THE KNOWLEDGE BASE FOR EDUCATION OF RESEARCH MANAGERS

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Paper presented at the 19th National Conference on the Administration of Research, Newport Beach, California, October 6-8, 1965, and published in the Proceedings by the Denver Research Institute, University of Denver, April, 1966.

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Dr. Williams has been looking at the past. I would like to turn our attention now to the future. We are interested in research management, first of course, because we are all in the business, but second, at least in my judgment, because at this moment in history research management is critical for the overall effective performance of industry, government, and universities.

I want to tell you why I believe this. At various times in history the difference between an effective organization has depended upon competence in different critical activities. Prior to the eighteenth century the critical difference between a successful and unsucessful company was how well they managed trade and commerce. After the industrial revolution the difference that made the difference was management of production.

In the latter part of the nineteenth century, when the great business empires were constructed, the critical difference was in financial management. During the first part of the twentieth century I think we would agree that the critical difference has been in marketing and distribution, with the advances in communication and transport making mass markets possible. At the present time the critical difference between an effective and a less effective organization is in the way they manage research, innovation, and the utilization of new technologies.

Research management is not only the critical difference between a good organization and an average one, but research is the most difficult to manage of all the functional activities. There are three sources of this special difficulty. The first is the degree of uncertainty. Compare, for example, the certainty with which you can plan and schedule production or inventory or sales or cash flow compared with what you can do in new product development.

The second source of difficulty is that you are managing a new kind of employee who views himself as a professional person. Scientists and engineers differ from other employees in their expectations, their values, their attitudes, and their motivations.

The third source is the difficulty in measuring results when each research task is unique and never repeated. Even if you could measure results, the delay in the feedback loop is so great that it is hard to use knowledge of results as a basis for planning in the future.

This analysis leads to the proposition that a good research manager is one who can deal capably with these special problems, and therefore he should be paid more than other managers. It is also of interest to note that in these times of rapid change, the top management tasks in an organization are coming to sound more and more like those of a research manager.

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Third Session

Top executives have to deal with the uncertainties of long-range planning, foreign competition, government policy, and so forth. They have to deal more and more with professional personnel in all parts of their organizations. They have to deal with the social, economic, and political environment of their organization with the inevitably long delays in the feedback loop that brings them knowledge of results. I firmly believe that if research management can be improved, it will be the best training ground for top executives.

The *Scientific American* recently documented the increasing proportion of technically trained men in top management, and I know one science laboratory manager in a very large company who says that one of his missions is to develop alumni of his laboratory to become presidents of all the divisions of his company. If we are going to talk about research managers we should acknowledge the fact that they are not a single species, but that there are many varieties of research managers.

The job of research manager differs by industry, as was made clear yesterday. Moreover there are certainly two entirely different situations in which we find research managers. One is the manager of a large organized laboratory, and anything over 50 people is a large laboratory requiring organization. The other is the technical entrepreneur who starts a small new enterprise which may eventually grow into a large organized business.

A study carried out last year at MIT by Harry Schrage, which will be published in the Nov.-Dec. 1965 issue of the *Harvard Business Review*, examined the characteristics of 20 individuals who had established new small technical enterprises in the Cambridge region. He found that the rate of growth in profitability of these enterprises was clearly related to certain characteristics of the founder.

The men whose companies showed a higher rate of increase in profitability had, compared to the others, a higher motivation for achievement as measured on McClelland's test; they had a lower desire for power and authority—they didn't try to hold the strings too tightly; and most important of all, they had an accurate perception of critical aspects of their environment. The two most important aspects for this type of enterprise were accurate perception of the customers and accurate perception of employees.

He devised a very ingenious method of getting at this. During a long talk with the founder-president of each company he would ask, "What do the customers think of your product?" He paid no attention to their reply but then asked a second question: "How do you know?" He found he could put the answers into four categories.

First, and lowest in terms of accuracy of perception, would be "no information at all." Next would be "information comes to me without my seeking it, and I interpret it to suit my wishes." Third, "information comes to me, and I pay careful attention to it." Fourth and most accurate, "I go out actively and deliberately seek information directly."

This is what we mean, then, by accurate perception. In a sample of only 20 companies there was a clearly significant relation between the score on accuracy of perception of customers and employees, and the hard facts of rate of increase in profitability and growth of the company.

Othe studies conducted by Edward B. Roberts have examined the formation of new technical enterprises by individuals who left government-funded laboratories to start their own companies. This is of interest to us as a channel by which technology is transferred from government-supported R and D to commercial research.

For example, the MIT Instrumentation Laboratory, which got started shortly after World War II, has graduated, or spun off, or lost, 27 employees who have started their own companies. Of these, 26 are successful and still in business, although they differ in their rate of growth and profitability. The differences depend upon some interesting factors.

One is how directly the technology of the parent company is taken into the new company—the more directly, the more successful the new enterprise. If there is a gap of two or three years (in which the founder of the new business works in his father's business or some other), then the probability of success of the new company is less.

Similarly from the MIT Lincoln Laboratories, which is a larger organization with perhaps 2000 employees, there have been formed in the last 15 years 50 new companies by individuals who are technical entrepreneurs, who want to take the technology that they have learned in the parent laboratory and use it as a basis for forming a new business. Of these 50, there are 45 still viable and successful. The aggregate volume of business of the spin-off companies from these two laboratories is rapidly approaching the point where it will exceed the volume of the two parent companies, and 40 percent of their business already is commercial.

We would go on and talk about other types of research managers. Certainly we ought to distinguish those who are managing a science-or discipline-oriented activity from those who are managing an applied research or advanced technology activity, those who are managing a development activity, and those who are concerned with carrying the products of research and development into manufacture and marketing.

We can also distinguish research managers by their level of responsibility: the team leaders or first-line supervisors, the project managers, the functional managers at several levels, the laboratory director, the vice president for research and development, and the president of the organization, who ought to be a research manager in the sense that he knows as much about research as he does about finance or marketing or production. Recognizing, then, that there are many kinds of research managers, what can we say about the education, training or development of research managers?

It seems to me that before we can talk about courses or curricula or on-the-job training, we need to know whether there are any principles known with sufficient certainty to be worth teaching. We need to know the difference between a superior and an average research manager. This foundation of sound management knowledge is what the announced title of my report refers to.

If you are teaching polymer chemistry or magnetohyrodynamics, there is an established body of knowledge, and you can test whether a student has mastered that body of knowledge. Is there anything comparable to that in research management?

The body of knowledge in research management accumulated over the past 19 years (which Dr. Williams is working to summarize) is pretty thin. I tend to think of this body of knowledge as derived from four sources: revelation, tradition, experience, and systematic investigation. Let me say a little about each.

Revelation. There are some research managers—of course none of them are here, but we all know some—who operate and who give speeches as if they went up on top of the mountain and received the word directly about how to manage research. This is the *a posteriori a prioristics* that Dr. Mesthene spoke of yesterday.

Tradition. The second and perhaps more common source of knowledge is tradition. The management of organized research and development is young, perhaps 20 years. But the traditions have grown up very rapidly, because traditions always grow quickly in the absence of other more soundly based knowledge. The traditions of research management have come from other forms of management, principally from the management of production facilities.

Experience. The third source of knowledge of research management, then, is experience; and I am sure that everyone here believes that he has learned from his experience how to improve his management of research. I am going to challenge that statement.

I contend that one of the hardest things in the world to learn from experience is research management. The reason is that you don't know for a long time the results of what you have done. Whether you are planning the allocation of resources, budgeting for your laboratory, selecting projects for approval or termination, it is very hard to operate on the basis of experience because your experience has been in only one or a few laboratories, it has been of a special kind, and—unlike in other fields—the results of a managerial action in R and D may not show up for three, five, ten, or fifteen years.

If this is the case, we would not expect much learning on the basis of experience, because the possibilities for it just don't exist. In the absence of learning by experience we see the growth of fables, myths, and rules of thumb—the sort of things that Dr. Mesthene was talking about yesterday—because we have to believe that there is some basis for our actions.

Systematic Investigation of Results. The fourth source of knowledge, and the one for which I want to put in a plug, is systematic investigation of the results of research management. It is difficult for managers to carry in memory the results of an action five years ago; besides, times have changed and this is a quite different situation.

I would like to suggest that in no field of management than research management is it more necessary to do systematic research. In no field is it more difficult, but, for reasons that I hope to have made clear already, it is worth the try.

Now, let's turn to what kind of systematic information on research managers we have. As a matter of fact, there is only one large-scale systematic study. This is one reported by Clarence Randall in 1956, based upon very careful assessments of research managers by Booz, Allen & Hamilton. It surveyed 1427 executives, of whom 300 were R and D managers, by systematic procedures in companies of all sorts throughout the country.

On the basis of the assessments and talking with colleagues and superiors, the managers were classified as those who were clearly promotable, those who would never be promoted, and a middle group of marginally promotable. About one-third were clearly promotable.

We are depending here upon a measure of performance based on evaluation by people who observed their work and who perhaps shared the myths and fables and rules of thumb of the person being evaluated. It turns out that eight characteristics distinguish the promotable from the not promotable manager in all fields. These are performance in his present position, drive, intellectual ability, leadership, administration, initiative, motivation, creativeness.

It sounds to me like the Boy Scout oath—faithful, honest, obedient, courteous, etc. I wouldn't have any idea how to move from this list of characteristics to the selection of research managers or to the design of a training program for them. It turns out that some of these characteristics are a little more critical for research managers than for managers in other fields.

One is creativeness, whatever that is. Another is social acceptability. (Well, if you have no other criterion that's always a good one: white Anglo-Saxon, and Protestant). Less important than in other fields, according to this very thorough systematic study, are initiative, administrative ability, leadership, and drive. This is what you can get with a very thorough study of people's opinions about who is a good research manager.

The alternative that I think is necessary, even though it seems difficult, is to analyze systematically the outcomes of research management by some objective measure of results. This is not nearly as easy as it sounds, but there are some promising approaches.

Two years ago, those of you who were at Estes Park heard a session in which Dr. Donald Pelz reported some very interesting results on factors in research effectiveness. Dr. Calvin Taylor, on the same program, talked about ways of selecting potentially good research people by the use of biographical information. The work that I described at that meeting was largely a promise, a direction, and a hope, and you were kind enough to say, "Go ahead, and when you have some results, let's see them."

The current report of the MIT Research Program on the Management of Science and Technology, copies of which are available here, represents the first time we have felt confident enough of our results to issue a report. I will not take your time now to try to survey the results of the 38 published papers and 70 theses which are summarized therein. I hope, instead, that this organization will, over the years, invite reports of the more substantial and objective research studies whose results have implications for management policies and procedures.

DISCUSSION OF THIRD PANEL SESSION

J. D. FRENCH, *Panel Chairman*

While you are thinking of something to question these gentlemen about I would like to ask some questions that occurred during the speeches. First of all, Mr. Williams, you indicated earlier concepts of the almost chaotic method of managing research on the one hand, as opposed to a more ordered attempt to structure research. (Incidentally, I was struck by the fact that the chaotic group seemed to do rather well in that interval of time).

Nevertheless, about this divergence of consideration regarding the management of research: In your view is there any pattern emerging, since I gather you believe more structured and predictable methods are going to emerge successfully? Is there any pattern emerging which indicates what the instructional problems these managers will face will be?

L. B. WILLIAMS, *Panelist*

I am not quite sure, Dr. French, what you mean by the word "instructional."

CHAIRMAN FRENCH: Teaching or learning to become an administrator of research.

L. B. WILLIAMS: Well, I think it is very, very clear, as I pointed out, that you cannot really expect a maximum of efficiency of your research of R and D organization to come from leaving it alone, leaving the situation in chaos. The word "chaos" was used here yesterday by Dr. Harris.

More and more you can see, as you read and re-read these Proceedings, that certainly some kind of order and system in the planning and executing of research and R and D effort is recognized now as not only desirable but necessary. Peltz' studies seem to have brought that out (his studies being at least an academic and scholarly attempt), so that whether or not we are ready to start teaching these methods of two-way channels of influence I am not qualified to say, and I don't think that the Proceedings would in any way reflect this.

CHAIRMAN FRENCH: It occurred to me, Dr. Marquis, when you were speaking, that you made a telling point when you suggested that there was a great deal in common between the responsibilities of management and of research management, and that perhaps research management might be a direct line of communication in the development of top management. It occurred to me that very few professional people, certainly very few people in research, go into government, and I mean political government.

Yet, it seems to me that a great deal of the influence that is being fed down to us all stems from government. I wonder if there is any relationship between what you said and the need to have more people in Congress, and perhaps in the administrative branch of the government, who have had experience in professional research.

D. G. MARQUIS, *Panelist*

Others yesterday spoke about the fact that government in the past has largely been in the hands of lawyers, and for very good reasons, because of the legal constraints upon the powers of government. Dr. Mesthene especially pointed out, and I don't need to elaborate on it, that the nature of government is changing—that the objectives of government are shifting and we are swallowing them, and that in this new kind of government, especially on the executive side and perhaps later on the legislative side, I would agree with you that there is a place for people who have had experience and have learned something as research managers.

M. HARRIS, *The Gillette Company*

Don Marquis and I have been pulling each other's legs for some time on some things, but this is a rather serious comment, because yesterday I stressed the importance of the project type of thing versus the functional, and I still stick to this. I would like to ask Don this question.

I have held discussions among what I call the consumer-oriented, generally non-government-funded industries (people who spend their own money on producing products such as in the chemical industry, the pharmaceutical industry, the textile industry, and the Gillette Company), and I find that people in these industries still think that, for most types of work, the project system is more effective. I am rather curious as to the source of the information that you have shown here, which distinctly showed that functional work is more productive. Is it more productive with the large projects of government-funded money, because, if so, that's a different story of the way to approach it than what I was discussing yesterday.

D. G. MARQUIS: You are correct that the data were, in 35 cases, for very large—$4 million to $6 million government-funded projects—performed by industry. The reason that we started our studies with government contract projects is that it is easier to get your hands around them. They have a beginning and an end, an initial estimate of cost, and so forth.

We started this summer trying to do the same sort of thing in commercial laboratories, and we welcome your offer of cooperation. We would like very much to study the eight laboratories in Gillette.

R. S. GORDON, *Panelist*

I would like to follow up Dr. Harris' question because I think it does bear on Professor Marquis' evaluation of functional versus project organization. It would be my understanding, then, sir, that the integrative function—that is to say, the utilization of this information for a mission—is really beyond the concern of the people performing the contract. It was done within the government.

If this is true, is this not different from the integration that occurs in laboratory mission or operational performance, say, within a private concern or even in an academic laboratory, such as the brain surgeons team that our chairman runs?

D. G. MARQUIS: Yes, of course it is different, and the integration involves a cooperative relationship between the industry's laboratory and the government's technical people. The follow-through, the implementation, is a government operation. But it was the government's evaluation of the success of the development, and these, incidentally, were all development projects. That evaluation is given in terms of its potential for use in an operational system by a government agency. These were a mixture of Air Force, Navy, Army, NASA and other government agencies.

N. KAPLAN: My usual morning cautionary note: Without wishing to detract at all from Don Marquis' very excellent presentation, for which I have nothing but great admiration, I think a word ought to be said about how these results can be used. This is something that I think was not stressed sufficiently.

In the world in which most of us live, which consists of these fables and myths, a little data might be just as harmful as the myths themselves, and I think this is indicated by the two questions which have been asked previously about project versus functional organization. I think it is going to take time; it is going to take a lot more of the kinds of studies that Don Marquis is talking about, with a lot more knowledge of the parameters involved, of precisely the types of questions that were asked about whether the projects are Air Force projects, whether we are dealing with commercial development, and so on.

The basic point that Don Marquis mentioned is the need to know much more about the evaluation of results. Although he and his group have moved very far beyond this kind of study that was mentioned earlier, the kind of study where you simply ask people for their opinions as to who is good and who is bad, I think that we are still a long way from the desired end product. That is the measurement of the actual results which, in the case of the Gillette Company, would be what kind of product and how profitable is it. In the case of an Air Force project it would be how good is the weapons system, and so on. The kinds of criteria will differ.

The important point is that this research on research has to be encouraged. It has to go forward much more so than it has in the past, and it can not be viewed as something that is ready for application as soon as the first result rolls off the computer.

D. G. MARQUIS: Although I disagree with your general proposition that for a starving man a little food is a bad thing, I would agree entirely, Norman, that we need a lot more research. For this particular sub-set of large development projects, you could split them into large and small. The results are the same. You could split them in a variety of ways, and the results that I reported are true, regardless of the number of other factors of that sort.

One other interesting fact came out, since we are talking about project managers in this case: the ones who ran projects that were judged more successful had less experience than those who ran projects which were judged less successful. I wouldn't, on the basis of that, argue that experience is a bad thing, but I would argue that the myth that the man with a lot of experience is the best project manager deserves reexamination.

J. T. GREY, JR., *Thiokol Chemical Corporation*

I look at these results from a somewhat different point of view, and I raise the following question in my mind. What was the breadth of the technical content in these programs?

My concern for my question arises from the fact that of late we have had many discussions of the stimulation of the interdisciplinary project team. However, in this case it might appear that if the technical content of the project was rather narrow, the technical people were rather stimulated by association with their technical peers.

D. G. MARQUIS: When you are talking about a $30 million project, which is a system, the technical content is interdisciplinary, although in general these were all aerospace, electronic and electric. These are exactly the ones for which project organization is recommended and, as a matter of fact, government agencies are now requiring it and insisting that the proposals outline the form of project management that will be used.

J. T. GREY: I address this question to Mr. Williams. Will your study of the Proceedings of the NCAR include a critical analysis to determine whether the apparent beneficial effects of the shift from chaotic to organized research may be a self-induced artifact of our concern for our *raison d'etre* as the research management establishment?

L. B. WILLIAMS: Well, certainly, if you mean by "artifact" a "straw man." I don't think that we can tell from the Proceedings whether such is the case or not, really. I don't think they will reveal that. These, after all, are experiences, and, as we heard yesterday, no doubt research administration is still an art. What happens, perhaps, and what is happening is the same thing that happened, for instance, to the medical profession. I don't know what the timing might be, but I think it is relatively comparable.

J. E. MAHONEY, *National Aeronautics and Space Administration*

The first comment is to Les Williams, and this is with reference to what I hope is evolution of the Conference form here. I see in these gatherings—I have seen a couple over the years now—a very unique bringing together of academic minds, industrial people, the guys who push the buttons on R and D management, and also the government people who, in some cases, are sponsoring either contracts under this R and D work, or they are actually doing R and D work in and of themselves.

I think, as Don Marquis brought out, we have over the years relied upon this Great Man coming forward and speaking on how he manages his project, and I am very happy to see Les' effort, where he is trying to pull this all together. I hope I see a research emphasis after this. In other words, I hope we use this forum, let us say, as a research mechanism, possibly every year reporting on some of the general problem areas that have been reported over the past 18 years. I hope this is one of the outputs of your effort.

Don, I hope you look into how you transfer the knowledge you are generating and how you get the word across to the R and D manager. The other thing is with reference to Norm Kaplan's example, and how it relates to his earlier comment. Let me give you a hypothetical case.

If I were a lab manager and I had a large number of projects under me along with the results of Tom Allen's data, which showed that the best project teams consider two approaches at a time, would I use his data from the standpoint of looking at my many projects and finding those project teams that are considering two approaches? Then I can do one of two things. The ones that are considering more than two approaches I can fire, or I can force them to consider only two at a time.

L. B. WILLIAMS: I would like to respond to your first comment. Certainly over the years we have recognized that several kinds of people have come into consideration of research administration problems from an academic viewpoint. First of all, I think we had the people interested in the behavioral sciences area, the sociologists and psychologists, who wanted to explore some of the problems of motivation and control and some of the things I mentioned that Dr. Peltz treated. These are people interested in the people of science and research.

A little later, or maybe about the same time, we had another kind of scholarly and academic person coming into the picture. This is the economist or the business manager or business-administrator type of person, and he generally is a person who is interested in things, not people.

More lately, of course, we have had the academicians, who generally can be classified as the operations research people, who would like somehow to tie together things and people into a working model for research administration. This is a noble type of approach, or a noble goal, I think. We are just so far back in the dark ages on the academic point of view that it is going to take quite a while before we learn how to describe in mathematical or even heuristic terms what is the real situation in research and research management. So, I think that our Conferences have included in the past some of these academicians and results of scholarly efforts.

I personally believe that it would be good to continue that. What the distribution between experience and theory should be is not, of course, up to me, but up to the Conference management directly from year to year. I am only expressing a personal opinion when I say I think this is good and we should have more of it.

R. L. HERSHEY, *E. I. du Pont de Nemours & Co.*

I want to make a few remarks on Professor Marquis' excellent talk. I couldn't agree with him more that the research function is the most difficult of the various functions a businessman has to face in managing. I myself have said so many times, and I have given the first of his reasons for my belief on that point.

On the other hand, I was a little startled to hear him apply so broadly the statement that you cannot learn from experience. I think this is right if you go to the extreme speculative end of the R and D spectrum, but my own experience would suggest to me that if you move over to the development side of it, at least in purely industrial research, it *is* possible to learn from experience. If not, then I have been deluding myself for 25 years in believing I had done so, in certain phases of the development process.

Finally, I would like to remark on the myth that one necessarily learns from experience. This certainly is a myth. One does not learn from experience unless one has the ability and the desire to examine experience in order to learn from it.

D. G. MARQUIS: From your experience, sir, in development contracts, what is the value of a PERT control system, as compared with a simple . . .

R. L. HERSHEY: My experience goes back to when there was no PERT, so I am disqualified to measure it. Let me tell you what I am talking about. This was a series of development projects in the 1930's, in which we had a problem of taking the discoveries of chemists and, as chemical engineers, converting them into operating plans. Now, we clearly learned over a succession of half a dozen of these how to go more successfully from a chemical reaction to the operating plant.

D. G. MARQUIS: I couldn't agree with you more, and I don't want anything that I have said to carry the implication that I don't have the highest admiration for the accomplishment of the R and D people over the last 20 or 30 years. I only think it could be done about ten times as effectively.

J. E. MAHONEY: This is a question which I would like to direct to Professor Marquis. I held it at the end of the first part in the hope that somebody would direct some remarks to it in the latter part. I don't think it was done specifically. Dr. Gordon may have come the closest.

Professor Marquis referred entirely to the single word "R and D." My question pertains to research of the kind which will be called fundamental research, I think, by anyone here, not the contentious part at the borderline of application and development. I justify this discrimination on the ground that the title of this Conference did not contain "D," nor does the title of this session, and I can limit the question perhaps, and decrease the argument.

There are those who believe that the time is not entirely past when the dramatic advances in fundamental knowledge may still be made by individual intellectual giants. Such giants in the past, I think, have grown in the soil of what is known here today as chaos.

The question is, as this management business gets much more efficient, much smoother, how do you propose to insure that the essential nutrients are retained in the soil in which these men are supposed to develop. And, quite aside from the immediate commercial utility to industrial concerns, to government, how do you satisfy yourselves that the development of such individuals was not actually the result of the environment that existed then, and may now be on the way out?

D. G. MARQUIS: That's not an easy question to answer. It is hard to use statistical evidence of any sort to talk about the unique events, the Lavoisiers, the Maxwells, and so forth. You don't try to predict them. Read Tom Pruden's new book on *Revolutions In Science*. These revolutions occur when these great breakthroughs come, and in between we little ants work piling up pebbles and grains of sand to confirm, to apply, to test aspects of a theory. New knowledge disagrees with the existing paradigm—that is Kuhn's word—but we explain it away. We try to fit it in with new *ad hoc* hypotheses; eventually it accumulates to the point where somebody with real insight can get us a new paradigm, and then we work for another 50 years building up both confirmatory and disconfirmatory evidence.

I don't think we need try to predict these great breakthroughs that make the history of science. What we can do, perhaps, is increase the effectiveness of this intermediate level of work that is so essential to lay the foundation for the genius.

Date Due