# WHAT MANAGEMENT EXPECTS OF RESEARCH*

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Before getting into the discussion of what good management expects from research, let me first say what they do not expect. They do not expect their research departments to be quality-control laboratories. They ought not to expect them to spend any major portion of their time in trouble-shooting, or on routine changes and formulations leading to minor changes in their products. These problems often require excellent technical ability, and they are important, even indispensable, but they are not research.

Unfortunately, poor presentation by technical men has often limited in the minds of management the potentialities of research. Management expects research to supplement its own activities in providing imaginative effort toward sound growth. They hope that thus the sales and profits of the company will be correspondingly increased, through a diversification of the company's interests by the development of new products, an increase in gross profits, entrance into fields where the profit margin is greater, and the maintenance of the company's competitive position. An organization cannot remain static, and unless it goes forward, it will go backward. We have found that successful and forward-looking management expects far more from research than mere technical excellence. Research thinking that limits itself to mere technical contributions is inadequate and outmoded. *Management looks to research as a responsible source of advice in high-level planning.* It looks upon its research direction as it does on legal counsel for expert guidance in fields calling for specialized knowledge.

Management contributes by providing facilities, for example, laboratories, working space, and other necessary tools. These are important, but they need not necessarily be extensive. Within the last ten years one man launched a rather large new business with his wife's stainless steel saucepans and her kitchen and his garage as his sole initial laboratory facilities. As a result of his success with saucepans, his new business now employs a substantial group at Arthur D. Little, Inc., as well as a research staff in his own organization.

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More important than the physical facilities are the people who use them. The main problem we face today is finding good research personnel. We simply do not have any adequate yardstick by which we can measure an individual's potential value. However, some things we do know. Technical background is necessary, and the more the better, but this alone does not qualify an individual. We sometimes wonder whether in obtaining a broad knowledge of basic physics and chemistry a man does not sacrifice something else. Management expects their top research personnel to have a rather intimate knowledge of their business, and, for example, not to embark on a long and expensive study of means for producing cheaper buggy whips.

It is presumed that the research people will always be on the alert for new products which may come to their attention. This, too, is not always associated with pure scientific excellence. I understand that Professor Kipping spent many years studying the silicones and attempting to produce crystalline products which he could characterize. He felt that his work had been unsuccessful because all he was able to make were gluey, resinous materials. Last year, these useless products were sold (with some modifications) by the three producers to the extent of twenty-five million dollars.

By contrast, a research worker disposed of his cigarette quickly when a visitor arrived—on a laboratory dish where it contaminated and was contaminated by a chemical under investigation. When "all clear" was sounded, he resumed his smoking, noticed an unusual sweetness—and a new competitor of saccharin was discovered. What was admittedly poor laboratory practice was offset by a brilliant combination of observation, further investigation, and, in a word, creative industrial research.

And this brings up the next point which I would like to make about industrial research personnel. Their effectiveness is dependent to a considerable extent on the broadness of their knowledge of industry. While they need not necessarily be experts in any field, an understanding of many industries and their problems can be tremendously helpful. Many useless gluey, resinous materials have a use if one has a wide enough range of interests.

One of our best industrial research men believes that the greatest single liability that the research man can have is knowing too much. An American humorist—I think it was Josh Billings—once said, "It ain't the things we don't know what makes us so ignorant, it's the things we know that ain't so."

In scientific discussions of papermaking, one usually finds a complete discussion of the structure of cellulose and the extensive work which has been done on this subject. But paper is not made of pure cellulose. Here again, exploratory technology may be hampered by knowing too much of what is in the books, worse still, uncritical belief in it, and still worse, allow-

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ing it to prevent courageous new research. This emphasizes another important requirement in research personnel—an open mind, an unwillingness to believe a thing cannot be done, or a lack of belief that means may not be found to surmount what appear to be insuperable obstacles. New industries are constantly blossoming because of individuals who disregard the sign "Unsolvable Problem, Keep Out" and sneak around and come in by the back door. Management has a right to expect this enthusiastic, optimistic, and unorthodox approach.

One test which quickly separates the men from the boys is the attitude of people toward problems. To anyone worth having around, a problem is a potential opportunity—not the kind of problem which results, for example, from a boiler explosion, but one like that described in this excerpt from a salesman's report. "Yesterday, I called on the X Company. Mr. Blank, their Purchasing Agent, said that they expect shortly to curtail greatly their use of our #1 grade wool felt. It seems that they have been experimenting with a new flocked paper product which, while it is much lower in tensile strength than our felt, is claimed to be adequate for their purposes in many applications." Crises like this often stimulate one to think of new approaches that lead to great technical and economic advances.

So far I have been talking of the technical phases of research—those which call for a knowledge of physics and chemistry. Today, management is looking more to research to co-ordinate the technical with the economic aspects of the program. This is not, of course, because economics are better recognized for their importance, but rather because it has been found that uncertainties of crystal ball gazing can be considerably replaced by systematic studies carried out by chemists, physicists, engineers, and marketing experts.

Patent matters are almost solely the responsibility of research working in conjunction with patent counsel. Research people may not be expected to know much patent law, but management certainly expects that research will initiate the actions necessary to ensure that the investment in research is protected so far as is feasible legally and economically.

Close collaboration between sales and research is usually essential. Not only can sales benefit by technical suggestions for favorable sales presentation, but more important, research is made aware of shortcomings which can lead to better products and is stimulated to greater productivity. Industrial research has a responsibility for support to advertising and sales.

In one instance, a company won the award for advertising excellence three years consecutively through a close working relationship with research. Lowering of the barrier between what some scientists call the hucksters and what some business men call the long-hairs would benefit both parties and, I believe, the public. But both would have to modify their extreme characteristics that keep them apart.

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An essential feature in a good research program is an adequate and effective presentation of the results. Too many reports to management resemble articles in our scientific journals. The important points in these reports simply never reach a management that has neither time nor ability to comb them through for significant findings. As a result, excellent work has been made useless. This matter of reports is so important that we recently acquired the services of a competent specialist on this subject. One of his contributions has been in helping our staff write summaries to go at the beginning of the report and which present the important findings so clearly and concisely that a busy management can appraise the project and appreciate the action indicated. Reports should permit management to answer these questions: Where have we been? Where are we now? Where do we go from here? What is it all worth, supposing we get there?

Conferences with management rarely center around the necessity for synthesizing alpha alpha′ bis para para′ something or other. Conferences are more likely to be concerned with something like an easing in the supply of sisal fiber and if its water absorption could be reduced and, if so, could it compete with Fiberglas on a pound-for-pound base. Above all, perhaps management expects of research an understanding of management objectives. Research leadership should first find where management wants to go and help it go there.

A glib answer is that management often does not know where it wants to go; that research should show it. Now one of the principal responsibilities of research directorship is that of helping management define its research objectives. Those objectives must be defined in terms both desirable and reasonably attainable. The early stages of a research program, when management and research agree upon an assignment, are vital.

But helping management define its objectives is *not* telling it where it wants to go. If management is competent, it has defined, or will define—perhaps with financial, market, and technological advice—its basic objectives. If it is in the cosmetic business, it does not welcome diversion of much of its research fund and effort from nail enamel to an automobile lacquer merely because a chemist dropped some on a fender and thought it had promise.

I am aware that unless I expand this point, I shall have my favorite research word, *serendipity*, thrown at me. I am aware that many great discoveries have been made in this way. But I am also aware of the stubbornness of some research men, and even research departments, in pursuing objectives outside the company's area of interest after management has evaluated the project from its point of view and decided not to pursue it. It may be that the company's working capital does not permit even serious preliminary explorations of a new product in the paint and lacquer field. It may be that management knows better than the chemist that its net

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returns, even given success, would not warrant the diversion of effort. Where and when to draw the line between dropping such a project and insisting on further support by management is a matter of judgment. Management expects research to understand and allow for its broad policies and purposes, and even for its financial and administrative capacities in making such decisions.

Nor is an industrial research laboratory a university. The fine line between acquisition of basic knowledge in the company's interest and acquisition of knowledge for the personal satisfaction of the research workers again calls for judgment. Most moderate-sized companies are reluctant to support fundamental research within their own walls that is not reasonably certain to pay off, in one way or another, within a reasonable time. If management is broad-gauged and farsighted, it may sponsor such work in its field at educational or other institutions. But it does not like to mix objectives, and most moderate-sized laboratories are expected to keep management targets in their sights. As laboratories grow larger, men competent to delve deeply into sound fundamental approaches in the company's field can be added to the staff, and feed the balance of the staff and the industry with basic and significant advances.

Perhaps most discouraging to management is a staff not qualified to compete with university researchers in advancing the art, and not imaginative enough to develop new products or significant improvements, that yet plods on, acquiring technical knowledge (often second-hand knowledge) with only minor increments of improvement and even of product knowledge. Men on such a staff become "experts" of a sort, and production and sales men lean on them for information and aid on minor problems. Important as is such production and sales service, it is not research. If it is confused with research, management soon begins to wonder why competition leads.

A related problem is the matter of significance of objectives and results. A moderate-sized company cannot afford large expenditures for improvements that have minor economic significance. Nor can it spend much to make only minor increments in the fixed or basic scientific knowledge in its field. Industrial research is a speculation. It may be a calculated risk, and the degree of speculation often varies inversely with the significance of the objectives. But it is a speculation. And management expects of its research leadership that when it backs either fundamental research, or expensive target-seeking research, the winnings if achieved will be in proportion to the size of the stake risked. Management is becoming more sophisticated in its understanding of research, either from continued experience with it, or from including research-experienced men within management. Able, driving, wise management likes, perhaps least of all, frittering away time and money on petty objectives.

Lest I be misunderstood, a word of caution here. Increasingly, it is diffi-

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cult to achieve major research objectives without use of the best in ability and knowledge on the scientific frontier. An industrial research staff must engage competent, up-to-date, creative basic scientists in its fields, preferably, in my view, within itself, but otherwise by collaboration. It is not sufficient for a research man ten years in an industrial atmosphere to depend on his recollection of his professor's statements, or even on the literature.

The liaison between the best scientists and the best industrial researchers should be established on an intimate working basis of mutual respect and collaboration.

Today's shibboleth of good management is communications. In few areas do management communications combine importance with difficulty to a greater extent than in the use of applied science and the work of scientists and engineers. The problem has spilled over from university and industry into the community. It is a problem to which every scientist and engineer owes time, effort, and attention. A sympathetic understanding and effective working relationship between the world of applied science and the world it serves is today vitally important—not only in the confined sphere of an industry, but in the world around it. Management expects research men to strive mightily to establish such a relationship.

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## CHICAGO SECTION NEWS

THE OPENING meeting for the fall was held on Sept. 7, 1954. Dr. Peter Flesch, Professor of Dermatology at the University of Pennsylvania, spoke on "Laboratory Methods for Studying the Chemical Effects of Topical Agents Upon the Skin." Dr. Flesch received his M.D. degree in Budapest, Hungary, and his Master's and Ph.D. in Pharmacology at the University of Chicago. His research work in dermatology includes studies on the chemical effects of human sebum and of epidermal keratinization, as well as work on hair growth and pigmentation.

The guest speaker for the meeting on Oct. 12, 1954, was Dr. Adolph Rostenberg, Professor of Dermatology at the University of Illinois, who spoke on "Sensitivity to Simple Chemicals as Used in Cosmetics." Dr. Rostenberg spent six years as a dermatologist with the Food and Drug Administration and is presently a consultant in dermatology for the FDA.

Dr. Donald H. Powers, Director of Research at Warner-Hudnut Inc., and President of the Society of Cosmetic Chemists, spoke on "Recent Developments in the Cosmetic Field" at the Nov. 9, 1954, meeting. Dr. Powers received his formal education at Boston and Princeton Universities and has had wide experience in the fields of cosmetic and textile chemistry. Dr. Powers is a former Chairman of the Scientific Section of the Toilet Goods Association.

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