this type are: Is the project one which will lead to a product which is in our field of business? Do we have competent manpower and equipment to do the job? Do we have a unique or good raw-material situation? How much investment will it require? What kind of a return can we expect on that investment? Will the product create new markets or will it enter into a market which is currently of sufficient stability and expansiveness to absorb the new material? The answer to these and many other similar questions add up to a composite answer to the question: Is it good for the health of our organization?

At the very beginning of a project and frequently throughout its life, a project should be subjected to a searching economic evaluation. To do this requires a background or framework of reference with which the project may be compared. Fortunately, two articles have recently appeared dealing with the financial facts of life of chemical and allied companies. The first article, by Pescatello, appeared in *Chemical Industries* early this year and dealt with twenty-five selected chemical companies.

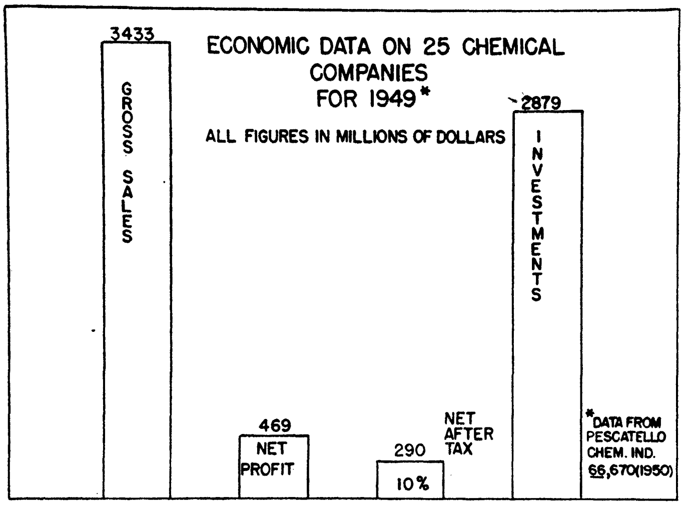


Figure 1

The economic data shown in Fig. 1 indicates the overall state of health of these twenty-five companies for the year 1949. It will be noted that their gross sales were somewhat in excess of the invested capital in the business with a ratio of approximately 1.15 to 1 and that the net profit after taxes (NAT) represented approximately 8.5 per cent of the gross sales volume and a return of 10 per cent on the investment.

A similar study by Aries and Spence which appeared in the *Facts and Figures* issue of *Industrial and Engineering Chemistry* this year dealt with one hundred chemical and allied companies and gave a rather exhaustive survey of the financial facts regarding these companies. A summary of the economic data

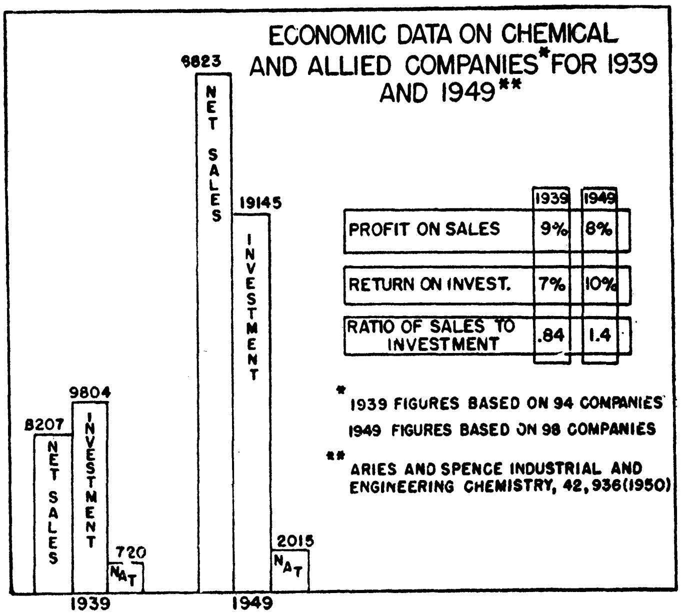


Figure 2

contained in this article for the years 1939 and 1949 is indicated in Fig. 2. It will be noted that here too, when dealing with the allied industries as well as the chemical industries, the volume of sales for the year 1949 is larger than the capital invested in the corporations, with a ratio of roughly 1.4 to 1. The return on investment in 1949 agrees exactly with that indicated in the previous article for the chemical industry alone, namely, 10 per cent, and the percentage of profit on sales for the year 1949 was 8 per cent. It is interesting to compare these data with those for 1939. Here net sales are less than capital investment, the ratio being 0.84 to 1, and the net return after taxes expressed as return on investment was only 7 per cent as compared to 10 per cent in 1949, whereas the profit on sales was 9 per cent as compared to 8 per cent for 1949.

This financial information provides a general background against which research projects in the chemical and allied industries may be evaluated in terms of their possible worth to an organization should they prove to be technically successful. Of course, similar information of interest to the research executive can be obtained for any selected group of companies by consulting the annual statements of the companies or by using information available from Standard and Poors, Dunn and Bradstreet, and other financial reporting houses.

For the purposes of this discussion, let us set up a hypothetical corporation which has approximately $100 million invested in its business and which at present has a net sales volume of approximately $110 million a year. Currently, this company is enjoying a net return after taxes of only $8 million a year. This company is considerably behind the parade in several respects: The ratio of sales to the capital invested is lower than it should be, this ratio being only 1.1 to 1, whereas the average for the industry as a whole is 1.4 to 1. The net return on investment is only 8 per cent as compared to a 10 per cent average for the industry, and the profit based on sales is only 7.3 per cent against an industry average of 8 per cent. Obviously, then, this company has a sincere interest in a research program which is aimed at projects yielding an unusually high return and would probably be willing to put a large proportion of its research funds into such projects, even though some of them may have considerable risk attached as compared to short-term, sure-payoff jobs which would not materially improve the relative position of the company.

It is naturally the intention of all research administrators to do their part in having their company operate with a profit picture that is considerably better than the average for the industry as a whole. It is obvious that the research administrator in this particular hypothetical company has quite a job on his hands.

What I said earlier about the attitude toward short-term research in this particular company does not, of course, preclude the possibility of its research director being very seriously interested in short-term projects which show a large return on investment. For example, let us consider a completely hypothetical case involving some hypothetical compounds. This corporation is manufacturing methyl esterate. The process has given them considerable difficulty and is not showing the kind of yields hoped for. It has been proposed in one of the research laboratories that the process could be materially improved and that considerable savings could be made if metallic copper were to be substituted for the present cupric chloride catalyst used in the process. The research division has spent to date approximately $2,000 in preliminary investigation and feels that the chances of success are excellent. Accordingly, the time has come for a decision to be made as to whether or not additional research effort is to be spent on this project. While the project has had a rather cursory economic evaluation in the very beginning, the time has now come for a somewhat more thorough evaluation.

At this stage in our own organization, the supervisor in charge of the project would address a memorandum to the director of research of the division in which he would cover the basic objectives of the program, provide background information (in a condensed form, since the director of research is assumed to be cognizant of the main features of the methyl esterate process), provide a research forecast which states the amount of money which has been spent in the months in which the program has had part-time attention, cover the present status of the project, estimate how much money will be needed to conclude the research phase of the project, estimate the date on which the project can be terminated, and outline the probability of technical success. He goes further and provides a production forecast which indicates the probable investment which would be required to put the findings of the research group into effect, assuming of course that they are positive and turn out as he expects, stating when the changes in equipment could be made and when, in his opinion, they should be made. He also provides information of an economic nature, indicating the savings which would result if the contemplated change were made.

The savings in this particular case fall into three categories: first, savings due to the use of less catalyst, since the feed rate of the copper powder could be more accurately controlled than that of the cupric chloride and the excess quantities which are being presently used would not be necessary; second, savings due to the fact that copper is a lower-priced material than cupric chloride; and third, savings which would result from the fact that there would be slightly increased yield of methyl esterate from the use of copper versus copper chloride.

This information is reviewed by the director of research and, after making some necessary changes, deletions, or additions, is finally turned over to a draftsman for the preparation of a chart indicating the salient features involved. This chart would look something like that in Fig. 3. It will be noted that

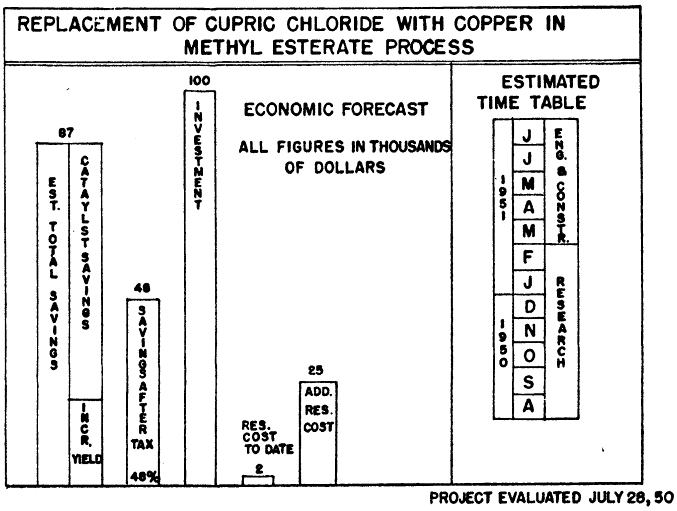


Figure 3

the estimated total savings resulting from this project are $87,000 per year, this being broken down into two parts, one dealing with the increase in yield and the other with the catalyst savings. After income taxes (based on the taxes which exist now and not on those which we are going to face in the future), the savings would be $48,000. It is estimated that the capital required to put the new process in production would be $100,000. The research supervisor in preparing this estimate of capital investment has, of course, checked with the engineering department, with production personnel, and with others who might be in a position to offer advice and counsel. Research cost to date, as stated earlier, is $2,000, and the supervisor has estimated an additional $25,000 will be required to complete the program.

The estimated time table indicates that the completion of the research phase will require until approximately March 1, 1951, and that engineering and construction could proceed very rapidly because of the very minor character of the changes involved and because of the fact that most of the required equipment is already on hand, so that savings from this change could be realized by our hypothetical corporation as early as August 1, 1951.

This project costing only a comparatively small amount of money and promising a return on investment of 48 per cent would appear to be a rather favorable one and accordingly would be placed on the considered program of the research and development division. Work now proceeds, and we move from July 28, 1950, to January 20, 1951. At this time, according to Fig. 4, we have spent an additional $17,000, bringing our research costs to date up to

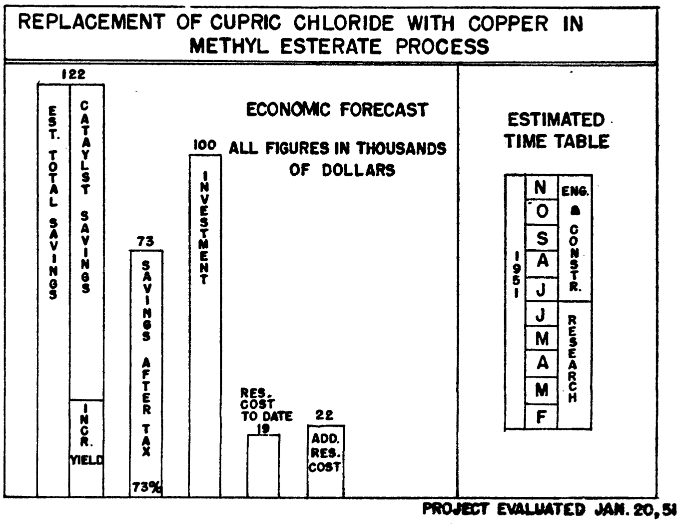


Figure 4

$19,000. We are approximately one month short of the completion date which our supervisor estimated earlier and have not been able to spend money quite as fast as he had anticipated. Also, you will note that the project has undergone considerable change. It is now estimated on the basis of the results obtained by research that the savings each year after taxes would be approximately $73,000 and that the investment figure has not changed and that therefore the return after taxes would be increased to approximately 73 per cent on the investment. We find also that the research group has not buttoned the project up as adequately as they would like and now estimate they must spend an additional $22,000 to complete the project. The time for completion of research has also advanced from the end of February, 1951, to the end of June, 1951, and the estimated completion of the project in the hands of the engineering and production departments is estimated as November, 1951, instead of August, 1951.

The project, however, still looks like an extremely good project—in fact, almost as good as it did before. The $17,000 has been well spent and the project would naturally be continued.

Let us take another look at how the project might have turned out, however, as indicated in Fig. 5. We

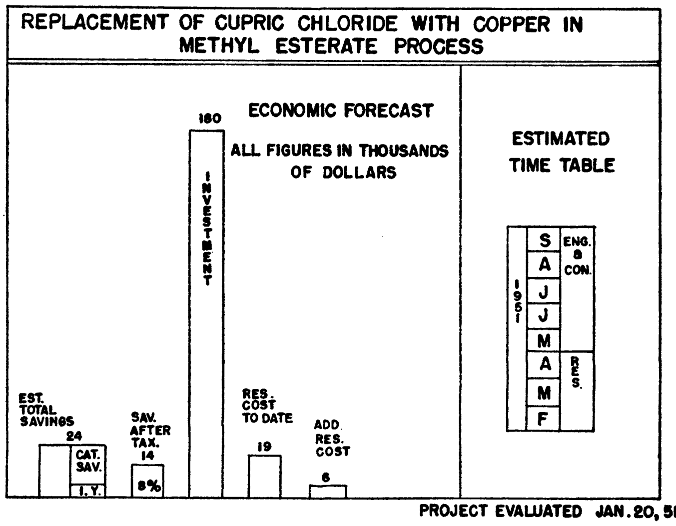
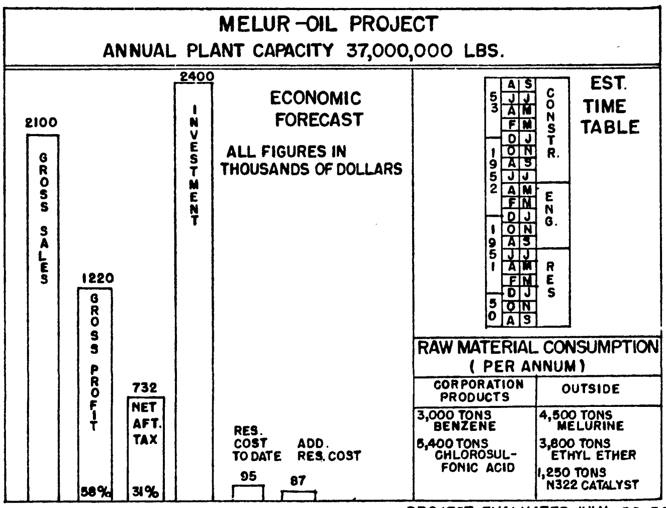


Figure 5

notice that several things have happened. We have spent our $19,000 on research as we did in the previous case, but the results of that research have indicated that the savings which we had in mind in the beginning are much lower than our previous estimate and that the investment has also risen because we have found that in order to handle copper, we must make other changes in processing, which involve the use of special alloy equipment instead of steel, and the investment figure is accordingly increased. The project now looks rather sick because the net savings after taxes would be only $14,000, necessitating an investment of $180,000. Now, despite the fact that the table looks more optimistic than in the previous situation and that the added research costs are only $6,000, the research director would probably terminate the project since it does not tend to improve the overall financial picture of the company.

I might say, however, that the information contained on this chart is not necessarily the whole story. It may well be that the plant is having such difficulty in operating the process with cupric chloride as a catalyst that the corporation would be ahead if it were to make the indicated change to copper even though it would cost them some money. This would be a reflection upon the research administrator and his sources of information because if that were the case, the estimated savings on the chart should reflect in some manner the difficulties which the production department is having with the process at the present time.

Now let us look at some of the other research projects which our hypothetical research and development division has under way. One project on which it has been engaged for some time and on which it has spent $95,000 to date is the Melur-oil project involving the synthesis of melurine-1, 5-disulfonic acid and its application as an oil-treating agent for automotive use. This project looked good when an economic forecast was made on it approximately two years ago, and our latest forecast, which was made on July 28, 1950, still looks exceedingly promising. It will be noted in Fig. 6 that this project, which calls for a capital investment of approximately $2,400,000, will show a gross sales of $2,100,000. This does not quite meet the needs of our hypothetical corporation because the ratio is still adverse, but, on the other hand, this project shows a good gross profit and a net return after taxes of approximately 31 per cent and will require the investment of less future research money to bring it to fruition than has previously been spent. In other words,

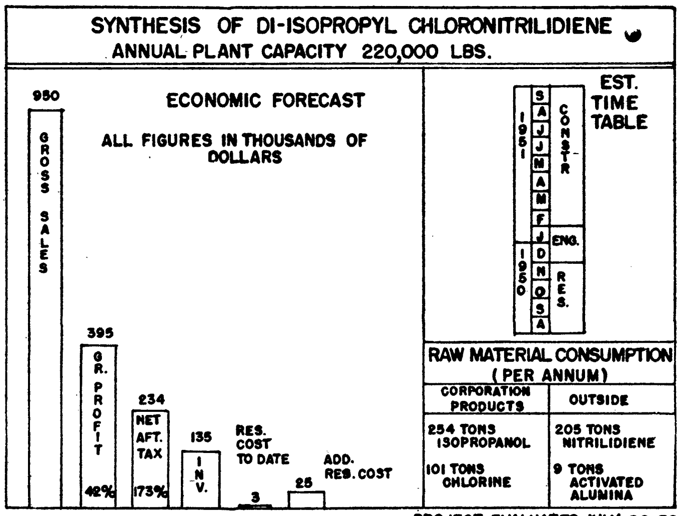


PROJECT EVALUATED JULY 28,50

Figure 6

the investment of the corporation is increased by about 2.4 per cent to add approximately 2 per cent to its annual sales volume, while increasing its profit by almost 10 per cent. This naturally appeals not only to the research director but also to the other executive officers of the company, and he, accordingly, will have no trouble in pushing the project through. Equally important is the fact that the project would consume products presently produced by our hypothetical corporation, namely, 3000 tons of benzene and 5400 tons of chlorsulfonic acid. It would also involve the purchase on the outside of other raw materials as indicated in Fig. 6, which, it happens, are extremely easy in supply at this time and which we will have no difficulty in obtaining in suitable quality and quantity for several years to come, according to our purchasing and market research departments. Consequently, our research director looks at this project in its present status and, after discussing it with other divisions of the corporation, approves the expenditure of the estimated additional $87,000. He does this because he believes that any project that will yield better than 25 per cent after taxes is a good one, provided all the other factors seem favorable. In this particular case, the sales department is extremely anxious to have the product, and, accordingly, there seems to be every reason to push the project.

Another project which is brought before the director of research in the form of an economic evaluation is the synthesis of diisopropyl chloronitrilidine (Fig. 7).



PROJECT EVALUATED JULY 28, 50

Figure 7

This material is a new compound, but application work in the corporation's laboratories has indicated that the product will serve as an intermediate for high-temperature resistant, shockproof plastics suitable for many industrial and military applications. The project is new, only $3,000 having been spent on it to date, but the prognostication of its future is extremely promising. It looks like one of these projects which can be brought into commercial production with an extremely small investment, a project which will add a rather large amount of gross sales to the corporation's total figures and which shows an extremely high net after taxes based on investment, namely, 173 per cent. The gross profit of 42 per cent, based on sales, is also high, and, accordingly, the project looks extremely promising—even more promising when it is estimated that only $25,000 need be spent on the project to bring it to fruition and that the plant can be completed for operation approximately October 1, 1951. The research director would push this project for all it is worth since it would appear to be an ideal project for the improvement of the corporation's picture, offering a very large return on a small investment and providing early profits for reinvestment in other projects.

Still another and the last of the hypothetical projects to be considered is the development of a continuous process for the production of sodium hexametasilicate (Fig. 8). Sodium hexametasilicate has been found by workers on the Continent to be an extremely suitable alkaline salt for water purification. The corporation has both a captive use for this product and the possibility of external sale. The product uses one of the corporation's products and requires the purchase on the outside of a relatively cheap and readily

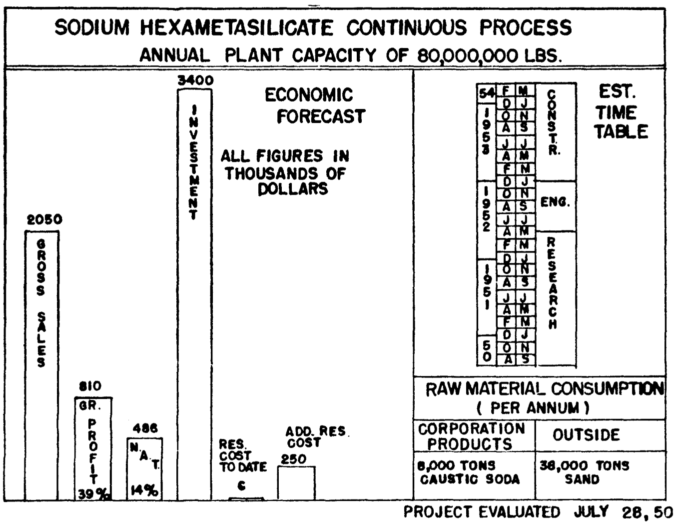


Figure 8

available material. This again is a new project since only $6,000 has been spent on research to date. The probable additional research costs are somewhat high —$250,000. The investment again is adverse, an investment of $3.4 million being required for a gross sale of $2,050,000. The gross picture is also not very good since only 39 per cent would be realized and the net after taxes is only 14 per cent. The research director looks this picture over and may decide to go one way or the other. In this particular case, however, the sales department is demanding a product of this type, and he knows that many of the corporation's present products could be markedly improved if sodium hexametasilicate were available at a reasonable cost. Therefore, although the net after taxes does not measure up to the 25 per cent, which he has arbitrarily adopted as being the standard for research projects, he goes ahead with good will and retains the project on his program.

Let us assume that these four projects constitute the entire research program of the corporation under discussion. This obviously is foolish, because the total *amount of research expenditure called for* by these projects is very much lower than that which a corporation of the size indicated should be investing in research each year. However, time is limited and the delineation of all the projects which such a corporation should have would serve no useful purpose.

In review, let us look at a summation of the economic forecasts for these evaluated projects covering the years 1951 to 1955, inclusive, as shown in Fig. 9. Here *we find the picture*, year to year, of what the corpora-

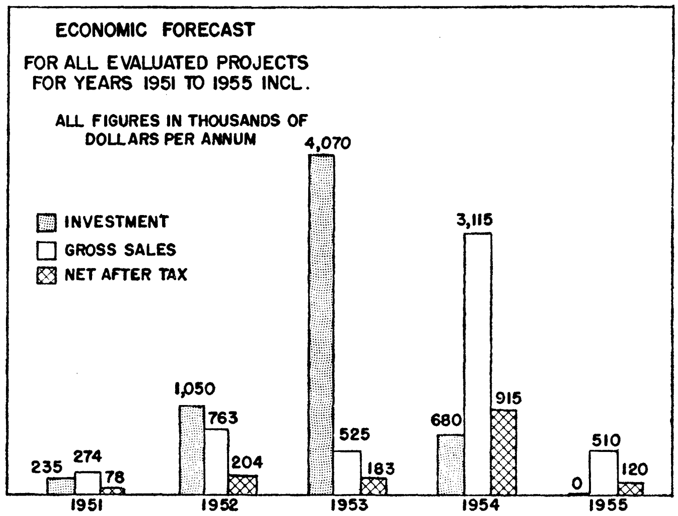


Figure 9

tion may have to invest, what the corporation may realize in the way of added gross sales, and what the net after taxes may be. This type of information is particularly helpful to other executives in the company since it shows them what their research program may lead to. The people primarily concerned with the financial fortunes of the corporation are put on guard, because the program which the research division has under way will require the investment of certain sums of capital. They may then decide either that this capital requirement is exorbitant or that it can be readily met, and that decision will naturally have an effect upon the research program. Similarly, the added sales which will result from year to year are of interest to the executive in charge of sales. He knows whether he needs to be training or hiring new salesmen and when; he knows, from a consideration of the individual projects, what kind of salesmen they should be, and thus he is placed on notice as to exactly what may be expected of him in the future. The material is of equally great importance to the production department and other groups.

Fig. 10 indicates the cumulative effect of the few research projects discussed on the future fortunes of the corporation. It shows that, if the projects were successful through 1955, a total investment of $6,035,-000 would be required, which would result in gross sales of approximately $5,200,000, with a return after taxes of $1,500,000. This would hold some appeal to the corporation as a whole because the return on the *new projects after taxes is very much better than that* presently enjoyed by the corporation. Although this portion of the corporation's total research program is decidedly deficient in that it does not improve the ratio of total sales to investment, it does have a very appreciable effect on the over-all return—both on capital investment and on sales. The net effect on corporate finances would be to increase the return on capital from 8.0 to 9.0 per cent and from 7.3 on sales to 8.3 per cent.

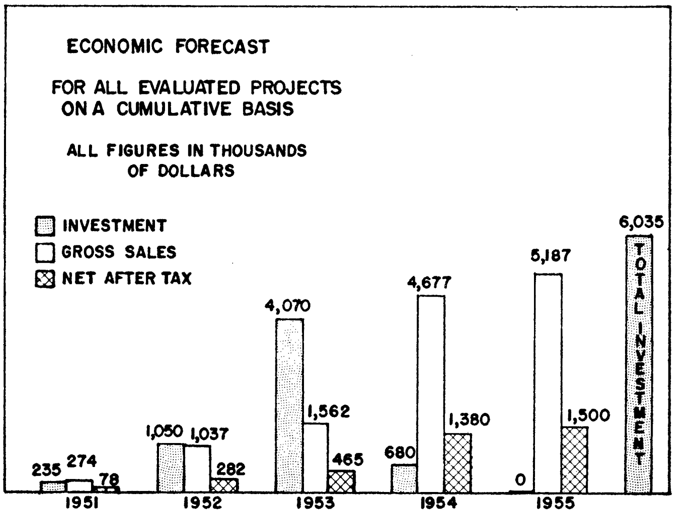


Figure 10

Another use of the information shown in the previous figures is to prepare a forecast of the demand on the corporation's present products to determine whether or not the present capacity of the corporation is adequate to meet the demands which the research division is trying to set up by the expenditure of the corporation's funds. Such a forecast is shown in Fig. 11, which indicates for the years 1951 through 1955 the requirements for raw materials produced by the corporation for the research program presently under way. This is of tremendous aid to the sales and production executives of the company. In the case under consideration we have no trouble because in every

ANNUAL CONSUMPTION OF CORPORATION'S PRODUCTS BY ALL PROJECTS

YEAR	CONSUMPTION TONS	PRODUCT	% OF ANNUAL CAPACITY	% OF UNSOLD CAPACITY 1950
1951	70	ISOPROPANOL	1.5	8.2
	30	CHLORINE	0.15	10.0
1952	254	ISOPROPANOL	6.5	35.5
	101	CHLORINE	0.5	34.0
1953	1000	BENZENE	2.3	5.4
	1800	CHLOROSULFONIC ACID	8.2	18.0
	254	ISOPROPANOL	6.5	35.5
	101	CHLORINE	0.5	34.0
1954	6000	CAUSTIC SODA	1.6	34.0
	3000	BENZENE	6.8	16.2
	5400	CHLOROSULFONIC ACID	25.4	54.0
	254	ISOPROPANOL	6.5	35.5
	101	CHLORINE	0.5	34.0
1955	8000	CAUSTIC SODA	2.2	51.0
	3000	BENZENE	6.8	16.2
	5400	CHLOROSULFONIC ACID	25.4	54.0
	254	ISOPROPANOL	6.5	35.5
	101	CHLORINE	0.5	34.0

Figure 11

instance the amount of product required as raw materials for the production of products which the research division hopes to develop is less than the unsold capacity of the company. They do, however, give the executive in charge of sales some reason for concern. He is definitely put on notice that as early as 1952 this corporation may demand from its present production capacity certain amounts of isopropanol and chlorine and that these amounts are not small but represent approximately one-third of the present unsold capacity of the company. He therefore should be somewhat reluctant to enter into long-term contracts to sell a greater volume of isopropanol and chlorine or, alternately, he should insist that if he decides to enter into such contracts, the production department be prepared to expand their capacity so that by 1952 adequate amounts will be available to supply the needs of the research projects presently under way. Actually, in the consideration of such figures, executives outside the research department should pay very little attention to figures projected for more than one or two years. After all, the fact that our requirement for chlorsulfonic acid is going to take 54 per cent of the present unsold capacity in 1955 is not a very serious matter as far as a sales manager is concerned. So many things will happen before 1955 that he need not be concerned about this requirement until it is more imminent.

The estimated chances of success of the various projects have been passed over very lightly in the previous discussion. In Fig. 12 is shown the estimate of the di-

ESTIMATED CHANCE OF SUCCESS

PROJECT	RETURN ON INVEST. %	PROBABILITY OF TECHNICAL SUCCESS, %	PROBABILITY OF OVERALL SUCCESS, %
COPPER CATALYST	48	100	95
SODIUM HEXAMETASILICATE	14	100	95
MELUR-OIL	31	95	90
PRE-PLAST	173	90	80

Figure 12

rector of research as to the probability of success, both technical and overall, of the projects discussed. About the copper-catalyst project, which shows a 48 per cent return on investment, he feels quite confident that there is no possibility of falling down on the job and that the copper catalyst will work. He does feel, however, that the plant may have some difficulty in putting this catalyst into use and therefore he rates down his probability of overall success to 95 per cent. For sodium hexametasilicate the same situation prevails. About Melur-oil he is not quite sure and feels only 95 per cent chance of success from the technical point of view, and overall, primarily because he is concerned about the reliability of his market information, he rates it at only 90 per cent. With Pre-plast, however, he has a still different situation. He scales his probability of technical success down to 90 per cent and the probability of overall success to only 80 per cent, but the 173 per cent return on investment stands out foremost in his mind, and he is perfectly willing to keep the project active.

Today, in our troubled world situation, there is one other type of calculated risk which any director of research must definitely consider, namely, how a project which he has under way fits into the overall planning of the nation, as he sees it. And here I might say parenthetically that "as he sees it" is a very important phrase. An important part of the job of a director of research today is making every effort to be sure that he sees it right. He must be informed on both national and international affairs and must be in definite rapport with the changing events about us. His ability to interpret may make the difference between success and total failure in keeping his research organization in business. Thus, a consideration of the data in Fig. 13 might well lead to some changes

MILITARY SIGNIFICANCE OF RATED PROJECTS

	IMPORTANCE IN WAR		
	HIGH	MED.	LOW
COPPER CATALYST			X
SODIUM HEXAMETASILICATE		X	
MELUR-OIL	X		
PRE-PLAST	X		

Figure 13

in the research program to keep its military significance of a properly high order.

After one has made all these calculations and looked at all these figures, one may believe to have a basis for estimating the risk involved in research projects. One is still faced, however, with the necessity of using judgment as a deciding factor. One knows that most of the figures he uses are not factual, that they are guesses and that the area of certainty represented by each guess is open to question. One must also allow the judgment of others on some of these points to influence his own thinking, even when he is relatively sure that the judgment being expressed is incorrect. After all, the modern industrial organization moves as a team, and teamwork needs the subordination of individual thinking and judgment to that of the group as a whole.

For example, suppose that $370,000 has been spent for research on a new project. Its technical success is predicted as 100 per cent, the return as only 12 per cent, but the controller, sales manager, production department, engineering department, and all others who have contact with the project are highly enthusiastic about its probability of success. It would therefore seem to me to be an ideal project to push, even though the return on investment is considerably lower than that which you would normally consider as good. On the other hand, if you have a project which shows a 42 per cent return on investment, and production and sales are against it for reasons which you feel quite confident are wrong but which represent judgment in areas where sales and production people should be, and probably are, better informed than you, then you should turn it down because it is definitely a poor risk. It is a poor risk probably because the bases of their judgments are better than yours. Moreover, if your facts are right and theirs are wrong, you will not have the kind of support from the organization as a whole that would be necessary to put the project over.

The system discussed here has been used in our own organization for a short time only, but we are highly gratified with the results. I do not say that this system is a perfect system or even that it is an extremely good system. I can say, however, that from our own experience it has been a very important tool in getting every man on our research and development team as well as the men in other divisions of the company to think in terms of calculated risks. We find that, after exposure to this type of system for only a very short period of time, the man at the bench begins to think in terms of ultimate economics, and I know of no healthier situation in an organization whose primary aim is the commercialization of research.

Obviously, most of what has been said would have no application whatsoever to fundamental research as conducted in our universities and some of our governmental laboratories. There, the primary aim is to push back the frontiers of knowledge rather than to put to work scientific information and data for immediate or at least early commercialization.

In conclusion, let me say that organized industrial research cannot achieve fullest success if the president and financial officer of the corporation, and for that matter, the research administrator are the only ones whose decisions are governed by economic principles. While the research worker must be primarily a person of scientific integrity, he should possess also an awareness of economics as an attitude and a means to an end. Such a philosophy does not require an adding machine for every chemist and a cash register in the laboratory storeroom, but it does call for a wrecking crew for every ivory tower unless exceptional justification can be found for its existence.

DISCUSSION OF MR. VAUGHN'S PAPER

MR. MARLOWE: I would like to raise one question, and that is how on earth can you make the background necessary for judging markets available to a research worker without spending a great deal of time on that necessary background information.

MR. VAUGHN: The way that works out is this: The research supervisor generally has some general familiarity with the field we are talking about, and we have a market research department, whose figures are available to him and to his group. The supervisor starts out with some ideas on markets and by digging and by talking his problem over with other informed people in the company, he learns more and more about markets and much faster than he otherwise would. Then, after he arrives at an overall idea, it is finally subjected to review by people who are usually still better informed.

You would be surprised to know how often the supervisor's information holds up, and what a good effect it has on the entire staff to have them thinking in such areas.

MR. BOYER: I was wondering to what extent you take into account the possibility that another company in a somewhat competitive field is working on about the same type of product development and therefore is going to be competing with any possible market or has patents which will dominate it.

MR. VAUGHN: Well, we try to take those things into account insofar as we can. We had a project just recently, with which we were pretty far along, and we learned that one of our competitors was at the present time expanding their capacity and would have their project in operation over a year before we could possibly have ours in operation. In that particular case, we decided that the market would expand considerably and absorb not only their increase but ours, and fortunately, we were right. Had we thought their expansion program would more than eat up the existing market, we certainly would have held our program in abeyance.

Generally, you do the best you can, based on the information before you, but there is no way to be certain that you always reach the right conclusion.

MR. DOAN: I would like to make one remark: Estimates of investment costs are subject to a lot of variation, depending on whether you are an optimist, a pessimist, or a realist. I suppose most research supervisors have had the experience at one time or another of seeing an otherwise attractive research development go "out the window" because of engineering estimates of high investment costs. If the estimates are realistic, as they should be, a reliable appraisal of the project is obtained. On the other hand, if the engineering group responsible for making the plant estimates employs an unduly high safety factor in order to insure that the actual costs of construction will always be comfortably below the estimated costs, you may come up with an investment figure which turns the project down.

MR. VAUGHN: That is one of the toughest problems in this whole job. Our estimates are normally based upon partial layout of a plant by an engineer who has had a lot of experience in "guesstimating," and we use his figures just as though we believed they were correct. The engineer shouldn't try to shade his figures either high or low, but whenever he foresees trouble, he should take it into consideration and take the most expensive way out. For example, if the question of using steel or stainless steel comes up, he should figure on the use of stainless steel, thus putting in a possible cushion.

I have seen plants, and everybody in this room has, that have been engineered, appropriated, and built which have cost 30, 40, and even 50 per cent more than they were supposed to. If this happens on a project that shows 25 to 30 per cent return on investment, it doesn't cripple you too much, but if you get it on one that shows only 12 per cent, you are in serious trouble.

So we have tried to keep our engineers from altering their guesses one way or another. We would rather get them exactly as they see them. If they see them right, that is good, but if they see them wrong and pad them too, you are in a bad way.

MR. WALKER: You and I have talked about stimulating productivity in research personnel. I am wondering whether in handling these economic values, you feel that your researchers are more productive than they used to be

MR. VAUGHN: There isn't any question about it, definitely yes.

MR. ABRAMS: Could I make one statement, just to throw in one more complicating situation? Do you make any allowance for the purchasing value of the dollar? I think that ought to hit some of these things that are three years off about as bad as anything.

We make an estimate, and then we add enough to overcome any inflationary effect. When the dollar can drop from $1.00 in 1940 down to 58¢ now, you can see what is going on.

MR. VAUGHN: I think there is some compensation built into the system you are talking about, that is, the sales dollar increases along with inflation, as a general sort of thing, so that the ratio between sales and investment remains somewhat constant.

The other thing is, don't worry too much about figures being valid three or four years from now, because we are spending money trying to refine the figures and find out what they actually are. The figures we like to have confidence in are those for 1950 and 1951. We like to think that they are close to being right.

On the project that I showed you that ends up in 1954, an engineer has had to design a plant, and we have the research just started. He is working with nothing, and you can't hold him too closely to his estimate. I think the uncertainty of his capital investment figure is much greater than any effect inflation might have on the value of the plant.

And then, as I say, you have the overall compensation of the value of the dollar reflected in both what you buy and what you sell.

— II —

by

E. D. REEVES
Executive Vice President
Standard Oil Development Company

Several years ago it became apparent to our research people that the heavy-duty diesel oils our company was marketing at that time would not give entirely satisfactory performance in a new type of diesel engine that was about to appear on the market. This meant that something had to be done immediately to correct the situation, and, after a thorough study of the problem, the specialists in this field felt that one of our products could be improved by the time the new engine got on the market. At the same time, however, they expressed the opinion that what this engine really needed was an entirely new type of product. Because of limitations of time and personnel, it did not seem possible, however, to work on the improvement of our current diesel oil and to develop an entirely new one at the same time. After some discussion of the matter it was finally decided that no work would be done on improving the existing product but that all our efforts would be concentrated on the entirely new product. I am glad to say that this work proved to be quite successful, and our company had a lubricant that gave entirely satisfactory performance in the new diesel engine when it appeared on the market. I mention this occurrence because it illustrates a typical decision that must be made in planning industrial research almost every day. I think you will easily recognize that we were forced to take a calculated risk here; that we had to choose between bringing one of our existing products to a point where it would get by or developing a substantially better product. In the first case, the risk was small and so were the benefits. In the second case, the risk was quite a bit greater but so were the advantages of a successful solution.

When we think about industrial research, it seems to me that there is one very important point that must always be kept in mind. This point is that the success of an industrial research organization is measured not primarily by its scientific achievements but rather by the usefulness of these achievements to the company supporting the research. The fact that industrial research results are useful is no mere coincidence; it is the result of endless planning of research programs for this specific purpose. Each research project must be carefully examined with this in mind, and only those projects selected which fully meet the criteria of ultimate usefulness. These decisions determine what research will be pushed and what will be abandoned, and each decision represents a thoughtfully calculated risk on the part of research management. Willingness to assume calculated risk is an inseparable part of the management of industrial research and is a large factor in determining its success. Though you all know many examples in which real technical achievement and success have not gone hand in hand, I might mention that if our research group concerned with the development of oil burners were to come up with a truly revolutionary coal stoker, our company would not be particularly impressed with this scientific achievement.

While it is easy to talk about calculated risks and their use in industrial research, it is not nearly so easy to put this discussion on the quantitative basis that the term implies. I think it can be said that what we are really trying to do when we take calculated risks is to secure maximum usefulness from a given effort. As applied to research, this means that we are trying to achieve some sort of a balance between the cost of doing research and its probable value, i.e., we are trying to direct our research efforts into the most profitable channels. This immediately raises a lot of questions about the value of research to a company that are not easy to answer. I hope that this afternoon's discussion will throw considerable light on this problem and would only like at this time to mention some of the things that have to be considered when we try to answer the question, "What good can research do?"

Whenever a company decides to carry out research or to support a given research effort, it takes the first of a series of calculated risks in industrial research. The fact that business managements have come up with so many different answers to this question clearly indicates the complexity of the problem and the difficulty of putting down the answer in black and white. It is, nevertheless, a very important factor in the evaluation of specific research projects because the usefulness of research to a business will depend on the capacity of the business itself to put research results to work. This is affected by the type of business, the character of its management, the availability of capital funds, etc. All of this must be kept in mind by research management in trying to evaluate its research programs.

If we now turn to the evaluation of specific research projects, we will again find that the research director needs an intimate knowledge of the business for which he is doing research. The first thing he must determine is what phases of his company's operations are in greatest need of research. Should he work on raw-material supplies? process improvements? the development of new products? or even improvements in marketing techniques? Although all these questions need to be answered with reasonable accuracy, it is not always easy for research management to make an accurate estimate of the importance to his own company of the successful solution to a given research project. At the risk of further complicating the problem, I might even mention other factors that have to be considered. One of these is the consequence of failure to work on certain projects and bring them to a successful conclusion. For example, if the company is faced with a loss of its normal raw-material supplies, it is much more important to develop an alternate raw material than a new processing method. There are also many corollary advantages associated with new products. In our business, the development of an outstanding grease not only often brings in new grease business for our company, but also gives our marketers an opportunity to sell fuel oil and many other products to the same customers. Finally, there is the question of timing. It frequently happens that the value of a new process or a new product to the company depends on its availability at a certain time. For example, if the company is planning to expand its plant, a new and cheaper process would be very important just before the expansion took place. Six months or a year later it might be of no importance at all.

In listing all the considerations I have mentioned, I am not just trying to make this sound completely confusing. All I am trying to point out is that in deciding how much a successful research project might be worth to his company, the research director is required to be aware of all of the factors that the owner of the business himself has to consider when he decides what he is going to do next. Failure on the part of research management to consider all these factors usually results in research work being done on problems where the value of research can be determined by fairly simple calculations, such as the direct replacement of an existing process by another one making the same products but having a lower operating cost. In the long run, concentration on the more obvious needs of the company for research is bound to result in a lack of balance between the research program and the company's real need for research even though the research that is done appears to be paying handsome returns.

The next thing that has to be considered in attempting to balance research effort against its value is the cost of the research itself. This is a much less complicated problem but still requires careful study, since many research costs are often hidden among the company's operating expenses. Some of the things that have to be considered are the costs of the laboratory work, pilot-plant operations, engineering, sales development, and possibly the cost of the plant that would be required to utilize research results. This last item could be important since, other things being equal, the research project that results in the least capital requirements would have attractive advantages over the others.

The final factor in relating research costs and the value of research is concerned with the probability of success for the research project. In looking at our own research costs for these purposes, we have come up with a term that we call the *equivalent cost of research;* that is, if a research project is estimated to cost $50,000 and we think it has a fifty-fifty chance of success, we then say that it has an equivalent cost of $100,000. In other words, we divide the estimated cost whether successful or not by the probability of success. This brings us to a relationship between the value of a research project to a company and its equivalent cost, which we call the *desirability factor.* I might illustrate how this works. Suppose, for example, that the successful solution of a given problem would be worth $10,000,000 to a company and we have two possible research projects by which this problem could be solved. If one of these projects were estimated to cost $50,000 and its chance of success one in twenty, and the other estimated to cost $200,000 with an estimated chance of success of one in two, the equivalent cost of the second would be $400,000. On this basis, the second method of attack represents the better risk, since the desirability ratio in the first case is only ten while it is twenty-five in the second.

With the above in mind, we might see how these various factors are applied in the selection, control, and termination of specific research projects. Starting with the selection of projects, there are three things to consider. The first involves what might be termed the use of categories in setting up an over-all framework to match research against the company's needs. This means that there must be some balance between research on products, processes, raw materials, etc., in accordance with the requirements for research of the various phases of the company's operations. Within each of these broad categories, it is also desirable to have what an investment banker would call a balanced portfolio. For example, let us say that we are considering research on processes. Part of the research ought to be equivalent to Government bonds; i.e., it ought to be work that is definitely needed by the company and where the chances for a successful solution are very high even though the returns are not too great. In this class would come work on improving the design of fractionating towers, improvements in the design of process equipment, the determination of optimum operating conditions, etc. The next category might be what is called the blue-chip stocks. This covers work on new processes for which the company has a definite need and on which the return to the company would be very high if the research were successful. In our company this might mean the development of an entirely new cracking process or a method of making synthetic lubricating oils cheaply. Finally, we come to the class of problems known as the old gold-mine stocks. These are projects which might take the company into entirely new fields if successful and could represent a real contribution to its expansion. The development of Butyl rubber by our company some years ago is probably a good example of work along these lines.

Once the company's research programs are set up to give a preliminary balance between what appears to be the needs of the company for research, it is necessary to take stock of the various individual projects. This can be done by calculating the desirability factors for each project and then comparing them with others of the same general type. Comparison of the average desirability factors for research projects in different fields can also be used to check on the preliminary balance that has been set up and to earmark the least desirable projects for replacement by new ones.

I believe it is apparent from the above discussion that pretty much the same considerations would apply to the control of the research projects themselves. In determining the magnitude of effort justified for a given project, our normal feeling is that the most important projects in each field should be given as much effort as they will stand. Our reason for thinking so is that it is always desirable to complete a project before the need for it disappears. Also, there are certain psychological advantages to winding up each project as quickly as possible. In the control of projects there arises a certain type of calculated risk that is more concerned with the planning of the detailed technical programs. Here again, it is important to streamline the project as much as possible and refrain from obtaining information that is not essential to the final solution of the problem, even though it has a certain amount of scientific interest.

If the procedures that have been outlined above are consistently followed, research projects will automatically terminate as the need for them drops off or as data obtained during the course of the project indicate that its chances of success have gone down. This does, however, require a constant and continuing review of all projects, with particular emphasis on the distinction between technical interest and usefulness. I know that during the war our company was very anxious to develop processes for the manufacture of triptane, which is a very high-quality component that would be useful for aviation gasoline. Having started on the project, we kept right on working even though about halfway through, the importance of making triptane had lost a great deal of its urgency. The reason we had so much trouble in stopping the project was that our research people did not want to admit that they could not solve the problem and objected to discontinuing the work without a suitable answer. While there are many other phases of industrial research that require the use of calculated risks, I think that the above discussion serves to illustrate the fact that the taking of calculated risks is an integral part of industrial research. I think that I can summarize our own feeling about this as follows:

In carrying out industrial research, the success of the work done is measured by its usefulness for a specific purpose. These specific purposes must be achieved by the research organization with a minimum amount of effort, and the over-all research program must be balanced against the needs of the company supporting it. For this reason, there are two things that are demanded of an industrial research organization. The first of these is technical competence in carrying out the research itself, and the second is wise planning or the use of calculated risks in meeting its responsibilities.

Calculated risks must be taken through all phases of the industrial research effort. This includes determination of the total research justified, its distribution in different fields, and the selection, control, and termination of specific projects. Calculated risks must also be taken in the direction of individual research programs if these programs are to be of maximum effectiveness.

I feel sure that failure on the part of research management to take calculated risks would soon doom that particular organization to mediocrity. On the other hand, the evaluation of the calculated risks requires all the skill and resources that research management can bring to bear on it. It requires an intimate knowledge of the business for which the research is being done, an intimate knowledge of the research organization itself, a knowledge of the actual and probable costs of carrying out research, and the ability to evaluate the real usefulness of the results of research.

With all of its importance, however, the assumption of calculated risks by research management is at best an intelligent effort to make the most of what is available. It is by no means a substitute for a good, hard-hitting research team with ideas for things to do. Its purpose is not to make poor research look good, but only to make good research useful.

DISCUSSION OF MR. REEVES' PAPER

A VOICE: Both of the speakers so far have outlined systems for the calculation of risks. As I understand it, both have also outlined direct calculation of some of the risks, followed by, let me call it, guessing at some of the other risks. You figure out the cost of the new plant and the quantity of new sales you will have, and then you guess a factor of anything between one and ten as to the chances of success in the research, and you multiply or divide by that.

That leads me to wonder how long these systems have been employed, where they have been employed, and whether any post mortem has been done on them. Can you cite the batting average of your system of calculation? How many gross errors do you discover years afterwards? Can you discover your errors within a year or within some years afterwards, and can you by any means of investigation determine your own past batting average?

MR. REEVES: We are somewhat like the doctors; we bury most of our mistakes. I think that in most cases where you really make an error, you fail to do something which you should have done, because, as you go through a particular project, you always have the opportunity of stopping it if it doesn't look profitable.

Now, we have made mistakes in the past, and I think the biggest mistake we ever made was in spending as much money as we did on hydrogenation back in 1927 and 1928. That was a mistake because we never got to use the process very much.

On the other hand, the work that we did at that time was justified on the same basis as the work we have done more recently on synthetic fuels. In other words, hydrogenation was of great interest to us in '28 and '30, because it was thought that there might be a shortage of oil, and hydrogenation was going to give us a much bigger yield of petroleum products from the crude.

That did not turn out to be the case, so I think we might say that we made a mistake because we thought there was going to be a shortage. The last time we did work on synthetic fuels, we didn't think there was going to be a shortage.

A VOICE: I can appreciate your taking a called third strike, and that is one of the worst things we do in its effect on our batting average. I am still interested and wondering whether you have calculated in any way, shape, or manner, the batting average over a long time and how it works out by the calculation you do. In other words, have you tested your method of calculating risk in the light of past experience?

MR. REEVES: Yes, I think some of that is going to come up this afternoon, because we made quite a study of it about seven or eight years ago and tried to calculate what we thought was the value for the company of the research we had done over a period of years. We then tried to estimate the value for the next five years and recently recalculated it to check on our estimates.

The only trouble with the calculations is that it is hard to get the different groups to agree on the estimates. In other words, who makes the money on the new product? the sales people, the manufacturing people, or the research people?

— III —

by

DONALD H. LOUGHRIDGE

Senior Scientific Advisor

Office of the Assistant Secretary, Department of the Army

At the time Dr. Work first approached me on the question of speaking at this year's Conference on the Administration of Research with the suggested title of "Calculated Risk," my first reaction was to wonder as to whether the military had taken over our annual meeting and as to whether we were to be subjected to the so-called "military-mind" type of thinking. My reaction was due to the fact that I did not recall having heard the term "Calculated Risk" applied to shrewd guessing before becoming closely associated with the military services. I replied to Dr. Work's invitation stating that I did not believe there was such a thing as "Calculated Risk." However, since I am an old member of the American Physical Society and have long lived under the necessity of submitting a short abstract about six weeks prior to a meeting of the Society to get placed on the program, I, along with a percentage of other members, have been known to stoop to the practice of sending in some preliminary research results with the hope that by the time the meeting rolled around, we would have time to obtain more convincing experimental data. On the basis of this principle I accepted Dr. Work's invitation to speak. Your chairman apparently was also cognizant of the above-mentioned general practice because he replied (to paraphrase his remarks) that it was not absolutely essential to talk on the subject nor to know much about the subject but that it was his hope that by the time our Conference convened I would have had the opportunity to learn something about this military figure of speech.

Having thus become involved in the implied tacit agreement to put some thought on the subject, I first went to Webster to ascertain what was meant by "calculated." I found the following: "Adapted by calculation, forethought, or contrivance to accomplish a purpose, hence loosely, likely to produce a certain effect."

For "risk" I found: "Hazard, danger, peril, exposure to loss, injury, disadvantage, or destruction."

Since to a physicist the interpretation of "adapted by calculation" could mean nothing except the use of precise numerical methods and since in the initial processes of selection of research projects it has been my experience that educated intuition plus mere accident has been the primary basis of outstanding scientific advances, it would appear that we must use Webster's last definition to represent truly what I believe we mean, namely, "likely to produce a certain effect."

As far as "risk" is concerned in our present terminology, it is clear that "exposure to loss" is the only meaning given by Webster which can be applicable. Hence, the combined meaning of our term in the present field of discourse must be "likely to produce a certain exposure to loss." I should hasten to add, however, that I would not restrict this loss solely to financial loss. It could be equally well represented by loss of man hours, loss in competitive standing, either from a business or military standpoint, loss in the rate of pushing back the scientific frontiers of knowledge, or the individual loss of the satisfaction of having given birth to a new, really fundamental concept.

Now, since I believe I should present a discussion of the application of this concept of calculated risk—or the likelihood of producing a certain exposure to loss—to the problem of military research and development, let me take a few minutes to explain to those of you who may not be sufficiently familiar with the methods used within the Department of the Army how the R & D program is set up.

The Research and Development Board issues annually its Program Guidance to the three sister services, Army, Navy, and Air Force. This guidance assumes the form of discussing in terms of eighteen catagories of the RDB Master Plan those fields in which the RDB believes more, or less, or equal, support should be given in an approaching fiscal year as contrasted with the present year. To mention a few of these categories we find such titles as Land Combat, Air Defense, Strategic Air, Sea Combat, Supply, Supporting Research, and Basic Research. Each of these broad categories is again broken down into Technical Objectives, numbering from a very few up to 20 or 25 within each of the broad categories.

The Department of the Army (and I presume a somewhat similar procedure is followed in the Navy and Air Force) uses this Program Guidance of the RDB to aid in the annual evaluation of its past R & D program and the formulation of its new one. It should be remembered that this formulation of program necessarily occurs approximately two years prior to the fiscal year in which it is to be executed. The procedure of Congress in granting yearly appropriations requires this long-range planning. At about the same time as the RDB Program Guidance is furnished to the Services, the Secretary of Defense gives to the Services through the RDB an interim R & D ceiling which their proposed program must not exceed.

Next begins the detailed breakdown of these budget ceilings into various classifications. Within the Army, the Office of the Deputy Director, G-4, for Research and Development proposes an interim breakdown of the Army's R & D ceiling figure into an interim ceiling for Ordnance, Signal Corps, Chemical Corps, Engineers, Quartermaster, Surgeon General, Transportation Corps, and an Army-Wide category. Each of these Technical Services in turn submits a series of projects, with estimated costs and tabulated in terms of the Technical Objectives within each of the Strategic Categories of the R D B Master Plan.

The Joint Chiefs of Staff meanwhile assign priority classifications to the eighteen catagories, and the R D B assigns technical promise evaluations for the technical objectives within each category.

Within the Department of the Army the next step is for the seven Technical Services to submit their proposed R & D program, properly budgeted within their respective ceilings, to the Deputy Director, G-4, for R & D, who now appoints a committee of three, of late usually consisting of himself, a representative of the Army Field Forces, and the present speaker, to hold a series of hearings with each Technical Service with a view of examining the proposed program in detail, project by project, requiring justification where desired, suggesting increases or decreases in the financing of particular projects, elimination of some, and addition of others. In this review, there naturally results a shift in budgetary ceilings between the Technical Services. Since the Review Board is made up of one member from the Army General Staff, one from the Army Field Forces, and one representing the scientific interest, the hope is that by this means a balanced Army R & D program will result.

An additional final check however is made by the Services presenting their budgets to the R D B. Committees, panels, and the General Secretariat go over the entire Department of Defense program project-wise and budgetarily, the theory being that a proper balance between the three Services and elimination of duplications and gaps will thus be achieved.

After the above dry discussion of the channels presently existing within the Army and the Department of Defense for the establishment of its R & D program, it becomes desirable to examine in somewhat more detail the presence or absence of calculated risk (as previously defined) in the procedure. Since the subtitle of this morning's series of papers emphasizes *research* projects, let us first be sure that we are all interpreting the word in the same way. It has become all too common practice within industry and the military services to lump too much under the glamorous name of research, whereas, in fact, most of the work is development and consequently directed toward the solution of specific problems. At the risk of being pedantic, I should like to establish clearly what I believe to be the difference between research and development. The basic objective of real research is to add previously unknown knowledge to our stockpile of scientific facts and to explore fields of science without any requirement as to a specific application. On the other hand, development or engineering work consists in the careful and logical application of previously known facts and conditions to the problem of satisfactorily attaining a previously desired end-objective or product.

Research problems of the kind above described can only be recognized by those actively engaged in the work. It is the immediate result of the solution of very closely allied problems by the investigator, or others working in the same field, and is conceptually impossible of even formulation by one not closely allied to the particular field of research endeavor. To go even further, many of the most outstanding and valuable discoveries are often the result of almost accidental observations by the carefully trained investigator of phenomena which came to his attention solely due to the fact that he was interested in a different, though perhaps closely allied problem. To mention such a specific case, one needs only to recall the wave-like nature of electrons discovered by Davison and Germer at the Bell Laboratories as a consequence of their initial studies on electron scattering by crystals. The Nobel Prize was given for this industrially-financed piece of research.

Although it is true that no one except a working scientist can intelligently select a real research problem, a good research director can support and encourage work in a particular field, and on the basis of his usually greater experience and more mature judgment, he materially assists in the choice of fields in which research work needs emphasis.

It has been my experience that many perfectly capable research men, especially in their younger days, become easily discouraged by being subjected to the request of management or superior administrative authority to justify budgetwise their needs for equipment and technical assistance. Quite frequently they may lack someone with whom they can discuss their present state of progress and receive a fresh interpretation or suggestion which puts the troublesome problem in a new light so that a logical deduction or interpretation becomes apparent.

Since we are talking about the place of calculated risk in *research projects,* I believe I have said enough to show that is is my belief that the question can be answered only on a specific basis. Given, let us say, one million dollars for research and development, what fraction of this sum should be used for real fundamental or basic research? The question as stated is still unanswerable until we specify the type and purpose of the organization administering the funds. If the institution is a university, the answer is easy—80 per cent should go to actual research with a 20 per cent allowance for overhead; but the consideration of calculated risk (meaning, likely to produce a certain exposure to loss) should still be carefully evaluated in terms of the distribution between research workers. The requirements of the scientifically more mature, who have proven their ability to obtain results, must be balanced against the eager young minds, who may be more imaginative and who have the extra incentive to produce in order to establish their scientific reputation. To restrict the opportunities of an exceptionally able but unproven young man can produce a great likelihood of exposure to scientific loss.

In the case of laboratories of large industrial organizations, 5 per cent and sometimes as high as 20 per cent of the total time can be spent on basic research, with the laboratory still yielding overall dividends to the industry which supports it.

However, if the institution spending the million dollars on research and development is a military organization, we have a complicated problem. The organization is not one primarily established for research and development purposes. It is not an organization which has to worry about paying dividends to its stockholders. It is an organization which, with the exception of the Office of Naval Research (and this is only about four years old), has never been really concerned with research projects. Even a very high percentage of its developmental accomplishments have been done by simply hiring outside organizations under contract to do the job. Methods suitable for the selection of programs for development are often applied to the choice of research problems. Research objectives are defined and a program is planned in terms of time and money. This practice is not only useless; it is downright harmful. The best way to conduct research is to decide on the fields of investigation, to explore these fields, and to feel one's way into the unknown, allowing competent investigators to follow the paths which in their competent judgment seem most promising.

But this willingness to allow self-direction (by either individual or group) in broad fields of research is a very difficult point of view to gain adoption in the military services. The desirability and, I might even say, necessity, of its adoption is nevertheless recognized in many levels of the command channels. However, there still exists a great desire to tie research projects to developmental end-items. As far as I can ascertain, this tendency is closely related to a justifiable belief that in times of retrenchment the research item which is standing on its own feet will be the first to be cut off. How true this is! On the other hand, during those critical times of financial expansion, there is always a concomitant demand for rapid completion of development items, a great need for money to purchase service test items of hardware, and a belief that in times of critical stress research must give way to development. There is no doubt that the latter viewpoint must, in general, be supported, and hence we find real research pushed out to a great extent at all times in the military R & D program. I must take this opportunity, however, to point out the exception which is always needed to prove the rule. The Office of Naval Research has done a brilliant piece of work during the past four years in its support of really basic research in the natural sciences. In the period since the war, when it became clear that this country must take the lead (previously held by the European nations) in fostering science, and before Congress succeeded in establishing a National Science Foundation, the Navy grabbed the ball and has pioneered very successfully in fighting the tendencies to which I previously referred. An examination of the budgetary support which the Navy has been able to provide for science in the last few years will readily show, however, that the ceiling provided in the bill lately passed by Congress creating a National Science Foundation will be inadequate unless the Office of Naval Research or other Service-supported activities are, at least in part, continued. Very roughly, the Navy has lately supported a $20-million research program out of a $200-million R & D budget. Thus we arrive at a figure of 10 per cent for Navy support of research. This would seem a reasonable ratio.

I have pointed out above that the selection of the most promising research projects can be intelligently accomplished only by the working scientist. His selection of projects must necessarily be limited to a great extent by the facilities and financial support which it is possible to accord him by his employer. The field of his researches will naturally grow out of those fields in which he has had experience. The research director to whom he reports has, naturally, the responsibility to select those broad fields in which the laboratory's work should be concentrated. The decision by the scientist as to what research projects he would recommend being pursued involves the calculated risk of his choices, involving such complicated techniques, either known or unknown, that a satisfactory solution of the problem may require too large a fraction of his active life and hence probably result in slow professional advancement for himself. The approval of suggested research projects by the research director in turn involves the calculated risk that he will be able to maintain proper support in facilities and budget until the answer to the problem is found. This requires a continuous close following of the progress of work by the director, so that he may be in a position to defend the support of the work whenever called upon to do so. In large research organizations this is oftentimes done by project supervisors. These continued reviews involve a control in the sense that a calculated-risk type of decision is frequently required in the way of recommending an increase of effort, a slight change in emphasis, or a decision to alter the method of attack.

Finally, and usually, the toughest decision, is the calculated risk involved in terminating a project. In the very few cases where a unique answer to the original problem has been achieved, which in turn suggests nothing needing further investigation, this decision is easy. In most cases, however, the matter does not turn out this simply. After a reasonable effort has been expended and the question has not been answered to the investigator's satisfaction, one is confronted with the question of the calculated risk involved in dropping the work altogether. This means usually a complete loss in the effort already expended, although in a few cases a negative result may mean progress. There is always the alternative that, with a continuation of the work for some time, useful information will be obtained. There is, of course, no formula by which to decide such questions. The inclination of the primary investigator is usually to continue, since a successful research man is always an optimist. In a few cases he may elect to stop the investigation due to discouragement, but research is always such a continuous series of discouragements that the proven successful researcher rarely fails to emulate the bull dog—he hangs on. Consequently, the research director, having followed closely the progress of the work and having the background of successful investigational experiences himself, must usually be the one to decide when the calculated risk would indicate that further work would probably result in waste of manpower and money.

Getting back a little more closely to the subject of military research and development, let us examine the present mission of the Army. It is so wide in scope that it covers not only an enormous military effort but also an enormous supply effort. The military effort of the Army requires above all competence in combat, including the military aspects of logistics. This part of the task can be carried out only under the leadership of professional military personnel. The problem of supply includes the efforts of numerous nonmilitary professions, beginning with research and ending with production. The required competence and experience in the professions involved can in most cases be found in civilians who have made these professions their lifetime careers. Military personnel cannot and should not be expected to develop high competence in such professions, unrelated as they are to their own primary field of specialization. In order to carry out the supply part of the Army task, it is essential that the personnel engaged in these activities, both those within the Army Staff in administrative or advisory capacities and particularly those in the Army laboratories and stations, be civilians experienced in specific professions. With rare exceptions—and they do exist—officers should be excluded from participation in the supply organization because they will not have the necessary competence in nonmilitary professions.

The conflict and inefficiency which has sometimes arisen within the Services with respect to this second task, that of providing the combat forces with the supplies they need, arises principally because of the insistence of the officer corps on immediate direction and control of this task. The difficulty can be avoided and competence can be assured within the Services with respect to this second task by separation of the military and civilian functions and by assignment of the two tasks to the personnel that is most qualified to carry them out. However, such a separation should not be a separation in the sense that there will be two teams, one military and one civilian, but rather a separation from the point of view that the most competent person should be chosen to direct and execute each specific function. The choice of the director for each organization, and of his subordinate personnel, should be made on the basis of competence and experience for the particular task. Military and civilian personnel must work within the Services on a basis of equality, each according to his abilities.

It is necessary that the eventual user of materiel control the detailed decisions which will lead to production, since this is materiel which will be used in combat. It is necessary, therefore, to provide means whereby the military users will control those decisions in research, development, engineering, and production which affect the *emphasis of effort* and the *nature* of the final product. The control should be from a center directly connected with strategic and tactical planning which maintain close liaison with the field commanders who are most capable of estimating the practicability of specific materiel items.

The philosophy underlying the above discussion is based upon the assumption that military and civilians alike have an equal duty with respect to the security of their country and must work as partners, each accepting, and being given authority within, those regions in which each possesses competence. In this partnership the civilians must accept the role of junior partners and must welcome guidance by the senior partners, the users of weapons and weapon systems. It should be pointed out that this same philosophy has been used by the British in the organization of the Ministry of Supply.

There appears to be a slowly growing appreciation within all the Services, particularly advanced in the Navy, that the scientific administration of military research and development laboratories must be in the hands of competent civilian directors. These directors must be distinguished professional civilians capable of commanding the respect and services of competent scientists and engineers and of maintaining a working partnership with the military organization. Such technical directors will then be able to assemble a group of professional personnel as highly skilled in their profession as are the military officers in theirs. In either case a lifetime of specialization is required. When such research and development laboratories are located, as is often the case, on a large military post or test facility, there is obviously the need for a military post commander to direct the administrative and housekeeping facilities and to ensure proper planning for the enormous nontechnical requirements of the research and development laboratory. But the authority and responsibility of the technical direction must be entirely in the hands of the technical director, who should report directly to the Service Headquarters. Organizations somewhat similar to that indicated above now exist at the Michelson Laboratory at Inyokern, the Naval Ordnance Laboratory at White Oak, the Naval Research Laboratory, and the Camp Detrick Laboratories of the Chemical Corps.

Calculated risk in military research and development does not mean taking long-shot gambles with our national security. It does mean, however, that we must "make haste slowly" in our planning and conduct ourselves in the manner of the astute chess player. As mature players at the international board we must think out each move carefully. We must take long looks before we move our pieces and we must never be in the embarrassing, and possibly disastrous position of moving in all directions at once. On the other hand, we must not be indecisive and delay to the point where we lose the advantage.

Only men of science, and a few others, realize fully the extent to which we depleted our research stockpile during the war years and to what an overwhelming extent that stockpile had been built up for us by European scientists. All that we had learned in the years between the wars and all that we had appropriated from foreign research agencies went into the developments which we accelerated to such an extent during the past war. We must replenish that store of proven research. In short, he who now discovers basically new facts of nature, the storehouse the door of which has only been cracked slightly ajar; he who now develops the skills and techniques for designing and employing new weapon systems far advanced over those that are currently being put into procurement; he who now refuses to limit his vision to further only short-range development of presently conceived weaponeering gadgets— will most certainly acquire a predominant equity in the role of fathering the security of his country by positively insuring the military superiority over any foreign aggressor.

Let us never forget that those really priceless products of human imagination, those strong skeletons of basic scientific theory which firmly support the branches and twigs of technological advance are never the result of over-all planning, or coordination, or of channelized administrative procedures. They are the result of freedom of thought, time for thought, and the establishment of a natural tradition that science has been proved essential in the continued fight for national security.

DISCUSSION OF MR. LOUGHRIDGE'S PAPER

DR. MAXFIELD: Mr. Loughridge has discussed the question of calculated risk in connection with research in the way the military establishment looks at it. We feel sure that there is practically no risk in true foundational research. However, when we speak about development, which is the subject that the two preceding speakers have been discussing, there is a great deal of risk involved.

In attempting to attain a certain objective, military agencies usually must consider several projects offering possibilities of success, and they must then decide which one shall be supported. All possibilities cannot be supported because, as Mr. Loughridge pointed out, the Research and Development Board fixes a ceiling within which research and development must be conducted.

The problem frequently arises, then, to decide which of several promising developments shall be pursued. An evaluation must be made to determine the most promising solution to satisfy the requirement for a future weapon or future device of value to the armed services.

I am not going to discuss this subject in detail; however, I want to point out some of the factors which, it seems to me, we have to consider in evaluating a project for military purposes when we have already done the foundational research and want to determine whether a particular proposal is worthy of development.

First, we must consider the estimated cost of this development, because we must stay within the limitations of the Research and Development Board budget.

Second, we must consider the development time involved, since usually we have target dates which have to be met.

Third, we must consider the facilities available to do this work, and whether this work can be done with existing facilities or whether it will be necessary to initiate a program for new facilities either by government contract or through a government laboratory.

Finally, we must consider the cost to manufacture and maintain the ultimate product resulting from the development. Since usually the products of military development are only going to be required in large number in time of war or emergency, we must consider what the product will cost under wartime conditions and how its manufacture will compete or interfere with civilian activities. Then we must consider the cost of maintaining this equipment once it has been procured and issued to the fleet. We must consider the cost of keeping the equipment up and keeping personnel trained in its use.

All these factors have to be considered in calculating and evaluating the risk involved in undertaking a given development. When a requirement exists and limited funds are available, we must ask ourselves, shall we undertake a particular project which will cost two or three million dollars and *may* produce the desired result, or shall we spend that two or three million dollars along an entirely different but promising line which appears to yield a similar solution, or shall we split it up among several projects and have several competing projects going along at the same time but at a slower rate?

I think this illustrates some of the important factors which must be considered in any calculation of the risk involved in military developments.

MR. HUGHES: I would like to inquire of the first two speakers what methods they use to allocate moneys for the research end of the deal as expressed in the last man's definition of research? I think we in the petroleum industry are accustomed to calling that exploratory research. Do you have any schemes for that kind of research?

MR. REEVES: I think that the allocation is more on the basis of necessity. In other words, we look at these projects from the standpoint of what information is needed, and, if it is basic information, then the money goes to basic research. If it is pilot-plant work, it goes into that. In other words, we do not draw a distinction between basic research and some other kind of research, such as pilot-plant work and development work.

We have an end to achieve, and we try and figure out what type of information we need to achieve it, and that is where the money is finally allocated.

COLONEL KEIRN: I would like to explore that question a little further. I noticed in Mr. Vaughn's speech that each project, before the research staff was able to present it to management, had \$3,000 or \$4,000 or something of that sort spent on it. There must be a percentage of profits or something that can be earmarked for these explorations before it is possible for you to picture or create a picture from which you can determine the degree of risk or the value of the project. Can you go further into that?

MR. VAUGHN: Well, in our own organization, and I think this is true in many others, any man has some opportunity to explore ideas that he believes may have some ultimate benefit to our business. I think I can say categorically that we do not sponsor deliberately and formally any so-called fundamental research work which has no possible application in our business, but we do some of that kind of research in the early periods of some projects.

The men in the laboratory should be able to spend some time, if they so desire, on a project without it being officially on the program.

In general, however, we try to keep such work within the confines of the area in which we are interested, etc.

I know that some companies have adopted a percentage basis for the allocation of time to such work. I know one organization (and a representative of that company is in this room) which permits its research men to put in 20 per cent of their time on anything they like, and there is no restriction whatsoever on what they do with that time.

We have adopted no particular percentage figure, but I guess it is running around 10 or 15 per cent of our research effort.

DR. SPENCER: Mr. Loughridge spoke about depleting our supply of basic knowledge. Such a statement gives a wrong impression. We do not, in any way, deplete our supply of basic knowledge by using it. By finding uses for basic knowledge, we increase our understanding of it and enhance its value.

What Mr. Loughridge meant, I believe, is that more fundamental research, aimed at the increase of basic knowledge, should be conducted and that technological research is not a substitute for basic research. Certainly, additional fundamental research is urgently needed.

There is one risk which none of the speakers mentioned—the risk of not doing research. When one calculates the risk associated with conducting a research project, he ought to calculate also the risk of not doing it. Suppose that a new project or a new process is needed. The new product or process will not be found if the research is not done. The risk of conducting the research may be high, but the risk of not conducting the research may be much greater.

MR. NORTHRUP: I have listened with interest to the beautifully simple and apparently reliable methods of calculation of risk presented by the first two speakers. I should like to raise a small voice in the wilderness of regimented research administration to the effect that very few nonhypothetical research projects ever follow such simplified charts.

Really significant research results often obtain from projects initially started for entirely different purposes or for no specific purpose at all. I am reminded of Mr. Gerald Johnson's discussion of the projects started by President Roosevelt in the New Deal era. In his book, *The Incredible Tale,* he cites many projects which Franklin Roosevelt started for one purpose and from which, entirely by accident, he achieved more important and wholly unpredicted results.

Mr. Johnson compared this phenomenon to the tales of Hugh Walpole of the three Princes of Serendip (ancient name for the island of Ceylon). These tales were always characterized by some entirely unexpected and delightful experience encountered by the princes during voyages originally planned for other and usually less interesting objectives.

Thus the term "serendipity" has come to mean "getting more by accident than you do on purpose."

Mr. Vaughn: Could I say that the principle you just discussed is one of the factors of safety in your calculation? It helps you a great deal sometimes.

Captain Furth: I would like to support Dr. Loughridge's statement regarding our having used up the fundamental knowledge which was available prior to World War II.

From the military point of view, he expresses his concern over the value of weapons which have been developed and which make use of techniques which are well known. The value of the weapon is measured by its effectiveness; it remains effective only as long as the enemy is unable to deny us its use by countermeasures. So, by having used up our fundamental knowledge, the techniques which are now available and which were developed prior to and during the last war are also available to probable enemies.

To have effective weapons, we must keep exploring new fields, looking for new techniques, with the hope that it will take an enemy a long time to find proper countermeasures and thereby deny us use of these weapons.

ROUND-TABLE DISCUSSION

Paul D. Foote, *Chairman*

The use of charts illustrating calculated risk, the estimated expense, and the possible returns from a research project are of primary advantage in the selling of research programs to management officials. A director of research, in many cases, sells a research program to his company, and since the results of the research program might well affect the future of his company, the responsibility in properly evaluating the factors involved in the performing of the research and the utilization of the results is of great magnitude. One of the very important considerations in this day of rising costs is the financial capability of the company to exploit the results of the research project upon its completion, and with long-range research programs the evaluation of the financial element becomes increasingly difficult.

Today's conditions necessitate a large part of the research budget going for those projects which tend to lower production costs and reduce capital expenditures. Such projects are largely short-term efforts quickly put into use; their effect may be estimated accurately. As a rule, the investment and cost is comparatively low, and the return on the research investment is not very high. The very life blood of an industry, the projects that will produce the new products which keep a company in business over the years, are the long-term research projects involving many elements of risk. They require a careful analysis and a strong selling effort on the part of research directors. However, these are the projects which are neglected when economic prospects are uncertain.

The tendency to avoid long-term projects because of the economic uncertainty existing today may, in the long run, seriously retard our scientific progress. It is often possible that these long-range projects present a field that logically might be most advantageously supported by the Government or by cooperative efforts in the laboratories of the universities and colleges.

The risk involved in failure to conduct research was also discussed, with the petroleum and chemical industries cited as examples of industries spending large sums for research, particularly in pilot-plant construction and operation. Here the risk involved in going directly from laboratory to full production is considered too great. It was estimated that 75 per cent of petroleum research money goes into pilot-plant construction and operation.

The current world situation has injected a further element of risk into the calculations of the industrial research director, namely, the problem of Government research. In the general discussion, the following points were raised with regard to Government research in industrial laboratories:

1. Large Government contracts disrupt industrial laboratories by forcing temporary staff expansions, with reductions in force upon termination of the contracts adversely affecting worker morale.
2. When the Government projects require the

industrial laboratory to go far afield from the interests of the parent company, the termination of the Government work often finds the company and its products in poor competitive position due to lack of continual product improvement.

3. The present method of administering Government contracts is so involved that some laboratories prefer not to accept them.

It was estimated that in the next two years 75 per cent of every research dollar will be spent for military purposes. It was recommended that if the Government were going to provide an increase in research and development money of such magnitude, an accurate survey of the men, facilities, and the effects of the military draft upon the various laboratories should be made at once if this vast sum is to be expended to the best advantage.

Second Session

Henry M. O'Bryan, *presiding*
Research and Development Board, Washington, D.C.

MEASURING THE RETURN FROM RESEARCH

– I –

by

ALLEN ABRAMS
Vice President, Marathon Corporation

IN THIS DISCUSSION we are concerned primarily with the evaluation of organized industrial research. Some directors of research may think it unnecessary to attempt the justification of a function which consumes such a minor portion of a company's expenditures. Other directors will feel that in their own organizations there is quite full recognition of research as the basic insurance of the business.

Yet there are good reasons why an evaluation of research is desirable. In an industrial concern research is a parallel function to production and sales. The production man can point with pride to the number of units he has turned out. The salesman can present an impressive report of his accomplishments. Through his operating statement and balance sheet the manager of a business is always cognizant of the money being poured into research. Yet often he has no adequate gauge of what flows out, so that the research man receives little or no recognition of his contribution.

Industrial research laboratories are incubators of ideas—converters of thoughts to things. But without some measuring stick, research men, particularly in younger organizations, may develop a sense of frustration which is good neither for their own morale nor for the well-being of the company. Many directors of research would be happier, and more companies might invest in research if they had means of evaluating the results. For even among the blue-chip industries listed on the New York Stock Exchange there are notable omissions of research departments.

There are a number of ways by which we may judge the value of research. The simplest of these is to note the growth of research organizations in the United States. In 1900 formal research was practically nonexistent, but World War I demonstrated such potential value of science that by 1920 there were 300 industrial research laboratories employing approximately 7000 scientists and spending $30,000,000 per year. In this year, 1950, there are ten times as many laboratories as there were in 1920, about 20 times as many people engaged in research, and the expenditures are 30 times as much, or nearly one billion dollars.

Industrialists do not spend money without good reason. It is, therefore, interesting to note some of their comments on research in the annual reports to stockholders. One of the large chemical companies, successful in introducing many new products, states: "The company believes that an aggressive and forward-looking research program is the dominant factor influencing the sound growth of business." A large electric company has an ambitious program for its research department, "providing the means of turning more energy to man's advantage . . . is the end result of our research." The pioneer company in photography says that its research laboratory is "responsible for the future of photography." An oil company headed by a chemical engineer believes that "In all our fields of activity, research continues to bring greater efficiency to our operations and better products to our customers." A paper manufacturer thinks that "constant work in research and development is the best assurance of the continuity of our business and hence the security of employees and management, stockholders and customers."

Another measure of the value of research is in the industries which have grown out of its findings and in the jobs which have been created by its application. Consider a list of 1700 of our large industrial companies. Nearly one-half of these were either nonexistent or were infants in 1900—aircraft, automobiles, chemicals, electrical equipment, petroleum, and rubber. It is not pure coincidence that in half a century the following changes have come about. Jobs have been created 25 per cent more rapidly than our population has grown; industrial production has increased three times as rapidly as population; industrial output per worker has multiplied by four times; today the United States consumes nearly one-half the energy output of the world. To a large degree it is the findings and applications of research which have caused these changes.

Another, and for our purpose the most pertinent way of evaluating research is through the procedures which have been worked out by various companies. Our discussion here is based largely on information obtained from about one hundred industrial research directors. A few of these expressed lack of interest in the subject and questioned the desirability of further investigation. But one scientist expressed the viewpoint of nearly all the respondents; he said: "Support of a research department is more an act of faith than it is a numerically calculated risk. However, any act of faith should be appraised sooner or later."

Several research directors point out the desirability of preliminary appraisal of projects in terms of expenditures required and the likelihood of successful returns. If such a study casts doubt on the project, then it should probably be abandoned. Several companies classify their programs primarily as defensive research, believing that the company would not be in business at all save for its research department. In the older companies with well established research laboratories, management appears well satisfied with the effectiveness of research, judging in most cases by the general health of the company. In one case this confidence in the research department is so implicit that the philosophy is to sell a product first and then hope that it can be made, rather than make the product and hope that it can be sold.

Some companies have developed formalized methods for crediting research with its accomplishments. In all cases the research directors point out the difficulties of such an appraisal and caution research not to claim too much credit. They believe that the evaluation can be carried out most satisfactorily by having representatives from production, sales, accounting, and research make the final appraisal.

A large chemical company uses the following procedure: When the research department has been responsible for most of the original studies, including application and process development, all future sales of the new product are credited to research. If, however, the product comes to research partly worked out, then the department completes the job and takes as credit to itself only some reasonable percentage of the sales. When a major improvement has been developed on an old process, resulting in higher yields or greater volume, the increment only is credited to research.

While not entirely satisfied with its method, a certain lumber company classifies its research results into three groups and rewards them accordingly: (1) Projects which have been concluded successfully and put into operation. It is assumed that the value of these discoveries is four times the cost of the research. (2) Projects which have been completed successfully and are of definite value to the company in improving operations but on which no well-defined action has been taken. An example of this might be an investigation which has kept the company from spending money on a questionable process. Projects of this sort are assumed to be worth twice the cost of the research. (3) If the project is unsuccessful, obviously, no credit is taken.

One manufacturer has developed a detailed procedure of evaluation. The research and development department submits proposals on new products to the factory manager. When the idea has been accepted and put to use, the accounting department studies the results and applies the "Index of Return." This index is composed of the savings for one year obtained through an improved process; 3 per cent of the net sales on a new product for a period of three years; 3 per cent of the net sales on an improved product for one year. The company believes that this index measures not only the effectiveness of the research department but the ability of the factory to accept and develop new and better products and the ability of the sales department to sell these products.

A large oil company evaluates its research about every five years. The principal benefits arise from: (1) elimination of royalty payments, which are easy to estimate; (2) added capacity due to improvements in existing products and processes, representing a saving in capital investment and lowering of operating costs (another advantage comes from increased business resulting from such improvements in quality); (3) additional profits from new products.

Another oil company measures the results of research as follows: On new and improved products, research is credited with the total profit in the best year of a five-year period immediately following the initial marketing of the product. On new manufacturing processes, research receives credit for the profits of one year. On improved manufacturing processes, savings in cost are credited to research for a period of one year. On patents acquired through research, all royalties or sales of such patents are credited to the department.

A manufacturer of paper products totals separately the annual sales and profits of products derived through research and of products which are not so derived. The net profit on research products is then compared with that on nonresearch products, and the profit increment only is credited to research. This increment may be divided by the cost of research for the year, yielding a figure showing the dollar return per dollar expended for research. At this time credit is not taken for process or product improvement nor for royalties on patents acquired through research.

The tangible returns from research expenditures vary widely, dependent to a considerable extent on the effectiveness of the research department in pushing its ideas through to successful completion. Perhaps even more important is the difference in the formulas which are used for determining these returns. For example, one company has a committee which decides how much royalty can be charged on the cost of a new product and then credits this to the research department. This company states that the successful developments more than pay for all the cost of the research department. Another manufacturer indicates that its research credits for a given year were about equal to its expenditures. Since this is a very successful company, it would appear that the formula is not predicting the proper returns of the research department.

During a period of seven years, one oil company showed annual results ranging from a profit of $1.45 to a loss of $0.25 on each research dollar, with a seven-year average profit of $1.35 per dollar expended on research. Another oil company estimated a yield of $15.40 for each dollar of research expense. This sum consisted of $3.70 savings in royalties which would otherwise have been paid to outside groups, $9.60 for profits resulting from process and product improvement, $2.10 for profits from new products. In one typical year, a paper company estimated a net return of about $10.00 for each research dollar used in producing that amount.

These figures are significant, for a company is thought to be doing well if it shows a profit of $0.20 for each dollar expended in producing that profit. Yet it should not be assumed that these are typical of all industrial-research earnings. Nor must it be forgotten that the law of diminishing returns holds good in evaluating research, else there would be no limit on expenditures for this purpose.

In the ultimate analysis posterity will judge research not alone by the dollar sign, nor by the great industries research has created, nor by our improved material standard of living. Profit is the immediate and essential incentive for industrial research, but there are far broader considerations than this. The findings of research may be applied in making fiber to clothe us, or to produce a deadly form of warfare; a drug may be used to heal the sick, or to inflict pain on the healthy. Atomic fission, born in the stress of war and first applied to the destruction of human life, may become the greatest benefactor of mankind.

Research has extended the horizon of our knowledge until at last we have reached a no-man's land bordering on the infinite. Yet, with all this great scientific advance, have we made the human race better or happier than it was fifty years or a thousand years ago? It is just possible that we have been better scientists than citizens. Now more than ever we should labor to direct the results of research into constructive use. If we succeed, that success itself will be the best measure of the return from our research.

— II —

by

W. S. PARSONS

Rear Admiral, U. S. Navy

It seems desirable to introduce the Weapons Systems Evaluation Group (WSEG) to you first by setting forth the problem, then indicating the mechanism proposed for solving this problem, and finally by giving a necessarily sketchy account of the manner in which the Group has actually operated in its one and one-half years of existence.

Concerning the scientific evaluation of weapons systems evolved in World War II, I shall quote the following from Dr. P. M. S. Blackett of England, who is reputed to be the father of operational research:

> "Many war operations involve considerations with which scientists are specially trained to compete, and in which serving officers are in general not trained. This is especially the case with all those aspects of operations into which probability considerations and the theory of error enter . . . the scientist can encourage numerical thinking on operational matters, and so can help avoid running the war by gusts of emotion . . . ."

A man from Mars might believe that several years after World War II, when the dust had settled and complete analyses could be made of what happened to all operators in that most extensive weapons laboratory in all history, we would enter a period of full and "final" agreement on the question of how mid-20th century wars are fought and won. In actuality, the clouds of dust, atomic and otherwise, seemed to rise higher as World War II receded into the past. From the technical standpoint this was mainly caused by the fact that snorkel submarines, guided missiles, jet aircraft, and atomic weapons appeared in the last year —some in the last days—of the war. So it is quite understandable that by 1948 Dr. Bush, as Chairman of the Research and Development Board, received from his Ad Hoc Committee of eminent scientists, called to consider the problem of overall weapons systems evaluation, a report which included the following:

> "The problem of national security is so serious that every reasonable effort must be made to get the best answers by the application of the scientific method. The speed with which the technology of war has changed and will change; and the complex interactions of technology with tactics, strategy, and logistics in total war are such that military judgment alone is not enough. If we are to exploit fully the power of modern analysis, scientific methods for examining the nature of future war should be employed in the broadest possible sense."

The need for weapons systems evaluation was voiced beyond official documents. *Foreign Affairs* contains the following paragraph in an article called "Science and Politics in the Twentieth Century," by Dr. J. B. Conant:

> "By what procedures are a free people to determine the answers to such complex questions as to whether a large amount of the taxpayer's money is to be spent on the development of a given weapon or its auxiliary? Granted the matter must be left to the people's elected representatives and the President exercising through subordinates his power as Commander-in-Chief, nevertheless the problem still remains, how are politicians to resolve conflicts of opinions among scientists and engineers? Have we devised as yet even the first approximation to a satisfactory procedure for evaluating technical judgments on matters connected with the national defense, including atomic energy? Some who have been close to the postwar scene in Washington and have followed some of the research and development projects must be inclined to answer this question in the negative."

James Forrestal, the Secretary of Defense, was in full agreement with Dr. Bush as to the need for weapons systems evaluation and concurred in the establishment of such a group by the Joint Chiefs of Staff and the Research and Development Board. The document establishing the Weapons Systems Evaluation Group states:

> "The purpose of the Group is to provide rigorous, unprejudiced and independent analyses and evaluations of present and future weapons systems under probable future combat conditions . . . ."

I think it will be apparent from what I have quoted so far that the concept of evaluation of major weapons and systems was fully accepted in what might be called the inner defense circles. The reaction in the press was almost embarrassingly good. As samples of the press reaction, I give the following:

The *Washington Star* commented:

> "With methods of warfare undergoing revolutionary changes, it has become necessary to use the greatest possible wisdom in discarding conventional weapons and techniques and choosing new ones. It is especially important . . . that careful thought be given not only to atomic and bacteriological warfare but to how far it is safe to go in dispensing with pre-atomic tactics strategy. This is the reasoning behind the creation by . . . Secretary Forrestal of the Weapons Systems Evaluation Group . . . ."

The *Christian Science Monitor* declared that Secretary Forrestal acted "boldly, constructively, and imaginatively in creating the Weapons Systems Evaluation Group."

The *Baltimore Sun* observed:

> " . . . The Joint Chiefs of Staff are weighed down with administrative duties which allow them little time for contemplation. Each of them also has certain complex instruments at his disposal. Hence it follows that they think in terms of doing a given job with the instruments already at hand instead of trying to find out what instruments would be best and how they are to be obtained . . . . If the problem were attacked first by determining the best instrument for doing the job, it seems probable that the differences . . . would be reduced in importance. That is the idea behind the new Weapons Systems Evaluation Group . . . ."

The *Kansas City Star* noted:

> " . . . the announcement that a Weapons Systems Evaluation Group has been formed is . . . one of the few really important milestones in the nation's continuing effort to make itself secure . . . the army, the navy, and the air force in the past have given their attention to it separately. The result has been much duplication and even some working at cross-purposes. . . . Our people . . . will be looking to the services for more of the solid assurance that can come from such actions as the centering of responsibility in the Weapons selection unit."

Lieutenant General Hull, who was General Marshall's Assistant Chief of Staff, Operations Division, during World War II, and who in 1948 commanded Joint Task Force 7, which tested three atomic bombs at Eniwetok Atoll, was a unanimous choice for the first director of the Weapons Systems Evaluation Group. His deputy and research director for the group was Dr. Philip Morse of the Massachusetts Institute of Technology, one of the founders of American antisubmarine operational research in World War II and first director of the Brookhaven Laboratory of the Atomic Energy Commission. Dr. Morse was relieved in June, 1950, by Dr. H. P. Robertson, eminent mathematical physicist from the California Institute of Technology, whose wartime scientific contributions earned him the admiration of all three services. My colleagues as senior service members of the Group are: Major General Gavin of the Army, of World War II paratroop fame, and Major General Barnes, with a distinguished record in the Air Force. Dr. Robertson has a civilian scientist, Dr. Welch, as his deputy and, as a rule, the project leader in each evaluation is a civilian scientist.

The operational research groups of the several services and such groups as the Air Force Project RAND at Santa Monica have been of great assistance to WSEG in its analyses and evaluations. The same applies to university laboratories engaged in weapons development, such as the Los Alamos Scientific Laboratory in the case of atomic weapons, and the Johns Hopkins Applied Physics Laboratory, in the case of guided missiles. WSEG borrows key personnel from these research activities, and the exchange has been found beneficial both to the parent group and WSEG.

As WSEG was being formed, sage advisors urged that we cut our teeth on one or two fairly easy problems. This was not to be our luck. One of the first problems handed to us related to strategic air warfare. By August 24, 1949, General Bradley, Chairman of the Joint Chiefs of Staff, testified as follows before the Armed Services Committee investigating the B-36 bomber program:

> "Insofar as we could, we believe that all weapons of a major type and of a major importance should be evaluated by an evaluation board, consisting of military people and scientists and there has been a board set up for this purpose."

The problem of evaluating weapons systems in peacetime is considerably different from wartime operational research. In war the time scale is fairly obvious and if combat reports are accurate there is not much risky extrapolation to be done, particularly if the enemy is sufficiently hard pressed so that he may be assumed to have laid his available weapons cards on the table. By comparison, weapons systems evaluation in peacetime is much less definite. Even to get reliable information on our own weapons and systems requires extensive travel to establish contacts and get first-hand technical and operational knowledge. In our war games we must pit our known weapons against assumed counterweapons which may be better or worse than our best but will certainly be different from ours.

From our vantage point close to the center of technical weapon development and operational analysis, WSEG is in a position to assess the value of applied research in many technical and operational fields. The need to see through and around each military weapons problem is more urgent certaintly in peacetime than any similar need in industry. The reason is that most industry is in "combat" in peacetime and therefore has the advantage of knowing how the game came out each year. A further advantage in industry lies in the fact that the scale of the budget can be determined on a fairly rational basis, whereas in defense planning the value of victory and cost of defeat are not predictable on a quantitative basis.

Need for formal evaluation of weapons has developed with the machine age in the 19th and 20th centuries. It is doubtful that operations analysis or higher mathematics of any kind were used before the decision was made to enter production of bows and arrows to replace rocks as weapons in primitive warfare. In those prehistoric times such a decision was evidently a "horse back" decision involving some energetic operational tests but probably no operational research as we use it in the Weapons Systems Evaluation Group. In the past there has always been a reluctance on the part of the military to accept new weapons until they have been tested in battle, and even then new weapons had to be "sold," so to speak, before they were accepted.

Lieutenant General Hull is my authority for this history of introduction of the machine gun in the U.S. and British armies. The first machine gun was invented by the Gattling brothers in 1859. They tried to get the Army to adopt their gun but without success. All through the Civil War the Gattling brothers followed the Union Army around trying to anticipate when a battle would occur where they could demonstrate their gun by firing at Confederates. It wasn't until 1866, after the war was over, that the Army finally adopted it. They did not make use of it in their military forces until some time later. As a matter of fact, the British Navy adopted it before the British Army did.

The realistic evaluation of ground weapons and close air support now going on in Korea naturally modifies and simplifies some of our problems. However, in several of the major weapons fields the effect of the United Nations reaction to Korea has been to intensify the rearmament effort, with a corresponding need for evaluation before committing ourselves to billion-dollar programs. In other words, some of the peacetime budget brakes are now off, and the responsibility to be *right* is correspondingly greater.

One field in which development is extremely important and expensive is that of guided missiles. These differ from manned aircraft, radar, etc., in that the more successful the missile the sooner it will reach the full-scale hardware stage in flight tests, and from then on expensive crashes will pace progress. While the magnitude of the missile development absolutely requires as much simulation and calculation as possible, it also brings in a hazard which may be expressed as follows: We thoroughly simulate and impose all of the diffiiculties we *know about,* but we pamper the model in the calulator by neglecting to impose any of the difficulties we do not know about. These are some of the reasons why we must have focused on the missile problem many minds which have been kept sharp and resourceful by basic and applied research.

Measuring the return from research from the standpoint of the Weapons Systems Evaluation Group is hardly a quantitative operation. Qualitatively, we can say that the growing fund of technical and operational knowledge bearing on weapons and systems is a direct product of applied research in many fields. Also, those scientists and engineers who have become veterans in these new fields of applied research have thereby qualified themselves to work in the more complicated game played with a crystal ball in one hand and fairly accurate proving ground and operational reports in the other.

As in all advanced fields of endeavor, in order to get results it is necessary first to have good men and then to organize around the problem. The organizational part is not difficult. Inside WSEG the color of the uniform and the differences between military and civilian points of view tend to resolve themselves easily. But the problem of getting first-class civilian talent is complicated by the idea held by many scientists, and to a less extent by engineers, that the closer they get to the center of the Pentagon, the more stifling will be the atmosphere dominated by the so-called "high brass" and "military minds." I think that one of the returns from our research to date has been the demonstration that civilian scientists can operate effectively in such an atmosphere.

Our experience in WSEG to date has been highly encouraging. We have had the utmost cooperation from the military services and from the civilian groups we have visited and with whom we have dealt. They even failed to make the usual screams to high heaven when we gently suggested that we needed to borrow some of their best people. Also, in spite of the difficulties of wrenching a problem and concept into a form amenable to evaluation, we have normally succeeded in doing this by the process of what might be called "approximation by argument." I might add that these arguments have very seldom been settled by Blackett's "gusts of emotion."

To summarize and conclude: From the standpoint of weapons systems evaluation, the measure of return from research in this anxious decade will be the degree to which it increases our national stockpile of flexibility, resourcefulness, and alertness, both in the human and material fields.

— III —

by

C. G. SUITS
Vice President and Director of Research
General Electric Company

Last month I attended a series of company meetings at Association Island on Lake Ontario in which each functional activity of our company was reviewed, starting with research and proceeding through engineering, manufacturing, marketing, as well as the principal administrative activities. These meetings provide, in our company, an indispensable source of integration and balance, and, incidentally, illustrate to an excellent degree the problems inherent in measuring the return from research. The proponents of each of these activities of an industrial enterprise feel, with proper enthusiasm, that theirs is the most important component of the whole. For example, we in the laboratories feel that without our pioneering scientific work the corporation's future would be in doubt. We must admit, however, that some of the output of our laboratory is hardly ready for market in the form in which it leaves our hands and that a great deal of rationalizing and reducing to practice must be accomplished by engineers as the essential next step.

The manufacturing people, in turn, are quick to point out that no matter how well conceived and engineered, unless the highest degree of manufacturing skill is applied to the product, it will never reach a satisfactory market position. Finally, the marketing groups say, with some justification, that without skillful selling there will be little or no need for manufacturing, engineering, or research. Although it is true that most successful products draw fully upon all these industrial functions, the problem of measuring the contribution of each of them separately is not easy. Yet this problem has been studied many times in the course of the history of our laboratory, and I can summarize my talk at once by saying that in our case we have never found an objective and complete solution of the problem of measuring the return from research. My comments will, of course, apply to a research laboratory in industry and, more particularly, to the laboratory in my company with which I am most familiar, which has long-range exploratory research as its primary function. As a necessary consequence of this specialization in long-range research, we depend heavily for the practical utilization of our work upon development laboratories and engineering-design divisions, where the application of science is the primary task. The contributions of many groups of specialists is thus generally involved in the successful life history of a new idea.

I propose to discuss this question in terms of a variety of case histories of actual developments, which will serve to illustrate the complexity of the problem, without, however, leading to any very workable means of measuring the return from research expenditures in a specialized industrial environment. I hope, however, that we may obtain a better understanding of the matter from a consideration of actual examples. Obviously, the character of the industry in which the company operates, the size and kind of business in which the company engages, and, finally, the kind of laboratory operation—all have a great deal to do with the question. Hence the conclusions drawn from case histories may not suggest generalizations at all. In specific cases the return from research may bear little relation to the cost of research; the return may in some cases be very much greater than any properly allocated cost, or, in other cases on which I would not like to dwell, very much less.

Now, measuring the return from research is generally desired for reasons of good business, for, if we can accomplish this result, we can judge whether our expenditures have been made wisely or whether they should be greater or less than at present. Another equally important, related question is the prediction of the future value of the results of research, so that this may, in turn, be compared to the cost of research in progress.

Everyone would, of course, like to be able to accomplish this admirable result with precision, for it would provide the final answer to many important questions of research planning, effort, and expenditures. Because the term "research" covers a great variety of technical activities, it must be admitted that some types of research activity may be susceptible to detailed planning at every stage, including the prediction of return.

Consider, for example, the hypothetical case of the laboratory activity, principally of an applied engineering character, involved in the substitution of a pressure die casting in a product which is now being manufactured with a fabricated part. The circumstances may be such as to permit the anticipation of a considerable saving in manufacturing cost if a die casting can be employed. The cost of doing the necessary laboratory, engineering, and pilot production work may be estimated with satisfactory accuracy, as may also be the expected manufacturing cost savings. Some of the expected savings may permit price reductions and a consequent expansion in volume, which may be determined by a market survey. In other words, enough information is available or obtainable to make possible an estimate of the cost of doing the work and the return which may be anticipated from this particular example of applied research. With such facts, or reasonable facsimiles, fairly complete planning of research effort may be feasible.

Everyone is familiar with the way in which preliminary estimates, particularly cost estimates, acquire an aura of exactitude with the passage of time, frequently to the embarrassment of the estimator. Planning the commercial future of an exploratory research project in its early stages is analogous to planning the education and career of a new baby and is fraught with as many uncertainties. It would be better to wait until something is known about the child's personality, intelligence, and character traits as basic factors upon which to plan.

This is not to imply that planning of this type of research is not possible. One can plan the scope of fundamental and exploratory research and can relate the scope of the work to the scientific opportunities presumed to exist in the field of work and to the plans and aspirations of the company in relevent industrial areas. Although it is frequently true that the detailed results of exploratory research are unpredictable, some of the gross results are not subject to the same uncertainty. For example, some work in our laboratory pointing toward new methods of interrupting electric arcs is more likely to lead to results of importance in the field of switchgear than in polymer chemistry, although I know of a case in which the opposite was true. Planning for the future of such work must be done on a much more expedient basis than in the case of engineering development work. The director must expect to do a good deal of playing by ear, bearing in mind that new experimental results may, without warning, completely alter the complexion of the work for better or for worse.

At the other end of the scale of research is the purely exploratory work in the frontier areas of modern science: research at temperatures near absolute zero involving superconductivity and superflow, investigations of the phenomena of semi-conduction in solids, exploration of nuclear reactions, to mention some current examples in progress in our laboratory. Since the results of exploratory research are thoroughly unpredictable, the application of planning methods which require a detailed knowledge of the time and cost of doing the work and the possible application of the results in terms of products and markets involve unknowns at every point. Planning the career of the research project in the case of truly exploratory work cannot be done in the definite terms with which one may deal in the case of applied reasearch or engineering development. In spite of the fact that principal information on which planning and forecasting depends is generally not available in the early stages of exploratory research, engineers frequently attempt to plan for the future results of the work. I feel that forecasting in such cases, for example, estimating the cost of a new chemical product from very incomplete early research data, frequently does far more harm than good. Once numbers are obtained, one is likely to associate with them a degree of reliability that is by no means justified by the assumptions upon which they are made.

The Case of the Silicones

I would now like to consider a relatively simple case of a research project on which a priori one ought to be able to measure the return from research. I refer to the silicones on which our laboratory has been working for quite a number of years. The initial exploratory work was instigated largely by a consideration of one of the primary weaknesses of organic materials, the carbon-to-carbon linkage. It is the failure of this bond which accounts for the long-time thermal decomposition of organic materials at a temperature of about 105°C; the carbon-hydrogen bond is much stronger. The silicon atom is similar to carbon in much of its chemistry and is not too much larger, so that silicon-substituted organic compounds were possible, and a few such materials had been synthesized. Although it was recognized by analogy with the numerous organic family that a very large number of compounds of silicon, carbon, and hydrogen must be possible, it was the potentialities of high-temperature insulating materials which were sufficiently attractive to justify an extensive investigation. It is important to point out that it was not the sale of silicone materials as such which provided the incentive for this work. It was rather the prospect of very greatly improved performance of electrical machinery which silicone materials might make possible. This project was carried through all of the stages of laboratory and pilot-plant research, and it finally led to the building of a silicone manufacturing plant at Waterford, New York. Concerned with other developments, such as formex and glyptal, it became the basis for the Chemical Department of our company, which was set up in January, 1945.

Although the early expectations concerning thermal stability have been well borne out by experimental results—the Si-C bond is stable at temperatures in the range 17°C to 200°C—the silicon analogues of the organics have been full of surprises. For example, nearly all members of the family have small thermal coefficients of mechanical properties. The oils have a small thermal coefficient of viscosity, and the elastomers a small thermal coefficient of elasticity. Many silicones have astounding antiwetting properties. Others have remarkable antifoaming performance. As these unusual properties have come to light, they have been put to use so that at the present time, well over half of the output of our silicone plant is going into uses that were not foreseen by early research workers on the project. The largest surprise of all has been, of course, the fact that we had in hand not only improved high-temperature materials, but that we had a very large new family of chemicals with unusual and versatile properties. Although the silicone business is still in its infancy, it seems abundantly clear to us that it is destined to become an important segment of the chemical industry.

The return from this research must be sought principally (1) in improvements in products which use the new materials or (2) from the sale of the materials themselves. In each of these directions precise estimates are difficult to make. We use silicones in many types of equipment with beneficial results.

Let me give you one example. Silicone rubber is compatible with pyranol, an important insulating dielectric liquid, so that we have, for the first time, an elastic gasket for pyranol-filled equipment. It has displaced all other material for sealing pyranol capacitors at an attractive annual saving in manufacturing cost. Although the saving in cost is important and readily calculable, it is probably not as important as the improvement in the product which it permits and the wider market which may thereby be served.

Considering now the rapidly growing silicone chemical business, it is probably too early to calculate the return in this direction. We have made large capital investments in this business and will presumably make more as it grows, all at an uncertain but ever present business risk. In any event, the return from this research will probably never be known. Without research there would have been no silicone business. But the same statement applies to the other industrial functions, without which the project could not succeed.

Finally, the prediction of the return from this research, during its early progress, would obviously have been subject to gross error because of the many reasons which are readily apparent.

The Formex Development

Since we both manufacture and sell new materials as well as apparatus which uses new materials, we have a double reason to be interested in the synthesis of new materials. I would like to describe a development in which the effect of a new material on our products is far more important than the sale of the material as such. I refer to formex, an organic product which satisfies to a superlative degree the requirements of an insulating enamel for copper wire. Prior to the development of formex, oil-base enamel was used for this purpose, primarily because it was priced at 15¢ a pound; it was not and is not now a very satisfactory material for this application. The technical requirements of an insulating enamel are very elaborate, but it is principally in mechanical strength that formex is outstanding; it is undoubtedly one of the toughest organic coatings known. Although formex is much more expensive than oil-base enamel and is more difficult to apply to wire, it has practically displaced the latter. Ninety per cent of all wire enamel used in this country is formex. The reason is that the toughness of the formex film makes its use possible where ordinary enamel will fail. It can be wound by automatic machines at high speed without damage to the integrity of the insulation film.

Because its electrical strength is also excellent, it can be used in a thinner film for a given electrical stress. To provide a given number of turns in a motor or transformer, therefore, less space needs to be allocated to insulation. This reduces the size, weight, and cost of the magnetic core. Thus, the ultimate gain in the use of this material is in the design of electrical equipment. Probably the most important single factor in the small size and inexpensive construction of modern small motors and transformers is formex. The potential market for wire enamel is not large; it is a specialty chemical product of limited application. However, improvements in wire insulation have a far-reaching effect on the cost of electrical machinery, and it is in this area that the important results of the research appear. If the costs of the research which led to formex are allocated against the end product, which is the wire enamel itself, the sale of the improved material readily permits a liquidation of the cost of the research. However, no accounting system we have devised measures the great importance of this development to the design, manufacturing methods, and hence the cost of electrical machinery made possible by the formex development. Hence the return from the research in this case is apparent and satisfying, but its measurement, as that term is generally understood, is a matter of great complexity.

The Electric Blanket

I would now like to describe a project of our laboratory which I hesitate to call research. In this case the difficulties of judging the value of the result and developing a market were prominent. Dr. Whitney became interested in the idea of the electric blanket in the early nineteen thirties, more or less for the fun of it. I don't think he was consciously trying to develop a new electrical appliance; he rather was motivated by a curiosity to see how an electrically heated comforter would work. He got Mr. William Kearsley to build a first sample, and it worked well from the beginning. A type of control was devised which automatically compensated for changes in room temperature, and a number of experimental blankets were built and used by willing laboratory collaborators. One distressing difficulty was discovered. When the blanket is properly spread on the bed, a satisfactory temperature requires a heat input of about 150 watts. If the blanket should be rolled up without being turned off, this 150 watts of heat, well insulated, will cause the center of the rolled-up blanket to become very hot and eventually to burn. Before we had a solution to this problem, a number of these early experimental electric blankets caught fire.

When this happened, Mr. Kearsley was invariably confronted the next morning, if not earlier, by the unfortunate individual who, clutching the charred remains in one hand, described his misfortune with the other. Happily, there were no serious accidents. Mr. Kearsley began to notice that people who burned up blankets, although naturally somewhat upset by the experience, were far more upset if there was to be any appreciable delay in repairing or replacing their blanket. The obvious enthusiasm of users of electric blankets, even under the somewhat difficult circumstances I have described, convinced us in the laboratory that there was much merit in the idea, and we promptly set about to interest our engineers in the results we had obtained. The danger of the rolled-up blanket was solved by placing a number of tiny safety thermostats in the blanket, which open the electric circuit if the temperature rises excessively. The latest design of electric blanket has a far more elegant solution of this problem.

It did not take long to interest our appliance engineers in the electric blanket; but everyone recognized that the very novelty of the idea would necessitate a considerable period of public education before one could hope for any general acceptance of the idea. The development of a new electrical appliance is frequently preceded by market surveys and market testing.

But a market survey in the case of a product as new as was the electric blanket in 1936 would have been useless; any housewife confronted by the question, "If an electric blanket were put on the market at a certain price, would you buy one?"—could only have given the questioner a puzzled expression. She had never seen an electric blanket and could hardly be expected to imagine what they were like. The first regular production of electric blankets took place in 1940. Production increased every year, with a hiatus during the war, but it was not until 1947 that a profit position was achieved. Meanwhile a great deal of engineering development went into progressive refinement of this basically simple device. At present, ten years after the blanket was first marketed, the national market is estimated to be some $12 million.

It is easy to measure the modest research expenditures which went into this development, but for reasons similar to those found in the previously mentioned cases, the determination of a return from the development which is assignable to research is quite another matter. The contribution of many groups was essential to the success of the project, but special credit is due to the marketing people, who must be particularly skillful in the case of any basically new product such as this.

Project Cirrus

A peculiarly intricate example of the difficulty of measuring the return from research is presented by Project Cirrus, the work on weather modification, in which our laboratory is participating in an advisory and analytical capacity.

During World War II, Langmuir and Schaefer undertook a study of aircraft-wing icing, which focused attention on the problem of supercooled clouds. Ice forms on aircraft structures when they come in contact with clouds in which the liquid water particles are at temperatures substantially below the freezing point. The meteorologists were familiar with supercooled clouds, but they didn't know very much about them, and the phenomena held a great fascination for Langmuir and Schaefer. So, at the end of the war, they set out to learn how the water in supercooled clouds can be made to freeze. A home freezing cabinet was the principal laboratory equipment required for the experiments, and in it Schaefer formed supercooled clouds and studied their properties. He discovered that a particle of dry ice would quickly nucleate the entire cloud, causing it to settle out as snow in the bottom of the chamber. Later Dr. Vonnegut determined that nucleation of a supercooled cloud could be accomplished by silver iodide particles. Both methods were successfully applied to actual clouds: by "seeding" the cloud from aircraft, dispensing dry ice or silver iodide, or by silver iodide generators operated on the ground. At this point we asked the armed services (and they agreed) to undertake a contract in partial support of the work, primarily because of the legal liability aspect which is inherent in parts of the task.

With this pioneering work has come into being a new science of experimental meteorology. Active experimenting on weather modification is known to be going on in Australia, Canada, Mexico, Hawaii, South Africa, in many South and Central American countries, and probably at many unannounced points. In the United States, in addition to the well-known reservoir-filling operation carried out by New York City, there are many local cloud-seeding projects going on in various parts of the country. It is somewhat astonishing to behold the rather casual and routine manner in which rain-making contractors carry out their operation in the southwestern part of the country. Probably far more important than local rain making is the growing evidence that continent-wide weather modification is feasible. Where this work will lead eventually is impossible to predict, but its potentialities are of the very greatest importance. It is certain that for the first time in history man has at hand a powerful means for influencing weather. These are the bare outlines of the case history of the project. Now let us try to measure the return from this research! I know of no single application of this work to the products which we manufacture. Yet it was decided in the highest councils of the company that the importance of the work to the economy as well as the national security was such that we should continue the work. This case illustrates the necessity and obligation of considering many factors, some of which are noneconomic, in measuring the results of research of this character.

The problem of measuring the return from research varies greatly with different types of work. In some cases the problem reduces to that of separating the contribution of research from the contributions of the many skills—engineering, manufacturing, and marketing, etc.—which have by their collective efforts made possible a new development. In other, less simple cases the effect of a new material, for example, formex, has so many ramifications in so many different areas of an industrial economy that it seems hopeless to measure it adequately. In the case of still other research, for example, Project Cirrus, the results of research extend so far beyond the boundaries of a given industrial enterprise—in this case they may well extend beyond the boundaries of the nation—that one cannot hope to measure the return from the research.

I have developed the thesis that measuring the return from research is difficult because of the manifold ways in which the beneficial results of research manifest themselves and because of the important and inter-related contributions of engineering, manufacturing, and marketing to a research result which attains a market. I would like to leave no reasonable doubt, however, on one point. Although the contributions of many groups are vital to the practical utilization of a research result, it is the scientist himself who produces the vital foundation on which the whole structure rests—the new fact of nature. The scientists and the laboratories which have the freedom to follow their curiosity in search of new facts of nature are among our most important national assets.

DISCUSSION OF DR. ABRAMS', ADMIRAL PARSONS', AND DR. SUITS' PAPERS

DR. STEVENS: In the various papers that have been presented for the quantitative measurement of the results of research, I see no base line, no measure of what would have happened had the research not been conducted, and I would like to ask Allen Abrams how he would cover that.

It seems to me there is a little hazard in telling manufacturers that you can measure the return from research. I would like to know what would have happened if you had not done the research.

I do not know whether there is any particular base. I know that in our own company, we can segregate the developments that have come through research, the new products we are now making, and the profit that comes from those products.

DR. ABRAMS: I have to refer to the various men who responded to our questionnaire. A number of companies said that if the research department were not there, they would no longer be in business.

A director of research who has been at the job for some time acquires an innate feeling that he has or has not accomplished certain things.

Research has developed something like 30 per cent of our present production, on which we show about 45 per cent of our total profit.

COLONEL SEILER: I was wondering whether the Weapons Systems Evaluation Group has any method for apprizing the research and development people in the defense department of the shortcomings as they appear to be evident from their evaluation?

ADMIRAL PARSONS: That brings up quite a question. Of course, in our travels and cruises, in B-36's and in submarines, and so on, we run into a lot of equipment which is apparently not operating right.

The question is how to handle such reports. If we handle it on a formal basis and blacken the name of the organization by reporting it after we get back to Washington, that would probably be the last trip on which we would get any information of value to us.

We would become confused with the Gestapo, or the Inspector General, and we would kill our sources of information.

We find that the best way to handle situations like that, when we find something hot, either in a good or bad sense, we come back and deal on a completely informal basis, at the points in Washington where it is likely to result in the most good. For instance, we actually went to the Naval Research Laboratory, and General Barnes of the Air Force was very struck with some new things of a classified nature going on. He proceeded to help the Air Force Major who was the liaison officer there by becoming a major general liaison officer for that particular project and seeing to it that it got pushed ahead.

I think that is the answer to your question. We do approach these things, but we very seldom write letters, unless it is a question of, well, almost international importance, such as the logistics of bases or something like that.

If we become aware of a serious problem affecting our ability to perform strategic bombing, for instance, we would perhaps write a letter to the joint chiefs in that particular case. That does not reflect on any group or organization that has been our host in the United States.

DR. O'BRYAN: That has been our experience. They not only have General Barnes, they have General Gavin and Admiral Parsons, and also other sources of information, and they very quickly get it to the persons who are concerned, and very rarely write letters.

DR. HOLLAND: I would like to make an observation about this index of returns. What would you do with an index of returns if you had one? How much research would industry support?

MR. HUGHES: I was wondering if there has been set up in any industry a body or division, an independent body, that is analagous to the weapons evaluation group. If so, what effect has it had on industry?

DR. O'BRYAN: I don't know whether there are any industry-wide evaluation groups. I am sure that every company has its evaluation, with its quality controls and market analysis.

A VOICE: We have set one up on a fair-sized scale and have men that have had experience in military work. We have applied it to an area that five years ago would not have been open to research.

DR. O'BRYAN: I think this sort of thing has been going on since the beginning of time, but it has never been so formal, or was ever called "operational analysis."

DR. KILLIAN: I might also mention that the National Research Council has a committee studying the application of operational research to industrial and civilian needs. It is quite an active committee. It is a new one and it is just making its first study.

DR. HOLLAND: It seems to me that for a long time there has been a need for something along that line, an evaluations group in industry, and to a limited extent there has been something of that kind going on.

For instance, at one time before the practice became illegal, testing laboratories used to test the products of the various laboratories in the country. But there is one fundamental difference in the job of the military and the corresponding job in industry. Military people in the time of peace cannot get operational data. We can.

If the products fail, we find it out immediately, because the customers cast their vote. So, in a sense, our weapons evaluation is carried on as a current activity.

Perhaps industry could go farther in anticipating customer reaction, and I suspect that there are some opportunities there that are being overlooked right now.

MR. FISHER: First, I would like to make a comment and then ask a question.

We do not have an opportunity to test our product without very large capital investment, and I have somewhat of a personal horror of industry, where everyone agrees that the indices have to be modified by judgment, but many fail to do it.

I would like to ask whether the people who are using these indices find that they are misused to any great extent, to any damaging extent?

MR. REEVES: I would like to comment on that question. I agree that it is very easy to misuse them, and one reason I was a little evasive this morning, when somebody asked what we had done with them, was that we have found that they can have some fairly surprising results. In other words, if you try to claim too much for research, other people start arguing in the other direction, and there is quite a battle on as to whether the research is doing any good or not.

In answer to the question of why you need these things, it seems to me that it is very important for a company to know what its different operations contribute to the overall operations. I know that in our industry we have had a very clear illustration of this because we make a great many products out of one raw material. As a result, it was extremely difficult in our early operations to try and find out how much it cost to make each product.

In other words, we needed to find out how much gasoline costs, how much heating oil costs, how much different lubricants cost, etc. At one time all products were sold for what we could get for them, and if the company made money, all thought they had done a pretty good job.

Later they set up a system whereby they could analyze the actual costs that went into the production of different products, and that revealed that there were many weak points in the operations. There were some products that were losing quite a bit of money while others were big money-makers. From this study it was possible for the company to readjust its operations so as to make the products which were the most successful from a financial standpoint and would increase the overall return.

I think the same applies to research. I believe that it is fairly certain and accepted by most companies that research is desirable and that it is worth-while. But the question is, how worth-while is it? Are we doing too much of it or too little, and are the research efforts in the right direction? If we had the answers to those questions, I suppose research would be much more effective than it is now.

DR. ABRAMS: I think that is a very good statement. After investigating a good many of the results of these research directors, my own feeling is that any kind of index you may set up is perhaps a morale builder, and as long as nobody in the company objects to it, the index may give someone a lift.

MR. HOLLAND: My question is addressed to Admiral Parsons. I was very much impressed with the Weapons Systems Evaluation Group. I followed your talk with a great deal of interest and I feel a lot more secure as a citizen, knowing that such work is going on.

My question has to do with this thought. Long before we had organized research, we had in this country a great talent for developing individual inventors. In fact, an ancestor of mine happens to have been one of them; his name is John Holland, who invented the submarine.

What part of the flow of material that comes in comes from organized research groups and what part comes from independent inventors? What provision is made for native American genius to flow into such an evaluation system?

ADMIRAL PARSONS: In the Weapons Systems Evaluation Group, we really get the fruit of research and development after it has reached or at least approached the prototype stage.

Now, in the case of the various departments, for example, the Air Force, the Army, and the Navy, the Research and Development Board may get a certain invention, and it will eventually end up in a contract for the development of that particular article. I believe that we would come in quite a little time after its inception, in that particular case.

We might come in as midwives, but we would not decide how the infant is to be reared. I believe that it is a function of the Research and Development Board and the department concerned at least to bear the child.

We then would look at it and be very interested, or perhaps not so interested in a particular case. Let me take the case of helicopters. I can think of several new systems in which the development of the helicopter is the principal bottleneck. The helicopter will have to become about four times as good, e.g., flyable in the dark, and on instruments, etc., before any particular hot idea will bear fruit; so we go around and talk up helicopters for the purpose of putting across some ideas which can only be used for helicopters. But I would like to say that we do not receive the invention at the earliest stage, which might be called the glint-in-the-eye stage.

A VOICE: Now offhand, if only to give the dogs a rag to chew on, so to speak, I would like to submit a suggestion for the agenda at the next meeting. Perhaps it might be possible not to have so many research directors but rather some of the people that handle the money, to discuss how they decide whether their research is any good. I believe we have been discussing it all from the point of view of the researcher, figuring out how to prove to the man with the money that our product is worth-while.

DR. O'BRYAN: I think that Dr. Abrams, with years with the Marathon Company, knows something about that.

DR. ABRAMS: The question you raise bothers the average research director. He wonders how good he is. I suppose that to be one reason you find so many people interested in getting some basis for a determination of what they are accomplishing.

It is interesting that such a minor part of a company's expense should be subject to so much discussion.

In the average company today it is only one or two per cent that is being spent on research, and the insurance on the building and equipment is more than the average research budget.

Management requires a feeling that the research department is producing the goods, and the general health of the company is good because of that.

A VOICE: To clarify what I was trying to say before, part of the difficulty arises out of a full formulation of the problem. When you try to find the linear relationship and can't find any, do you leave it there or do you reformulate the problem?

It seems to me it is a fairly common occurrence to seek a solution along a certain line, and the discovery that you cannot find it along that line is frequently the precious discovery that permits you to reformulate it and find it in another direction.

Is that perhaps true in this case? That is the question.

DR. O'BRYAN: It is not necessarily inverse, is it?

A VOICE: No, not necessarily inverse at all.

DR. O'BRYAN: Dr. Suits will be unable to be here tomorrow, so he won't be in the round-table conferences, and he has agreed to be a special target for the next few seconds.

DR. SUITS: If I may comment on the last few remarks, I agree with Dr. Abrams and the other gentlemen, that an important problem is that of determining the over-all matter of how much research the company should do, as well as the detailed evaluation of what has come from the individual projects. As Dr. Reeves pointed out, there is an unfortunate aspect of the attempt to evaluate separately the contribution of research to the success of the project. If you attempt to ascribe to the research contribution an unrealistic share in the success of the over-all project, you may endanger the continuing cooperation of all the groups which by their joint efforts have done the job.

There is a well-known technique in our company of ensuring that the step following research will be taken, the next step being an engineering step. When a project leaves the laboratory, there is quite a bit left undone; otherwise it would be difficult to interest the engineers in the job. At least that is what we claim. The fact is that we usually don't know how to take the next step, so that a great deal is left to the ingenuity of the engineers in the development laboratories. Since cooperation and teamwork all along the line are required on large and technically complex developments, it seems to me that it is desirable tactically for the research laboratory to show restraint in claiming credit for its part of the work.

DR. FOOTE: I would like to enlarge upon that a bit. Some of the reluctance of corporate officers to expand research and research facilities is due to the fact that, except for routine improvement of operating problems, the results of research usually necessitate capital assets expansion. Research either renders existing processes obsolete or it introduces a new product or process, both results requiring further capital investment for plant equipment. The industrialists in general would like to encourage every development of the research laboratory but are limited by funds that are necessary to capitalize upon these developments. In fact, it may be possible that the quantity of research that a corporation can finance should be determined by the amount of capital asset expansion that should be diverted to putting the results of research into commercial operation. This is many times greater than the research budget itself and constitutes a major problem of finance, especially in our present inflationary spiral, where funds arising from amortization are so totally inadequate.

A VOICE: I don't think you can place too much stress on the inter-relationship between departments. I know a man who is running a research organization for a company selling about ten million dollars' worth of products a year in a highly competitive industry, and this organization during the past ten years has developed about five per cent of all its products that the corporation is currently selling. The products are good as a whole, and so on, and yet the corporation is losing half a million dollars a year. What kind of an index can you dream up that will give that research department the break it needs? There is another part of the business in this case, which, I am sure, would fall down under any index system that I have ever heard discussed. You cannot get a break for the research department under such conditions.

ROUND-TABLE DISCUSSIONS *

—I—

LEROY A. BROTHERS, *Chairman*

The discussion centered around indices as a means of measuring the return from research. Agreement was reached on the view that the use of indices generally should be restricted to development, to operations research, and to applied research, and that they should not be used for fundamental and basic research.

Concern was expressed that the misuse of indices might be more dangerous than if no use were made of indices. This was countered strongly by those who held that intelligent use of indices was so valuable

* Two round-table discussions were necessary because of the large attendance.

as to warrant the minor risk that some harm might be done through misuse. It was stated that a research worker who could recognize how his efforts might create a return for his organization might easily be influenced to change his efforts from a research project of small potential return to an area of greater apparent return. The importance of a set of convincing figures to be presented to a board of directors was also emphasized.

For fundamental research it was recognized that indices were probably of very little value. Other measurements of the return from research were stated to be the personal satisfaction to the research worker and the vitality of a research organization.

Indices of the return from research may be of many kinds, some capable of being assigned numerical values and others which are relatively indefinite. Some of these enumerated were: dollar return, capital requirements, manpower costs, scheduled time of completion, casualties in warfare, consumption of strategic materials, patent royalties, morale, and public relations. Emphasis was placed on long-term evaluation, ending with more rephrasing than occurred in the talks before the Conference. These included: vice president in charge of "change," vice president in charge of "no change," and president in charge of "profit."

– II –

ALLEN ABRAMS and W. T. BLAKE, *Co-Chairmen*

Evaluating the results of research is a subject on which there is a wealth of interest but a dearth of knowledge. Evaluating research and development is done to some extent by all research directors and their superiors. Occasionally one finds formality in evaluating research, including the techniques of the statistician and chartist. More often, however, one finds the procedures informal, even obscure.

The extent of interest in this subject is based on the realization that research evaluation is vital to research and development growth. There are two recognized valuable elements in evaluating the results of research, whether formal or informal, namely, the element of good salesmanship to those who hold the purse strings and the element of better control of the research program, both present and future.

Some means of evaluation is important in order to put research and development on a comparable basis with other departments which have tangible products. It is easy for a manufacturing department to point to the number of units which they produced during a certain period. The research and development department also needs to be able to count the quality and quantity of its "products" during any time span. The need for this is great at all times for the furthering of the research worker's feeling that he is making a definite contribution, particularly great in times of austerity, when research and development is often considered a luxury that can be retrenched. Data on research results is helpful, not only to the research director himself in selling the value of his services to his superiors, but to each successive echelon above him, in their salesmanship to those to whom they are responsible. Thus, even the board of directors in industry must be armed with facts on research results when subjected to the interrogations of stockholders. Research has been likened to insurance, yet, since it doesn't guarantee protection from techno-economic hazards, it requires salesmanship.

Evaluating the results of research to the research director is perhaps the most important reason why the interest in this topic is so great. Precise facts on the results of past research can help the research director to judge the potential value of new programs of research. Records of past expenditures of time and money and the effect of these expenditures on the profit and loss statement of the company can be of great service in appraising the calculated risks of new research programs.

The need for formal procedures of evaluating research results depends to some extent on the need for research salesmanship. A research director who is a good salesman puts the story of research results in the language of the businessman, which often means that it must be related to the dollar sign. In contrast, some companies are old and experienced in research and have managements that understand and have faith in the value of research. Therefore, evaluation procedures for purposes of salesmanship are less important. These are often companies that can point with pride to the fact that their whole existence now depends on products which came up the "research way."

There are but a few organizations with specific, precise methods for evaluating the results of research because the problem is fraught with great difficulties. Precision in method is easier for a research and development department which devotes almost all its energies to product and process improvements than for a department that devotes a great share of its energy to research of a more fundamental nature. It is easier to evaluate the results of a process which improves yield than the results of some fundamental research which is applied in varying degrees over a time span in a number of company operations.

One research department makes periodic checks of its profits resulting from products that came up the "research way" vs. those obtained otherwise. This research department evaluated its usefulness by pointing to the profit increment on "research items" vs. "nonresearch items" and divided this increment by the annual cost of research. In this case, the ratio of profit increment to research costs on an annual basis was 10:1. Another company divided the value for one year of projects completed during a fiscal year resulting in reduced product or process costs by the annual cost of research. The ratio in this case was 14:10. The techniques used by Olin Industries can be referred to as perhaps the most elaborate effort at precision in the area of evaluating the results of research. (See *Business Week,* December 3, 1949, p. 26).

Some companies have minor techniques that serve the purpose of evaluating research to some extent, one of these being the maintenance of a log book as to time and place that research and development findings are adopted by the production departments of the company. Another company lists the annual sales volume of products that resulted from research and leaves the precise dollar credit due research an open question. Some companies "sell" research to some extent not by evaluation but rather to "keep up with the Jones'," either for public relations or out of fear of being left behind in the techno-economic race. Other research directors, not having a system for evaluating research, admit to the real need for their keeping their "powder dry" to protect them against all who assail research.

There are many precautions to be taken in techniques that place a dollar value on research results. The greatest hazard is the resistance from other departments of the company that will result from lack of modesty and over-salesmanship. Any formal procedure for evaluating research should have the blessing of other departments, and savings ascribed to research should be agreed upon by representatives from other departments concerned. Failure to recognize that research is but one member of the team can cause more harm than good. Research and development results often cause the production man the "trouble" of abandoning the status quo and changing to a process that expects more of him in terms of control, watchfulness, and direction.

There is always a possibility also that formal procedures for evaluating research will result in building a research program around those types of projects where research results can most easily and most quickly be recognized. This could act as a substantial deterrent to the undertaking of long-range research of a more fundamental nature—research which in the final analysis could be vital to the company's existence. Therefore, a research director must guard against allowing formal procedures for evaluating research to unbalance his research program, However, a realistic research director, especially in a company new in research, will have in his portfolio of projects a sufficient number of the short-term tangible-results nature to provide him with facts for his salesmanship.

Problems of the evaluating of results of Government research are somewhat unique, yet the need for salesmanship at the time when research appropriations are being considered is often great. There are many Government research projects costing substantial sums of money that can never be properly evaluated until time of war.

Dinner Meeting

James P. Adams, *presiding*
Provost, University of Michigan

ENGINEERING RESEARCH IN MODERN CORPORATIONS

by

JAMES C. ZEDER
Director of Engineering and Research
Chrysler Corporation

I CAN'T BEGIN to tell you how honored I am to be invited to address this distinguished audience. As a graduate of this University, I am proud that it can serve as host to a gathering of the leading minds of this country on a subject so fundamental to progress as research. As an engineer from industry, I am pleased to share in this joint effort by universities, Government, and industry to reach solutions to problems which we have in common. As an individual, however, I would feel much more comfortable sitting out there listening to some one of you speak from this platform in my place.

Research is a controversial subject. Nearly everyone has strong opinions about it, and yet even among research men opinions differ widely as to what it is and how it is done. Outside the research laboratory the opinions get wilder and wilder. In some quarters, the belief obtains that all good—political and social harmony, health, and complete freedom from economic drudgery—in short, heaven itself, will eventually emerge from the laboratories if we will wait just a little longer and spend just a few more dollars.

Much of this is generated by our commercial attitude toward research. Research is a word which has become a symbol for progress, and, in the highly competitive atmosphere of modern merchandising, industry tends to divert attention from the heardheaded pick-and-shovel job that a research organization has to do to its more romantic but specious aspects.

I cannot tell this group anything new about research. However, I can explain the thinking and attitudes on this subject which derive from the experience of one industrial engineering and research organization. At Chrysler Corporation, the sequence of these activities from the uncovering of new knowledge in the engineering sciences to the production of finished goods may be classified under four headings, and I believe that this same method of classification will apply to most modern corporations, even though different terms may be used. These classifications, according to the names which I prefer for them, are: production testing, product engineering and development, engineering research, and academic research.

The terms "production testing" and "product engineering and development" are self-explanatory. Production testing is the devising and performing of tests on production samples to insure that the product meets the standards of performance and life desired for it. Product engineering and development is the activity of bringing out the annual new models—including the initial establishment of all specifications, the preparation of drawings which enable the product to be manufactured, and the constant refining and improvement of the product after it is in production.

"Engineering research" is a little farther upstream from the product than production engineering and development and is therefore harder to define. What we mean at Chrysler Corporation by this term is the organized effort to apply new materials, new processes, and new principles to the improvement of our products by basic revisions in design, the introduction of entirely new features, or even the addition of new products to the line.

The fourth category, "academic research," we identify as a search for knowledge without regard to its possible application. It is sometimes referred to as "fundamental" or "basic" and has been called research for research's sake. For reasons which I will explain in a few moments, we consider it to be outside of the scope of an industrial organization.

I have reviewed these classifications only as a matter of orientation, for the only one I intend to talk about is engineering research. Obviously, both production testing and product engineering and development are essential to a successful corporation. Unless a product is being manufactured which has no unknown quantities and unless that product can hold public acceptance indefinitely without change, no industrial organization can long continue without both these functions.

In regard to academic research, on the other hand, our experience at Chrysler Corporation has definitely indicated that it can be carried on more economically in the long view by universities and research foundations. Academic research is the fountainhead of all future progress; but to carry on such research in an industrial corporation imposes too much of a strain on normal viewpoints and attitudes. After all, a corporation is run for the express purpose of turning out a product at a profit, and setting up an academic research group within corporate confines and then trying to wall it off from the motivating philosophy of the company is almost impossible. Too soon you will find your academic research group joyously—and profitably—engaged in engineering research or product development.

The most important element in a successful engineering research group is the quality of the man who heads it. Research must be an *organized* effort to be effective; hence, a research head must be primarily a good organizer and administrator. We have had the experience of taking a research department out of the hands of a highly skilled researcher with little administrative ability, turning it over to a good technical man who was not a researcher but who knew how to organize, and seeing the effective research output of that department increase enormously with the same facilities and staff.

While a research head need not be the top scientist in the organization, he must certainly have a thorough knowledge of his field, a good understanding of what research is, and an overwhelming desire to do it. Too many research directors tend to fool both themselves and their managements with dramatic demonstrations of man-made lightning, frying eggs on newspapers, and the like. They confuse exhibitionism with research and as a result accomplish little that is good and lasting.

On the other hand, a little showmanship is sometimes necessary to get an idea across. A research head has one of the most frustrating jobs in the world—selling new ideas to people who are operating successfully with old ideas—and he must have not only the courage of his convictions but also the salesmanship necessary to get them accepted by his management. By salesmanship I don't just mean aggressiveness, either, but rather the skillful use of all the refined techniques which have been developed by modern business to gain acceptance for ideas and products. If management is consistently turning down ideas from its research department, the research director must take some share of the blame because *selling* research is a substantial part of his job.

Sometimes, you will find a research engineer who will ride a project he likes for as long as you will let him. Give him one job that interests him and he is fixed for life. You can never get him to wind it up. Then there is another type who will stick to a project until he loses interest or the going gets tough, and then he will switch to another project. This kind never gets anything done, either. A good research director must constantly be on his guard against people of this kind in his organization.

Finally, a good research head must keep his eye on company objectives and not be easily lured away from what is best for the corporation to a pursuit of his own special interests. Sometimes this means having the courage to stop a project, even though some sacrifice of personal prestige is involved, when it becomes obvious that the results to be attained will never justify the expenditures.

Successful operation of a research group demands a continuous flow of new projects, as well as an output of successful and valuable developments. As a matter of fact, the quality of research output depends to a very large extent on the selection of the projects.

Research in an industrial organization must have a definite objective; it cannot move in the line of least resistance, or necessarily that of greatest scientific interest, but must be acknowledged from the beginning as a step in product improvement. In the selection of these projects at Chrysler Corporation we ask pertinent questions:

1) If the work is successful, will the results be used in the product or for the direct benefit of other corporation groups?

2) Will the probable cost of doing the research justify the results obtained and be within the financial means of the corporation?

3) Will it be logical to use the results at the time they become available or will they be so far in advance of manufacturing techniques and market requirements that public acceptance will be questionable?

There are many other questions such as: Is manpower available and capable of handling the job? Are adequate facilities and instrumentation available? Is its scope too broad? Will it interfere with the everyday business of the corporation?

We do not lay the burden of answering these questions solely upon our research group. After a preliminary investigation by this group has convinced them that the project has value, the project and its proposed scope is discussed with the heads of the production engineering and development groups in whose field of activity the project may eventually lie. This not only aids the research group in a determination of whether the project should be undertaken but also establishes a close liaison between research and development from the very inception of the project, which aids greatly in the utilization of the results if the project proves successful.

Before we had this arrangement at Chrysler Corporation, the development laboratories were inclined to be antagonistic toward any projects carried on by the research group. The complaints were that they should have been assigned the job, or that they could do it better, or that the work should not be done at all. By acquainting the development laboratories with the research project at its very start and making them a party to the decision to start it, we eliminated this friction and made the production engineering and development groups much more receptive to the end results of the project.

Despite the care with which these early stages are considered, it does not always preclude the possibility that engineering compromises or unforeseen difficulties will make the project completely unattractive before it is finished; and, consequently, progress reviews are held with the original group, in order that they may be acquainted with any change which might affect their interest or later use of the results.

We have learned through sad experience that nothing is quite as disorganizing to a research group or as disturbing to its relations to the organization as a whole, as a project which has been approved merely because it was a hobby horse for someone to ride or was oversold originally by its sponsoring group. The unwillingness to give sufficient time to the initial analysis of the problem or to spend enough money on its early exploration inevitably results in a high percentage of rejected effort and the quiet burial of the work of capable men in the chilly dawn of realization that the work should never have been started in the first place.

The operation of a research department has no royal road to success. To stretch the metaphor a little further, we might say that it is an extremely rocky path, if there is any path at all, since, of course, the job of research is that of the pathfinder and the pioneer. Just as the pioneer will find a place to settle down and take root, so the researcher must occasionally stop long enough to consolidate his gains into usable form. If this work is successful, it becomes the problem of the corporation management to decide when it shall be taken out of research, or, expressing it from the research man's point of view, torn from the sympathetic arms of its fond parents and thrown to the development wolves.

From the viewpoint of the research department, the new idea is seldom old enough to stand on its own feet when it is taken from them and passed over to a development group. It will probably always be that way, and in extreme cases it may be necessary to allow the research group to continue to do the developmental work. This may arise because it would take too long to reeducate a development group to the point of progress already achieved by research, and it is faster and better to allow research to complete the job almost to the point of commercialization. This condition, however, is unusual and it should not be allowed to interfere with the normal transfer of research projects into the development and production engineering groups.

We have learned one thing at Chrysler, however, and that is to allow the research idea to continue to the point where its acceptance by development or production engineering groups is assured on at least an equal basis with their own developments, since only too often we attempt to pour the new wine of research into the old bottles of conventionalism and lose both the wine and the bottles.

The direct transmission of projects and ideas from research to the production engineering and development group is not the only way in which a research department serves its corporation. Frequently, a new design which comes out of research is rejected on the grounds that it is too far in advance of its time, or that immediate application of it cannot be made, or simply that it is "no good." But the idea nevertheless has had its influence on the thinking of the organization. It may just help pave the way for the next idea which comes along, which by contrast may not seem quite so radical. Or it may simply act like a seed which lies dormant for a time and then germinates into a new plant on the development side of the fence, with the fact forgotten that it originated in the research department. In these ways, a good research program is constantly having its impact on the development operations of the company, even though there does not seem to be any direct mechanical linkage between the two. At the very least, research is a stimulating influence which a progressive and growing company cannot afford to be without.

It is an oversimplification, however, simply to assert that research is a good thing and that industrial corporations should do it. There are a lot of companies which are rich enough to have extensive research facilities but not smart enough to direct them properly. I have had the experience of having to exhibit polite admiration when being shown around such laboratories while being privately amazed at how poorly organized they were and how little value was coming out of them.

The mere fact of having a research department is no justification for a feeling of smugness. No one will attest more vigorously to the value of research than we at Chrysler, but I don't mind admitting that we made several false starts over a period of years before we developed the type of research organization which was right for us. We found through experience that a research department must be tailored to the particular job it has to do. There is no general prescription that can be followed. What is right for one corporation with a certain product selling in a given market may be all wrong for another combination of conditions; so each company must devise its own formula.

The most productive results are obtained when the research department is integrated into the engineering organization instead of separated from it. There must be a balance, a harmony; one must not be tied to the other. Isolating a research facility from the rest of the corporation—either geographically or organizationally—has definite disadvantages. The only outlet which research has for its products is the production engineering and development group, and you can't sell to a customer without maintaining a good contact with him. Also, research needs a reasonably close association with the rest of the engineering department in order to keep informed on the problems which exist and the directions which future research should take.

How much should be spent on research? If enthusiasm is allowed to run away with good business judgment, it is possible to spend too much. Research itself has become more and more costly. The instruments and equipment required are much more extensive and complex than they were in the past. Also, in the products of today we have run out of the simple, obvious improvements which could be made, and now it takes much more research manpower and facilities to get even a little new information. So research is an expensive commodity, and in some ways it suggests a parallel to the prospective buyer who asked the yacht salesman how much a week it would cost to operate one of his larger models. The answer was, "Mister, if you have to ask that, then you shouldn't be buying a boat like this."

In some companies the idea prevails that a fixed percentage of each sales dollar should be set aside for research. At Chrysler Corporation we are definitely opposed to any such rule. A good research program depends on an intelligent understanding of the job to be done, not on a knowledge of how much is available to be spent. When you have more money than projects, the research director has to look around for additional ways to spend it, and you have robbed him of the stimulation of having to compete with other divisions of the corporation for his budget allocations. On the other hand, during periods of low sales volume, the fixed percentage system may result in drastic reductions in the research program at the very time when research should be expanding instead of contracting. It is far better for the corporation if the research department is required to sell the management on every dollar of its appropriation on the basis of the probable benefits to be derived from the projects undertaken.

But while research cannot be bought by the pound or percentage, industrial corporations still have the right to expect it to pay its own way and much more. When it does not, it is probably not because the research department is too big or because it is spending too much, but rather because it has not been properly organized and directed to meet the specific needs of the company. In such cases, the trouble is not with the job which they are doing, but with the management policies under which they are operating. With intelligent management, good direction, and sound organization, industrial research is *sure* to pay off.

I have left the sixty-four dollar question to the last. Why must industrial organizations carry on long expensive research programs and spend millions of dollars for permanent research facilities? There are undoubtedly a good many reasons that I have not thought of, but, fundamentally, it is true that at no time in history was there any indication that man was satisfied with his lot. He has experimented with almost every phase of his experience and has continuously sought for improved transportation, housing, food production, distribution, social organization, defense, and health. This desire to achieve a closer approach to what man considers perfection in all fields of activity is so fundamental that we can almost conclude that it is a law of human behavior.

Viewed from this standpoint, research is good business, and by an organized effort in this direction we are making use of a fundamental law of human nature in satisfying the desires of individuals for constant progress. Competition certainly spurs industrial organizations into this activity. As an example, no one could compete in today's automobile market with a 1930 automobile, but competition itself is merely an exemplification of the way research works to gratify the human desire for better things.

Staying in business in a competitive economy means that research must be a business venture, and that as such it has to be done with good business judgment. The good business judgment demanded by research is like the business judgment that would be exercised with any other capital investment. Patience is required, because research cannot be turned on and off like a machine. Once having begun it, there is no turning back because you have committed yourself to progress and your customers will demand it. Tomorrow's customers will have grown to expect even more from all of us in industry, and if we wish their approval the output of our research efforts will have to be even better than before.

Research has sometimes been characterized as a gamble. In the sense that it is an investment in an enterprise with an unpredictable outcome, it *is* a gamble; but the fact that its success is dependent on the quality of its direction and execution makes it a game of skill rather than of chance. The great industrial might of this country has been built on gambles of this kind, and it is becoming increasingly apparent that the biggest and riskiest gamble which industrial corporations can take is to do no research at all.

Third Session

Merritt A. Williamson, *presiding*
Associate Director, Research and Development Department
Pullman Standard Car Manufacturing Company

WHAT IS NEEDED IN A RESEARCH EXECUTIVE

– I –

by
JOHN C. FLANAGAN
Director of Research, American Institute for Research

DURING WORLD WAR II a number of personnel psychologists were fortunate in being called on to assist in the solution of armed-forces problems on the classification, training, and efficient utilization of manpower. The urgency and scope of the problems and the large number of personnel involved provided an excellent opportunity to do decisive research on the effectiveness of many of the personnel procedures which had been developed in the period prior to the war. These tests of procedures confirmed some practices, such as the effectiveness of certain types of aptitude tests, and raised doubts about others.

Probably the most significant outcome of these studies was in the increased realization on the part of the research workers that the development of accurate job definitions must precede all other types of studies on personnel problems. If it is not known in precise behavior terms what an individual is supposed to be doing, including a description of the specific ways in which he may succeed or fail, it is impossible to evaluate any part of the personnel program concerned with this individual.

The underlining of this need led to the development of new and more precise techniques for defining job requirements. One of the new concepts growing out of these studies was that of "critical requirements." This was a substitute for the long lists of traits, duties, and requirements for various jobs which in many cases were more comprehensive than they were enlightening. It was proposed that techniques be developed for obtaining short lists of the really critical requirements. The critical requirements for an activity are those that are crucial in the sense that they have been frequently observed to make the difference between success and failure in that activity. An efficient technique for determining critical requirements has been developed. This technique, called the "critical incident technique," tends to substitute data for impressions and opinions. It provides a relatively precise and comprehensive definition of effectiveness on a job in terms of what people actually do on this job. The critical incidents are reports by qualified observers of things done that were especially effective or ineffective in accomplishing important parts of their job. The vague hunches, the stereotypes, and the poorly defined traits, such as character, imagination, and foresight, are replaced by reports of observed incidents which are detailed and specific.

To illustrate the technique and report some data for a related field, a study of officers in the armed forces will be summarized briefly. Several hundred officers were asked to report incidents they had observed in which something especially effective or ineffective was done. Two or three illustrations will indicate the type of data obtained.

The first example is an incident relative to making decisions. This is an ineffective incident about a deputy for operations:

> About two or three times a week he would come into my office and start the conversation by saying, "Say, Colonel, you have to make a dicision." He would tell me the problem on which he should have rendered a decision since he had all the facts and I didn't. He was afraid to make a decision that I would not approve. The final straw: A decision was needed about sending a ship out in bad weather—he knew the facts but didn't have the courage to decide. He came to me, and I told him to make the decision and notify me about it. He wrote a letter to all subordinates telling them that he had been on the carpet, and the

reason was because his subordinates had failed to do as they were told. This was untrue, and several of his subordinates complained to me about the letter, so I requested his relief.

The second incident regards the matter of taking prompt action. This is an ineffective incident about a squadron adjutant:

This officer was an adjutant in my squadron. My former adjutant was very energetic and handled most of the administrative functions with little or no supervision. This new officer started out and was given the same authority as my former one. The administration began to slow down, reports were going in late, so I began to check. I found that this officer was very thorough, so thorough that even the most ordinary function was slowed up waiting for his signature. One afternoon I overheard E.M. talking. The conversation was to the effect that they were through work at noon and here it was 3 P.M. and the passes were not signed yet. I checked with the adjutant and found that he had been too busy at noon to sign the passes but would get to it as soon as he finished a roster he was making up. I picked up the passes, signed them and got the men started on their way. I checked with some of the men in the unit and found that this delay had become a common occurrence. I then decided that this man was too slow for my unit and got rid of him on the next shipment.

TABLE I

Relative frequency in per cent of incidents reported for each item on the *Check List of Critical Requirements for Officer Evaluation,* comparing incidents reported concerning Colonels and Generals with those reported for all officers.

Item	Per Cent for All Officers (N = 2,907)	Per Cent for Col. and Gen. (N = 412)
I. Proficiency in Handling Administrative Details	6.9	3.6
1. Understanding instructions	0.2	0.0
2. Scheduling work	0.3	0.2
3. Getting information from records	0.1	0.0
4. Getting ideas from others	0.1	0.0
5. Checking accuracy of work	0.9	0.5
6. Writing letters and reports	0.8	0.5
7. Getting cooperation	2.0	1.5
8. Presenting finished work	0.5	0.2
9. Keeping records	1.1	0.2
10. Keeping others informed	0.5	0.2
11. Rendering effectiveness reports	0.1	0.2
11. Proficiency in Supervising Personnel	13.6	29.6
12. Matching personnel and jobs	1.0	0.7
13. Delegating authority	2.0	4.1
14. Giving orders and instructions	1.9	2.9
15. Insuring comprehension	0.2	0.2
16. Giving reasons and explanations	0.6	1.2
17. Supporting authorized action	0.6	2.2
18. Encouraging ideas	0.3	0.7
19. Developing team work	1.3	4.4
20. Setting a good example	1.8	4.1
21. Assisting subordinates in their work	0.6	1.7
22. Evaluating subordinate's work	0.6	2.2
23. Looking out for subordinate's welfare	2.0	4.9
24. Maintaining relations with subordinates	0.6	0.2
III. Proficiency in Planning and Directing Action	16.8	39.8
25. Taking responsibility	2.3	3.4
26. Solving problems	2.6	4.6
27. Making use of experience	1.9	8.3
28. Long-range planning	1.5	5.1
29. Taking prompt action	1.0	2.9
30. Suspending judgment	0.7	1.7
31. Making correct decisions	2.8	7.3
32. Making forceful efforts	2.8	2.4
33. Absorbing materials	1.2	3.2
IV. Acceptance of Organizational Responsibility	13.0	9.2
34. Complying with orders and directives	1.8	0.0
35. Accepting organizational procedure	2.7	0.7
36. Subordinating personal interests	4.2	3.4
37. Cooperating with associates	1.6	1.7
38. Showing loyalty	1.9	1.2
39. Taking responsibility	0.9	2.2
V. Acceptance of Personal Responsibility	35.3	11.7
40. Attending to duty	6.7	0.5
41. Attending to details	4.4	0.2
42. Reporting for appointments	0.4	0.0
43. Meeting commitments	0.5	0.0
44. Being fair and scrupulous	5.2	3.9
45. Maintaining military appearance	1.0	0.5
46. Adapting to associates	5.0	2.4
47. Adapting to the job	5.7	1.7
48. Conforming to civil standards	6.3	2.4
VI. Proficiency in Duty Military Occupational Specialty	14.4	6.1
49. Possessing fundamental training	3.5	1.7
50. Improving effectiveness	1.5	0.7
51. Keeping well informed in specialty	1.1	0.5
52. Applying training and information	4.5	1.0
53. Showing ingenuity in specialty	3.0	1.7
54. Handling related assignments	0.8	0.2
TOTAL FOR ALL AREAS	100.0	100.0

The last incident concerns effective behavior regarding forcefulness:

A directive was received from higher headquarters requesting certain detailed information that appeared on the surface to be impossible to obtain in the limited time allowed. In the discussion on the matter, various ideas came up such as reporting certain phases of the required information as unknown, requesting higher headquarters to extend the reporting date, etc. This officer spoke up and said, "This is a directive. We should be getting the information instead of discussing alternatives; give me the job, and I'll get it." He was given the job as requested and immediately formed a plan of attack and expedited it. Information had to be obtained from over 100 locations all over the United States. He formed a unit to bring the information in and another to compile it in the required phases. The deadline was met on the report due to this officer's initiative and force in handling a seemingly impossible problem. This officer at the time was not assigned to specific duties, having just reported for duty. I forcibly requested this officer and he is working for me now.

On the basis of nearly three thousand such descriptions of outstanding and unsatisfactory job performance, a *Check List of Critical Requirements for Officer Evaluation* was prepared. For each of the fifty-four critical requirements, the proportion of the incidents obtained from the total group of reporting officers is shown in the first column in Table I. The third column shows the corresponding proportions of incidents concerning Colonels and Generals only. The table shows that about 30 per cent of the incidents for the senior officers relate to proficiency in supervising personnel, and an even larger proportion, about 40 per cent, has to do with proficiency in planning, initiating, and directing action. Practically no incidents describing either outstandingly effective or ineffective behavior for these officers are found in the area relating to proficiency in handling administrative details. A few incidents were reported regarding the senior officers' fundamental training and ingenuity in their specialties. The remaining incidents dealt with more personal aspects of their behavior, such as subordinating personal interests, cooperating with associates, taking responsibility for subordinates, being fair and scrupulous, adapting to associates, adapting to the job, and conforming to civil standards.

In the general area of supervising personnel, the points which are most frequently observed for senior officers as compared with junior officers are: developing team work and looking out for subordinates' welfare. In the area of planning, initiating and directing action, solving problems, making effective use of experience, long-range planning, and making correct decisions are the types of critical incidents which are generally responsible for observed effective or ineffective behavior on the part of senior officers.

Turning more specifically to the problem of the research executive, limited data are available from a study of research personnel carried out by the American Institute for Research under the sponsorship of the Office of Naval Research. In this study, critical incidents were obtained from five hundred scientists in twenty research laboratories. About twenty-five hundred incidents were reported by supervisors describing something that a research worker did which was especially effective or ineffective in getting his job done. In the detailed list of observed critical behaviors regarding research personnel, the two types of incidents which for research workers classified in the top two professional grades of Federal Civil Service employees are observed as critical more frequently than for those in lower grades have to do with Area I, Formulating Problems and Hypotheses, and Area VI, Administering Research Projects. The data concerning these incidents is reported in Table II.

Within the first area, the critical behaviors observed

TABLE II.

Relative frequency in per cent of incidents reported for each item on the "Observational Record Form for Research Personnel," comparing the incidents reported concerning research workers classified in the two top professional grades with those reported for all research workers in two of the middle grades.

ITEM		% for middle levels of res. workers (N=641)	% for 2 highest levels of res. workers (N=115)
I.	Formulating Problems and Hypotheses	5.0	6.9
A.	Identifying and exploring problems	1.4	2.6
B.	Defining the problem	2.0	2.6
C.	Setting up hypotheses	1.6	1.7
II.	Planning, Designing the Investigation	15.6	11.3
A.	Collecting background information	5.0	5.2
B.	Setting up assumptions	.6	.9
C.	Identifying & controlling important variables	2.0	2.6
D.	Developing systematic & inclusive plans	3.1	1.7
E.	Developing plans for the use of equipment, materials, or techniques	3.1	.9
F.	Anticipating difficulties	1.6	.0
G.	Determining the number of observations	.2	.0
III.	Conducting the Investigation	18.2	20.8
A.	Developing methods, materials, or equip.	7.5	11.3
B.	Applying methods and techniques	3.0	2.6
C.	Modifying planned procedures	1.4	.9
D.	Applying theory	3.1	3.5
E.	Attending to and checking details	3.1	1.7
F.	Analyzing the data	.3	.9
IV.	Interpreting Research Results	5.3	3.5
A.	Evaluating findings	3.7	3.5
B.	Pointing out implications of data	1.6	.0
V.	Preparing Reports	9.2	5.2
A.	Describing and illustrating work	2.5	.0
B.	Substantiating procedures and findings	3.4	1.7
C.	Organizing the report	3.0	2.6
D.	Using appropriate style in presenting report	.3	.9
VI.	Administering Research Projects	27.0	33.9
A.	Selecting and training personnel	5.9	6.1
B.	Dealing with subordinates	9.5	10.4
C.	Planning and coordinating the work of groups	6.2	8.7
D.	Making administrative decisions	2.7	4.3
E.	Working with other groups	2.7	4.3
VII.	Accepting Organizational Responsibility	9.2	9.6
A.	Performing own work	3.0	3.5
B.	Assisting in the work of others	3.0	4.3
C.	Subordinating personal interests	1.7	.9
D.	Accepting regulations and supervision	1.6	.9
VIII.	Accepting Personal Responsibility	10.3	8.7
A.	Adapting to associates	1.2	1.7
B.	Adapting to job demands	3.9	2.6
C.	Meeting personal commitments	.5	.9
D.	Being fair and ethical	3.4	.9
E.	Showing interest in work	1.2	2.6
TOTAL FOR ALL AREAS		100.0	100.0

regarding the research executives included identifying and exploring problems, defining the problem, and setting up hypotheses, but the behavior most distinctive of the research executive as compared with his subordinates was identifying and exploring problems. In the area concerned with administering research projects, all the subcategories appear to be important factors in evaluating the effectiveness of the research executive. These included: selecting and training personnel, dealing with subordinates, planning and coordinating the work of groups, making administrative decisions, and working with other groups. These types of behavior are also critical for research personnel on the next lower levels. The administrative subcategory in which responsibility appears to be most frequently shared with the next lower level of research supervision is that of selecting and training personnel.

A few examples illustrating the behavior of research executives are given below. The first incident describes ineffective behavior on the part of a research administrator:

> An administrator was informed that one of his subordinates, a section supervisor, had recommended for promotion a laboratory worker who was notoriously lazy and incompetent. Without obtaining the complete story or checking the facts, the administrator summoned his subordinate and began administering a severe reprimand. During the course of this reprimand, it developed the complaint was based on misinformation and that the subordinate supervisor was completely innocent. Thereupon, the administrator merely stated he would look into the matter further. Several weeks have passed, and the incident is apparently considered closed, and the subordinate was left dangling in a justifiably offended frame of mind.

The second incident illustrates ineffective behavior on the part of a supervisor in one of the lower grades of research supervision. Data of this type should be valuable in deciding whom to promote to higher supervisory positions:

> This supervisor refused to accept the recommendation made by a shop foreman to simplify and improve the design of a new supporting bracket. The supervisor had assigned the project to a subordinate who was inexperienced in mechanical design and did little research prior to issuing the plan. The supervisor did not spend any time assisting his subordinate or laying down any general requirements but gave him the job to do completely. The foreman's idea was approved by the supervisor's superior but discarded by the supervisor because he felt the "production department had no business telling him what to do." This inflexible attitude kept production forces away from him, wasted time and effort, and produced a poorer product.

The last example illustrates effective behavior on the part of an administrator having to do with supervising personnel:

> A division head learned indirectly of the success of one of his subordinates in obtaining qualifications under a Civil Service Examination after a long period of delay, correspondence, and appeal. Recognizing the importance of this event to the subordinate concerned, he immediately contacted the man and invited him to his office for interview. Upon arrival, the man was warmly congratulated, given assistance and suggestions in connection with the final routines necessary to certification, and then complimented upon the work he was doing. Discussion of the latter followed and resulted in beneficial suggestions for pursuance of the problem. In so doing, the division head gratified his subordinate and made him feel that his place in the organization was important, and inspired him to a greater degree of effort and initiative. Thus, the over-all progress and effectiveness of both the individual and the institution were measurably enhanced at the expenditure of only a few minutes of the executive's time.

Because of the differences in the categories used in tabulating the data regarding military executives and research executives, a precise comparison is not possible. Certain comparisons which can be made do throw some light on these positions. In both instances, it appears that effectiveness of performance with respect to the type of administrative detail involved in staff work, such as checking, record-keeping, getting information, and keeping others informed are not reported as critical behaviors for either of these types of executives. Proficiency in administration in the sense of office detail does not appear to be a critical requirement for these types of executives.

In both groups, the general area designated "Supervising Personnel" or "Dealing with Subordinates" is a substantial critical factor. Judging from the proportion of critical incidents reported, it is not quite as important for the research executive as for the military executive. Proficiency in planning, initiating, and directing action include the largest proportion of the critical incidents reported regarding both types of executives. The major difference in the requirements of these two groups in this general area is indicated by a larger proportion of incidents for the research executive dealing with getting new ideas or using imagination in formulating a problem, plan, or program. Fewer incidents are reported regarding fairness and ethics for the research executive. The final comparison is in regard to the area designated "Accepting Organizational Responsibility." The proportion of critical incidents concerning matters of fitting into the work plans of others is slightly larger for the research executive. It should be emphasized that the relative frequency of critical incidents of either an effective or ineffective nature is not an indication of superiority or inferiority of one group over the other in the comparisons discussed above. It is merely an indication of the extent to which the problems and activities in a particular area lead to critical behaviors as observed by the reporting research workers.

The foregoing analyses should be regarded in the nature of a pilot study with respect to requirements for executive personnel. Although several thousand incidents were collected in each instance, the number relating to individuals in high-level executive positions was only something more than a hundred in each case. For an adequate study, it would be desirable to collect a few thousand incidents.

The basis of this new approach to job and personnel study is the desire to substitute relatively objective data for opinions and hunches. In dealing with the behavior of research executives or any other groups of human beings, it has been found very difficult to measure with anything like the precision of the physical sciences. Usually we must fall back on counting. As a rough approximation, this works out quite well if we take care in collecting and classifying the items which we later count.

In establishing the critical requirements in terms of actual job behavior for the research executive, five specific conditions must be satisfied if valid results are to be obtained. These are as follows:

1. It is essential that actual observations be made of the on-the-job activity and the product of such activity.

2. The aims and objectives of the activity must be known to the observer. Unless this condition is fulfilled, it will be impossible for the observer or judge to identify success or failure. For example, a research director might be rated as very successful if the objective of his activity were taken as getting along well with the workmen under him. At the same time, he might be rated as very unsatisfactory if the objective were the quality and quantity of research accomplished.

3. The basis for the specific judgments to be made by the observer must be clearly defined. The data can be objective only if all observers are following the same rules. All observers must have the same criteria for making judgments. For example, the definition must clearly state whether or not a minor imperfection will be regarded as evidence of failure or whether the results must be completely unusable to be classified as unsatisfactory.

4. The observer must be qualified to make judgments regarding the actvity observed. Typically, the supervisor on the job is in a much better position to make judgment as to whether behavior is outstanding or unsatisfactory than is the job analyst or psychologist. On the other hand, the supervisor on the job is ordinarily lacking in the training essential to make an inference as to the particular mental trait which caused the behavior to be successful or unsuccessful.

5. The last necessary condition is that reporting be accurate. The principal problems here are those of memory and communication. It is also important that the observer's attention be directed to the essential aspects of the behavior being observed.

Even with a very careful formulation of our procedures and the use of large samples, the problems involved in studying human behavior are great, and our results must usually be reported in terms of probabilities which are very far from certainties. Recent studies have demonstrated that improved techniques such as the "critical incident technique" can provide data of verifiable predictive value. The day of expert opinion regarding problems of selection and classification, training needs, job requirements, the evaluation of performance, and job design is coming to a close. Research of the type which enables the personnel psychologist to stand on the shoulders of the preceding contributors in this field is taking over. Much work needs to be done, but we can predict with confidence that in the next few years it will become possible to read a paper stating clearly, and with known precision, the needs of the research executive.

DISCUSSION OF DR. FLANAGAN'S PAPER

DR. FURNAS: I would like to compliment Dr. Flanagan on the pioneer work which they are doing. At last I see some element of objectivity entering into the personnel problems.

I would like to ask: What is the next step? What are you going to do with this information after you have it? Can your findings be directly translated into some effective system for merit rating?

DR. FLANAGAN: Yes, we think so. We regard this as a first step. As a definition of what is required for success on the job, in terms of behavior, it seems to be a first step, and we believe that work on selection, work on training, and work on evaluation of success on the job comes after that.

We have, for example, prepared the officer-effectiveness report that some of you have. This was prepared for the U.S. Air Force and is being used with slight revisions and some additions in the Air Force at the present time.

This study also forms the basis for a new form being put into use in the Marine Corps. The Marine Corps, after a general study of a lot of procedures, decided that this approach was best. They made some changes. For example, they cut the number of items down to twenty-seven from more than fifty in the original form.

We are now working on a project to survey experience in the Air Force and are trying to improve the present form, so that it will be even more effective.

We are also working on the general problem of merit rating of hourly-wage workers with one of the large automobile manufacturers.

We have done some preliminary research on developing a merit-rating procedure in connection with studies of research personnel for the Office of Naval Research.

This study was conceived as a five-year program, and we are now in the third year. We have completed the first step, determining the critical job requirements, and the second step, developing a selection test. We have also done quite a bit of work on the problem of developing a merit-rating procedure for research personnel. We are now working on two things: the development of a proficiency standard to measure skills and the readiness of the person to accomplish independent research. This study does not aim to evaluate a person's aptitude to learn to do research but rather his present ability to do it. Besides, we are trying to develop more objective procedures for evaluating products, specifically research reports. There are a number of unsolved problems. We have established that general impressions are practically worthless in most situations for merit rating and that some method has to be developed for getting records of observations of actual behaviors.

How long people can remember observations and report them with accuracy, we don't know. In working with foremen in industry, we found the foremen lost three-fourths of the incidents at the end of two weeks, as compared with what they would have had if they had reported them each morning. They put a check mark in the correct space on the man's record each morning to indicate things they had seen the previous day. With some twenty-five workers they usually had three or four incidents to check each morning. Just what the right period of time is for making observations for research personnel, we don't know.

DR. FURNAS: Do you consider that the merit-rating forms of the usual kind, which are more or less helter-skelter, are useless? Also, we apparently will have to wait a number of years before the world's best rating system is available. Is that correct?

DR. FLANAGAN: I would not like to put a date on it, and I would hesitate to say anything was the world's best.

DR. FURNAS: Putting it another way, considering the time for the culmination of your work, it will be at least a couple of years before you will have a recommended rating system.

DR. FLANAGAN: We hope to be able to submit a report to the Naval Research personnel in a couple of years and say: "This is the system we think you ought to put in."

When we tell them to put it in we will probably also tell them that it has shortcomings and that further research is desirable. In the end we hope to have something which is practical and also follows sound principles, but we haven't got there yet in the research-personnel field.

DR. FURNAS: Is my first impression correct that you consider the present or available rating systems to be essentially useless? Perhaps that question is unfair.

DR. FLANAGAN: We would not use them in our organization.

MR. DOW: Dr. Flanagan just passed over very briefly the question of aptitudes. The performance of any individual in a given situation is probably a function of both his inheritance and his developmental personality.

The field of developmental personality is going to be a very complex field to work out in this approach. It is an exceedingly interesting one, but the field of inherited aptitudes may be a little simpler and more immediately useful. It would not be the final answer, because a man may have the ability and still not perform up to his ability because of personality restrictions. You mentioned that during the war you have done some work on aptitudes, which in my mind are largely inherited, and I wonder if it would be possible to go a little further into that.

DR. FLANAGAN: I might just say that that work has been reported. We wrote nineteen volumes, which the Government Printing Office put out at the end of the war. The volumes run from 200 to 1000 pages each.

But to summarize very briefly, we can say that we did achieve success: firmly established, verifiable success in predicting success in pilot, navigator, and bombardier training, and also more limited success in predicting combat results.

It is very hard to find out whether a person is successful in combat, and we did not get started on the problem until late. However, we did considerable follow-up work to check on the tests. In pilot training, for example, we took in a special experimental sample of one thousand people that came to Aviation Cadet Boards and asked to be trained as pilots. This study was hard to sell since it cost several million dollars. We took these applicants no matter what they did on their aptitude test, provided they were physically and otherwise qualified and had not been in prison. There was self-selection in that they came in to apply, but the boys that came in were given the test and, no matter what they did on the tests, they were sent on to pilot training.

It was not a bad group; nor was it a good group; just a random selection from boards all over the country: one thousand people who said they wanted to be pilots. After taking the first screening examination, they were given a battery of twenty tests; they were assigned numbers, as were all applicants, from one (lowest aptitude) to nine (highest aptitude).

Because of the failure to use the first screening, there were as many as one hundred and fifty with the lowest aptitude (score of one).

To report very briefly: out of the one hundred and fifty Number 1's there was not a single one left by the time they got ready to graduate from advance training. Of the 2's and 3's there were sixteen or seventeen who graduated. Of the 8's and 9's there were very few, only sixteen or seventeen, who failed.

To improve our procedures we made a special follow-up on those that failed or succeeded contrary to our expectations. In general, the correlation (the predictive accuracy) was about .70. This was fairly close to the maximum we could get with the criteria of success in pilot training that we had.

Further work is being done now. For example, modifications are being made in procedures and equipment to select jet pilots.

Very little information was available at the beginning of the war on what was necessary for success or failure as a pilot. Vocabulary tests were no criterion of probable success. Ability to do simple addition showed a little consistent, negative correlation with ability to assimilate pilot training. But we did find that certain coordination tests, reaction-time tests, mechanical, spatial, and visual tests predict quite accurately.

COL. SEILER: I am somewhat interested in the new rating forms for officers, because, as I recall, the criticism of the old form was that we had too many people ending up with the same rating; in other words, we did not have enough spread of the rating of the people to get a good distribution of their effectiveness.

In reviewing the new rating forms on the subordinate officers, I find that this form, for a small segment of officers, also tends to end in ratings that are very close to each other, but I was wondering whether you had an opportunity to examine a large number of these, to see whether this effectiveness report has attained in some way a better effectiveness rating of the military personnel.

DR. FLANAGAN: "Better" either implies a criterion, something to compare it with, or some sort of logical analysis or judgement factor, and I do not believe that we have any such criterion.

I would say that the officers in the field try-out, about two thousand officers, liked it better than other forms they had used. I think that is still true in general.

In the recent Marine Corps try-out, in which four thousand officers used a number of different forms, the new form was preferred as giving a fairer, more valid estimate, in spite of the fact that it entailed a little more work.

This does not prove that it is better. There are many difficulties in obtaining valid effectiveness reports, and we have just made a survey of what difficulties are being encountered with the Air Force officer report used at the present time.

I do not want to go into a lengthy discussion, but it is being misused in a lot of instances. For example, it has been used to determine whether the person gets a promotion to the next grade. Rumors have gotten around that you have to have a 4.0 rating to be promoted and that certain commands are rating higher than others. This affects the ratings. For example, one rater said: "I went through and marked this fellow just the way he ought to be marked, then I looked at it, and I recognized the fact that I am traditionally a low rater, so I went through and changed all my marks, so as to raise him one point."

Now, if you have that sort of thing being done, administrative actions based on just this one report, you are not going to get satisfactory results. We are trying to work out a procedure which will have the officer make some of his judgments about administrative actions independently of whatever the description of actual performance on the part of this officer is.

If anybody has any other ideas for improvement, I would like to have them.

In one command, the Air Materiel Command, they required their officers for a period of time to make observations frequently and to keep records. The procedure had one thing lacking, which we have put in our other procedures. The officers were supposed to observe and report all actions, whether they were mediocre, poor, very good, and so forth.

Recently, while working on some procedures, we said, "Let's not record everything, let's report only the critical behaviors that are especially effective or especially ineffective."

This saves a great deal of observational time. Just exactly how this can be made into a system that the busy superior will be able to use, we don't know as yet, but we are trying out some new approaches.

— II —

by

ALBERT A. LOMBARD, JR.

Scientific Assistant, Directorate of Research and Development
Headquarters, United States Air Force

Introduction

I am pleased to participate in this program on the discussion of "What is Needed in a Research Executive." In view of my connection with the Air Force my talk will be slanted toward Air Force research, recognizing that many of the same factors also apply to research in other Government departments and to some extent to non-Government research as well.

Why Air Force Research?

First of all, it is essential that the Air Force research executive understand the role of the Government, and of the Air Force particularly, in the research field. There are some people who would have the Government take over everything. However, our nation has been built up on the basis of private initiative and resourcefulness with incentives and rewards as results of the effectiveness of private enterprise. We want this to continue in the research field, as in others, to the greatest possible degree. Many types of research have been and are being supported completely by private capital.

Industries are supporting research to an increasing degree today, as is evidenced by the interest in attendance at this conference. As several speakers have indicated, industrial research is primarily pointed toward the development of new products or production methods of value to the individual company, although many companies do sponsor some basic research as well. In addition, considerable basic research is being carried on in universities and other research establishments, supported largely by direct grants and contributions from philanthropic persons and organizations who expect no direct personal return but are interested in the conduct of this research because of its obvious benefits to mankind. This research and its support must continue by all means; it provides the broadest base for scientific inquiry with the greatest possible freedom to the individual.

With the substantial funds for research available from the public treasuries today, the Government research executive must have a keen sense of balance to avoid the pitfall of putting Federal Government funds unnecessarily in competition with other funds in research, which might unduly influence the trend of research and might drive private enterprise out of profitable and fruitful fields.

It is, therefore, the policy of the Air Force to perform or sponsor necessary studies and experimentation to insure the timely improvements in concepts, techniques, and materiel of promise to the Air Force mission, taking fullest advantage of research being conducted outside the Air Force. An important responsibility of the research executive is to see that this policy is carried out in practice.

Air Force Research and Development Program

Administration of Air Force research is a technical job. The research executive must be technically trained in order to determine what projects will be undertaken and to judge the competence of individuals or organizations conducting the work. In order to explain these technical requirements in some detail, I would like to discuss the various phases of the Air Force research and developmental program in terms of its broad objectives.

Determine Most Economical Program

Background research is being conducted on a broad scale today to determine the most economical program for the development of weapons and weapons systems of maximum effectiveness with a minimum cost. Important studies in this field are being performed by the RAND Corporation, a nonprofit corporation operating under a contract, to investigate and improve methods and techniques in the conduct of intercontinental warfare. These studies consider the performance, reliability, vulnerability, and logistic factors pertaining to the weapons and to the weapons systems as a whole. In such an analysis as this, the evaluation of a new development in relation to a time scale is necessarily very important. For example, we must predict with reasonable accuracy the expected time at which new heat-resistant materials will be available for incorporation in engines, which in turn will require further predictions as to when these engines will be available for aircraft. The engine development with its substantial influence on speed of aircraft will thereby necessarily have a substantial influence on the armament development program and on the percentage of effort which should be devoted to new armament.

The research executive must therefore not only evaluate the fields of promise but must, to the fullest extent possible, predict the time table on which basic items can ultimately result in improved weapons systems.

Because we have weapons systems that are continually changing or subject to change, the Air Force is sponsoring basic research in many areas of promise to the Air Force. Necessarily, these areas are very broad, including all branches of science. The Office of Naval Research is sponsoring research in physical sciences. Other Air Force activities are dealing with other sciences. While many research executives are devoting their full time to one or more of these sciences, there is also a need for individuals of broad appreciation who can recognize, evaluate, and coordinate this research across the board, looking always for greater effectiveness and greater economy.

Rapid and Efficient Transport

A large part of the Air Force mission involves the rapid and efficient transport of bombs, ammunition, personnel, and cargo. Two fundamental military characteristics of great importance are speed and range. Aviation has grown from the development of these two characteristics. Speed is essential for effective penetration and survival of aerial vehicles and, in many cases, for the accomplishment of a mission within a limited time scale. Range is likewise important.

Increased performance in both these areas depends primarily upon research in aerodynamics and propulsion.

Fundamental research in aerodynamics and propulsion is being financed directly by the Air Force. In addition, a substantial and significant contribution is made by the National Advisory Committee for Aeronautics, an independent Government agency which is devoting its primary attention to these two fields. I am glad to say that there is close coordination between the administration of Air Force research and that of the NACA. The Air Force is represented on the NACA committees and subcommittees, and, in turn, the NACA is represented on the Air Force Scientific Advisory Board. Thus, you will see that a substantial phase of the job of an Air Force research executive is one of *coordination,* of keeping the Air Force program from duplicating that of the NACA in the development of rapid and efficient transport.

Locate Targets and Recognize Them

Aviation has long since passed the time when mere ability to get off the ground has significance. Many phases of military operations require locating targets and recognizing them.

In many of these instances long-distance navigation becomes an essential factor, guided by celestial observations, dead reckoning using inertial systems, Loran systems, radar, or a combination of these. In order to simplify the work of the air crews and to make guided missiles possible, projects are under way to make these systems automatic to the fullest extent.

Also, the target must be recognized. Visual methods of recognition are hampered by weather conditions and by the altitudes and speeds at which airplanes are now being operated. A substantial effort is therefore being devoted toward improved radar techniques. This objective of locating targets and recognizing them necessarily brings the research executive again into contact with many problems of present-day physics, electronics, mechanics, and mathematics.

Hit Target Accurately

Once the target is located and recognized, weapon effectiveness is measured directly in ability to hit the target accurately. The research executive must see to it that all promising avenues are explored which improve the accuracy of hitting the target. For each per cent increase in efficiency here, there can be a corresponding reduction in materials, personnel, and dollars to accomplish a given mission.

Ability to Cause Destruction

Also, we need to maximize destruction. Through research and development we must improve bombs, warheads, and fuzes, which involve the whole range of developments of explosives, propellants, and the like. It is interesting and somewhat gratifying to see how explosives research is tying in with the propulsion fields. Both these phenomena involve chemical reactions. The only difference is that explosions are often more rapid than propulsion phenomena. A whole new field of chemistry is opening up to understand better the kinetics of chemical reactions, to understand why some reactions are fast and others slow, why heat, cold, and catalysts accelerate or retard reactions. Our research executive must, therefore, appreciate the problems of chemistry.

Function Independently of Weather and Darkness

Research and development is pointed toward the requirement that the Air Force be able to function completely independent of weather and darkness, to take off and land in a continuous procession, using a relatively narrow flight strip in conditions of poor weather and visibility, as well as operate under all weather conditions. In addition, we want to take advantage of weather, as, for example, using winds to increase the range of aircraft, clouds for cover, and bad weather to reduce enemy fighter opposition. The Air Force Cambridge Research Laboratories, in addition to doing electronic research, are therefore pursuing a geophysical research program aimed at a better understanding of the weather and predicting future meteorlogical conditions as well as determining what effort is required to operate independently of the weather. This activity increases the fields of interest of the research executive.

Defeat Enemy Interference

We must defeat enemy interference to insure that our airborne vehicles can perform their assigned missions. In the active phase of this program, long-range-penetration fighters are being developed to strike at the source of enemy resistance, with new promising developments to increase the effective range of these fighters. The passive phase of defeating enemy interference can be accomplished through superior performance of aircraft, confusion of defenses, evasive tactics, countermeasures, and bomber-defensive armament. This requires considerable attention by the Air Force research executive to aerodynamics, propulsion, electronics, armament, materials, etc.

COMMUNICATION FROM GROUND-TO-AIR AND AIR-TO-AIR

Utilization of aircraft for long-range bombing and reconnaissance, for defense, for short-range tactical offense, and for intercontinental transport require distinct types of communication equipment from ground-to-air and air-to-air. Here attention is being given not only to the power and frequency of equipment but also to the provision for multiple channels, automatic control, and miniaturization to conserve space and weight. The principal requirement of this development falls on the electronic research executive, but there are also human aspects when it comes to making communications intelligible.

DEFEND HOME TERRITORY

The possession of nuclear weapons by a potential enemy increases the necessity for defense of home territory, which likewise involves research in aerodynamics, electronics, propulsion, and armament. The existence of high-speed aircraft reduces the time available to the defensive forces and emphasizes the need for superior performance in interceptors, superior performance of the detection equipment, and reduced time to translate the warning of an enemy approach into the direction of the interceptor aircraft. This is a challenging problem in systems development.

EFFECTIVE UTILIZATION OF PERSONNEL

Until now we have discussed primarily the Air Force research and development program in relation to physical equipment. Equally important is the research program in human resources to increase the utilization of personnel by the best possible selection, technical training, and job engineering. The research executive must be familiar with and understand the techniques and results of this research program in human resources.

Conduct of Research

Having discussed the types of research in which the Air Force is interested and with which the research executive must be familiar, I would like to discuss the manner in which the Air Force conducts its research. While in many organizations research is conducted almost completely within the organization itself, Air Force research is, to a very large extent, done under contracts with universities, research institutes, industrial concerns, and through other governmental agencies, such as the Bureau of Standards. The predominant characteristic of the research executive must therefore be a great receptivity to new ideas regardless of origin, and a humble desire to aid and encourage the research organizations and workers themselves. In this way, he can make certain that public funds are wisely spent to bring results of value to the Air Force. A good administrator is therefore much more valuable as an executive than a clever technician or inventive genius. Furthermore, the research executive must be a good salesman, not in the high-pressure sense but rather in the true sense of what constitutes salesmanship. He must know his product – research – and the capabilities of his organization to undertake research and to produce. He must see the applications of his products to the solution of useful problems of his sponsor, and he must be able to present through reports, talks, and other sales techniques the ideas which he desires to put across.

Facilities

It is an Air Force policy to use existing facilities, wherever situated, to the fullest extent rather than go out and build additional facilities. In many cases, however, new fields of research do require new facilities, and the costs of these modern research facilities are so great and so specialized that private capital is neither available nor disposed to make the necessary investments. In these cases, the Government necessarily must construct research facilities. Examples of such facilities are the following:

1) *Wind Tunnels.* A few years ago very acceptable wind tunnels could be built for a few hundred thousand dollars. They were usually built with private funds and located either at industrial plants or at colleges and universities. However, today with the great emphasis on transonic and supersonic flight, very much larger and more expensive facilities are required. Under the unitary plan, Congress has given the NACA $75,000,000 for new supersonic tunnels and has authorized the Air Force to construct the Arnold Development Center, at a cost of $100,000,000, which will embrace three major facilities:

a) A high-altitude engine test facility for testing new power plants.

b) A 16-foot propulsion wind tunnel for full-scale testing at supersonic speeds under simulated altitude conditions.

c) A 40-inch hypersonic wind tunnel operating up to speeds of approximately Mach 10. Necessarily the location of the site for this facility was determined to a large extent on considerations of electric power. Consequently, Tullahoma, Tennessee, was chosen among the possible sites.

The cost of facilities such as the above-mentioned places a significant responsibilty on the research executive, for he must determine that they are needed and properly utilized.

2. *Flight Test Range for the Airplane and Guided Missile.* An important part of the research and develop-

ment program involves actual flight testing of the finished experimental vehicle to determine its performance, behavior, and capabilities. For this purpose, the Air Force has established a large flight test base known as Edwards Air Force Base, at Muroc, California, on the Mojave Desert, where many natural runways and landing areas abound. The Navy, as well as the NACA, uses this facility for aircraft testing.

For flight testing of missiles, the military departments operate missile test ranges at Pt. Mugu, California, and White Sands, Almagordo, New Mexico. The Air Force is developing an additional long-range proving ground under the joint cooperation of the United States and Bahamanian governments, in which missiles will be fired from Banana River, Florida, southeasterly, along the chain of the Bahamas. This location was chosen because of the geography of the area and the suitability of intermediate siting stations. Thus, the facility picture broadens our research executive still further as many specialized facilities are involved in the program.

In all these facility matters, the fluidity of the tactical developments is a very important factor. Besides, the facilities must be completely modern. It is rather obvious that a subsonic wind tunnel would be extremely limited in predicting the performance of a supersonic airplane. It is not always quite so obvious that other research and development facilities must likewise be kept in step with new technical developments. Often the facilities will be the key to a new development. Moreover, the facility must be completed before research can be undertaken. The research executive must, therefore, be continuously alert to these new developments in order that the facilities can be initiated in time to become available when needed.

Conclusion

In conclusion, therefore, the research executive, whether an officer or civilian, must be a person with the following qualifications:

1) He must understand why the Air Force is conducting research and under what conditions;

2) he must be familiar with the over-all mission of the Air Force and able to translate that mission into all fields of science, and, in turn, translate scientific capabilities back into military ones;

3) he must be able to see research in a clear enough perspective to predict and evaluate its rate of development on an adequate time scale in order to keep all elements in step, shift emphasis from one area to another, and plan facilities as needed;

4) he must be a leader, receptive to new ideas regardless of their origin, who aids and encourages research, but does not dictate it; and

5) he has to be a salesman, who knows his product and the capabilities of his organization, sees application to solution of problems of his parent organization, and presents his ideas convincingly.

DISCUSSION OF DR. LOMBARD'S PAPER

COL. SEILER: The comments of the last two days indicate to me that industry is operating research on a fairly short-time basis, that is, if the project does not seem to indicate a possible payoff in two or three years, there is small probability that it might be undertaken. I was wondering whether Dr. Lombard would care to comment on the time range of research for the Air Force.

DR. LOMBARD: Well, our time scale is much longer than that of some industries. It seems that any new development in the physical sciences, in fact, in any of the sciences, can have a direct application to Air Force activity. That is a very broad statement. It has been our observation that research has enormous fields of application to the Air Force, and our time scale is not only the solution of immediate problems on a two- or three- or four-year basis (and those are definitely the fire-drill program problems), but it is at the same time looking forward to research that should be conducted and will be conducted on a much longer time scale.

The Arnold Engineering Development Center, just to take one concrete example, has the facilities to do research in hypersonic aerodynamics, among other things. We visualize that the results of this research will be of great value in 1960. We are looking at the facilities picture in terms of the kind of research we will be doing in 1960, which means that we should start to process the facility expansions now, if we are going to have the facilities fully operating by that time, or shortly before that time. I would say, therefore, that at least elements of the Air Force research program are aimed at very long-term objectives.

Even the problem of developing an airplane itself, is a five- or eight-year project, and we are looking for research that will lead to new developments far away in the future.

DR. KLOPSTEG: I wonder if Dr. Lombard has a comment on the "research mindedness" of Congress, considering the fact that the committee on appropriations cut out the $475,000 proposed for the National Science Foundation.

DR. LOMBARD: I think Dr. Killian is giving a presentation of the National Science Foundation this afternoon. I will let Dr. Killian answer that for the Department of Defense. I have no comment on that.

I do have one comment, even though it may be trespassing on this afternoon's conversation on the National Science Foundation. It is our concept that the National Science Foundation will not represent a monopoly of Government research.

The NSF will attack certain phases of the problem, but we still visualize a very substantial usefulness for Air Force research, for Navy research, Army research, as well as for private research, and research in the NACA, and other Government agencies, even with and after the full operation of the National Science Foundation.

DR. SEEGER: May I add a footnote: Government research, particularly military research, is often distinctive in that Government must support long-range basic research in certain fields in which the law of supply and demand may be inoperative for certain stagnant periods, e.g., ordnance research and development, including aeroballistics, high explosives, etc.

Incidentally, an additional question is not just what we are to expect of research executives, but rather where we are to get persons for this type of work.

MR. MORSE: I am interested to know: (1) to what extent is the Air Corps going to attempt to centralize any of their research activities and engineering facilities; (2) is the Office of Air Research really going to exist or not?

DR. LOMBARD: Colonel Seiler is the chief of the Office of Air Research, and it is his intention, and it is mine, and it is that of many of the higher Air Force officers that the Office of Air Research will exist and become an important agency for basic research.

In answer to your other question as to centralization of facilities, I believe that the tendency is going to be in the opposite direction. The tendency in the future will be to locate research facilities where the predominant resources are that are essential for the operation of that facility.

At the AEDC the predominant requirement is electric power, and consequently it was built in Tennessee. In the case of electronics research, the predominant requirement is not electric power but manpower skilled in the fields of electronics, guidance mechanisms, and the like. There is a very substantial resource in that field in the Cambridge, Massachusetts, area. That is why the Cambridge Research Laboratory is located there.

Other research activities are being located in other areas where there is a predominant characteristic which makes it favorable to the particular kind of research.

We have research going on in Alaska and on Mt. Washington in New Hampshire because the geophysical and climatic conditions are there. We also have a climatic hangar in Florida, where we can get down minus sixty-five degrees, even in Florida.

A VOICE: One approach in technological research is the cost of the production of airplanes. Have you any information as to the cost of research and development as related to the cost of airplane instrumentation and things of that nature?

DR. LOMBARD: Let me see if I understand. We know what our research and development appropriations are. They are approximately a couple hundred million dollars a year—that is, the entire research and development appropriation, including the building of prototype aircrafts and all the equipment that goes into them.

The production appropriations are one or two billion dollars a year, so that the research and development budget is of the order of ten to twenty per cent of the overall "hardware" program of the Air Force.

In view of the conversation we have already had on definitions, I am reluctant to state how much basic research we do in the Air Force. We do consider it to be something of the order of twenty million dollars, depending on how you define it.

MR. MAIER: With your permission, Dr. Lombard, I think I can answer that question. On an engine development, it normally takes a period of eight years. One specific engine that I know about cost fifty-two million dollars to develop, and the price of the engine in mass production runs somewhere around eighty thousand dollars apiece. That will give you a time scale, as well as the cost. That does not include, however, the cost of special facilities for producing the engine that had to be supplied.

DR. HOLLAND: One other related question, to get a kind of basic comparison with costs of doing research in industry, is the investment in research and development per professional research worker. I don't attempt to differentiate between research and development, but taking the Air Force overall R and D activity, what is the investment in facilities per professional research worker?

DR. LOMBARD: I am sorry, I just can't answer that question. I wish I could, but I don't know. I know what the question is, but I don't know the answer to it.

DR. FURNAS: If I remember correctly, the NACA investment in facilities at the present time is about $150,000,000, and they have seven thousand employees.

DR. LOMBARD: The AEDC will be a very expensive project. They will have facilities valued at $157,500,000 and one to two thousand employees. That will be fifty to one hundred thousand dollars per man.

– III –

by

RAYMOND STEVENS
Vice President, Arthur D. Little, Inc.

The attack on any research problem is helped by restatement in new terms and in new perspective. Frequently a new and more basic problem emerges. Less frequently, what seemed an irritating difficulty reappears as an attractive opportunity. In this instance an intriguing and constructive new aspect of general corporate management seems involved.

The problem as given originally was a description of the qualifications of a Director of Industrial Research. To resolve the matter properly we need a fresh view of the organized provision made by management for a company's future. The professional requirements are stated by Dr. Norman Shepard in a chapter in the Industrial Research Institute book, *Research in Industry*. Technical knowledge and skill coupled with experience in research are accepted as essential prerequisites. The research executive must have absolute authority over the research program, and its formulation is his major task. He must prepare the research budget, with the technical and business judgment that is presupposed. He must sell his budget to management. He must maintain a healthy and constructive mental atmosphere within his staff and must concern himself with salaries and status factors. He must build and maintain adequate liaison with production and sales.

Dr. Shepard also notes the frequency with which men are found in the post without the necessary scientific training and research experience. Unfortunately, it is equally true that the post is frequently filled by a man unwilling or unable to qualify himself in the policy, financial, and top administrative aspects of his important assignment. His stature in the company's policy deliberations does not then equal that of other executives. He and his associates are frustrated by decisions made for them "above the point where the facts are gathered," and research fails to realize its potential in the company.

Giles E. Hopkins has written that research management provides the company with background for company policy, for interpreting technological trends with an understading of economic values, and selling the results to those who should utilize them. Appreciation of dollar values and leadership are among the required abilities. In his opinion research administration is a growing new profession. Rector emphasized that the research director needs the drive which translates research accomplishments into business results, and a few other elements of a job specification for a research director are found scattered in the technical literature.

If we now supplement a literature search with observations on industrial research and research administration as it exists in some of the three or four thousand organizations, we are struck at once with the tremendous variation in policy and practice. It is almost like looking at a series of embryos and tracing the history of the evolution of the organism. The man in charge has various titles, with about four out of five called "Research Director." Other designations are obviously dated, being still embedded in the past history of research and management. They describe the eras when research was an odd man, not a department or even a permanent staff member. Then, a little later, he was a chemist troubleshooting for management. At a later period we see the chief chemist and the cornerstone of a research department. When the physicists, biologists, mathematicians, and economists entered the conception of research, the embryo took on the characteristics of the species. Then the director of research appeared. By the 1930's it was recognized generally that research belonged in some way to top management, and we find the title Vice President—Research. Without spending much time on the company whose research executive is still a "chief chemist" concerned primarily with technical quality control, we pass through the various stages to the higher development where an officer of the company sitting with the executive committee and the board of directors superintends a modern profit- and growth-producing activity that has far outgrown the industrial research function we knew even a few years ago.

One of the most recent phases of functional development in industrial research is systematic operations research, an outgrowth of the war. It brings applied mathematicians into the research orbit in a large way. It emphasizes the breadth essential in the modern research executive, for it is concerned with top policies—and with top policies in which science and engineering in their narrow sense are negligible factors. Such matters as, for example, distribution, traffic, plant location, and inventory policy are among the more simple and obvious elements that are involved. Yet the basic method is that of the research man, and the administrative experience and the skill required of him are identical with the requirements for highly developed research. It is merely another phase of the developing embryo We are not even sure it is the last and highest.

We find a correlation between this high development and the strength, growth, and profit record of the companies concerned. In some companies with spectacular growth and diversification records we find the term "Research Director" clearly inadequate as title for the responsible officer. In the gradual development of research, connotations have grown like barnacles on the words used. The term "research" itself has become inadequate, and the term "director" may carry an implication that his responsibilities lie below the top policy-creating echelon. The dictionary gives little support for "manager" outranking "director," but in some companies the precedence exists. Where it does, unrealized opportunities probably also exist.

Many attempts have been made to define industrial research briefly. Industrial research needs redefinition, and redefinition in more than mere words. Only when that redefinition is accomplished can the qualifications of a research director be defined properly. It is not an easy task, but some aspects of it can be considered to advantage. In the face of the observed tremendous difference between the various kinds of "industrial research," a brief and all-inclusive definition is impossible. The terms "research," "industrial research," and "research director" now have many meanings to many men.

It may help to drop the barnacle-clustered words and start without them. How can we describe in simple English the highly developed and still developing operation that makes some companies living, growing, dynamic organisms, companies so much more humanly interesting and financially attractive than those which lack this new thing?

As vague outlines begin to emerge, the first reaction may be a bit of a shock. Human and financial aspects loom large. Dominant is the general approach to problems with which physical scientists have been indoctrinated, an approach that requires a period of patient apprenticeship for its full comprehension and most certainly for its skillful application. The general method is useful over a wide range of problem types—many of them far outside the laboratory.

Another familiar touch is emphasis on measurement. Both metric and English units are required—and curiously the one series of metric units most universally used is that based on the dollar. It is a unit familiar to the executives of the older functions of industry and one into which scientific measurements must be translated for talks with them.

There also looms brutal realism, an insistence on findings facts, facing them, and acting on them. Every effort is aimed at combatting wishful thinking, rationalization of unsound situations, impulsive action. When facts are available, some of the human touches of old-time management seem in danger, but in their place come a broader vision, fuller realization of possibilities, fewer tragic failures. The whole industrial organization is catalyzed into a new and better way of life.

There are other characteristics. We find, for example, a large percentage of men schooled in a particular class of subjects. The physical sciences, engineering, and mathematics are especially prominent. But the most important and essential characteristic of the new activity at its best is a new basic philosophy. This philosophy is often the one significant difference between the successful and the unsuccessful sponsorship of research.

An industrial corporation should be a living thing, recreating itself in frequent generations, adapting itself to changing conditions, growing where growth is desirable, alive, and healthy. It cannot be life-like if its various functions are all static, as were so many gay-nineties production and sales functions, for example. It is indeed the exception to find the otherwise undesirable sales, production, or financial executive who is constantly seeking major change. The one function occupied particularly with responsibility for change, growth, and adaptation to surroundings is the function with which we are now concerned; call it research if you will, but drop any limitations to the term acquired by early industrial usage.

If we start with this concept of the responsibility of an executive for whom we are writing a "job specification" sheet, we have at least a reasonable chance at a definition that will fit the few known highly developed types of our species. The "job specification" sheet becomes fairly easy. Matching it with an available candidate is another matter.

It may be of interest to look at a description of a "man wanted" for research director at the policy level. Here is one used recently by Rogers and Slade, a New York firm specializing in the location of industrial executives:

> "He will be a member of the company's top management group. In addition to planning, directing and supervising his own Division, he will be expected to maintain effective liaison with the sales, procurement, plant engineering and production departments. . . .
>
> "Works in cooperation with top management in the planning of an over-all technical research program, designed to meet both long-range and immediate operating problems.
>
> "Plans, develops and directs the activities of the Research Division, designed to assist the company in maintaining its excellent position of leadership in the industry.
>
> "In collaboration with operating executives, assists in finding solutions to production and related problems.
>
> "Prepares and issues practical interpretations to management regarding current technical problems and trends affecting the industry."

Another states that:

> "Since the company is deeply interested in the development of a long-range, practical and broad-gauged reresearch program, a man would be preferred who possesses these general qualifications:
>
> "'Skill in defining and presenting recommendations with respect to both long-range and immediate objectives;

in planning work programs in accordance with approved objectives and policies, and in directing and supervising a technical staff in securing results in conformity with established objectives.

"'A keen appreciation of the problems of department heads and their respective staffs, particularly with respect to production, sales and finance. He must be practical and possess sound business judgment, balanced with vision and the courage of his convictions.

"'He should possess a highly developed sense of team play, and the capacity to put into practice the standards by which top management measures the effectiveness of the activities for which he is responsible. Too, he must be able to interpret to top management the potential results which may be attained through an adequate research program and activities related to such a program.'"

One used recently by Handy Associates, Inc., states in part:

"A qualified Director of Research will be experienced in laboratory techniques and practices . . . He will have a valid sense of commercial application and can objectively evaluate products in respect of merit, cost and consumer acceptance . . . .

"As a counselor in research matters to the management of his company, a research director will have a capacity of personal salesmanship to the point where he can get acceptance of his ideas by those responsible for carrying them out."

These specifications are at a high level. They are far removed indeed from George Eastman's first outline of the research man, back in 1890: "It will not be long before your firm will need a practical chemist . . . the best way is to make application to the Professor of Chemistry in some good technical school and have him recommend two or three first-class boys. You can . . . take your choice. If he is any good he will be the most profitable man you can hire."

Yet, returning to the delineation of the good present-day research executive, even these specifications when compared with a broad definition of the function involved leave something to be desired. What we really are talking about is responsibility for the *future* of the company insofar as that future differs from the present activity. We know that many a highly successful modern company will be completely different twenty years from now—in size, in product, in market, certainly in personnel. The production, sales, and personnel managers are *at no one time* primarily concerned with this future. The executive we are now discussing is almost solely concerned with the future.

It is here that we begin to define our problem. Insofar as the many restrictions permit, our man is responsible for the character of changes in his company for much of its future. He must be a prophet, a gambler, a man of vision, a man dissatisfied. He must change, adapt, create. He and his staff must know better than any others the limitations and possibilities of the company's products and markets, of its facilities and—yes, of its personnel. For only with this knowledge can the future be planned and shaped.

With this type of definition we see the shortcomings of any narrowly trained technician. We see what handicaps must be surmounted by the scientist or engineer who seeks this type of responsibility. We begin to see why men who have recently become great in this field are seldom recognized for their personal accomplishments as scientists or engineers. But, by the same token, the truly successful men have been thoroughly indoctrinated in the philosophy of the scientific method and the methodology of physical-science research. They have known how to use the methods and how to talk the language of their staff of scientists, engineers, and economists.

Because the language of science is little known to the business man, and because the language of the business dollar is little known to the scientist, the top executive is, above all things, an ambassador and an interpreter between two worlds. Because the language of science and engineering is an obvious requirement, he is most apt to be deficient in the language of the dollar. He must know the sources and limitations of funds and the relative merits of the many demands for the corporation's money. Balance sheets and statements must be as familiar to him as critical tables. He must know something of markets and practical commercial economics generally. Dr. Shepard has with reason termed "The research director's job . . . the most important one of all the technical positions in a company, and, without danger of over-emphasis, one of the most important in the entire executive family, when the long-term success of a company is considered."

Summarizing: We need a new concept of industrial research devoid of the limiting connotations acquired in its early development. We visualize a crystallized corporate function concerned with company growth and change. The executive responsible should be indoctrinated in the basic methods and philosophies developed since Bacon in the laboratories of the physical scientists. He must maintain and inspire a staff of specialists whose language he must speak fluently and whose mode of thought he must instinctively comprehend. He must interpret staff findings honestly and clearly to his lay associates, and, conversely, he must be qualified to interpret the area of management policy and finance to his staff in their guidance toward proper targets. He must have breadth, vision, something of the urge of the crusader, and something of the enthusiasm of the pioneer and the promoter—all tempered with awareness of the practical possibilities and an honest and realistic allowance for the limitations of men and money.

This is no place for a small or lazy man, but, fortunately, a superman is not required either. The officer in charge can now supplement his own blind spots with men experienced in any of the several principal areas. The general procedures and policies to be followed can be examined in a few working models now in successful operation. Only a very few indeed are highly developed, but many of the others need only expansion in scope in order to cover wider fields. If we are to judge by the results of the very few pioneering leaders in this activity, the promise is great to those companies which follow.

DISCUSSION OF MR. STEVENS' PAPER

MR. ABRAMS: I would like to inquire how successful these management-engineering corporations are in selecting research executives, as proved by subsequent performance.

MR. STEVENS: I would say that they have been vastly more successful than the choices that have been made on the recommendation of one or two men in the field.

I have seen several failures of men who were appointed on the recommendation of some research director who was a friend of the president of the company.

DR. FURNAS: I wonder if you can summarize your remarks in one phrase? Is the essence of your observations that the man you are talking about is the vice president in charge of change?

MR. STEVENS: Dr. Maurice Holland used a term last night, "Vice President in Charge of the Future." But the old-time organization chart had a president, a sales manager, production manager, and a treasurer. The president probably thought that all the functions he supervised were covered. Obviously, they weren't, because he had left out the one life-giving function that has been sorely neglected ever since industrial corporations began to assume their present form. And the title for that sort of a man might well be "Vice President in Charge of Change."

MR. ABRAMS: What recommendation would you make to a young man, let us say around thirty, as to how he can become a research director some day? What sort of action should he follow?

MR. STEVENS: My first recommendation would be to decide whether he really wants to be one or not. It is a hard life, because one has to have two fields and cover them thoroughly.

One has to be an expert in the scientific field and at the same time command respect at the policy level and in the administrative field. Otherwise, he is not a competent man of the type that I am talking about. He certainly should take every chance to meet with and obtain the feeling and the sense of the lay business man, who is concerned primarily with policy.

If he doesn't understand the language of these men, he is not competent. Our technical schools do not teach the men much along that line, certainly none of the detailed knowledge of balance sheets and statements and financial policy.

In addition to his scientific technical training, he must get the equivalent of what some of the business administration schools are giving. And he must acquaint himself and learn how to deal with and talk the language of the businessman.

I believe that most of the men here would meet those requirements.

MR. ASBURY: Many of Mr. Stevens' remarks are pertinent when it is a question of bringing in a man from the outside to fill an important job in the research organization, but there is nothing that helps the morale of an organization more than to take a man from within the organization.

I presume Mr. Stevens was thinking of companies who are starting a new research organization and are trying to find somebody not available in the present organization.

However, with a going research organization, the problem is to have a system for developing men to take the place of the executives that are already there and to fill new executive positions, and under such circumstances executive training becomes an important factor.

MR. STEVENS: I am seriously concerned if I left that impression, because bringing in a man at the top level on some of these organizations is a very serious problem, and the record has not been too good, as you know.

I would much prefer to see the men in the organization begin to work toward the new positions and begin to see some of the responsibilities it envolves.

DR. HOLLAND: I would like to make one point that was touched on yesterday and that is the question, whether we shall grow from the hard technological core of our own company research organization or grow by the merger route.

A friend of mine in one of the large petroleum corporations for years has been making a study of some of the chemical companies in the United States. I think his studies clearly indicate that the growth is largely by accretion, the merger route. I note that some of our friends are becoming the interpreters to management as to what organized research means in terms of company growth and development. They are becoming technical advisers to the president on what I call "the technological strategy" of company growth.

I would like to present this idea: Let us come up with the concept of the Vice President in Charge of Research, . . . of the Future, . . . of Change, or whatever you want to call this overall assignment. Our Vice President of the Future is a product of a technological background and of business management, who keeps his fingers on the pulse of business and competition and taxes and all the rest of it in order to be in fact a "technological strategist" for his company. That is what the job amounts to.

Some of the chemical companies, such as General Aniline and Film, have a director of development, who is a Ph.D. He is not director of development in the sense we think about; he is director of the development of the corporation. His job is to view the corporation as a whole and how best to use technologically trained men. I think such a concept is beginning to emerge.

DR. KLOPSTEG: If the word development is synonymous with or connotes "change," it would seem appropriate to have a vice president in charge of development, tying in with Dr. Furnas' idea. Incidentally, the use of the word "development" in that connotation might help wean us away from the misuse of the word "research."

MR. WILLIAMSON: I entered the research business by way of metallurgy and chemistry. In thinking about what is needed in a research director, I began to think of applicable metallurgical and chemical terminology. Some of these terms are rather interesting and appropriate, others are interesting but not appropriate. "Elasticity" is one, also "high endurance limit" and "resistance to corrosion."

The research director is "abraded" and "eroded" by sales, management, production, engineering, and all the rest.

"High strength" is also good, and "low fatigue." In metallurgy, we hear an "order-disorder transformation" that might be applicable, since a research director should be able to transform things in a disordered state into an ordered one.

You can go on to say "bright and polished, like a metal surface." "Miscibility," the ability to mix well, is another attribute, for the research director should be a good mixer. "Orientation" is another term—seeing himself in the proper perspective in the organization. You might have a little fun kicking around "boiling point," too, also "freezing point," also "throwing power" and "toughness."

Of course there is the phenomenon that is very well known in metallurgy under two names. One is "aging," the other "age hardening," which means getting harder as you grow older! I don't know whether you can have any fun kicking that one around or not!

As far as "brass" is concerned, I prefer to leave that out!

ROUND-TABLE DISCUSSION

R. J. SEEGER, *Secretary*

It was generally agreed that there is not yet any valid concept of an ideal research executive. Such an individual, at present, may be called upon to perform various functions, the exact pattern depending upon the specific job and its place in a particular organization. The essential responsibilities seem to be: (1) obtaining ideas, i.e., planning; (2) selling ideas up and down, i.e., promoting; (3) administering the resultant program, i.e., supervising, etc.

Most individuals, however, were of the opinion that agreement could be reached on the basic virtues desired in a research executive. A research background is regarded as the primary requisite for such an executive. The ability and desire to handle people were also stressed.

The primary need now is a reliable list of items in a usable form for evaluating or selecting research executives. The concepts must be defined precisely in as quantitative terms as possible. Records of specific instances of good and bad performance were recommended as an initial procedure in this direction.

This Round-Table group, consisting of about 30 persons, recommends that the Conference request Dr. Flanagan to conduct an investigation to ascertain a short list of critical requirements for a research executive on the basis of information to be solicited from the members of this conference, et al.

Fourth Session

Albert E. White, *presiding*
Director, Engineering Research Institute
University of Michigan

OVERHEAD
As a Factor in Sponsored Research

W. K. PIERPONT
Controller, University of Michigan

THE SUBJECT of overhead as a factor in sponsored research reminds me sometimes of the White Rabbit in *Alice in Wonderland.* Wherever Alice was or whatever Alice was doing, the White Rabbit usually appeared on the scene under one disguise or another. A discussion of overhead, it seems to me, almost always appears on any program devoted to the problems involved in the administration of research, and I am very happy to have the privilege today to discuss this subject with you.

I think it might be helpful to take a brief look at sponsored research itself, the amount of such research in dollars and cents, and the place of such research in the Government, in the universities, and in private industry.

When World War II ended, all of us were well aware of the contributions made toward the winning of the war by research work in many fields of knowledge. Not only were we impressed by our great achievements in producing the machines for war, but we began to appreciate more fully than ever before the great contribution of physicists, chemists, engineers, and others working from the theories of basic science to the drawing boards of manufacturing plants. There has been a definite recognition on the part of Government officials, industrial leaders, and university personnel that research in basic, fundamental knowledge should be continued if we are to have a sound base upon which to build better techniques and better processes for better products and better services of all kinds.

That sponsored research is a well-recognized and well-supported program of research is evidenced by the fact that the Atomic Energy Commission expects to spend about 14 million dollars this year outside of its own laboratories. Its own laboratories, of course, will spend many millions more. The Public Health Service, financed by the Federal Government, also expects to spend about 14 million dollars this year. The Office of Naval Research will very likely spend somewhere between 20 and 25 million dollars in colleges and universities. It has been estimated that the Federal Government through all its agencies will spend possibly up to 100 million dollars this year for research work in colleges and universities. This large amount of money to be spent on the part of the Federal Government indicates that research has a very important place in the operations of the Federal Government, whether for the protection of the people or for their well-being in other ways. From the point of view of the universities, this sum of money spent on research gives sponsored research an important place in universities. For example, the total research program at the University of Michigan for the past year amounted to about 5 million dollars, so that the Federal Government's research funds would carry twenty programs the size of ours in twenty different universities. And our program here is not small by any means, as university programs go.

In addition to the funds provided by the Federal Government, there are, of course, large sums provided for research by grants from foundations such as the National Foundation for Infantile Paralysis, the Rockefeller Foundation, the Carnegie Corporation, the Kellogg Foundation, and others.

The expenditure of these sums of money, when related to the previous amounts spent on sponsored research or to the laboratory facilities and personnel available for research work, has resulted in the establishment of new facilities on a large scale in many industrial plants and universities and in a fuller utilization of previously unused capacities. The use of these new facilities and previously unused facilities costs money, to cover both direct costs and the overhead costs which are created in substantial proportions. With this brief background, let us look in detail at the costs of research and the specific problem of overhead costs.

I think we should recognize first of all that research is an activity for a purpose. There is a product to be developed, and this product may be, and is in fact, an idea, a theory tested, a technique evolved, or any one of a limitless number of possibilities in the realm of knowledge. From the point of view of the nature of the costs involved, this kind of a product is not different from any of the products of industry or commerce, of the universities, or of the Federal Government itself.

The costs of a product or service, tangible or intangible, from the point of view of budgeting and financing such costs and from the point of view of controlling such costs, may be separated into three major elements. Almost every elementary cost-accounting textbook devotes the first several chapters to three elements:

1) Personnel employed specifically, exclusively, and measurably in a convenient sense for the particular product, or job, or service, or process under study—ordinarily called direct labor;

(2) material, supplies, equipment, communications (such as long distance telephone tolls and telegrams), travel of personnel, and other nonprofessional items of cost purchased or used specifically, and also in measurable quantities from a practical sense—ordinarily called direct material and supplies and other direct costs; and

3) personnel, material, and supplies, equipment, utilities, and other costs which are not easily measurable in terms of a specific product or operation or which for purposes of convenience are not charged directly to the product or operation under study—ordinarily called indirect or overhead costs.

In determining the total cost of a particular product or service, overhead costs are assigned to the product on the basis of the amount of a known factor of cost. In most cases the amount of direct personnel costs is used for the assignment of overhead, since, by and large and in most cases, there is a direct relationship between the amount of direct personnel costs and the amount of overhead costs properly assignable thereto. The relationship of direct personnel costs and overhead costs for an entire enterprise or for a particular part of an enterprise establishes a rate applicable to each particular product, and this rate is known as the overhead rate.

Now, one of the first points to keep in mind in this question of overhead is that overhead as a cost practically always exists in obtaining any product or service. It may exist to a more or less extent, but it will always be present. This is true for various reasons. We, at the University of Michigan, and we are not different in this respect from others, do not begin a research project for a sponsoring agency until it has been reviewed by certain members of our administrative staff to determine if (1) the project proposed is compatible with our overall objectives, (2) we have a contractual agreement to be reimbursed for all costs incurred, (3) salary rates to be paid are in line with established rates for our institution, and (4) we have the space available for the project. Once this is done, we have to select and appoint the personnel for the project; we have to pay them, and, of course, we have to be sure that their net pay is correct after deducting income taxes, medical and hospitalization payments, retirement payments, bond deductions, and what not—each one of these deductions must of course balance exactly to two decimal points, and each one of these deductions requires a separate record for each individual—; we have to purchase materials, supplies, and equipment for the project and do this right now, or else the project will be held up; we have to keep an accurate accounting of all funds involved and prepare invoices itemizing the costs incurred; we have to heat and light the areas used in the project; we have to provide water and sewage facilities; we have to sweep the floors, clean the ice and snow off the front steps, build and maintain parking lots; and a thousand other things which, if not done, would be used as good excuses for poor research results. On top of all this, in most instances, we have to advance the funds to pay salaries and other expenses and then seek reimbursement. Usually, we obtain reimbursement without any trouble, but if we stray from the well-worn path, we have to spend our time, money, and energy to obtain repayment for the funds which we have previously advanced.

The first point I wish to emphasize today is that overhead costs exist in all reality; they are not the figment of an overactive imagination nor the result of accounting hocus-pocus. We have innumerable items of cost which we incur because of the sponsored research carried on by our staff and which we cannot, or do not choose to, charge directly to a research project. I wanted to stress particularly that point, as it seems to me that oftentimes we overlook this fact and are concerned only with paying for direct labor or direct material, or travel, or equipment and are not quite sure of the existence or the extent of indirect costs of one kind or another.

You may say, to be sure, that we would build and maintain parking lots anyway. That is right, we would build enough for our instructional and administrative staff, but without research personnel on the campus financed by sponsored research funds, we would not need to acquire the land and build and maintain so many lots. If some of you do think that parking lots are not a serious problem here, I suggest you come back in a couple of weeks when we are in full operation and try to find a place to park your car, after 8:45

in the morning, somewhere near the campus.

A second point to keep in mind in studying the costs of products or services and the overhead rate is that there is considerable overlapping between the three elements of cost, that is, direct labor, direct material, and overhead, and that the amount of costs represented by each element in a given situation and within a given total may vary, depending upon the organizational structure of the enterprise, the accounting procedures used, the objectives of those who control budgeting operations, and other such considerations. For example, the total cost of a given project may consist of the following distribution of costs:

	Example A		Example B		Example C	
Personnel	60		65		70	
Materials and Supplies	10		10		15	
Overhead	30	(50%)	25	(38%)	15	(21%)
	100		100		100	

The overhead rate in these examples varies from 50 per cent on personnel in example A, to 38 per cent in example B, to 21 per cent in example C. Specifically, items of cost which may be considered as direct costs or as overhead costs, depending upon the circumstances in the given case, include vacation pay, retirement and pension payments, telephone costs, secretarial help, janitors, and freight bills, and the treatment of these items as direct or indirect in the cost of a project will shift the overhead rate as much as was indicated in the three examples just given.

Low overhead by itself is no indication at all of an efficient operation, and decisions made on the basis of overhead rates or percentages without proper study of all the costs involved may just as likely be incorrect decisions as correct ones!

The second point I wish to make today is that the amount of overhead or the overhead rate should not be a controlling factor in the placing of research with a given enterprise. The rate of overhead is a factor in the reimbursement of costs incurred in carrying on research, and that is all.

It is not within the scope of this discussion to elaborate on the factors involved in determining where research work shall be carried on. It is sufficient to state that the particular training of the personnel available, the books and pamphlets in the library, and the interest of the personnel in the objective of the research are examples of real controlling factors in selecting the site for research and far outweigh in importance the factor of overhead. In research, probably more than anywhere else, it is very easy to enter into a low-cost project and obtain low-grade results. It is not money that obtains research results, it is a research worker with a desire to find something new.

There has recently been advanced an idea or a proposition which is somewhat intriguing on the face of it but which, in my opinion and upon close analysis, has little to support it. The proposition, as I understand it, is essentially this: Overhead is admitted to exist, but the payment for overhead costs or, more precisely, the reimbursement for overhead costs, depends upon who initiates a request for the particular research project. To be practical about this proposition, it means that we have to identify and attribute to a particular person or group the origin of an idea or a theory which will be the subject of a research project. If—this would be unfortunate for the university involved—a professor thinks of something which should be investigated further (and that, by the way, is one of the primary functions of professors), presumably the university should be appreciative of any financial assistance provided and should be content with reimbursement for direct salaries and materials needed for the research project. If, however, the Government or industry wants something investigated, the total costs, including overhead, are a proper charge to the sponsoring agency.

You are all well aware, of course, that ideas flow from discussions and from conversations as well as from solitary thought in a library cubicle. To determine the source of an idea for a research project is not only a subject for argument but also a waste of time. Whoever sponsors a research project should have no particular reason for differentiating direct costs from indirect costs; whoever initiates a project must recognize that direct costs are always accompanied by indirect costs. And both types of cost have to be covered by some source of funds.

A variation of this proposition has been introduced by the Atomic Energy Commission. This Commission has introduced two new terms to separate research projects on the basis of interest. "Programmatic research" is research primarily of interest to the Atomic Energy Commission and is directly related to the objectives of this commission. "Nonprogrammatic research" is research primarily of interest to educational institutions or to industrial enterprises. For programmatic research, the Atomic Energy Commission is willing to negotiate the reimbursement for overhead costs incurred. For nonprogrammatic research, the Atomic Energy Commission is willing to allow 8 per cent of total costs for overhead reimbursement on the assumption that if the university is interested in the research work to be done, the university should provide funds for the rest of the overhead from other sources. (This figure of 8 per cent, by the way, has no particular validity and, apparently, was picked up from the overhead rate used in Public Health Service grants.)

With such a proposition to work with, if I were in the Atomic Energy Commission, I should search the country for as many individuals and institutions interested in Atomic Energy Commission projects as I could find. Then I would let all work on a nonprogrammatic basis and do no work on a programmatic basis, or in Atomic Energy Commission laboratories. I have not seen a financial report of the activities of the laboratories operated by the Atomic Energy Commission, but I venture to guess that they are not operated on an 8 per cent factor for overhead.

There is a fundamental issue involved in this proposition. If the Atomic Energy Commission is not interested primarily in a project, why should it sponsor any of the costs? Are taxpayers' dollars less rigorously controlled if they are expended for direct costs than for indirect costs? On the other hand, if a university is not primarily interested in a project, why should it undertake the project at all, regardless of the financial support proposed. It seems to me that a mutual interest in a project by a sponsor and by an organization willing to undertake the project is the only sound basis upon which to work. If either party is not interested, the project should go elsewhere or be sponsored by someone else. And this decision should be made without consideration of the problem of who shall pay for overhead costs.

You will recognize, of course, that research work for which reimbursement is not obtained for total costs is financially sponsored by some one, in a university like ours, either by student fees, state appropriation, or other income of an unrestricted nature. It is this assumption of overhead costs to be met by other than research funds which must not continue to go on in an unrecognized fashion.

This idea of sharing the cost of research projects, particularly the overhead costs, has had wide acceptance in the medical field during the past few years. Public Health Service grants, American Cancer Society grants, and other grants have been made without full recognition of the overhead-cost problem and have included in their grants an item of 8 per cent or 5 per cent of total costs to cover the overhead costs of the work to be done under the grant. At the same time that medical schools are carrying on research without proper consideration being given to the overhead problem, these same schools are experiencing severe financial troubles. This may not be any more than pure coincidence, and I would not want to say that a full recognition of overhead costs for medical research would solve the financial difficulties of these schools. But I would draw your attention to the fact that we are discussing these days Federal subsidies for medical education at the same time that the Federal Government through its Public Health Service is being subsidized by medical schools and universities in the research work carried on with grants which do not include sufficient funds for overhead costs. I suggest that a clarification of this situation will stop any further aggravation from this source of the financial problems of these schools.

Also, I would like to suggest that other schools and colleges should not undertake research projects or accept research grants without recognizing that the eventual burden inherent in a failure to cover all costs of research, even if on only a few projects in the beginning, will create a serious financial problem.

There is one other point of view I would like to examine briefly today. In carrying on research, which to us is our business and which has many of the characteristics of any business, it is one thing to estimate or budget the cost of a project and another to agree on an amount or, in other words, a price to carry on the research project under consideration. In these agreements not much difficulty is encountered with direct costs, but overhead costs appear to be different.

There are, in general, two basic procedures which may be followed in determining the amount of overhead costs which must be covered to finance fully the total cost of a research project. One procedure is to negotiate an amount for overhead or an overhead rate for each project. And this negotiation between the parties may be repeated for each renewal or extension of the project. In this negotiation, all the factors which are pertinent may be brought to bear on the discussion in determining the price, and finally a price is mutually agreed upon. This procedure will soon result in the overhead rate of each project being the main point of argument, and the price will vary directly with the amount or rate of overhead finally agreed upon.

A second procedure is to use a standard or normal overhead rate applied to a common factor in all research projects to cover the overhead costs incurred. Such a rate, admittedly, would be an average rate, high on some projects, low on others, but it would cover in total the overhead costs applicable to research work. Since considerable discussion has taken place recently over the propriety of a standard rate for overhead costs for all the projects of a particular university, I would like to consider certain aspects of the use of such a rate.

Universities, as you know, are nonprofit institutions. They also hope to be "nonloss" institutions. In such institutions, there is no desire for more than a fair overhead rate to cover actual overhead costs. Dividends are not going to be increased, and the bonus is not going to be increased, since such distributions of income do not exist in our operations. There is a desire, however, to hold administrative costs to a minimum and to obtain the most effective use of the trained scholars and research staff on the university payroll. When a policy is followed of negotiating each project or contract independently of all others, it is necessary to arrive at an overhead rate for the reimbursement of overhead costs which is applicable to a particular project and that project only. Let us see where such a procedure leads us.

From a university's point of view, this procedure requires internal financial analyses of an endless variety. In our own case, we have several hundred separate research projects using varying amounts of space; of equipment, of heat, light, and power, of purchasing department personnel, and administrative, supervisory, and consulting personnel time and effort. Accordingly, we would need to have a full-time staff working with our technical and research personnel if we were to try to determine the overhead costs applicable to each specific project. This requirement would increase by many times our administrative costs, which must be covered by overhead amounts received, and would be a serious drain on the time and energy of our research personnel who should presumably be devoting all their efforts to the objective of the research project and not to determining the total cost of the project.

I might add that these analyses take some time to complete, and their completion before the beginning of a project would be a necessity if those of us who are concerned about financial matters were to have any peace of mind. We can all imagine what would happen if the beginning of a research project were held up to any significant extent because the parties involved had not yet decided upon the overhead applicable to that particular project.

From the point of view of a sponsor of research work, the preparation of overhead-cost analyses and the concluding of an agreement as to the amount of overhead properly reimbursable will postpone the beginning of a project and will call for a certain amount of administrative effort on his part. If a sponsor has only one or at most a few projects, the cost involved may not be significant, but there still is a delay in obtaining the research results wanted.

But, let us look at the Federal Government as a sponsor of research to be carried on by industrial enterprises or by educational institutions. This sponsor has many agencies, all with certain objectives to accomplish on what always seems to them as a limited budget. Moreover, these agencies are sufficiently independent so that the operations of one may differ markedly from the operations of another. Does it not seem self-evident, then, that there should be a consistent treatment of overhead costs by all the agencies of one sponsor? On the other hand, should not the universities consciously avoid preferential treatment of any one agency of the Federal Government?

I realize that the operating personnel of a single agency of the Government may wish to reduce the payments made by the agency to a minimum for each project in order to accomplish as much as possible with its limited budget. And in trying to accomplish this minimum expenditure per project, an attempt is likely to be made to reduce the payments for overhead costs to as low a figure as possible. Parenthetically, I should like to add at this point that no one ever seems to think about increasing the amount paid for overhead over and above the actual cost incurred.

If universities were to enter into an agreement which resulted in less than fair reimbursement for overhead costs, with which agency of the Federal Government should it enter into such an agreement? Should it be the agency with the smallest amount of research on the theory that the financial significance of this research is not of much concern? Should it be with the agency with the largest amount of work on the theory that such a sponsor is a good customer and must be retained? Should it be with all agencies to preclude unfair treatment of one as compared with another?

A privilege granted to one sponsor in one project, if allowed to spread to other projects or other sponsors, will soon be a privilege lost to all projects and to all sponsors. It will become an intolerable financial burden on an institution and will be reflected in a general lowering of the quality of operations of the institution.

These considerations indicate the desirability of using a standard rate which covers all costs of an overhead character. Such a standard rate precludes overhead-cost analyses for individual projects and provides a fair share of overhead costs in each project. The use of a standard rate takes the question of reimbursement for overhead costs out of the sphere of argument and reduces it to a place of minor significance in a negotiation of a research contract, which is its proper place.

During the war period, which ended five years ago, some rough calculations were made as to the amount of overhead costs applicable to research projects carried on at universities, and standard overhead rates were used in many wartime contracts. In 1947 the use of a standard rate was given further impetus by the issuance of a set of principles entitled *Explanation of Principles for Determination of Cost Contracts with Educational Institutions.*

These principles were approved for use by the military services and were acceptable to the universities. They were not when adopted and they are not now a perfect set of principles fully agreeable to either the military services or to the universities, but they offer a way to determine a standard rate of overhead for research projects which can be used for all research sponsored by the Federal Government. With the use of these principles to determine a standard rate, it is now a matter of ordinary contract procedure to include a proper allowance for overhead costs in research contracts with the military services.

It is hoped that this practice of using a standard rate of overhead, which has been found successful in the administration of many contracts for research, will be adopted by agencies of the Federal Government which do not now adhere to that practice. It may also be adopted, in a proper manner, by nongovernmental organizations for sponsored research projects.

In conclusion, I should like to state that—

1) when full recognition is given to the existence of overhead costs on sponsored research projects,

2) when an allowance for overhead costs is considered a normal item in the budgeted cost of a sponsored research project, and

3) when the amount of this allowance can be computed in a simple but accurate manner by the use of a standard overhead rate adjusted whenever necessary for special circumstances—

then the problems of overhead in sponsored research will be considerably lessened, if not substantially eliminated.

DISCUSSION OF DR. PIERPONT'S PAPER

DR. FURNAS: I don't know whether I can add anything in particular to Mr. Pierpont's excellent paper, except perhaps to state my experience from another point of view.

I feel that the gripes, some valid and some otherwise, were very much too modest, and I believe that it is really a quite serious situation, and his explanation of the seriousness was also much too modest.

I think I can speak objectively, because my own organization, while it is owned by a university, is a separate, though wholly-owned, subsidiary of a university. It does not operate on the university type of contract; it operates on the industrial type of contract, and it has to be completely self-sustaining financially. In other words, it has to pay its own living, in that it has to recover *all* its costs.

Is is my opinion that the governmental agencies, and I am including all, seriously short-changed the universities in the matter of overhead, and I think it is not in the national interest to do so.

In the matter of reimbursement of overhead to the universities, the Navy uses the so-called "blue book," the Air Force and the Army use the "university type" of contract, and these do not, I am convinced, allow for complete and full costs. Of course, there is always the matter of the bargaining position, and I do not know whether that kind of bargaining is particularly different from any other type of bargaining, except that you are dealing with a nonprofit institution, which doesn't have the bargaining position of saying, "In order for us to exist, we must have a profit."

Apparently, the basis of the bargaining, as nearly as I can tell, is that the increment of overhead which is taken into the discussion is that which is associated with the added increment of cost to the university for undertaking this work. I have sat in on some of these arguments, and I have heard a number of them second hand, and I have never seen any philosophy entering in the bargaining, which indicates that the full costs of doing research will be recognized if you take into account these things which are supposed to constitute the heart of the university, namely a community of scholars.

If I may be more specific, I don't believe, for instance, that in the computing of the overhead costs of the universities, any consideration is ever given to the necessary existence of a department of English. Nevertheless, the matter of having people with adequate cultural background and ability to express themselves is very much an indirect part of a whole research program. As a result, the universities always come out on the short end of the horn as far as covering the full costs of the running of their institution is concerned. Merely paying for the added increment of taking on a few more research people does not pay for the full costs. Who, then, covers these complete costs? The person who covers them in some instances is the poor student paying the fees. That means he does not get as much return on his educational expenditure as he has a right to expect. Sometimes the intention of the man who has supplied an endowment is not followed. In other cases, those costs are paid by those who are presumably paying for the strictly educational activities of the university, sometimes the state legislature itself.

The results are either a trend towards financial bankruptcy or a trend towards educational bankruptcy.

I think I can point out some examples, the first being the case of the medical colleges referred to by Dr. Pierpont. In *Harper's Magazine*—I believe it is the June, 1950, issue—there is an article which asks the question: "Can we afford our medical schools," or something like that. It goes into a discussion on the plight of the financing of the medical schools of the country. It makes quite a strong point of the topic which was brought up, that the more research they do, the poorer they are, simply because of the way the overhead is computed, since the reimbursement they get does not by any means cover the complete cost. All of you who are interested in the general structure of the overhead as financed on research by the Federal Government should be interested in reading that article.

In suggesting the possibility of educational bankruptcy, one speaks with a great deal of trepidation, for such remarks tend to make enemies. One of the country's leading institutions, according to its annual statement last year, had approximately seventy-five per cent of its budget involved in research, largely on contracts. That means that there are certain people who are presumably hired for academic pursuits that happen to be riding two horses, and one can't ride two horses very satisfactorily.

I have heard certain rumors to the effect in the last few years that there have been certain repercussions in the academic aspects of this institution, simply because the best people—and they are the ones involved in research—do not have the time to spend with the students who come there to get an education. Perhaps this is not as dangerous as it appears, but it is a trend which is more or less prevalent in all universities. I am just pointing out that there is a possibility that this might possibly lead to the bankruptcy of academic activity as well as toward financial bankruptcy.

The more research you have, the poorer you are because the contracting agency does not allow payment of all the costs involved, so that the money has to be taken from somewhere else. In the case of universities, the Federal Government is definitely not acting in the national interest in its policy of paying less than full overhead.

I believe that the universities as a group have been too modest in their attempts to get adequate overhead. As a national trend the situation is serious. It is not in the national interest for the Federal agencies to be so penny-wise and pound-foolish, and the universities should try collectively to help them see the light; otherwise the next generation will be seriously injured educationally.

Dr. Spencer: The remark made by Dr. Furnas needs emphasis. Overhead is a part of the cost of conducting research and it must be paid. If the sponsor does not pay the full overhead, it is paid from some other source. It may come from student fees, endowment income, state taxes, or anonymous donors.

The Government employs industry to conduct a large amount of research and development. It pays the full cost, which includes all direct charges plus the full overhead. In addition, it usually pays a small profit. Moreover, industry's overhead is higher than any university's overhead. It is difficult to see why the Government should expect the universities to conduct research for less than its costs.

The difficulty is, undoubtedly, associated with a university tradition. For years, universities have sought and accepted 'grants-in-aid.' With such a grant it is understood that the university must add to the grant some of its own funds. Under such a procedure it would be possible for universities to reach a position where they receive only grants-in-aid and research contracts that do not pay full overhead. They then might find it impossible to proceed with any of the research projects, because the additional funds required for each project could not be found.

Universities ought to encourage or require sponsors of research projects to pay the full cost, whether or not the money comes from a grant or a research contract.

Mr. Tour: I would like to compliment Dr. Pierpont. It is certainly pleasant to hear that universities are beginning to appreciate that overhead is a real factor in the cost of industrial research.

I would like to comment on several items, however. Dr. Pierpont mentioned that according to various ways of figuring overhead, the result might be forty per cent or thirty per cent or even as low as fifteen per cent. He included such things as vacation and ill-time in that overhead. I am surprised at the arithmetic. It is impossible to include holidays, vacations, and sick leave in an overhead and have so low an overhead as fifteen per cent. Professional and technical employees may average more than fifteen per cent of a year's time as lost time.

A survey was made of our staff as to the available time per year of our productive professional and technical employees. The initial deductions were for two weeks' vacation, two weeks' ill-time, and eight holidays each year. Next came, how many days are spent at technical-society conventions and committee meetings each year, how many hours or days are spent each year on reports and papers for committees and technical societies. We expect each professional and technical member of our staff to be active in that type of work in his field, and we must allow him the time to do so. A research worker that will not keep up to date in his field is not the man we want to keep on our payroll. Making these additional deductions from gross yearly time and plotting the available time as the ordinate and annual salaries as the abscissa gave almost a straight line. A three-thousand-dollar-a-year man has available something of the order of eighteen hundred work hours per year. A fifteen-thousand-dollar-a-year man has only fourteen hundred hours a year available for productive work. The higher the salary, the lower the number of work hours available.

In the ordinary cost-accounting system based on a forty-hour week and fifty-two weeks a year, there are two thousand eighty hours a year, and annual salaries are divided by this figure to arrive at a direct labor cost per hour. For example, a man receiving a $6,240.00 per-year salary is figured at a direct labor cost of $3.00 per hour. However, he only has 1700 hours of time available for productive work. If the missing 380 hours are charged to overhead, this item of lost time becomes a 22.3 per cent overhead on direct time. In the case of a $12,000.00-a-year man with only 1500 hours of productive time available, this overhead item of lost time becomes 38.6 per cent on direct time. This time is not lost because there is no pay work for the man to do but is lost because of the nature of his work. The $6,240.00 man must be charged as $3.67 for direct labor, and the $12,000.00 man as $8.00 for direct labor, and this type of lost time must be included in overhead on direct labor, or else the overhead rates must start somewhere above 33 per cent.

How can this item be included in the fifteen per cent overhead that Dr. Pierpont spoke about? Already we are over fifteen per cent in the hole, and we haven't started to pay rent, light, heat, insurance, and a host of other items.

I would like to point out another item of overhead that seems to be missed by a number of our universities and institutions. That terrible little item is called depreciation and amortization.

There is a tendency on the part of the Federal departments to say that if you got your building for nothing, and if you got your equipment for next to nothing, you have no right to charge the Government with depreciation and amortization. Some day the institutions should get to the point of living on their own incomes. Some day buildings and equipment will have to be replaced. Depreciation and amortization of buildings and equipment should be based upon fair appraised values and not on first costs. Depreciation and amortization costs based on such appraised values should be included in overhead.

Other items of overhead that were mentioned by Dr. Pierpont are Federal old-age benefits, social security taxes, workmen's compensation, and so forth. In the State of New York these items amount to five per cent of gross payroll. Productive payroll is not gross payroll. Productive payroll is only that portion of the payroll that covers the productive work hours of the productive workers. Office workers, cost accountants, time clerks, telephone operators, and all the people who argue with the Government auditors about how costs are kept, janitors, charwomen, and messengers are nonproductive workers on the payroll. The five per cent of gross payroll for direct payroll taxes, when converted into a cost on direct or productive labor payroll, becomes close to fifteen per cent overhead.

Where is a fifteen per cent or even a forty per cent total overhead a possibility?

I enjoyed what Dr. Pierpont has said about overhead as a factor in sponsored research, but I am sorry he did not start with one hundred and fifty per cent overhead and say that perhaps it could be gotten down to one hundred per cent, rather than starting with forty and getting it down to fifteen per cent.

Dr. Pierpont: You would get down below fifteen per cent, if you charge everything "direct." Fifteen has no validity, ten has no validity. It might take five hundred per cent if you had the choice of what is direct and what is indirect. Take the point on vacation pay, for example. I will agree wholeheartedly with the comments on sick-time and vacation time and so forth. As a matter of fact, the so-called fringe benefits that the University of Michigan has for its labor force, including vacation, group insurance, retirement, sick-leave, and what else, amount to 15.1 per cent of the payroll, just those items of payroll cost.

The point that I tried to make and that we want to be sure we recognize is that we should not have any preconceived idea of what a percentage rate ought to be. We have several kinds of operations here. In some cases some items are charged "direct" and in some cases the same item may be charged "indirect." It is very easy, of course, to set up a project by itself and charge almost everything conceivable as a direct cost of the project, and probably no questions will be asked or answered.

We have some Government auditors here in the audience who will be able to substantiate my ideas. They know the books better than I do, I believe, in some cases. The point that, I think, we want to be sure we recognize is that a rate of one hundred and fifty per cent or two hundred and fifty per cent or fifteen or fifty per cent, or whatever it is, can be obtained. Every institution may have a different way to get at its rate of overhead.

The point I want to make beyond that is that the overhead rate is not a particularly significant thing in determining who should get a contract for a research project. We have been told—and it has been used in the negotiations in setting a price—: "Well, we will go some place else."

If you want to come to the University of Michigan or Ohio State or the University of California or Mellon Institute or some place else with a research project, it is because there is somebody there whom you want to do the work. That is really the number one requirement. When you get off that particular point, then you begin to worry about getting a low price, and you may get what you pay for.

So that is the thing that, I believe, we ought to be sure we think about on the overhead rate. It is something that is a matter of reimbursement and comes after the project is pretty well set up.

I agree with Mr. Tour, fifteen per cent would not cover the down time. We charge both vacations and sick-leave as direct costs to the project. It seems to me the best procedure is to charge everything you can charge to the direct cost of the contract, and you won't have so much to argue about.

Dr. Waterman: I feel that perhaps somebody ought to speak for the Government. Perhaps I had better do it, because I am not particularly competent on the subject of overhead, nor have I very much to do with contract negotiations, but I would like to make a few observations from the standpoint of one who has taken a very active interest in the effect on the actual work that is going on in these discussions about overhead.

As we see it, the effect that should be considered is the effect on the institution and the effect on the general program of research throughout the country.

Another reason I am glad to make these remarks is because it gives me a chance to talk on the other side of the question of overhead.

I have been arguing with so many people that overhead is justifiable, that I welcome a chance to talk against some of the remarks that were made earlier, about what the Government is trying to do in the case of overhead. There are two extremes, it seems to me. One is the case where the supporting agency is interested in supporting a field of research that is an old subject with the institution, one in which it is very much interested and in which it has unusually competent men. That is why the supporting agency goes there.

In this case the university is just as much interested in fostering the research and encouraging it as is the supporting agency. It becomes much more of a cooperative venture, and I think that is helpful.

It means that in this case a rate of support may be set, as often done by O N R, that permits additional people to be secured for research, while the permanent staff of the institution is still paid by the institution. I think this is an excellent arrangement.

In an academic institution, the permanent officers in a sense are the board of directors, with regard to educational policy, and it does not seem right in the long run, that these should be supported in any major way by an outside agency. Those who are afraid of Government pressure should welcome this arrangement. In fundamental research, then, it makes a great deal of sense to divide the cost between the institution and the supporting agency.

Nor should one forget that research is a real asset to the institution—it gives prestige, it will give favorable publicity, and it has a money value. All these reasons prompt me to say that in fundamental research already established at the institution, the overhead expense may justifiably be shared.

Now take the other extreme, the case of a truly academic institution having a staff member who has a flare for a certain type of development which is outside the normal activities of the institution. An outside agency knows this and wants to get the man to develop a piece of hardware, and it approaches the institution. This is not a normal venture for the institution. In this case, it seems to me, the supporting agency, since it needs the work done and wants it done there, ought to pay the entire cost. This is quite reasonable, since the problem is one not normally undertaken by the educational institution.

Another observation: if all work sponsored in a nonprofit institution were to be supported entirely by outside agencies, with all salaries and the entire overhead paid by supporting agencies, then, I believe, the institution would tend to take on a business rather than an educational character. In time this might interfere very seriously with the educational and research standing of the institution. If this were done by institutions generally, it would affect higher education very fundamentally and, I believe, adversely.

I have no formula to apply here, but only a few observations to make. It seems to me, in the interest of making sponsored research a success, the best move is to approach the matter in a cooperative way, bearing in mind the interest of the institution and the interest of the agency.

Dr. Klopsteg: I am glad that Dr. Waterman made his observations, because I have several that fall in the same category.

I fully agree with everything that Dr. Pierpont says regarding what we might call development or industrial research done by educational institutions, namely, the type of work in which the agency makes the approach, just as it would in case of a commercial consulting laboratory. This is in the hardware category that Dr. Waterman mentioned. Without question, the sponsoring agency should pay the whole bill, including all the applicable overhead.

Now, on the other point that I want to make I speak not as a representative of a particular institution, but as one who has been observing institutions in their efforts to do research, especially institutions like Northwestern, that are operated out of private endowments and student fees and do not have a legislature to hand out the great sums of money each biennium. We rather feel that the state institutions are in a much better situation that way.

In private institutions, the amount of money that is available to support the scholarly research that the faculty may want to do is definitely limited. Now, suppose a Government agency has funds which may be devoted to the building up of this backlog of knowledge that we have been hearing about the last few days, and that, I believe, was first emphasized in the report by Dr. Bush, *Science, the Endless Frontier.* Moreover, suppose an institution is interested in adding to that backlog of knowledge, and its own funds are rather scarce, and an agency which has ample funds comes along, and a person in the institution eager to do a piece of research sees the opportunity of getting funds from that source. Now, the question that I want to raise is:

If the university can strengthen its basic research activity by accepting such funds, whether or not there is overhead, should it do so? Or, should it reject the funds, because the supporting agency is not going to pay the forty or fifty or whatever the per cent of overhead is?

In my estimation, there are really two categories of research of which we must take cognizance. The one, the actual sponsored research, where the Government seeks to get tangible, demonstrable value out of what it buys; the other, where the agency has funds for the support of basic research, and where the university can strengthen its basic research program by accepting such funds.

If the answer in the second case, that is in the case of supported basic research, is: "No, we can't accept those funds unless the agency pays all the applicable overhead," then I raise the question: Should the university take the same position toward the granting of such funds by private individuals?

DR. SEEGER: Having a full-time relationship with the Government and a part-time one with a university, I can perhaps see something on both sides of the question.

Dr. Pierpont's comprehensive presentation of a university's point of view should undoubtedly have been followed by equally factual arguments on the part of a representative from Government. I am certain that he would have begun in the same vein as Dr. Pierpont, namely, "We are tired of having all these different rates of overhead with so many types of universities and industries, with private institutions, and with state institutions. We desire a uniform rate." In either case, the same question persists, "How does one *determine* a single rate for even one university, or for all universities, for even a single Government agency or for all Government agencies?"

Take, for instance, the cited eight per cent rate. Apparently, that is customary, but certain people object to it because of its very uniformity.

A VOICE: It is too low.

DR. SEEGER: All right, it is regarded by some as uniformly too low. I am familiar with a university which has such a uniform rate of its own for all contractors and accepted by all Government agencies, apparently because it is uniformly low. The underlying reason is that the university regards itself as a partner in these enterprises and not as an institution selling something to the Government. Government support is regarded by this university more as a federal grant-in-aid than as a purchase of information. In such a case, overhead becomes of minor importance, being merely an administrative procedure for rendering account of a public trust. One's viewpoint, therefore, determines largely what is to be included in overhead for Government research contracts. It is amazing what one finds sometimes included in it on a quid pro quo basis. I have heard of demands for partial support of such items as, not just parking lots, but dormitories and stadiums.

On the other hand, starting from the premises that a university is essentially an educational institution and that higher education is intimately linked with basic research, one arrives at the following university attitude: "We have a research program which our faculty and students cannot prosecute as they desire. We invite the Government to share in this program, which we believe is of specific interest not only to ourselves but also to the Government." In short, the university is not asking for complete support of an extraneous project but for the supplementary contribution to an integrated program. (I would, to be sure, differentiate this type of undertaking from a development project, which, I personally believe, should be done by a university only in cases of emergency.) This principle will be actuated differently in different types of universities: the poor ones and the rich ones, those publicly supported and those privately. Each case will have to be decided on its own merits; uniformity seems to be undesirable—if not impracticable.

DR. MORGEN: I would like to agree with Dr. Klopsteg and Dr. Waterman. However, there is one point which, I think, we ought to make clear. In our university budgets we assume that we are going to do a certain amount of research. We assume that a certain staff member is interested in research and that he is going to do it. Therefore, we have included in the overhead of the university, from our regular appropriations, wherever they are, provision for that research.

Then, if we could get a grant or research aid for graduate students or other assistance for that man, we could in that particular case take the aid without receiving the full overhead. That amount of overhead has been calculated in the original institutional budget.

Each institution, then, will have to determine from its own budget how much of that type of aid it can take before it begins to exceed the calculated overhead of the university. I don't think we can give a specific answer to the question for all universities. Each university is going to have to determine that for itself, on the particular budget that it has.

DR. WHITE: I would like to make two observations. I suppose most of us have dealings with the faculty and find it rather difficult at times to convince them of the necessity of overhead. Also, we talk about Government, and my experience is that Government is of the opinion of some one individual that has been vested with authority. It is an individual, rather than a combined viewpoint.

Then I find that different institutions are treated in different ways. We cannot help but raise our eyebrows when two or three months ago considerable pressure was brought to bear on the University to take a contract from a Government unit at eight per cent, and the following Sunday a gentleman came through from the Pacific Coast who had a contract for a similar type of work, only about ten times greater in amount, which, we were led to understand, carried an overhead of about forty per cent.

Instances of this kind make us question the fairness with which overhead is administered.

May I now ask Dr. Pierpont to close the session with further discussion?

DR. PIERPONT: I will take but just a few minutes.

There are two or three things I would like to comment on.

I tried to indicate that I think this overhead problem is considered too serious a problem and that we get too much wrapped up in it. On the other hand, I don't want to minimize the problem either.

I think we should all recognize that the university research business is somewhat different from what it was fifteen years ago. Certainly it is larger. It certainly is more diversified, it covers many more fields, it has many more people in it.

I would like to say that we do not expect one overhead rate for all contracts. That would be a pretty easy way out, but it will never work. Neither do we expect one rate for all universities. We believe that would be a grave error.

It has been suggested that universities are looking for research and should reflect this in overhead charges. I don't think that has anything to do with who pays the overhead, whether Washington people are here or whether we are in Washington. I believe that we get off base in our thinking on overhead if we are concerned about what kind of a project it is and who wants it.

I agree that research should often be a cooperative deal. We should not have any project on this campus unless we want it. Nor should we have it unless the Government wants us to have it.

The philosophy of taking a contract to support a few graduate students and not worrying too much about the overhead, I agree to. But let us have just one contract without overhead reimbursement or a low overhead rate, and every Government agency will quote us the example, chapter and verse, day in and day out.

Against the advice of Professor White and Professor Good, we signed a contract a couple of years ago at five per cent overhead. We have been plagued many times since with this rate, but these two gentlemen have been very kind not to say to me, "Well, you should have known better."

We have had that particular project brought up so many times that I really wish it would never be renewed. We don't want it any more, it is a very small project, but every Government agency I can think of has used that project as an example to try and obtain a low overhead rate.

The overhead problem is not entirely a problem of the Government beating us down. We are still independent organizations, we still have people working on the job of running the University.

The problem of the high-paid faculty members, the top-flight faculty members, not teaching school but doing research, is not the Government's problem. Let us not lay that to the Government. It is our business to find out who is to do the teaching at our place and to see to it that it gets done. It is our job to see that it is financed. I think that sometimes we lay an awful lot to the Government, which is nothing but a way for us to escape some of our responsibility. It is our responsibility to decide who is to teach school, how many hours they should teach, how many hours they should spend on research time.

We don't expect at our place that the Government overhead rate on every contract will be the same rate. I think that that is a mistake. We do not expect our rate to be like Maryland's or that of any other place in the country. I doubt very much if you will find the overhead cost of automobiles in General Motors, Chrysler, Ford, or Kaiser-Frazer alike. It is an entirely erroneous conception of overhead to think it is going to come out the same no matter where you are.

Comment was made that maybe some universities should charge overhead and some should not; that the ones who are well able to pay the overhead should pay the overhead, and the ones that cannot afford it should not pay it. The rich will be poor and the poor will be rich under this philosophy, I am afraid, and it won't be very long coming.

There is one other point I would like to make. Dr. Waterman pointed out and very well, I think, that one of the philosophies that is inherent in the Government approach to this problem is that they want to be sure that we study the situation very thoroughly before we take a research project, that we are aware of its cost, and that we are willing to bear part of its cost. If the Government were to offer full costs on all research projects, we would be rushing for everything we could get, and pretty soon we would be only a research organization and forget our twenty thousand students.

I think such a situation is of concern, but I don't believe it is a Government problem. That is our problem. I think that we should be the ones to be sure that we teach school properly. Our first and fundamental objective, as was said before, is to teach school. Along with that objective is the objective of research. It is up to us to decide how much research we can do at our institution and handle it properly.

One of the reasons we have project directors, and one of the reasons we have administrators of research, is to see to it that from every particular project that we have here somebody gets some good results.

Part of the problem of research in universities is that it has increased pretty fast. We haven't control of all the problems, either internally or externally. We have gone a long way in the last few years in trying to solve the problems involved. The so-called blue book was certainly a good step in the right direction.

The arguments we have in these discussions and privately with people in Washington or here in our own place, I think, are all to the good in bringing out the essential nature of the problem. I think that owing to these discussions we shall keep going down the right road. Nevertheless, we probably will come back a year from now, five years from now, twenty-five years from now, and still have some problems on research overhead matters.

ROUND-TABLE DISCUSSION

W. K. Pierpont, *Chairman*

The Round Table devoted to a discussion of overhead problems discussed the following specific points:

1. What precisely is sponsored research?
2. What items of cost should be included in overhead and is it possible to establish some norms for overhead rates?
3. The problems involved in the determination of the cost of a research project should be distinguished from the problems involved in determining the amount which should be received or paid for research.
4. Universities must recognize the nonmonetary rewards obtained from research work and must relate these rewards to the amounts received for research work.
5. Research work accepted for less than full reimbursement of cost must be recognized as such by universities and the full implication of such a procedure on the financial position of universities must be considered.

In the discussion on what is sponsored research, there were quite a few comments to the effect that research work varies all the way from that done by graduate students under fellowship grants to research work done by full-time research personnel under contracts with outside sponsors for work expressly requested by the sponsors. Such wide differences in the kind of research work done on university campuses should be taken into consideration in any discussion of the overhead reimbursement to the university by the sponsoring agency. It was pointed out that corporation or governmental grants which are used in the form of fellowship grants for graduate students to carry on research work possibly should not bear a full cost of overhead expense, whereas, at the other extreme overhead costs should be fully reimbursed. There was a feeling that the term "sponsored research" should be expressly defined, at least in connection with a determination of the amount of overhead costs on sponsored research.

Following some pertinent discussion there was an expressed desire on the part of several members of the Round Table for a detailed study of what items of cost should be included in overhead and what items of cost should be considered direct labor or direct materials and supplies costs. It was pointed out that the recent work of the Armed Services and the universities in preparing the *Principles for the Determination of Cost under Government Research and Development Contracts with Educational Institutions* was a good beginning in defining the elements of cost but that additional work could be done to the benefit of all parties concerned. There was an indication also that it might be possible to establish certain norms for testing overhead rates of universities, though it was also pointed out in this connection that such norms might vary so greatly that they would be useless in practice.

There was a discussion on the influence on the overhead rate of various internal university procedures and policies in accounting, in purchasing, in the treatment of retirement and pension costs, and in other areas of university activity. If this is the case—and it was generally agreed that it was – it is desirable to separate the problem of determining the amount paid for research.

The cost of research is determined by using accounting procedures which are not exact and which introduce a number of considered judgments, whereas, the amount paid for research involves the problem of determining a precise payment for a given research project. By separating these problems, it is possible to relate financial considerations to the specific character of the research work to be done, to the source of funds sponsoring the research, and to other such matters which might affect the amount paid for research though they do not necessarily affect the cost of research.

A number of the members of the Round Table pointed out that universities derive considerable nonmonetary rewards, such as the publicity given to the university and to its personnel for research work carried on with funds provided by sponsors and the admitted aid to the teaching work of the university from the point of view of the students and faculty. Recognizing these nonmonetary rewards, it may be possible to relate them to the amount of the reimbursement to the university for some of the costs incurred in the research work. In any case, however, the university which accepts research work and receives less than full reimbursement for cost must recognize that the costs are still inherent in the operation and must be covered by some other source or sources of funds.

There was a request by several members of the Round Table for the inclusion of a session on overhead and various aspects of the overhead problem on the agenda of next year's conference.

Fifth Session

John I. Mattill, *presiding*
Secretary, Engineering College Research Council

NEW GOVERNMENT SERVICES TO RESEARCH

— I —

THE NATIONAL SCIENCE FOUNDATION AND RESEARCH

by

THOMAS J. KILLIAN
Science Director, Research Division
Office of Naval Research

SINCE WORLD WAR II the United States Government has rendered many tangible services to research. It has recognized that basic research is the foundation of the science and technology vital to our national welfare and security.

In particular, three outstanding steps have been taken: (1) About four years ago, the Office of Naval Research was established, which since its inception enjoyed the full-hearted cooperation of the Army and the Air Force, both of which have been well aware of the importance of basic research; (2) the State Department Scientific Office has been established recently, with plans for Overseas Scientific Staffs; and, (3) the National Science Foundation has finally been authorized.

Each of these governmental actions, when studied separately, seems logical and clear cut. But there are strong interrelationships. Let us examine some of the present and future complex interrelating effects.

We will begin with the National Science Foundation. On May 10 of this year, President Truman signed Public Law 507, which authorized a National Science Foundation. The stated purposes of this "National Science Foundation Act of 1950" are:

> "to promote the progress of science; to advance the national health, prosperity and welfare; to secure the national defense; and for other purposes."

Thus, after more than five years of effort, a new phase of Government activity has begun—science for its own sake. For the first time our Government has acted in positive recognition of the vital importance of science to our national health, prosperity, and security. It has assumed new responsibilities for the promotion of basic research and the development of scientific talent.

The immediate history of this official recognition of basic scientific research as a national resource began in 1944. On November 18 of that year President Roosevelt addressed an historic letter to Dr. Vannevar Bush asking for a plan in which the successful research experience developed by the Office of Scientific Research and Development could be used after the war to improve national health and the national standard of living. In particular, President Roosevelt was concerned about what the Government could do to increase our future research strength and to discover and develop scientific talent.

Science, the Endless Frontier was the stirring answer to this request. This report Dr. Bush submitted to President Truman on July 5, 1945, one month before the surrender of Japan. Dr. Bush recommended the creation of a National Research Foundation to promote a national policy for scientific research and education, to support basic research in nonprofit institutions, to develop scientific talent in American youth, and to support long-range research on military matters.

Then the legislative wheels began to grind. It took a five-year gestation period for the recommendations to bear fruit.

The principal recommendations of the Bush Report were soon embodied in a bill introduced by Senator Warren Magnuson of Washington. A short time later Senator Harvey Kilgore of West Virginia introduced an alternative bill. Most of the controversies over the legislation then and later were focused on three important differences between these two bills.

First, with respect to organization and administration, the Bush Report and the Magnuson Bill recommended that the Foundation be governed by a board of nine members, none of whom should be full-time government employees, but who should be recruited from private life, chiefly, though not exclusively, eminent scientists. The Kilgore Bill, on the other hand, provided that the Foundation should be headed by an administrator appointed by the President and aided by an advisory board without direct power. The advisory board was to be larger than that proposed by the Magnuson Bill. It was to be composed equally of persons from Government agencies and of others outside Government.

Second, there was a marked difference in the proposed patent policies. The Magnuson Bill followed the OSRD patent policy (and incidentally that of the Navy), which left it open to private interests to patent results of work supported in whole or in part by Federal funds, unless there were specific contract restrictions to the contrary. The Kilgore Bill went to the other extreme. It included a blanket prohibition of patenting results growing out of Government-supported research.

And third, the Magnuson Bill did not include any specific provision for the social sciences. Although the original Kilgore Bill did not provide for the social sciences either, a revised form introduced in October, 1945, did include the social sciences.

In a message to Congress on September 5, 1945, President Truman broadly endorsed the policies of the Kilgore Bill. He strongly recommended that Congress enact legislation establishing a Science Foundation and that an administrator and not a board have primary responsibility. He further recommended that the social sciences be included.

Then began a series of events which some people feel could happen only in Washington. The Kilgore Bill was referred to the Committee on Military Affairs while the Magnuson Bill was referred to the Commerce Committee. However, joint hearings were arranged under the auspices of the Subcommittee on War Mobilization of the Committee on Military Affairs, of which Senator Kilgore was chairman.

In the meantime the division between certain scientific groups began to widen. The number of bills introduced in the Senate and House began to increase, and they varied greatly. For instance, Senator Willis of Indiana introduced a bill establishing a National Science Foundation consisting of fifty members appointed by the President from nominees of the National Academy of Sciences. The Foundation was to set up its own constitution and define its scope of activity subject to the approval of Congress. The final clause asked for the appropriation of $100,000 for the making of an "initial report and recommendations." Thus, it was to start from scratch as if the work of Bush and others had never been done. It was no wonder that all action on a National Science Foundation was tabled in the summer of 1946, blocking further action before a new session of Congress.

Nevertheless, an important positive step in the implementation of a national policy recognizing the significance of science to national security was the action of President Truman in signing Public Law 588 on August 1, 1946. This law established the Office of Naval Research. Let us look at the introduction to Public Law 588:

> "To establish an Office of Naval Research in the Department of the Navy; to plan, foster, and encourage scientific research in recognition of its paramount importance as related to the *maintenance of future naval power,* and the *preservation of national security;* to provide within the Department of the Navy a single office, which, by contract and otherwise, shall be able to obtain, coordinate, and make available to all bureaus and activities of the Department of the Navy, world-wide scientific information and the necessary services for conducting specialized and imaginative research; to establish a Naval Research Advisory Committee consisting of persons preeminent in the fields of science and research, to consult with and advise the Chief of such office in matters pertaining to research."

All the aforementioned controversial issues were avoided. O N R is under the Secretary of the Navy. The social sciences are not directly mentioned. No change in patent policy is indicated. Furthermore, except for the Naval Research Advisory Committee, the elements of the organization were already in existence. The Naval Research Laboratory and the Special Devices Center were active field organizations, while the Planning Division of the Office of Research and Inventions had already launched its university contract program.

The situation late in 1946 was this: A compromise bill had passed the Senate but died in a pigeonhole in the House. Nonetheless, an agency unique in Government had been established and was at work, the Office of Naval Research.

A new note was being added. Late in 1946 the President appointed Dr. John R. Steelman as chairman of the President's Scientific Research Board and directed him to make a thorough survey of all research and development activities, both in and out of Government, and to make recommendations for insuring that the scientific personnel and training and research facilities of the country are most effectively used in the national interest. This was a Herculean task. The board consists of the Secretary of Agriculture, the Secretary of Commerce, the Secretary of the Interior, the Secretary of the Navy, the Secretary of War, as well as the heads of the Federal Loan Agency, the Federal Security Agency, the Federal Works Agency, the Federal Communications Commission, the National Advisory Committee on Aeronautics, the Atomic Energy

Commission, the Tennessee Valley Authority, and the Veterans Administration. With the aid of a Board of Alternates, a report consisting of five volumes was issued on August 27, 1947.

The conclusions parallel those of the Bush Report. Specifically, it recommended that Congress be urged to establish at its next session a National Science Foundation within the Executive Office of the President and that it be authorized to spend $50,000,000 in support of basic research during the first year of its existence and increasing amounts thereafter, rising to an annual expenditure of *at least* $250,000,000 in 1957! It recommended that no restrictions should be placed upon the fields of inquiry eligible for support.

In the spring of 1947 Congress did pass a bill establishing a National Science Foundation. It provided for 24 part-time members appointed by the President with the consent of the Senate. The members, in turn, were to appoint their own Director who would be responsible to them and not to the President. Expressing his deep regret for being forced to take such a course, the President vetoed the bill on August 6, 1947, because it provided "a marked departure from sound principles" of administration. Many students of government are in agreement with the validity of the President's decision.

Thus, the issue remained dormant during 1947, 1948, and 1949. In the *Bulletin of Atomic Scientists* of February, 1950, Dr. L. A. DuBridge wrote:

> "After four years of waiting and working there is still no Science Foundation.
>
> "This failure of the National Science Foundation legislation to pass might easily lead one to suppose either that this proposal was not of very great importance to national welfare, or else that it had stirred up an extremely active opposition. Yet neither of these suppositions is true. The proposed Foundation *is* of vital importance to the nation, and it has not encountered major opposition. It apparently suffers solely from its own inertia. No individual or group in Congress has taken the responsibility of pushing this particular piece of legislation over the many obstacles which stand in the way of its passage and approval.
>
> "Time is now running out. Unless the present bill before the House of Representatives is passed at the next session of the 81st Congress, the chances of ever having a Science Foundation may be small indeed.
>
> "The case for a Science Foundation has never been more adequately stated than in the orignal Bush Report, *Science, the Endless Frontier.* The arguments set forth in that report are as sound today as when first presented. In fact, four years of experience have strengthened some of the most essential arguments for an independent Foundation."

In our rapid survey of science and Government, let us go back to 1948, when we find the Department of State being systematically studied by the Hoover Commission. A special "task force" recommended that a scientist of national repute be asked by the Department to serve as a temporary consultant to analyze and submit recommendations on (a) the role of the State Department in national scientific policy and activities and their interrelationships with foreign policy, and (b) appropriate organization and staffing required to carry out this responsibility.

Dr. L. V. Berkner was appointed special consultant on October 4, 1949, and immediately went to work with his characteristic energy and enthusiasm. There were the traditional high-level steering and advisory committees. The Steering Committee consisted of ten Assistant Secretaries of State with the Under-Secretary as Chairman. The Advisory Committee was headed by Roger Adams and included Vannevar Bush, I. I. Rabi, Alexander Wetmore, Robert E. Wilson, Alfred N. Richards, and Detlev W. Bronk. And there were the usual low-level, but hard working groups.

Again the result is a report, in this case a rather long one (170 pages), entitled, "Science and Foreign Relations," which was submitted April 28, 1950. In addition to general recommendations that the State Department become aware of the scientific implications of foreign policy and of the international character of science and technology, there are two recommendations of great significance to science in this country.

The first recommended the establishment in the Department of State of a Science Office headed by a Scientific Advisory appointed as Special Assistant to the Under-Secretary of State and supported by a small staff of three or four scientists. Dr. Herman A. Spoehr has been appointed to this office.

The second recommendation provided for the establishment of Science Staffs at selected United States diplomatic missions abroad. These would be of two categories, differing in size and geographical coverage. Scientific staffs at a few key posts would act as centers for large geographic areas, not limited to the country to which the staff is assigned. Initially, staffs in this category would be established as follows: London, (Western Europe), Johannesburg (South Africa), Rio de Janeiro (South America), and Canberra (Australasia).

The second category would involve very small staffs in individual countries. Initially the posts might be: Paris, Rome, Berne, Stockholm, Ottawa, Lima, Oslo, Copenhagen, The Hague, and Brussels.

The report was approved unanimously by the Advisory Committee and the Steering Committee. The Secretary of State directed that the recommendations be implemented. This is now being done. Thus, the second and third significant steps have been taken in transforming positive scientific policy to reality.

Let us now look at the "National Science Foundation Act of 1950." It provides for a National Science Board and a Director. The Board of 24 members is appointed by the President with the advice and consent of the Senate. The Director is also appointed by the President with the advice and consent of the Senate. The Board may make recommendations with respect to the appointment of the Director, and the Director is not to be appointed until the Board has had an opportunity to make such recommendations.

At present four divisions are provided for: (a) Medical Research, (b) Mathematical, Physical, and Engineering Sciences, (c) Biological Sciences, and (4) Scientific Personnel and Education. However, other divisions may be established as the Board deems necessary. Thus, the door is open for the social sciences. The controversial problem of patents is disposed of by a generalization calling for the protection of the public interest and the equities of the individual or organization concerned. Foreign activities of the National Science Foundation are subject to the approval of the Secretary of State. The Science Office of the Department of State will, of course, provide the necessary channels. The act authorized $500,000 to be appropriated the first year and sums not to exceed $15,000,000 each later fiscal year.

This third step is very important. Not only have nearly all the objections to previous forms of the bill been removed, but numerous improvements have been added. This is particularly true with regard to security provisions and research related to national defense. The Foundation is not required to carry on research related to national defense, although it may initiate such research. Research not connected with defense is free from security regulations.

During the past few years, the Office of Naval Research has given a great deal of consideration to the problems of its relationships with a National Science Foundation. The establishment of the National Science Foundation will undoubtedly have effects on the planning, programming, and budgeting of O N R. There is general agreement among the committees of R D B that the National Military Establishment and the Navy, in particular, should be allowed and encouraged to continue the support of basic research at approximately the present levels. This is in accordance with the sound policy that every agency with large development responsibilities should have a basic research program in its fields of interest to insure scientific balance. Yet it is obvious that certain projects on our research program can and possibly should be transferred to the Foundation. Precisely which projects cannot be determined at this time. Each must be considered individually. Any transfer must be through mutual agreement with the Foundation, the Office of Naval Research, the university, and the investigator.

It is worth mentioning some of our thoughts on this subject. First, using the stipulation that applied research be undertaken with a definite naval application in mind, we can screen all projects which can be classified as applied research. These would not be considered for transfer. Second, there are certain areas of the frontiers of science in which the Navy has a very vital interest. I may mention a few as examples: hydrography and oceanography; underwater acoustics; electromagnetic wave propagation; studies of the mechanical, electrical, and magnetic properties of matter as they may bear on materials used in ships and aircraft; nuclear power for ship propulsion; underwater explosives; hydrodynamics; cavitation studies; all types of background noise, such as thermal, magnetic, electromagnetic, and acoustic; mathematics essential for the improvement of fire control, weather forecasting, operational analysis, and computers; safety and health-hazard studies with special reference to nuclear radiation; human resources, personnel selection, aptitude studies, and training; human engineering; studies of man in relation to his environment, with particular reference to arctic, tropic, desert, and other extreme conditions; studies of the normal man, etc., etc. A careful analysis of projects in these and other fields of Navy interest will indicate that many should not be considered for transfer to the Foundation.

A third screening of the program may be for projects which bring the activities of outstanding scientists into the research and development program of the Navy. O N R's basic research program has made available to the Navy and the Department of Defense, on a broad scope, the advice and counsel of many of the best scientists of the country. The university program has built up a vast network of experts in most of the fields of scientific interest to the Navy. The Navy should continue to have available an effective and complete group of consultants in these areas of science.

The effect of a National Science Foundation on O N R personnel has also been considered. We may be requested to loan, or to transfer permanently, some of our people to the Foundation. This may include administrative and contract, as well as scientific, personnel. We are prepared to give all possible assistance in this respect. In addition, we must consider possible demands for an O N R liaison group to represent the Navy in the Foundation.

Let us remember that what I have loosely called the O N R program includes many joint projects to which the Army, Air Force, A E C, and other Government agencies not only contribute but which, in some cases, they administrate. It is a program of nearly 1200 projects in 200 institutions. It amounts to about $20,000,000 a year and involves about 3000 scientists and 2500 graduate students. Such joint activities truly indicate the excellent practicabilities for joint and cooperative programs between O N R and the N S F.

The State Department program at present involves only two small offices, but it is growing. To date the N S F has no Board and no money, and is struggling to come into being this year (1950).* However, we are

* Since this talk was given, an appropriation of $225,000 was alloted and the twenty-four members of the N S F Board have been appointed by President Truman. They include some of the nation's highest-level educators, engineers, scientists, and industrialists. The members are:

dealing in futures. O N R looks forward to the healthy growth of both. We hope they will profit from our mistakes as well as our successes. In such a case the future health, welfare, and security of this country are assured.

Sophie D. Aberle, Special Research Director, University of New Mexico
Chester I. Barnard, President, Rockefeller Foundation
Robert Percy Barnes, Head, Department of Chemistry, Howard University
Detlev Wulf Bronk, President, Johns Hopkins University
Gerti Theresa Cari, Professor of Biological Chemistry, Washington University Medical School, St. Louis
James Bryant Conant, Harvard University
John W. Davis, President, West Virginia State College
Charles Dollard, President, Carnegie Corporation, New York
Lee A. DuBridge, President, California Institute of Technology
Edwin B. Fred, President, University of Wisconsin
Paul M. Gross, Dean of Graduate School of Arts and Sciences, Duke University
George D. Humphrey, President, University of Wyoming
O. W. Hyman, Dean of Medical School and Vice President, University of Tennessee
Robert F. Loeb, Bard Professor of Medical Services, College of Physicians and Surgeons, Columbia University
Donald H. McLaughlin, President, Homestake Mining Co., San Francisco
Frederick A. Middlebush, President, University of Missouri
Edward L. Moreland, of Jackson and Moreland, Consulting Engineers, Boston
Jos. C. Morris, Head of Physics Department and Vice President, Tulane University, New Orleans
Harold Marston Morse, Professor of Mathematics, Princeton University
Andrey A. Potter, Dean of Engineering, Purdue University
James A. Reyniers, Director, Bacteriology Laboratories, Notre Dame University
Elvin C. Stakman, Division of Plant Pathology and Botany, University of Minnesota
Charles Edward Wilson, President, General Electric Company
Patrick Henry Yancey, Professor of Biology, Spring Hill College, Spring Hill, Alabama

DISCUSSION OF DR. KILLIAN'S PAPER

DR. FURNAS: I would like to know what is the chance or the probability at this time of getting out enough money to get this started or implemented this year. Or is that asking too much of the crystal ball?

DR. KILLIAN: I tried to find out yesterday, but I could not. You probably are concerned with the article in the *New York Times,* that the House Committee felt that the President did not want any new programs to interfere with the national defense efforts.

I think that was farthest from the President's mind. I think that has now been made clear to the Appropriations Committee.

DR. WALKER: I believe that I can give you an answer as of last Friday. Last Friday the House Appropriations Committee passed an omnibus bill, a bill with $471,000 for starting the work of the National Science Foundation. It was not in the Senate appropriation bill; however, hearings were held last Friday. Larry Halstead was there, I was there and two other people, and the Senate Committee, what there was of it, was very cordial. Leverett Saltonstall was really in there swinging for us, and we came out feeling rather encouraged. Instead of saying, "Do you think it will pass?", we made up a pool on who could guess the closest amount of money we would get.

We have a lot of hope for getting the bill before Congress.

THE FEDERAL HOUSING RESEARCH PROGRAM

by

RICHARD U. RATCLIFF
Director, Housing Research, Housing and Home Finance Agency

This conference itself – the fact that we have gathered here–illustrates strikingly the extent to which research has achieved recognition as an essential factor in progress. During the past decade, research in some fields has moved swiftly and dramatically, enhancing the recognition of research generally. But this has made research efforts in other areas seem plodding by comparison. The relative progress of research in housing during these years and that in nuclear physics or plastics, for example, has made me feel sometimes a bit as Alice must have felt when the Red Queen said to her: "It takes all the running you can do to keep in the same place. If you want to get somewhere else, you must run at least twice as fast as that."

These days, however, I am feeling much less like Alice than I used to. Because now we have a robust housing research program, which is going places. It is certainly something new in Government research, as I see it billed on this morning's program, and it is indeed something new under the sun.

History and Objectives

The Division of Housing Research as presently set up came into existence late in 1949. Since then, we have conceived and put into action a program designed to meet critical research needs of the housing and housing finance industries. Research projects well underway are focused on a wide range of subject matter, ranging from light-weight aggregate concrete to family residential mobility. The current international situation has given defense-related activities of the Division an added pulse of urgency.

Congress authorized the Division of Housing Research in the Housing Act of 1949. Funds were made available late in the year. Title IV of this Act created the Division as part of the Housing and Home Finance Agency. The Housing Act recognizes clearly that research is essential to the production of better, less expensive housing. A study of the legislative history behind the Act reveals also an increasing recognition of the chronic need for speeding up, expanding, and integrating housing research throughout the housing industry, and across the Nation.

The language of the law and its legislative history reveal further that no narrow definition of housing is admissable in the administration of a housing research program. In the minds of the legislators, the subject of housing was not bounded by the four walls of a building. The scope of our program therefore reflects a broad concept of housing. The substance of housing research is imbedded in all categories of knowledge, across the full breadth of nature and society.

Congress defined the goals of Government housing activities in terms of several national housing objectives. These goals give the research program a definite direction and thrust. In general terms, the national housing objective may be summarized as follows:

1) *Improve housing and its community environment.* This improvement should satisfy both physical standards of strength and durability, and human values of health, safety, comfort, and personal satisfaction.

2) *Reduce the financial burden of shelter to consumers.* This involves cutting costs of production, operation, and maintenance, and providing sound, efficient channels for housing finance.

3) *Stabilize the housing and housing finance industries.* The goal is a balanced supply of housing and housing funds. This would contribute greatly to the harmonious growth of the national economy as a whole.

Mounting defense needs put a special emphasis on the objectives of lower costs and economic stability. In this situation, two considerations become central: First, the conservation of manpower and materials in housing construction and operation; second, application of controls over housing credit and construction, based on a sturdy foundation of fact and analysis. The value of research in our national strivings toward all these objectives is clear.

The Role of Government in Housing Research

The need for housing research is apparent enough. But why is a program to meet this need being sponsored by the Federal Government? The answer can be put simply: The current program was created because critical research needs are not being met by the housing industry itself, or by other nongovernmental means. The job of Government in this field is defined by these unmet needs. As housing research by industry, business, and other private groups expands, Federal activities will be modified to avoid overlapping and duplication.

This approach to the problem conditioned the molding of the Title IV legislation. It has become a guiding principle to us in planning our research program. It is linked with the conviction that this program must be as broad as the society it serves. Preoccupation with any one segment of the housing and home finance industries must be avoided. The program must consider them all.

The Division must constantly seek to comprehend the housing process, structure, and environment, in their entirety. The thinking behind all major decisions in the program must penetrate to the end product of the housing process—to the individual home—for this reason: Giving maximum benefit to the housing consumer yields the greatest benefit to all of the housing industry; at the same time it achieves the primary goals of housing research set by Congress.

Thus, in addition to sponsoring research projects, the Division serves as an observation tower, as it were, from which the housing industry can view the vast and intricate panorama of its own process. This service is especially important in housing, where there has been no central vantage point or clearing house for research. The Division has a unique and challenging opportunity.

In addition to direct research activities, the functions of the Housing and Home Finance Agency research program are these: To *integrate* all housing research activity through full interchange of information to the end that the impact of research results will be *cumulative;* to *stimulate* housing research activity by others, by spot-lighting research needs and providing technical guidance, to translate scientific findings into practical applications, to disseminate information on research developments to all potential users; finally, to encourage the general adoption of proven innovations. All these are part of the process of achieving the greatest possible yield for business and industry out of every quantum of energy expended on housing research.

Research and the "H H F A Family"

Before talking about specific research activities and how they work, I want to give you a quick look at the Division's structure and its relation to the Housing and Home Finance Agency "family."

H H F A was created by Congress in 1947 to combine and integrate most of the permanent housing activities of the Federal Government under a single administrator. Currently, its major components are the Home Loan Bank Board, the Federal Housing Administration, the Public Housing Administration, and the Office of the Administrator. The Administrator's office includes the Division of Slum Clearance and Redevelopment and the Division of Research, both of which were born in the Housing Act of 1949.

Inside the Division of Research, the administrative structure comprises four major staff units and five main branches. The staff units are labeled: (1) Statistical Research and Development, (2) Agency Reports and Statistics, (3) Research Intelligence, and (4) Publication. The functional branches are (1) Housing Technology, (2) Housing Economics, (3) Housing Finance, (4) Local Housing Regulations, and (5) Urban Studies.

Contract Method

Under the Housing Act, the Division is instructed to use existing facilities for research wherever feasible rather than setting up elaborate machinery of its own. The contract method is the obvious device for carrying out this instruction. Our experience with the contract method has been more pleasant than it might have been otherwise by reason of the familiarity of most universities with Government contract work. But in one respect this familiarity has made it more difficult, since most institutions have become familiar with contracts with the military establishment. They did not initially appreciate, in some cases, the fact that our program was operating under a different basic law and thus under a different set of limitations.

One of the features of our contracts with universities which resulted in considerable discussion is that relating to publication. The basic problem was reconciling the statutory requirement that research results must be placed in the public domain with the practical necessity of reserving for the Government the right of prior publication for a reasonable length of time, and the natural and proper desire of university scientists for independence and free right of discussion and publication. These differences were resolved to the satisfaction of all our contractors in a provision which reserved the right of prior publication for the Government during a six-month period, and permitted free and unrestricted publication privileges for the contractor at the expiration of that period.

Another matter requiring some attention was the arrangement for inspection of the research in progress by representatives of our Agency. No real difficulty arose here since the Division of Housing Research is as anxious as the university contractor that he be permitted complete freedom in pursuing the research project. However, it is important that our staff keep in close touch with the work, partly to permit integration with related projects at other institutions, and, partly, to be sure that there are no substantial modifications in original objectives and scope.

The contracts are on a reimbursable basis and provide for an allowance for overhead costs. Reimbursement is for actual cost, but the Agency endeavors to operate without requiring a detailed and comprehensive audit. In determining overhead costs the most recent available audit, regardless of its purpose, is usually accepted as sufficient for establishing a provisional rate applied to the total of salaries and wages. In most cases the Naval Cost Inspection Service has been able to furnish the necessary information.

At the time the contract is negotiated, a provisional rate is established. Then arrangements are made for the ultimate determination of a fixed rate to apply for the duration of the contract. The fixed rate is intended to compensate for actual cost and follows the general principles outlined in the War Department-Navy Department *Explanation of Principles of Determination of Costs Under Government Research* and *Development Contracts With Educational Institutions,* dated August, 1947.

The range in overhead returns in contracts already entered into is approximately from 10 to 55 per cent, with most cases in the vicinity of 40 per cent. These differences are accounted for largely by the degree to which items of direct cost can be identified and estimated so that direct reimbursement can be made. Thus, the greater the coverage of direct costs, the less will be the residual overhead charge. Another factor influencing the overhead rate is the proportion of the contract work to be conducted off-campus, where the direct costs are more readily identified and where certain of the overhead costs such as heat and light would not apply.

One other minor problem has arisen from our contracts which are in force. This relates to the purchase of equipment by the contractor. We have been asked, for example, to authorize the purchase of typewriters and filing cabinets. As a matter of policy it is assumed that normal facilities are to be provided by the contractor both in laboratory and office equipment. It is only in the procurement of special equipment or that which is substantially in excess of the normal needs of the contractor that purchase may be allowed out of Government funds. Ownership of this equipment vests in the Government when reimbursement of its purchase price has been made.

Current Research Activities

Fifty-eight research projects have been undertaken so far by contracts between H H F A and other organizations, private and governmental. Contract funds committed to these projects total $1.4 million. These contracts have been signed with twenty-one universities, eight governmental agencies, one private nonprofit organization, and the Academy of Sciences.

The Bureau of Census, Bureau of Labor Statistics, and Forest Products Laboratory are among the governmental agencies conducting housing research for us. Universities handling H H F A projects include Columbia, Harvard, Illinois, Tuskegee, California, and Michigan. Other contracts include the Bureau of Entomology and Plant Quarantine of the Department of Agriculture, and the Southwest Research Institute. The first final research report on any of these projects was received in our offices two weeks ago. Scheduled completion dates for other projects range up through May, 1954, but the majority are to be finished during 1951.

Some current projects are aimed at economies through improved materials and better use of materials. Studies on the use and properties of light-weight aggregate concretes in housebuilding are examples. A project of this kind is being done for us now by the National Bureau of Standards.

Another group of projects is aimed at savings through more efficient structural systems and structural components. The Division's work on the development of simplified plumbing systems and more economical floor construction are examples of this.

More economical procurement of materials and equipment is another field of concentration. Current projects in this area include an intensive study of present channels used to distribute all kinds of building materials.

And the Division is striving to help the housing industry develop more effective assembly and erection management practices. One project is an analysis of the best methods now in use, including the contribution of factory fabrication.

Three current research projects are being conducted for H H F A right here at the University of Michigan. Aside from the "local angle," it is appropriate for me to specify them here because they give a good indication of the scope of subject matter our program covers.

The first of these projects is:

Development of Cost Accounting Systems for Home Builders

This research will provide home builders with effective means for cost control and cost reduction. Part of the research job will be preparing texts or manuals on cost accounting for house builders. The project is under the direction of Professor Herbert F. Taggart, in the School of Business Administration.

The second Michigan project is:

Labor Relations in the Building Industry

One of the largest components of cost in house construction is, of course, site labor. This study is exploratory in objective and aims at describing current conditions in the labor market as they relate to the building trades and at identifying major trouble spots to which future research can be directed. The work is being supervised by Professor William Haber, long recognized as the leading authority in this field.

The third project here at Michigan is:

A Survey of Buyer Considerations in Recent House Purchases

This project is to be carried on by the Michigan Survey Research Center through the use of the interview techniques which they have developed to such a high degree of effectiveness. The purpose is to identify and evaluate the behavior of recent home purchasers in choosing among various housing features. The ultimate objective is to provide sounder design criteria in home planning, particularly in the small house.

In addition to sponsoring individual research projects, the Division is seeking by other means to facilitate sound business and public policy decisions affecting housing production, financing, distribution, and providing Government agencies and business organizations with a steady flow of market information for their use and guidance. Especially in connection with defense activities, the research staff is devoting an increasing amount of time to gathering this information and presenting it most usefully.

All told, thirteen current contracts are being handled by the Housing Economics Branch of the Division of Housing Research, thirty-six are assigned to the Housing Technology Branch, five to the Housing Finance Branch, and four to the Urban Studies Branch. Examples of urban studies projects are a *Study of Residential Mobility,* i.e., the movement of families inside urban communities, and *Growth Patterns in Metropolitan Areas in the United States.*

Housing Research Contribution to Defense

Adjusting activities of the Division to the international situation is primarily a matter of gaining a maximum yield for defense purposes from research directed toward the long-term housing objectives set by Congress. The artificial situation created by economic controls and specialized defense needs increases the difficulty of economic analysis, but makes such analysis even more important.

Insuring the greatest contribution of housing research to national defense does not involve a basic redirection of the program but rather a narrowing of its scope to bring about a sharper focus on problems aggravated by necessities of national security. The policy of concentrating more intensively upon defense problems will, in most cases, produce results not only of immediate value but of broader application to the long-term chronic housing ills.

Housing Research Movement in America

We hope that the current H H F A housing research program will serve as the catalyst for development of a real housing research movement in this country. The yield from each dollar now being spent on research in housing can be multiplied if such a movement emerges and matures. There is substantial evidence that it is now being born. It is the product of a growing confidence in the H H F A research program. It will be nourished by the recognition of its results.

Not only the results themselves must be recognized, *but the fact that they were produced by research.* This is necessary in nurturing any research movement. It will assist us in multiplying the impetus of the H H F A program so that housing research activity will be generated far beyond the limits of our own work. It will lead to creation of a permanent fund of activity in housing research, attracting minds which penetrate housing problems from new viewpoints of study and experience. Ultimately, the cultivation of a housing research movement in America may be the most important aspect of our program.

[Extemporaneous Remarks:]

Now, how has the Korean situation affected this program? It has very clearly affected everything that goes on in Washington, including research.

Our basic aim is to find ways of saving materials and manpower, of conserving resources—all of which objectives become even more pointed, in a time of labor and material shortages, when substitutes have to be used. And so, in housing technology research, in particular, we have no great problem in diverting our activity and focusing it on current as well as anticipated defense problems.

If we get into the materials allocations at some later stage, which I hope we can avoid, then we will necessarily become involved in research pertinent to allocations problems.

DISCUSSION OF MR. RATCLIFF'S PAPER

Mr. Worthington: I wonder, Mr. Ratcliff, if you could tell us how your agency's program will tie in with the industry-sponsored research agency called the Building Research Advisory Board.

Mr. Ratcliff: While you are quite right that industry groups are responsible for its creation, B R A B is part of the National Academy of Sciences.

Their objectives are much the same as ours, in the sense of aiming to integrate and stimulate housing research activity. Their field goes beyond housing, though, into all kinds of construction, whereas ours is limited largely to housing problems.

We have one contract project with them now—we are financing the research and they are carrying it out. It involves bringing together information on all housing research activities now being conducted by the Government, industry, universities, and others. We hope this will develop into a kind of a research reference service for the housing industry, carried on by our division. We have other joint activities with B R A B, and we are working very closely with them.

Dr. Owens: I believe it is true that unless the scientists in the universities can actually publish the results of their investigations under their own names any agency, industrial or Government, will have a difficult time getting research done under contract by the caliber of personnel that is desirable.

The first publication should be in an established scientific or technical journal. Then if the Government agency wishes to publish a more complete report on the same subject, that is perfectly all right.

There is a way of doing this that the Structural Clay Products Institute has used, with which some of you may be familiar, i.e., issuing a series of research publications after the material has previously appeared in the technical journals. The article carries the author's name and institutional affiliation directly beneath the title, with a legend across the bottom of the cover, "Number blank of a series of reports on the progress of research activities in the Structural Clay Products Industry." This is satisfactory and suitable since it shows who did the work and where it was done, while certainly providing proper acknowledgment of the support of the work by the sponsoring organization which it needs to obtain support for subsequent appropriations.

Mr. Ratcliff: I don't suppose we want to institute a debate on this subject. We have wrestled with it a great deal during our operation and discussed it with our contractors at great length.

Being fresh from a university campus, I think that I was able to see both sides of the question, and in most cases, not in every one, we have come to an understanding that the university people can have freedom of discussion of the technical aspects of the problem with other scientists and other people in the field.

We have found, in discussing the problem with them, that the kind of thing they want to publish is not the kind of thing we want to publish. However, arrangements can be made, so that very likely publication will be simultaneous. No matter in what order publication occurs, the problem, when you think of it in general terms, is a more serious one than when you think of it in specific terms.

We tried to work it out, and we came to that conclusion. All we ask for is the right if we choose to elect it—and in many cases we certainly would elect it—of getting out a publication somewhat in advance of another publication which would deal with the vital parts of the project.

There are many other kinds of publications that will develop as the project proceeds, which we will encourage; but I don't think you can settle the point now, and it is one on which we are open-minded and will negotiate within the limits of the law under which we operate.

DR. SPENCER: Mr. Ratcliff, why does this particular Government agency want the prior right of publication? It would appear that the results of this type of research should be published as quickly as possible.

MR. RATCLIFF: Well, there are a number of influencing factors. One is that our law rather specifically places an obligation to do just what you say is desirable, namely, to disseminate the results of research as widely as possible. This obligation obviously involves publication in most cases.

Our law specifically provides that technical developments, and in fact all the research product, must be put in the public domain. Thus, no contractor may patent a process or device growing out of contract work. The philosophy is to secure wide distribution without restriction. Take the copyright problem. When a university publishes, or even most scientific journals, the material is generally copyrighted. Our lawyers raised the question of whether such copyright was a restriction of the use of the research product and thus untenable under our basic law.

There is also a practical problem, our being able to demonstrate to Congress that the money they put into research produced useful results. Since publications are the form in which research results are generally "packaged," they constitute primary evidence of work completed. And as I mentioned a moment ago, the law under which we operate makes us responsible for dissemination of our research results. Naturally, we do not choose to dodge that responsibility on the assumption that publication by the contractor will suffice.

Certainly, it seems to us perfectly possible to let everybody get all the credit they want and to get the widest possible dissemination of results. Our rather mild restrictions do not impede this—involving our right to publish in the channels in which we want to publish. These often would not be the channels in which the university professor wants to publish the results of the same project.

MR. LOUGHRIDGE: It seems to me this is somewhat analogous to an experience I ran into about a year ago. I would first like to say that I agree with Dr. Killian on the desirability of getting research of a military nature published as quickly as possible, taking due cognizance of security restrictions. I ran into a rather peculiar and, to me, surprising result about a year ago. A question came up as to the degree of duplication between some of the Quartermaster's research in the field of foods and that of the Department of Agriculture in the same field.

I was asked to look into the matter and see whether this duplication did or did not exist. I, of course, contacted both the Quartermaster and the Department of Agriculture and ran into this rather peculiar attitude on the part of the Department of Agriculture. They admitted, as did the Quartermaster, that there was some duplication, as, of course, you find in most fields. In research you do not worry too much about that. However, some research results had been withheld from the Quartermaster, due to the fact that they had had some unfortunate experiences and criticism coming to the Department of Agriculture from industry. The complaints were based upon the claim that the Quartermaster published too much and that in the past, when they had freely allowed information to go back and forth between the Department of Agriculture and the Quartermaster Corps, the Quartermaster had released in their quarterly publications some information which industry objected to on account of its being in the nature of trade secrets.

This is exactly the reverse of what we usually hear, namely, that the military services keep too much close to their chest and release essentially nothing. I am wondering whether this element may not enter here since, without doubt, you are tied up, and necessarily quite closely, with some industries. May there not be an objection by industry in releasing some of this information?

MR. RATCLIFF: I don't think there is in our case any particular problem with industry. All our problems are worked out with industry, with advisory committees, and we usually obtain representation from industry.

Furthermore, we have no control over what the contractor publishes, except this one limitation—having prior right of publication for the duration of the contract plus six months. Ultimately, the contractor can publish the whole works, or any part of it, without any limitation whatever, so that in the end we cannot control what comes out of the project in any way.

Our only interest is in taking a part that is pertinent—that scientific knowledge be applied to specific problems in industry and that information comes out in such a fashion that it is going to be put to work. Our whole objective is lost, unless we can effect economies in the industrial processes of house building.

MR. GREEN: Perhaps I can shed a little light on this. Five years ago the President signed an order, and this order created the inter-departmental Publication Board. There are six officers of cabinet rank, and I happen to be the executive secretary of that board. I can say that their policy has been that it is not too important who has the prior publication, whether it is the university or the Government, as long as it is reasonably prompt.

But we are concerned with the degree of dissemination. I think we will agree that sometimes publication in university bulletins does not reach as wide an audience of taxpayers, who actually paid for that research, as might be done if a Government agency, which has a direct channel, develops at least a supplementary operation.

I am saying for the Publication Board, which actually has overall authority on this subject, that we believe it should be worked out on a case-by-case basis, that priority of publication is not significant, but rather the degree of dissemination.

DR. LOMBARD: Having heard from the Army and Navy, I think the Air Force should also be heard on this point. Research is not worth anything if you do not publish it, if you are hiding your light under a bushel.

Our idea is to get as much out and disseminate it as widely and as quickly as possible. Established journals usually have wider distribution than Government agency journals.

As far as the point of getting credit is concerned, it is reasonable to require that any article describing research done under the auspices of the Housing Administration should give due credit to the sponsoring agency. The publication of an article in a recognized journal, giving credit to a sponsoring agency, has weight in requesting additional appropriations as has the publication of an article in the agency's own journal.

In many research contracts, a report is all that is physically submitted to satisfy contractual requirements. It is practicable and desirable to include a provision that the publication of an article disseminating the results of the investigation in a recognized technical journal will satisfy the contractual requirements for a final report.

DR. KILLIAN: I want to bring up some points. When research is published in a professional journal, it is brought before the peers of the man who published it. Now, perhaps it would not be accepted for publication, in the case of the AIET or the IRE. The latter is a board that goes over this material, and they may decide that it is too trivial to be published. That is not true of a governmental publication. If publication funds are available, it will be published.

DR. MORGEN: There is one implication, I don't know whether it is true or not, in your statement that you reserve the right for publication by the agency. There is a possibility that you would publish without giving due credit to the persons who conducted the research for the institution. That is one implication which, I believe, ought to be clarified.

MR. RATCLIFF: No, the matter of giving credit has to be very carefully protected. In fact, we protect the scientist in two ways. We agree to give him full credit for the work done on the basis of the publication; but, at his option, so that if we should by chance twist the results or express them in such a fashion that he is not willing to be associated with the publication, he may decide not to be recognized.

A VOICE: What right have you to twist his findings?

MR. RATCLIFF: Well, it might be done unintentionally.

DR. MORGEN: It seems to me that this raises a very important point which is relevant to this discussion. There is a distinction between publishing results in the social sciences and publishing results in the physical sciences (these remarks are directed to Dr. Ratcliff). It is quite possible, isn't it, in social science research that (1) the conclusions may be based on qualitative information, and (2) the conclusions may have policy implications. Further, almost reverse policy implications can be drawn from the same set of results by two different people. Consequently, the Government agency publishing the research results may publish conclusions of a different sort from those which an individual might publish. Since the first conclusions of a research project that reach the press are the ones that get accepted, there is some problem of publishing in this area because of the real difference in interpretation given to the same data by two different people.

DR. RATCLIFF: I agree substantially with what you said; but it has not been an important consideration in our thinking about the contract deal. We very carefully protected the contractor, so that he can publish any interpretation he wants to.

However, I don't think that is too important a point in the kind of things we are interested in, because we are more interested in techniques and processes (even in the social sciences) and in ways of effecting economies, and much less interested in basic policy, in ideological approaches to housing problems.

I would agree with you, but I say it has not been too important in this connection.

DR. SEEGER: I would like to make two observations that may be of general interest. Certain Government organizations have to be exceedingly careful about all projects under their cognizance owing to possible repercussions from industry in the event of premature conclusions. In this case, the administration is bound to be quite stringent. On the other hand, certain laboratories prefer to make their preliminary observations available at once in informal laboratory reports, which are of immediate interest to workers in the field (one or two months' time requisite), and then to submit the material for formal publication in national journals, which reach a large public (three to twelve months' time requisite).

DR. OWENS: I want to dispel any illusion that the universities do not want wide distribution of results. They most certainly do.

Dr. Ratcliff and the universities are in entire agreement that the results should have the widest possible distribution.

I agree with Mr. Lombard that proper acknowledgment of an agency's support is good evidence of the worth of the agency's activities. I believe to have seen a publication of the H H F A which contained in the text an acknowledgment to the persons and institution at which the work was done, but this was the only indication that the H H F A did not itself conduct the research. A far more prominent acknowledgment of the authorship of the work would leave sufficient evidence of the sponsorship and support of H H F A as a basis for future appropriations.

I believe that all university representatives will agree with me that there are two things that are important:

One is that you can still obtain good technical men and scientists for work on sponsored research in universities, even though they are not getting rich out of it, because part of what they desire is the prestige and the commendation, if you wish, of their colleagues in their field. This comes through publication of research results under their own names.

The second important thing on which I disagree completely with Dr. Ratcliff concerns his statement that the university scientists can publish whatever they wish afterward. After material has once been published, most technical journals—and I've checked with several on this—will not accept an article that contains substantially the same information as that which has been published previously in any source readily available. I do not believe that such precedence is necessary in Government publications.

That, I think, is the really important point. We want to be sure that research results get into appropriate technical journals, and the only way I know of that will assure that is to have them appear there first; or, if some journals will agree to a simultaneous release, so that the university research men are not kept from publishing in appropriate journals, there is no desire to keep the Government from publishing simultaneously or at a later date because we all want wide dissemination of the results.

DR. RATCLIFF: Our program obviously cannot succeed unless we have the cooperation of university scientists. We are depending on them, so that for our survival, if for nothing else, we have to work out a deal which is acceptable.

These things are always matters of negotiation, trying to arrive at an arrangement that is mutually satisfactory. But it involves some concessions on everybody's part.

I suppose that I am influenced by the fact that in all the negotiations we have had in the past nine months, we did not lose one single university contract by reason of our publication clause. That is not to say we didn't get a little resistance here and there, but somehow we persuaded the university scientist that it was not as bad as it looked. At any rate, the facts are that we did not lose any of them, and our contracts are with leading universities in the country, from Harvard, in the East, to the University of California, in the West.

So I cannot believe that it is as bad as it sounds. However, we are open-minded and we will make such modifications as are necessary to carry out our objective and at the same time protect academic freedom, in which we are just as much interested as are the universities.

— III —
RESEARCH FOR SMALL INDUSTRIES

by

JOHN C. GREEN
Director, Office of Technical Services
U. S. Department of Commerce

"Assistance to Small Business" is a popular phrase in Washington because of a genuine interest in the preservation of this vital segment of our economy and because of an equally genuine appreciation of the values at the polls involved in espousing the cause. However, it is not at the seat of Government alone that the phrase rings in the air. There are many commercial and trade groups representing a diversity of industries or a segment of a single industry which are equally vocal and vigorous in their support of the smaller competitor.

Among the types of "aids" which have been offered as panaceas for the ills of smaller firms is "research." Permit me to strike a discordant note at this conference on the merits and importance of research—to indicate a belief that the public has been "over-sold" on the benefits to be derived from this term. Certainly it is understandable that the tremendous scientific achievements of World War II, the Atomic Energy program, the new industry, television, and the many other glistening products ably reported by the advertising agencies through all information media have built up a public impression that the scientist is a magician capable of solving all ills and insuring peace and prosperity. Perhaps the scientist himself is a bit guilty too; he remembers all too well the relative obscurity of scientific workers in universities and industry prior to World War II. And it is true that where responsible people have spoken out concerning the difficulty of predicting results and the probabilities against amazing overnight strides, their comments have not been widely appreciated.

Research should be given its proper importance in the business operation. However, it can never substitute for other and equally important factors of industrial success. Capable management, a skilled labor force, a competent sales organization backed by a product of popular appeal priced to meet competition are at least of equal importance.

If we admit that small firms can fail because of normal errors of judgment, incompetency, and plain bad luck, we can take a more objective look at the role of smaller businesses in the economy and methods proposed for maintaining their vigor. For example, it is easy to argue that small firms face an insuperable disadvantage in manufacturing competition because they simply do not do a sufficient volume of business to afford an adequate research staff and facilities.

You may have heard the problem of the small business firm and research stated in this way:

Studies have shown that two and one-half per cent of sales is a typical figure for an industrial firm's research budget. That means that a fifty-million-dollar firm has over a million to spend on research every year. But the small concern whose sales would probably run less than half a million a year—and over 95 per cent of American businesses fall in this category—would have only about $10,000 available. Ten thousand dollars does not give much of a permanent research staff, nor the initial cost and expense of maintaining a laboratory.

To the extent that the preceding computations are true, however, they overlook a number of important distinctions between large and small business. For one thing, the large firm is usually working on types of research where enormous investments are necessary simply to tackle the kinds of problems undertaken. In fields like acetylene chemistry, it would do little good for a small company to invest heavily in research because a large concentration of capital and managerial talent is needed simply to capitalize on the findings. Again, small business profits time and again from big business research. Thus, the manufacturers of women's stockings did not have to develop Nylon but it was made available for their use; producers of store lighting fixtures did not have to invent fluorescent lighting, but that invention created vast new sales opportunities for them, and many relatively small radio manufacturers today are successfully using inventions in television resulting from pioneering research performed by large corporations.

Large-scale industrial research can only be afforded by large companies and we should recognize that fact. This does not mean in any sense that research is the prerogative of the "big." The small, research-conscious firm with its closely-knit management, labor, and sales forces has definite advantages in technological competition since it can "turn around" more quickly than its larger competitor and get the improved product to market fast and aggressively.

To get back to our firm with less than $500,000 a year gross sales and with $10,000 a year to spend on research. How can they produce new and improved products (the end items of industrial research) at a rate commensurate with their larger competitor with a research operation budgeted at more than our small friend's annual gross sales? There are a number of techniques, none perfect, but applied intelligently to the problems of the individual firm, they can narrow the competitive gap.

Let us analyze some methods of procuring research benefits, realizing that all is not necessarily applicable to every company. Let us also imagine that our "small business" is reasonably competent in all aspects of the business operation except research. They have just heard of research—perhaps over the radio, in the corner bar, or through their children's comic books. Naturally they want to buy some. What to do? By this intellectual osmosis they have surmounted the first obstacle—they appreciate the potentials of research and want to know how to apply it. This is truly a significant step since all too many firms take an attitude toward research: (a) we don't need it, (b) we couldn't possibly afford it, and/or (c) what good is it anyhow?

But our firm is alert and wants to take advantage of the powerful competitive tool inherent in research. First, they should have a technically trained man on their staff. He will be a part of management able to talk the language of the researcher and to interpret management's problems. This liaison runs two ways since research accomplishments will have to be effectively integrated into shop production. If our man has time to keep abreast of the literature, attend scientific meetings and keep aware of the latest developments and their possible significance to his company, continuously reporting them to management, so much the better.

At present the greatest number of technically trained graduates go into large firms. The reasons are obvious. It is the awareness by such concerns of science and technology as a competitive factor that provides job opportunities. If small manufacturing firms would systematically employ technically trained men, the problem of placing graduates in industry would be forgotten. Certainly jobs in small business with its chances for rapid advancement, exercise of initiative and early responsibility would be attractive to graduate personnel.

Next, the engineering and research departments of material suppliers are a source of technical assistance. Many firms maintain sales engineers, backed up by research personnel and facilities, to solve problems relating to the use of their material. The supplier is always interested in remedying difficulties which restrict sales or in exploring new uses which would expand the market for his material.

The advice and guidance of a consulting engineer or firm can be of great value. Consultants are specialists who can assist small firms on a part-time basis for a reasonable fee because they have a number of clients, the sum total of whose business provides satisfactory annual remuneration. They can be most helpful in ironing out production difficulties, suggesting improved methods or materials, and in advising on most fruitful areas for research projects.

The commercial laboratories provide another area of research for hire. These tax-paying enterprises are usually small businesses themselves, qualified to give technical advice and guidance at reasonable charges. They usually carry out testing of the manufacturer's products and some have facilities, equipment, and personnel to undertake research of a high order. Our technical liaison should acquaint himself with the facilities reasonably adjacent to him and those which are specially qualified in the industrial area he represents.

Next, the land-grant colleges, state universities, and other educational institutions often have highly trained faculty personnel and equipment available for industry help. Our liaison should develop contacts with these bodies in order to use them where appropriate. There is a lively argument as to the propriety of this type of research in educational institutions. Such a philosophical argument is beyond the purpose of this paper. It is sufficient to say that if the small businessman wants the technical assistance available through a local institution, he is certainly at liberty to go to the appropriate persons there and hire their facilities and assistance. It is one type of tangible help to the businessman who lacks his own research unit.

The nonprofit research foundations are a rapidly expanding type of research support for business. As a matter of fact, the charters of many of these organizations "spell out" service to small business as a basic objective. Unfortunately, lack of understanding of their qualifications and abilities has deterred small firms from making as effective use as their larger brothers. Here the research foundations themselves carry on an active program of public education designed to acquaint more and more businessmen with their existence and specific aids. Many of these foundations bring together aggregations of talent, equipment, and experience easily the equal of the research organizations maintained by the largest firms. The volume of business annually handled by a typical foundation is comparable to that of a medium-sized company in our economic structure.

The Government itself, through its diversified agencies, develops a mass of new and vital research information which our technical officer should know of and bring to the attention of his firm when appropriate.

We have found that many small firms which think they need research actually need better access to known technical information. Their problem has been solved and reported somewhere in the literature but they have lacked the ability to locate the answer. It is in this type of small-business technical assistance, short of research, that the Federal Government, our great libraries, and the engineering societies perform a needed service.

Again, the National Bureau of Standards maintains a "Research Associate Plan" whereby industry groups are able to maintain personnel at the Bureau to participate in related work and take advantage of the unique facilities and experience available there.

Trade and industry associations are more and more sponsoring "cooperative" research wherein a number of companies jointly underwrite a project of benefit to all. Here there are no arbitrary barriers of size of firm; all may contribute and enjoy the results. This is one of the most important techniques whereby a small, alert firm may help shape a research program of direct benefit to them at a relatively modest expenditure.

It should be noted, however, that "cooperative" research is usually devoted to overcoming fundamental problems which hinder the growth of an industry. It does not encompass the development of a new product.

Product research is directed toward the development and commercial introduction of a new item of manufacture. Here the businessman is faced with the task of creating a market and selling competitively. He wants patent protection to justify his investment risk and naturally is uninterested in a cooperative venture with sharing of results.

Today product research, which leads to diversification of industry, the replacement of "slipping" sales items with new and better ones, is vital to the small manufacturer who aims to continue as an aggressive competitor. However, he must perform this type of research in his own plant or hire on a contract basis the services of a qualified research organization. This latter is common practice, and the ethics of the laboratory personnel in university, private laboratory, research foundations, or others insures that proper secrecy and patent protection will be obtained.

Within the Federal Government, the U.S. Department of Commerce, under Secretary Charles Sawyer, is the branch concerned with the fostering of business—large, medium, and small. In the Department we often think of ourselves as a "clearing house" of nonconfidential information to business. This information falls into many categories—statistical, marketing, foreign trade, economic, technical, and scientific. In the last-named field, my own office is in daily contact, either directly or by letter, with small firms all over the country. As earlier expressed, we find an urgent need for education in small business (a) as to the values of research, and (b) how to effectively apply research to their own operations.

As one modest step in this direction, we embarked on an experiment designed to bridge the gap between technical problems brought to our attention by small business and the many types of research assistance available if effectively employed. We viewed our role as primarily that of a "catalyst," organizing these problems in a form suitable for research, then bringing together the small businessmen primarily concerned and the interested research organization. After thorough discussion with men in industry, Government, and educational institutions, the following principles for the program emerged:

1) It should be undertaken at the local level with a minimum of Federal participation.
2) Representative advisory councils of competent and respected men should be enlisted. A typical council might include a financier, a publisher, a small businessman, and a research specialist.
3) These councils would undertake the task of handling research proposals, developing suitable programs, including financing, and, in general, stimulating research for business in their area. In performing this function, they would take advantage of all qualified and interested research facilities, public and private.
4) When the problem could not be handled on the local level, the Department of Commerce and/or suitable Federal research agencies would be invited to take cooperative action.

A number of state industrial commissions, manufacturing organizations, and like groups expressed interest in participating, and we have been moving slowly in this direction.

A legislative development which will be of interest to this group is contained in the Small Business Act of 1950. This legislation, S. 3625, is designed to offer assistance to small business on a variety of fronts. In "Title V" of the bill, section 503, the following language will be found:

> "The Secretary is hereby authorized to undertake, through the National Bureau of Standards, other Federal laboratories, nonprofit research foundations and educational institutions, or other facilities available to him, engineering and technological research on industrial, commercial and related programs of interest to small business on nonagricultural commodities and products. No such project shall be undertaken unless he finds that it is unlikely that the objective of such project will be equally well achieved within a reasonable period of time (1) by private enterprise or (2) by any other research development undertaken or sponsored by the Government or other public authority."

This language is quite general. It would appear that the Federal Government participation in research for small business of a financial nature should be exercised only on projects thoroughly supported by a broad base of business and in which the business firms themselves agree to participate, both financially and administratively. The Federal grant, as I see it, should be an incentive to small firms which are not doing research now to tackle cooperatively some of the basic problems which have been holding them back. This would seem as far as the Federal Government could properly go in the field of business research without finding itself in competition with existing private firms.

Since the type of research to be undertaken would be cooperative in nature, the development of new products with attendant patent problems would fall outside of the legislative purpose. Accordingly, I do not foresee any very difficult administrative problems should the Congress enact the legislation in its present form.

I should mention that the international situation and mobilization planning has brought up for consideration an operation modeled on the Office of Production Research and Development which was a component of the War Production Board. The OPRD supported research by contract with qualified facilities on methods and processes to speed war production or provide acceptable substitutes. If such an activity is again created, it probably will be within the Department of Commerce which has the OPRD records and was assigned the responsibility of liquidating that operation when the War Production Board was dissolved.

The research provisions of the Small Business Act of 1950, mentioned earlier, and the creation of an Office of Production Research and Development should not be confused. The pending legislation contemplates a normal, peacetime activity designed to assist small firms which are in difficulties and need incentives to join together and plan research. The OPRD type of operation is geared to national mobilization; it seeks information which will strengthen our industrial capacity for defense. The two might employ similar techniques of project planning and administration; however, their basic objectives are distinctly different.

Summary of Addresses and Discussions

ERIC A. WALKER
Research and Development Board
Washington, D.C.

I HAVE SAT through four of these conferences, usually trying to get somewhere in the back of the room, where I can get the feel of the conference rather than the feel of the individual talk, and at the risk of using an analogy which isn't too good, I have a feeling that this series of four conferences can be described, if you are musically-minded, pretty much as a symphony, discordant at times, but pleasing in its overall effect.

The symphony that this conference has reminded me of most is Dvorak's New World Symphony. Perhaps most of you know it. It has a theme of six chords which starts in the first movement. You hear it once, then you hear it again. Finally, in the fourth and last movement, the theme takes over. The annotators of this symphony have often called this, "The Call of the Future."

There is in this whole series of conferences, a call of the future. The first time I heard it was in the first conference, in what seemed to be a perfectly casual discussion on the part of Maurice Holland, when he said, "The vice president in charge of research is the vice president in charge of the future."

The theme recurred spontaneously a number of times last year, and this year we heard it again in a number of different guises, "The vice president in charge of research is the man who plans the future of the company," "Not to do military research is to endanger the future of the country," "The director of research should look upwards and not downwards; he should look upwards towards the development of his company."

This note has been struck again and again. As a matter of fact, Thomas H. Vaughn, Vice President for Research and Development, Wyandotte Chemicals Corporation, in his talk on Monday morning on calculated risk, quickly sounded the theme, risk on research is the risk on the future health of the organization. The Board decides the areas in which the research organization shall operate, but from then on the research director is on his own.

"The research director," says Tom Vaughn, "shall make a technical evaluation of every new research project. Can we as a technical organization do it? Is the product in our line? Do we have the manpower to carry it through the production stages? Will we have the raw materials, can we get the capital, and where are the markets, new or old, where we shall put this new product?"*

And then an economic evaluation: "What are the ratios—and for this we use the ratios of similar companies in our line—and what is the ratio of sales to investment for companies in our line? What is the return on our investment, ten per cent, twenty per cent, or some other per cent? What is the profit on sales, eight per cent, six per cent, or minus per cent? And if we are out of line, should we get into line and how should we do it?

So the research director makes out a prospectus for a project on research, and in this prospectus he puts himself and his staff on the spot. "What will be the sales per year from the results of this project? What investment will it take on the part of my company? What profits can we expect to make? How long will it take us to get there? What will be our materials position?"

From these individual project analyses, we put together a composite research picture and a forecast of the future of the company:

"What projects must we undertake to attain this position? What per cent return can we expect as a result of my research program? What chance have we of achieving technical success? And then, if the direct returns are low, what is our chance of over-all success?"

This same theme was taken up by E. Duer Reeves, Executive Vice President, Standard Oil Development Corporation. He said many of these things in different words. First: "Research, if it is to be undertaken by industry, must be useful. We must have the supporting technical organization to carry through to the end. We must know what is needed, and we must have the courage to take the risks.

"We should try as research directors to aim for high usefulness per dollar of cost, in other words, high return for dollars put in. However, we should not for-

* Virtually all the quotations in this summary are paraphrased.—Editor.

get that there are subsidiary results of any research program. Sometimes we need to complete a line so that our sales organization will be happy; sometimes we look for prestige effects; but in calculating these things, let us not mislead ourselves on cost."

"In chemical industries the cost of a research program is not just the cost of operating the laboratory. Someone has to build a pilot plant. Pilot plants cost money, and pilot plants must be tested, and although very often we can slough this cost off on the operating department, it is still a real cost."

"What is the final plant going to cost us in new capital, and if there is a risk—and there is always a risk—what is the equivalent cost of the research project? This equivalent cost can be defined as the total cost divided by the probability of success."

We have been led to believe, by the figures presented by these two chemical concerns, that the research directors are exceedingly good; because the probability of success was always high, sixty or eighty per cent.

And the desirability of the research project, what is that? Is it the chance of return divided by the cost? If the sum is positive, we should undertake the project.

A research director is also charged with making up a balanced portfolio, just as a banker or trust-investment officer is. We need some bonds in the portfolio, whose return may be low, but sure enough. We need some blue-chip stocks, whose return is good if they work out our way, and then, if we have any adventurous blood, we will want a few gold-mine stocks —those that will pay off handsomely if only they work.

And finally, we have to provide for this research program and for the program of our company, a steady flow of men, money, and materials.

Donald H. Loughridge, Senior Scientific Adviser to the Department of the Army, took another definition of calculated risk. He said that it leads to that action which is likely to produce a certain exposure to loss. It was twenty-four hours before I caught on to this and saw how applicable the definition is to military research. For if you do research, you may expose yourself to some degree of loss; but if you don't do research, military loss is inevitable.

Then Loughridge reviewed the military course of action leading to the decision as to the categories in which research must be done—air defense, strategic air warfare, land warfare, and so on. I might add as a commentary that we have spent a lot of our time on strategic air and air defense operations, and in the present war we have not had to use them.* Perhaps we should have stressed land warfare, but who can say what might have happened if we had not spent our money on air defense? It might have turned out to be what we needed. How does one measure the risk of this sort of decision?

* These remarks are as of September 13, 1950.—Editor.

After the military set up these categories of research, they tell us what the desirable technical objectives are. What do we hope to do in these categories of research? And then the three departments must subject their programs and budgets for achieving these technical objectives.

In the Army, the technical services send their proposals for research and development to a review board for approval. The review board must consider the risk of exposure to a loss if the research is not undertaken.

Many of us in the military are concerned about basic research. In this area, it is exceedingly difficult to calculate our exposure to loss, for the only purpose of basic research is to add to our store of knowledge. Development, which is the greater part of the military's effort, is a careful application of that store of knowledge. In the military, we do little basic research; it is approximately ten per cent of our total effort. We can only hope that the universities will provide a reservoir of basic research sufficient for the national need.

Monday afternoon we talked again about the evaluation of research. Allen Abrams, Vice President of the Marathon Corporation, continued this theme of evaluation, which many of you will remember was started by Olsen of Olin Industries at last year's conference. He said research and development is usually a very small percentage of the total outlay of any company.

"We can measure the products of our engineering department, our manufacturing department, our sales department, but it is exceedingly difficult to measure the results of the research department. This is tough on the research director, and it is tough on the individual researcher, because he would like to know just how much good he is doing. Furthermore, the growth of laboratories in dollars and in facilities depends upon the returns of research. Yet in spite of the difficulties of accurately measuring those returns, the growth of laboratories in industry and in Government, as well as in universities, has been phenomenal during the past twenty years."

Olsen commented that a questionnaire showed that nearly everyone was for research, and that many companies have formal methods of measuring the results of research; but it is quite evident that these formal methods are quite arbitrary. Although they produce a very good base line from which a single research director in a single company can measure progress from year to year, these base lines are not applicable to other companies, not even to other companies in the same line of endeavor.

"There is only one real measure of the results of research," says Abrams, "one which will stand the test of history: Will it produce a new industry, new goods, new happiness, and most of all, a better world than we had fifty years ago?"

Rear Admiral W. S. Parsons, of the Weapons Systems Evaluation Group, spoke on operational research which began in the military and is rapidly spreading to industry. He quoted Dr. P.M.S. Blackett of England, father of operational research:

> "Many war operations involve considerations with which scientists are specially trained to compete, and in which serving officers are in general not trained. This is especially the case with all those aspects of operations into which probability considerations and the theory of error enters . . . the scientist can encourage numerical thinking on operational matters, and so can help avoid running the war by gusts of emotion . . ."

Admiral Parsons gave an historical outline of the growth of the Weapons Systems Evaluation Group, sketching how it was first an idea and is now an organization in being, and he reiterated that it is very difficult to estimate the cost of defeat or even the cost of forestalling an effort on the part of the enemy.

"We can calculate the cost of sinking a battleship, or we can measure the cost of downing an enemy plane; the method is really very simple. We set up a paper problem based on numbers and probabilities, and work out the cost of carefully achieving the desired result. Then, admitting that this system is a paper system based on theory only, we look around in our experimental laboratories—and for this type of operation our experimental laboratory was World War II—and find out what results our own records and the records of the enemy show. By making a comparison of our paper probabilities and paper analysis with the actual facts we have some test of the validity of the method before we attempt to use it on new problems."

Admiral Parsons ended on this thought, which I think is worth repeating: "But in the over-all view, the measure of return from research in this anxious decade will be the degrees to which it increases our national stockpile of flexibility, resourcefulness, and alertness, both in the human and the material fields."

Guy Suits, Vice President of the General Electric Company, took us quickly to the other end of the scale and showed us by object lessons the difficulty in measuring the results of basic research. He started by saying that it is perfectly easy to measure the results of a piece of research which has as its object the substitution of a die casting for a fabricated part. But looking at research in semi-conductor solids or work in silicons, where they began by trying to replace a carbon molecule by one of silicon—how can one measure such research?

Suits continued: "We thought this would be a good idea, because the silicon bond gives us a higher temperature stability of certain materials, but we never thought that it would give us bounceless rubber and rubber which would be compatible with pyranol in transformers.

"At times we do research in the phase of increased costs, because there are other benefits to be derived from such research. Let us take a look at Formex. Formex costs more per pound than any other enamel for insulation, yet because we can pack more copper in a given space, and because we can wind it at higher speeds, we chose Formex instead of enamel. This again is an illustration of the difficulty in calculating the results of more fundamental research."

Then he gave his story on electric blankets, and left us with the question, "How would you measure the results of that research?"

Last of all, Suits talked about Project Cirrus, weather modification. It looks today as though they are doing an awfully good job on that particular type of research. Again I would like to quote his closing remarks, for they were well chosen.

He said: "I had developed the thesis that measuring the return from exploratory research is difficult because of the manifold forms in which the beneficial results appear, and because of the importance and interrelated contributions of engineering, manufacturing, and marketing. I should like to leave no doubt on the one point, however, that although contributions of many are vital to the practical utilization of a research result, it is the scientist himself who produces the vital foundation on which the whole structure of research rests, namely, the new fact of nature. The scientists and the laboratories that have the freedom to follow their curiosity in search of new facts are among our most important national resources."

Most of you heard the talk by Dr. James P. Adams, Provost of the University of Michigan, at the dinner on Monday evening, in which he brought out the fact that our geographical frontiers are ended, that we are left now with a desire to achieve the best that can be accomplished, and that this leads through research to new knowledge, new materials, new packages of power. "This," says Professor Adams, "is our new frontier."

James Zeder, Director of Engineering and Research for Chrysler Corporation, gave us some new slants on the problem, the key to which we find in his title, "Director of Engineering and Research."

He started by saying that in the Chrysler corporation there are four types of research activity: (1) product testing, (2) product engineering and development, (3) engineering research, and (4) academic research.

Zeder confined his remarks to the third, engineering research. He said: "A research director must be an organizer and a leader. He must sell but not oversell. He must recognize the important facts and not confuse them with toys. He must not hang on too long, and he must not drop off too early.

"From the viewpoint of the company, he must consider: (1) how will the company use the products of this research? (2) have we got the dollars to exploit them? (3) is it timely? and (4) can we do it?"

He said in closing that he thought we should not spend a given percentage of our budget each year, but we ought to spend on research when the going is tough, and go easy when the going is easy. He introduced one thought that has bothered me a great deal and which I might bring out here to make sure that I understood it. He said that in the early days the ratio of dollar returns per dollar spent on research was very high, but in the past few years salaries have been rising, the materials and equipment that a research man must use have been going up in cost, and I think he said the easy things that can be done have been done, all of which result in the higher cost of research.

Now, if I heard right, this means the ratio of return per dollar spent is coming down, and if this figure of $1.35 as called for or quoted in some of our earlier papers is correct, it isn't far from $1.35 to $1.05 and to 95 cents. And when we get below the dollar mark, will industry continue to do research, and why?

On Tuesday morning we gathered to talk about what is required of a research director. This meeting might well have taken on the aspect of a whitewash, in which several of our prominent research directors, certain that they were good research directors, could have described their own characteristics, enhanced by their own peculiar point of view, and thus reassured themselves, and incidentally reassured us that all of us are safe in our jobs.

However, the program committee in its all-seeing wisdom, made sure that this would not happen, because it called in a rank outsider, John C. Flanagan, Professor of Psychology at the University of Pittsburgh, to tell us what made a good research director.

Flanagan, making no use of a couch, a supine position, or a darkened room, and making no references to complex, libido, or prenatal frustrations, proceeded to describe an objective measurement of the characteristics of a good and bad research director as judged by research directors themselves.

The measurement consisted of an examination of critical incidents on the job, in which the observer, familiar with the subject and the subject's job requirements and his objective in the particular incident being reported, judged why the accomplishment was good or bad.

The test has been used on a group of colonels and generals, and also on scientists, rates P-5 to P-8. The comparison of a colonel to a P-5 and a general to a P-8, provoked considerable amusement and interest.

The results of these tests do not yet allow us to make comparisons, except in a preliminary way, but they show that a scientist is expected to treat a new idea more tenderly than a general or a colonel would; and that usually the scientists are less handicapped by personnel problems than are the military. However, everyone seems to think that planning, or the ability to plan, is an important attribute in both cases.

If I may be permitted a personal observation in this summary, I could not help but notice the importance in all these analyses of a pat on the back applied often enough, vigorously enough, and high or low, as the occasion demanded.

Raymond Stevens, Vice President of Arthur D. Little, asked us to take a new look at the problem of what a research director does. He is not charged, says Stevens, with the production of new gadgets. He is perhaps the vice president in charge of the future, but more truly he is charged with the development of the company.

It is obvious he should be able to run his shop, to determine what research is to be done, how it is to be done and who is to do it; but far above this, he should have a knowledge of economic, human, financial, and Government matters, which will enable him to meet other officers of the company on their own ground, and to make his wishes heard in the determination of company policy.

In successful companies, the man who is responsible for research holds a position more important than is implied by the title "director of research." He helps to manage the company. His training and method of thought will assist him, for "his industrial corporation should be a living thing, recreating itself, adapting to changing conditions, growing where growth is desirable, alive and healthy."

The corporation cannot be lively if its functions are static.

This concern for the future is not the primary concern of the sales, production, and personnel men. It is the job of the company's research officer, who must interpret his staff findings honestly and clearly for his lay associates and who must interpret management policy and finance to his staff for guidance and direct them toward proper targets.

He must have breadth, vision, and something of the urge of a crusader, something of the enthusiasm of the pioneer and promoter, tempered with an awareness of practical possibilities, and with honest and realistic allowance for the limitations of men and money.

This is no job for the small or lazy man.

Albert E. Lombard, Jr., Scientific Assistant, Air Force Directorate of Research and Development, contributed his thoughts as to what is needed in a research executive for the Air Force:

1) He must understand the role of the Government and the Air Force in research, as well as the role of industry and of private endowment organizations, and this is the tightrope he must learn to walk.

2) He must find the most economic program by making and using a time table of interlocking developments which will produce no major upheavals.

3) He must not produce a plane and forget the armament. He must not produce a reconnaisance system and forget the camera. He must understand the objectives of the Air Force to provide rapid and effi-

cient transport, to locate and recognize targets, to hit the targets, to cause destruction, to operate independently of weather and darkness, to counter enemy interference, to communicate air-to-ground and air-to-air, to defend our home territory, and to utilize the money and personnel effectively.

He must obviously promote a research program; and he must plan the facilities over a rather long time-scale, because of the length of time it takes for construction.

One of the most satisfying of the talks was devoted to a subject which we decided a year or two ago we ought to bring out into the open, namely, this bugaboo of overhead. Wilbur K. Pierpont, Controller of the University of Michigan, pointed out that approximately one hundred million dollars was being spent by the Federal Government in universities for research every year. This has created a need for new facilities and has added to the load on existing facilities.

The results of Government research differ slightly from those of other jobs, but the costs of the research are just as real as the costs of manufacturing or anything else. These are arbitrarily divided into direct labor costs, other direct costs such as supplies, communications, and so forth, and a catch-all known as indirect costs.

The measure of the indirect costs, or overhead, is determined by what the accountant, comptroller, or other fiscal officer decides can be juggled and can differ in any way you wish to juggle it. This is not the point of overhead at all.

If the Government wants the work done, and done at a given place by a given man, then the overhead should not figure in the argument. The only question is: Is the requesting agency willing to pay the proper costs?

Some schools, especially in the field of medicine and public health, fool themselves in this manner, and they will fool themselves into bankruptcy; or else they will have to rob the student who pays the fees, or the state which gives the appropriations, to make up the difference.

A uniform overhead figure for all universities and all governmental agencies does not appear feasible and probably is not desirable; but we might agree more generally on what is direct and what is indirect cost.

Doctor C. C. Furnas, Executive Vice President and Director of the Cornell Aeronautical Laboratory, in the discussion of this problem, concurred in many of these arguments, and added that some schools, by diverting their best teachers to research, are facing not only economic bankruptcy but academic bankruptcy as well.

Doctor Seeger raised some points that I think are well worth bringing out here. He pointed out that the conditions for applied research, on which this whole discussion seems to be based, might not necessarily hold for basic research; and basic research is what the universities and the foundations support.

The attitude of the Office of Naval Research is that they are helping support a man where he is, doing what he is doing, because the Department of Defense is interested in basic research. And because that particular basic research might be useful to the Department of Defense, they are willing to pay part of the cost. It is important to notice that in the support of basic research and in the support of applied research the objectives are different, and it is reasonable to expect that the cost angle would be treated differently.

There were four or five conferences in the afternoon. I have received the minutes on these conferences and would have liked to include them if time had permitted. I strongly urge the committee in charge of getting out the results of this conference in the form of proceedings to publish them as part of the minutes. They are as important as any of the speeches.

The first conference on measuring research returns was run in two sessions by Abrams and Brothers. They pointed out that one measures the research returns for five reasons:

1) To be sure that research and development get the credit for the returns of research. I gathered that this was desirable in order to get funds for research.

2) Such measurement is necessary in order for the directors of the company to justify research and development to the stockholders.

3) It is necessary in order to get capital for building new laboratories.

4) It is desirable in selling research to companies who do not have research programs.

5) It is desirable because such a measurement is a check which the research and development director can use on his own activities.

They then went on to discuss the procedures and such hidden gains in a research program as not having to expand the plant because the yield can be increased as a result of a research program.

This particular conference succeeded in listing sixteen different kinds of gains which could accrue from a research program.

The second session, under LeRoy Brothers, attacked this problem from an entirely different point of view. They began by pointing out that you cannot apply to fundamental or basic research any indices discussed in the meetings, because worker satisfaction and worker morale is probably the biggest reason for undertaking that type of research.

In applied research, one deals with many things, the dollar return, the saving of manpower, the saving of casualties, the use of strategic materials, capital requirements, human relations, etc. Any attempt to find an efficiency figure will be a difficult one and one which has to be done on the long-term basis.

I am going to read in toto another set of minutes on what is needed in a research executive, because it is a charge to the conference.

"It is generally agreed there is no valid concept of an ideal research executive, inasmuch as such an individual may be called upon to perform many functions, the exact pattern depending upon the specific job and its place in a particular organization.

"The essential responsibilities seem to be: (1) Obtaining ideas, that is, planning; (2) selling ideas up and down, that is, promoting; and (3) administering the resulting program, including supervision, training, and so on.

"A research background is regarded as essential for such an executive. Most individuals were of the opinion that agreement could be reached as to the primary elements listed in the research executive. The ability and desire to handle people was stressed.

"The primary requisite at present is the unavailability of a reliable list of items in a usable form for evaluating and selecting research executives. The concepts must be defined precisely and in as quantitative terms as possible. Records of specific instances of good and bad performance were recommended as an initial procedure."

The Round-Table group recommends that the conference request Dr. Flanagan to conduct an investigation to ascertain a short list of critical requirements for a research executive, on the basis of information to be solicited from members of the conference.

At another conference, on overhead, five points were brought up:

(1) What is sponsored research?

(2) What items should be included in overhead?

(3) How can we distinguish clearly between costs determined by accounting procedures and the amount paid?

(4) If contracts are accepted for less than the total cost, then a university must recognize its monetary contribution; and

(5) A university also should recognize the nonmonetary rewards of a research program, such as publicity, patents, contributions to teaching, etc.

The conference on calculated risk agreed that charts are primarily for the use of management. Rising costs bring pressures on research directors to find ways of cutting costs. Very few industrial companies are doing long-term research but seem to be concentrating on the near future. Is this where the Government and universities may have to step in to fill a gap?

Calculated risk in some industries, such as the chemical industry, seems to be low, but here the cost of pilot plants is high. Is the converse true in other industries?

Then we came to this morning, which I will summarize quickly, because most of you were present.

Tom Killian, Director of Research for the Office of Naval Research, spoke on the National Science Foundation, making three points. He described the gestation period of the National Science Foundation, during which the O N R and the State Department Scientific Office have both been born. If the National Science Foundation is established, O N R will have to decide on some meeting ground with it. Is is obvious that the applied research program would not be transferred to the Foundation, and those things which the Navy considers basic research should not be transferred, nor should those programs which are used by the Department of Defense as "scientist catchers," to interest good scientists in the military program.

Richard U. Ratcliff, Director of Housing Research of the Housing and Home Finance Agency, said (and I am liberally paraphrasing his remarks):

It has been said that one of the criteria by which we decide if the Government should do research is the portion of the population that it benefits directly. The higher the proportion of the population to benefit, the more responsibility there is for the Government to support that type of research.

We all use houses. It is therefore comforting to see a program of research on the physical aspects of housing getting under way. The problem of publication of the results, I can only say, was left unresolved.

John C. Green, Director, Office of Technical Services, Department of Commerce, pointed out that aid to small industry and business is a catch phrase, almost as research is; and so the two can be linked together in aid or research for small industry, but research is only one factor in the success of a business. Sales, promotion, skill, and so forth, are also factors.

Most small firms operate around the level of one hundred to five hundred thousand dollars a year, and if we use the usual percentage of gross sales for research, we end up with about ten thousand dollars per year available for research in many of these small industries. All research directors know that ten thousand dollars per year buys about one-half a senior scientific man.

Many of the small companies depend upon the results of research done by the larger companies. Many small companies want research because other small companies have it and seem to be successful, while they don't have it and are not successful. Many of them will be able to get the benefits of research through the use of one man, or through the use of part of one man whom they can get through universities, research organizations, and science foundations. Most small industries need access to the results of research, and therefore it is probable that the Department of Commerce will take every step to put those who need research in touch with the research that has been done and with the people who can do research for them.