An Executive View of Researc

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ANY DISCUSSION OF MANAGEMENT'S VIEWS of research must be coldly practical, down to earth, and deliberately far from the technical or romantic, because management itself must be all of these things. The first problem in the operation of research in any field is the reconciliation of the viewpoints of those in the theoretical phases of research and those in the less dramatic, possibly more practical approaches to research problems. We are led immediately to look for answers to the question: "Why do people do research?" It is certainly evident that there are two very basic attitudes, which supply apparently simple, although really very complex, answers to this question. Sometimes, and I am sure too seldom, we find a research man who is a purist doing research of a fundamental nature for the love of the field itself. If this question were addressed to him the answer might be like that of a famous mountaineer who when questioned as to why he wanted to climb Mt. Everest, answered: "Because it is there." In this day, the "because it's there" attitude toward research is not sufficient to provide laboratory facilities, salaries, and operating expenses required for modern programs.

On the other extreme, we find the practical, hard-fisted, financially minded businessman whose reply to the same question would certainly be: "To make profits and to enable me to pay dividends to my stockholders." Somewhere between the two, there must be a practical solution. When we find this solution we will be far along the road to perfect management-research understanding and the research director will have less difficulty in justifying his requests for appropriations. Before stating my beliefs concerning management's viewpoint toward research and some of management's responsibilities related to research we might give some thought to the past.

Progress in a Half Century

Forty to fifty years ago certain animal products and products of agriculture were alleged to have definite therapeutic values. How many of us have heard: "Eat your spinach; it's full of iron and will build you a big, strong body"? Who, at that time, could say that the iron was sufficiently metabolized, or that it had any effect whatsoever in connection with body building? The iron content was known but the metabolic fate of this iron was largely supposition.

In the same era, we were told to eat our fish because fish contained phosphorus and, consequently, was a brain food. The phosphorous content of fish is not radically higher than that of many other foods and at the time of these statements the iodine content and its effect on the thyroid were not considered. The Eskimos, who are notorious fish eaters, have never been particularly noted for their mental ability, but they are subject to other dietary deficiencies.

These miscellaneous statements regarding iron and phosphorus represent problems that could be, and were, definitely settled by the application of what is now fairly simple nutritional research. Sometime between 1900 and 1911 the minimum requirements for iron were established and phosphorus was recognized as a nutritional essential.

In the year of 1893 the average Yale freshman was 19 years of age, 68 inches tall, and weighed 138 pounds. Today he is younger, taller, and heavier—averaging 70 inches in height, weighing 156 pounds, and entering college at the age of 18 years. His feminine contemporary is an inch taller and somewhat more rugged than her mother was at an equivalent age. The application of a higher standard of living has slowly begun to have its effect. Future increases in stature will doubtless take place at a lower rate, but they will continue as nutritional theories, derived from research, are put into practice.

A half century ago we were approaching the refrigerator and aspirin tablet age. It was recognized that ice was a satisfactory product and that it would contribute to food preservation. But it was difficult to come by. Canning was practiced to provide winter food; but only at the expense of hard labor or high

Management must take a hardheaded, practical attitude toward research, but it must keep in mind the importance of fundamental studies and the possibilities in the support of research outside company laboratories

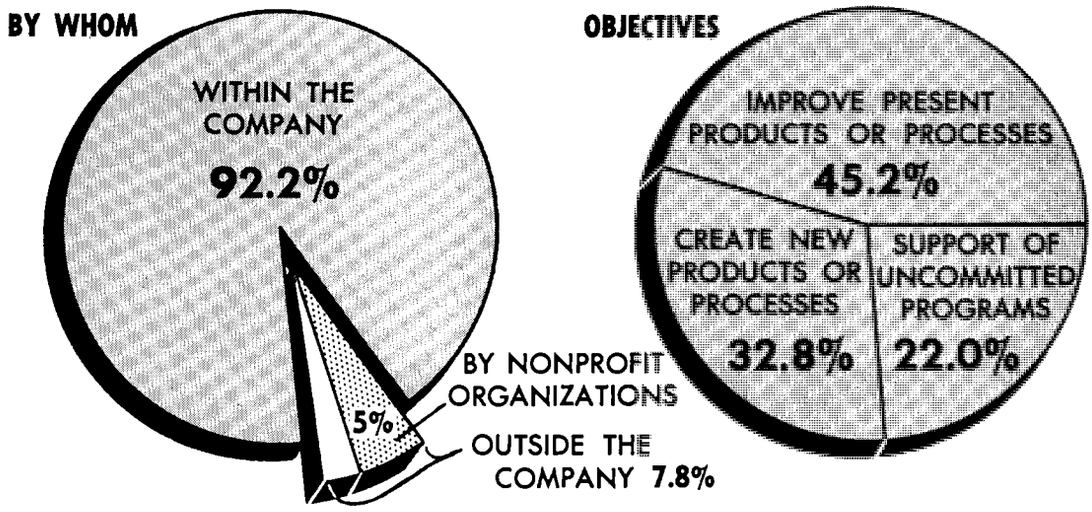
in the Food Industry

INDUSTRIAL RESEARCH SPENDING[a]

	1952 Median Figures for 191 Leading Companies			
	% Research to Sales	Increase in Spending over 1951	Dollars Spent per Research Worker	% Professional to All Research Workers
ALL FIRMS	1.3%	13%	$ 8,400	46%
INDUSTRY				
FOOD AND KINDRED PRODUCTS	0.3%	8%	$ 8,300	46%
Textile and Apparel	0.7	7	8,200	39
Furniture	0.5	9	6,500	19
Paper, Lumber, and Wood Products	0.7	15	7,100	49
Industrial Chemicals	2.9	15	9,400	45
Drugs	4.9	12	9,600	46
Paints	1.7	8	6,800	52
Miscellaneous Chemicals	2.4	9	7,700	39
Petroleum and Coal Products	0.7	14	9,000	43
Rubber	0.9	21	8,500	66
Stone, Clay, and Glass	1.3[b]	14	7,700	37
Primary Metal	0.9	11	8,500	40
Fabricated Metal	0.7	10	9,600	53
Machinery except Electrical	1.4	12	7,300	39
Electrical Machinery	2.7	17	8,200	44
Transportation Equipment	1.4	9	9,700	36
Laboratory Instruments	3.0	40	7,300	37
Mechanical Instruments	2.0	11	8,900	52
Other Professional, Scientific, and Controlling Instruments	2.0	14	10,000	50
SIZE				
Less than 500 Employees	3.4%	10%	$ 8,200	59%
500– 2,000 Employees	2.0	16	8,100	49
2,000– 5,000 Employees	1.4	12	8,000	45
5,000– 7,000 Employees	1.2	12	8,200	50
7,000–10,000 Employees	1.5	13	8,500	46
10,000 Employees or More	0.8	14	8,700	41

[a] Based on study of 191 leading companies in 19 industries. Reproduced from "Spending for Industrial Research 1951-52," published by Division of Research, Harvard Business School. Reproduced by permission of copyright owner.
[b] Estimated.

COMPANY FINANCED RESEARCH IN THE FOOD INDUSTRY



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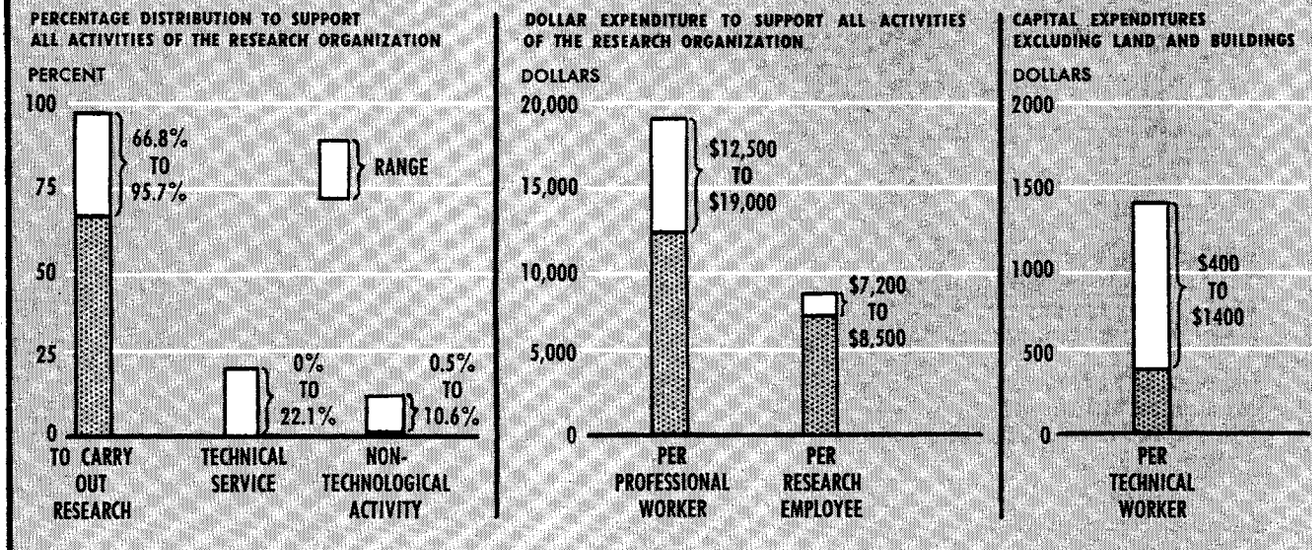
EXPENDITURE RANGES



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cost. The medical profession lived on a relatively few drugs and most of these were based on trial and error or had come down through history. Physiology was studied but the function of various glands was not understood; bacteriology was in its infancy; the synthetic organic chemical industry had not started. The vitamin pill was unknown. Some packaged foods were marketed but by and large the food industry was just beginning.

Today's Situation

Where are we today? The facts concerning our advances in 50 years surround us; we are deluged with them by the radio, television, smoke writing, highway signs, the press, and through innumerable other media. The scientific journals themselves are now too much for assimilation; abstracts alone now take more space and weigh more than the journals of 50 years ago. We have reached the day of abstracted abstracts. This problem is one that management and research must overcome and its solution requires more research. Microfilm, punchcards, computers, machines—all have provided a partial solution, but our warehouse of knowledge in any field grows faster than our ability to classify, disseminate, and comprehend. Specialization was once thought to be the answer but we have now reached the point where there must be further subdivision of specialized fields. It seems inevitable that many facts will be lost and rediscovered time and time again; and this situation plagues not only the scientist, but also the engineer, the banker, the lawyer, and all their contemporaries.

Management's Expectations

What does management expect from research? Management expects economical research. Research facilities are costly and require continual replacement and continuing capital investment. Research manpower is short in supply and expensive to procure; modern hiring methods for technical personnel require the expenditure of hundreds of dollars or thousands of dollars per person hired and this cost increases year by year.

Once a laboratory is established the most important continuing expenditure is manpower since it is not infrequent for salaries and wages to constitute 80% of the research budget exclusive of overhead. It is difficult to get the research worker to understand this and frequently his efforts to save money by spending time result in an over-all loss.

Proper utilization of manpower is the most important single factor to be considered in providing economical research. Technical personnel working inefficiently constitute a more-than-complete loss, since not only is their time nonproductive but the funds that paid for it could have been used in other ways to guarantee certain productivity.

Generally, management is not allergic to the devotion of funds for research operations, since management realizes diversification and expansion of the product line is essential to continued corporate existence. A sound management, however, will not be sold on fads, nor will it generally accept the problems of marketing short term products. But any proposal that has a reasonable chance for success is usually received enthusiastically. Any product suitable for management's consideration must fill a need, must have a place in the market, and there must be a reasonable chance that its development and production can be achieved within a few years or, preferably, months. When development of the products of research is slow, the market is sure to be lost to competition.

Frequently, during the economic discussions concerning a new product, the statement occurs that "we must take a calculated risk." I have heard this many times and always challenge it because I have yet to see any of the calculations involved in the background. In other words, a calculated risk is most frequently a wild guess, rather than a carefully considered economic solution to a problem.

Project Selection

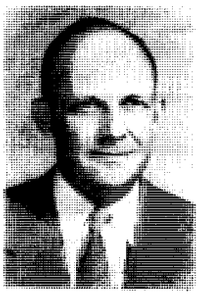
The question of quality *vs.* economy frequently occurs early in research project discussions. In my personal opinion, initial research should be directed toward quality alone. In the early stages, the research man should not be plagued with problems of economics, but should be allowed to produce the finest material possible without regard to cost. Once the product has been created, the fight toward cost reduction, preferably without quality sacrifice, can begin. It has been often emphasized that raw material cost has little effect on the economics of bringing a product to market during the early years of its production. This is particularly true in days of high capital investment requirements and high rates of taxation in the presence of relatively low rates of depreciation.

Management expects practical research. Research outside of the field of endeavor may be interesting but is seldom fruitful; in other words the research organization of a food company should base its work on the raw materials available to its management and direct its efforts toward the market outlets normally used. Long term projects may be based on other raw materials and management may build new facilities for products radically different from the normal line and create the marketing organization to service these products. Management must understand this from the very beginning of a research project. A completed research project directed toward manufacturing locomotives would not be greeted with enthusiasm if it were presented to the executive committee of a food company without prior warning, even if the research were well done and the project was economically sound.

The ability to choose a practical project for research is a gift with which few persons are endowed. There is certainly no dearth of ideas, for each individual development creates a new field of its own, and frequently improvements on products have greater value than the original development. It is essential that an idea be conceived and given a preliminary evaluation before a research director can subject it to practical evaluation, but before a project can receive serious management consideration it must be studied carefully from many viewpoints. The research and development cost must be estimated, market estimates must be made, the competitive features studied, and some estimate of capital requirements is definitely necessary.

If the embryonic idea can survive these studies a sound management will back the research and development work fully and enthusiastically. If the idea does not pass these tests both the research organization and its management are far better off to drop it entirely. The stockholder's money goes into more productive lines and nothing can react more unfavorably to any research group than a series of near hits; they not only discourage management but ultimately they break the morale of the research team. Practical projects seldom come from individual strokes of genius; but more often from rigid research and management appraisal.

Robert G. Ruark became a vice president of Corn Products Refining Co. last year, after serving as executive assistant to the company's president for a year. Before that he was assistant general manager of the chemical division for four years, during which time he attended the Harvard School of Business Administration. Mr. Ruark graduated from the University of Pittsburgh and attended the graduate school there while working as a Mellon Institute fellow. During the war, he served for three years as chief of the aromatics and intermediates bureau of the Chemical Division of the War Production Board, returning to Mellon as director of sales research for four years until he joined Corn Products.



Often a practical idea can result from the complete reversal of a proved product, and it is not difficult to cite recent examples of successful products' applying this principle. For years we concentrated on highly nutritious foods but today a sizable industry has grown up supplying foods having specialized or low nutritive character to accommodate the requirements of the diabetic and the obese. For years manufacturers produced foods and promoted them for the population in general. In the past two decades established producers and newcomers have built an industry devoted to pediatric foods. The application of this principle in reverse is now under way, and all of us here will see the growth of tremendous new markets for geriatric products. This reversal probably was not contemplated by the research workers of three, four, and five decades ago, who devoted their efforts toward curing or stamping out disease and to the study of the fundamental principles of nutrition and nutritional elements. The extension of the life term has created problems in the fields of retirement and life insurance but at the same time it has created vast markets which are now beginning to open up for all industry.

Fundamental Research

Management only demands economical research and practical research from its own research organization; but realizes full well that the community needs more than this and that industry must contribute its share. No corporation that devotes all of its efforts to the application of fundamentals developed by others, excluding this type of development from its own thinking, can continue to operate very long in the America of today and the future. Each industrial entity must devote a significant portion of its research endeavors to basic studies of the unknowns peculiar to its own products. In the fields of food research and nutrition research we have come far in the past 50 years, but we have only opened the door. When we have definitely and completely established the structure of such everyday commodities as starches, sugars, proteins, vitamins, and fats and when we have definite and complete knowledge of their metabolic function and fate, we can start to think about decreasing our fundamental research efforts. I don't expect to be here when that day arrives.

To carry out the required fundamental research it is necessary that each industrial laboratory must do its own share but management must do more than this and the responsibility of management in this regard is increasing rapidly. Management must support research outside of its own field of endeavor. It must accept some responsibility for financing specialized nonprofit research groups devoted to expanding our fundamental knowledge. In the case of the food industry the support of groups devoted to the study of nutrition, physiology, other branches of medicine, health, fundamental chemistry, physics, including particularly nuclear physics, is certainly in order. As a mercenary note I might add that all of us should certainly support any organization devoted to the development of analytical chemists since this field has certainly been neglected, and there are some fears that this fundamentally important species may become extinct.

No sound management will expect or allow its own individual research organization to be equipped to carry out all of the diverse, specialized, and intricate phases of modern research. Rather it will equip its own laboratories with the facilities most used and most adaptable to its own staff and operation and seek the services of specialists to fill in the gap. Not even the largest and richest corporations in this country can do all of their own research. The use of the consultants, the specialists, the universities, the industrial laboratories, and the support of foundations can do much to supplement the individual research operations of any company.

The Nutrition Foundation is a good example. The placement of research funds with that foundation, the placement of other research funds through its guidance, and the support of its endeavors in fundamental work have yielded tangible values to every company contributing to its support. The food industry is dependent on the public acceptance of its products so its interests and those of the Nutrition Foundation are mutual. The protection of the public is the most certain assurance of our corporate futures, and continued fundamental research of the type carried out by this foundation provides that protection.

Support of Educational Institutions

Support of college programs is an additional responsibility that cannot be ignored. Fellowships and grants to established programs either through foundations, or directly, increase our fundamental background and keep the teachers in the teaching profession. In recent years the laboratories of universities have proved to be happy hunting grounds for personnel to man the organizations of the unthinking and perhaps the unscrupulous. There is nothing wrong with the hiring of an individual who is leaving the field of teaching, or university research, but deliberate raiding serves only to kill the goose that lays the golden egg. Far better that we should make up some of the inadequacies of university salaries, not as a gift, but as pay for work done in the interest of our own future. At least give the teachers time to train their successors.

Support long has been given to brilliant students and to those who had little opportunity because of financial status. This endeavor, a responsibility of industry and community, must be extended. I do not care to dwell on the financial problems facing the universities nor do I know the answer to these problems. I do know from my day-to-day work that there is a serious problem and until a better solution appears, industrial management must continue to grant and increase its support.

Research and the Future

Currently there are clouds of uncertainty on our horizon: will the trend of national productivity continue beyond its recent all-time high? Competition from low-cost imports is growing. Military demands are changing, and foreign aid is reduced.

What happens to my company, what happens to its research program, and what happens to me are the questions many research men are asking today. The answers to the latter two questions are obviously management viewpoints on research. Finding the fundamentals, establishing the knowledge, creating the techniques, and finally producing new products should clear up these clouds and provide new opportunities and new responsibilities to those engaged in research fields.

Radical changes in viewpoint and entirely new ideas will be required but that is why research men are at work. The surpluses of product and productive capacity of World War I and World War II were usefully absorbed and it is fairly certain that we will again be successful in using up our inventories and extra capacity. In our industry we can look forward to years of increasing volume on the basis of population growth alone; but such an attitude is one of defeatism.

The companies that were most successful in lasting through the depression were those who carried out research before it. Those who have continued to be leaders are those who have established research programs or increased their research programs during and since the depression. Thought, fundamental research, and practical research can lead us to diversification, new fields of endeavor, a higher standard of living, increased volume and continued existence as a "growth industry."

The research man need not worry about his future for management must support his work to ensure its continued existence; management has no other choice.

(Based on an address presented before the Food Industries Advisory Committee of the Nutrition Foundation, Skytop, Pa., May 6, 1953)