

# WHAT RESEARCH EXPECTS OF MANAGEMENT\*

By WALTER J. HAMBURGER

*Director, Fabric Research Laboratories, Inc., Boston, Mass.*

WHAT DOES RESEARCH expect of management is a question which is or should be continuously in the minds of progressive scientific personnel. The answer has rarely been offered audibly or formally, but it appears timely to consider at least some of the beliefs in and attitudes toward research which the research worker may justifiably expect of management.

Management for the most part is not involved in the doing of scientific research, but the success of its enterprises will depend upon its beliefs in, and attitudes toward scientific research. Both its beliefs and its attitudes are vitally important to its employees, to industry as a whole, and to society.

It seems prudent first to establish the definitions of some words that are in frequent use, but seldom considered, in a truly applied sense.

*Research:* Studious inquiry, usually critical and exhaustive investigation or experimentation, having for its aim the revision of accepted conclusions in the light of newly discovered facts.

*Critical:* Of, or pertaining to a critic or criticism.

*Analytical:* Thorough, exact.

*Exhaustive:* Having a tendency to exhaust, as by thorough and complete discussion, covering all points or items as in an investigation or discussion.

*Investigation:* Systematic inquiry.

*Experimentation:* Action or operation designed as a test, trial, or attempt.

*Conclusion:* A conviction from inference, a practical determination, decision.

When considered objectively, it becomes immediately obvious that research is not exclusively an activity reserved for the scientist or the engineer. Indeed, persons in any walk of life may, and do, conduct research, unwittingly perhaps, in the course of their day-to-day associations with

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\* Presented at the May 14, 1954, Meeting, New York City.

their fellow men. Those who seek truth in the quest for accomplishment, constantly engage in "studious inquiry, usually critical and exhaustive investigation or experimentation, having for its aim the revision of accepted conclusions in the light of newly discovered facts." To express it more simply and succinctly they strive to keep up-to-date in a complex and ever-changing society. They conduct research at an informal level.

To live and act intelligently—as researchers—they must not of necessity, be scientists. To be successful they must be "analytical, thorough, and exact by means of thorough and complete discussion (or consideration) covering all points or items. . . ." They must see to it that they consistently engage in systematic inquiry by means of action or operation designed as a test, trial, or attempt. Their prowess will be judged by the conclusions at which they arrive, their "convictions from inference," their "practical determinations," their "decisions."

#### BELIEF IN RESEARCH

To achieve the major objective—success in the modern industrial race for technological supremacy—management must sincerely believe in research. Its actions must be research-oriented; its belief in the fruitfulness of research should be intuitive and unhesitant, so that when faced with technical problems it instinctively seeks answers with an open mind and critical experimentation, rather than with prejudice, opinion, and a mythical recantation of some prior art. Management should not have to be persuaded to support research.

This statement does not mean that management should support research uncritically, or that all research is either worthy of support or timely. Frequently, political and economic facts may overrule the logic of a research program. Frequently the answers to industrial problems are not technical but rather, social in nature, such as a breakdown in production because of faulty human relations between labor and management. The fact remains, however, that the great industrial development of the present era is the brainchild of scientific research, and no industrial organization can compete successfully unless it is convinced and aggressive about it.

#### RESEARCH IS LIKE INSURANCE

No respected, capable, business executive would ever dream of conducting his business regardless of economics or politics without the benefit of the protection afforded by adequate insurance coverage. To this end, a wager is made by business with the insurance company on a basis of odds actuarially established. Business bets (at these odds) with an insurance company that it will have a fire and hopes that *it will not*, and with the solace gained from such a protective arrangement, management puts its mind to the pressing problems at hand.

In the case of research, management should adopt a similar course. Whether it establishes a research department, or whether it engages the services of a consulting research laboratory, the magnitude of the project should be related to the economics of the business in such manner that the research is assured of continuity. The odds must obviously be properly established as in the case for insurance, since in this case, management wagers with the research laboratory that it will not succeed, but hopes that *it will*.

Both insurance and research are contemplated to relieve the burdens of the future rainy day. Insurance provides cash, research provides know-how in terms of sound, scientific knowledge that makes possible intelligent, practical operations, preparation to meet competition, keeping ahead of competition, product improvement, and product development.

#### ATTITUDES TOWARD RESEARCH

So much for beliefs. Now, what should management's attitudes be about research and research scientists? Management's attitudes are fundamentally as important as its beliefs. Attainments will depend upon the sincerity of purpose, the conscientiousness, and the enthusiasm expressed and evidenced by management. The proper attitudes toward research must never be lost sight of, irrespective of the immediate administrative pressures, if management's attitudes toward research are to be effective.

##### *First—Imaginative*

The executive will inevitably be faced with problems attendant to dealing with vast bodies of knowledge or the necessity for such knowledge. These areas will doubtlessly involve abstract concepts, many of which will be beyond his experience. Yet because of his responsibilities, it will be his duty to develop and retain the ability to appreciate the importance of applying new fields of understanding to local problems. Not only must this attitude be developed, but it must be imparted to his superiors as well as to his subordinates.

Management must not forget that research, as broadly defined, has for its aim "the revision of accepted conclusions in the light of newly discovered facts." Keen, imaginative powers are potent tools in achieving this aim. The road to achievement is often fraught with discouraging circumstances, but it must be remembered that any worth-while objective is rarely easily attainable. The apparent deterrents must be used only to further whet the imagination.

##### *Second—Discriminating*

Science in all fields is developing at such a rapid rate that it is impossible

to keep intimately in touch with all aspects, or even one individual field of activity. As a result, there seems to be a tendency to reject the possibility of ever being able sufficiently to understand a portion of a given field or fields to be capable of being imaginative and capable of making intelligent decisions in research matters. This certainly need not be the case, if management develops powers of discrimination. It may and should demand of any professional worker, whether he be scientist, engineer, attorney, or accountant, an interpretative explanation of the application of his work to the problem at hand; and it should be possible to obtain this interpretation in language intelligible to the uninitiated. If the executive is not successful in obtaining an answer in lay terms, his education should stand him in sufficient good stead to resolve the situation to one of intelligent communication. With a proper understanding of the specific work at hand, he must apply an unbiased, analytical viewpoint, in judging the relevance of any field of science to the problem or problems facing his organization. He must discriminate between relevance and irrelevance, since unwarranted and irrelevant research has been the downfall of many a research organization.

Actually, from a practical standpoint, imagination and discrimination usually function simultaneously. As a concrete illustration, let us take the problem of predicting the mechanical behavior of textile yarns from a knowledge of the mechanical properties of textile fibers. For thousands of years, textiles have been made "by guess and by gosh." While tribute must be paid to the remarkable ingenuity and skills of textile craftsmen who have produced a breathtaking variety of useful and beautiful fabrics, craftsmanship has become inadequate in meeting the demands on textile use in our complex society. With the advent of two world wars, the tremendous growth in industry, a flood of new fibers, finishes, and processes, the need has become pressing for quantitative methods of predicting and engineering textile products.

If textiles are to be engineered even as bridges and highways, then they must be first conceived as engineering materials, and subject to the same kind of mathematical treatment as metal, wood, or concrete. But it is well known that textiles are soft, flexible, and porous, and are made up of millions of individual fibers. Who can imagine such structures behaving as a solid piece of wood or steel? A qualified scientist may state that because of the multitude of fibers in the yarn it is impossible to calculate how the stresses applied to yarns are precisely distributed among the fibers. Yet the most striking advances in textile physics and the prediction of yarn performance in recent years has come by imagining yarns to be ideal engineering structures, e.g., solid, round, homogeneous, without any variability along the length or over the entire cross-sectional area. Then by measuring fiber properties and calculating how a yarn should perform, the difference

between theoretical and actual performance has provided the key to discriminating among the many possible causes of deviations, and rationalizing these into mathematical expression. Knowing from empirical experience some of the properties of fibers and yarns, such as crimp and twist, the effect of these variables has been gradually determined.

### *Third—Tolerant*

Research is the product of human beings, working with and among other human beings. For all the striking advances in science, we still have far, far to go before we can be assured in advance that in setting up any attack upon a problem, we have chosen proper methods for solution. Research progresses by small steps, frequently very slowly. The time of solution and the scope of the work, can rarely, if ever, be guaranteed. Many of the small steps, and much of the time consumed as a result of added wisdom, stem from experimental failure.

There is a danger in these times to think of the research method and science as being ahuman and infallible, and thus, when an experiment fails, to question the competence of the scientist. This attitude may be most unfair, and management should not take such a stand. Rather, it should realize that research methods are fruitful as much because they profit from failure as because they solve a practical problem. The most intelligent, effective expenditure of research funds depends upon a tolerant understanding of the limitations of what is known and what can be done.

Those who have labored diligently in the field of research, realize the problems that arise when the results of theoretical determinations are first explored at a practical level. Seldom is success achieved on the first trial. More than likely, failure will be apparent on several trials. Such failures should not necessarily be looked upon by management to connote a lack of soundness of work done at the fundamental level, but rather point to the need of additional research at the applied level. Here, the attributes of tolerance are paramount to achieving success. Management must be willing to sift the wheat from the chaff. It must determine which lines of attack upon the newly uncovered applied problems will be most fruitful. It must if need be, stand ready to revise its accepted conclusions in the light of newly discovered facts that become evident in the process of application. Such action must be taken forcefully, but not critically, for the wise man is he who recognizes the limitations of his fellow man, as he should recognize his own.

### *Fourth—Humble*

Science makes no policy—people make policy. Science may tell us how to do something. Science cannot tell us when or whether to do something. The plight of the world today and the shadow of the hydrogen bomb bear grim witness to these facts.

No matter what management's other attitudes toward science may be, it should not demand that science relieve it of its responsibility for action. Science has been so productive, and the authority of science has come to be so persuasive, that society has come to place an unfortunate faith in the ability of science. Humility is to be regarded more highly than authority in all fields of endeavor, and research is no exception.



Good research can be fostered by good publicity. However, the use of advertising copy which unwarrantedly glorifies research findings, and which makes spectacular announcements of and claims for products and processes, far beyond the ability of the research group to produce, is damaging to both management and research. All too frequently "research" is conducted for the purpose of creating extravaganza-type advertising programs.

False claims or overrated statements inevitably impose serious problems upon management and the responsibility for them is frequently, albeit unfairly charged to research. Management should avoid such practices since they can result only in disappointment and disillusionment to all concerned.

John Boulton, Director of the Dye Research Laboratory of Courtaulds Limited at Droylsden, England, in his opening remarks to the 1949 Annual Conference of the Textile Institute commented as follows:

"It is one thing to build up a large research potential, to make use of its results in industry is another. Management should ask themselves if in fact, they are making use of research. Research purely of itself, is a cure for nothing. Research is an activity the first product of which is ideas of a speculative kind and ideas of an established kind. The first provide a reservoir for further and eventual production of the second kind, which constitute new knowledge. Industry should be concerned, therefore, with the following questions:

- "(a) Are we making the fullest use of the available research potential, and if we are not, can we find reasons for this and can we point to practical means by which we can, with due speed, tap the available sources of new knowledge?
- "(b) Apart from the use we may make of its results, are we doing all that could be done to initiate research? The relation between the sources of new ideas and their application to industry should be a two-way relation. It is not sufficient for industry to apply itself, no matter how diligently, to the receiving end of the research activity.

"Is industry actively seeking the right *kind* of new knowledge?

"Is industry asking the right *kind* of questions of the research worker?"

Management and research may both profit from his sage advice.