SUMMARY OF A WORKSHOP ON DIMENSIONAL ANALYSIS FOR DESIGN, DEVELOPMENT, AND RESEARCH EXECUTIVES

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INTRODUCTION

This is a report of a rather unusual undertaking. The general study area of the management of science and technology contains a great number of findings but few applications. Research executives find rather little of general value in the reported literature. In an attempt to discover an approach which would divert the post course of enquiry and to expand the usefulness of present research, the authors convened a WORKSHOP ON DIMENSIONAL ANALYSIS. The following pages provide a report of this workshop.

In the first section the verbatim typescript of the workshop is summarized in chronological order. This is baseline data for the reader as he reads into the following sections. Section II is an expansion of the introductory paragraph to more clearly present the intent of the workshop and the hoped for value of the dimensional analysis approach. The immediately following section presents the theoretical model of the research and development process that served as the central theme of the workshop. The fourth and final section presents an analysis of the workshop and a tentative new model for further research.

I. WORKSHOP ON DIMENSIONAL ANALYSIS: SESSION SUMMARY

In early May, 1969 a group of twenty research and development executives from a wide variety of institutions gathered for a three day workshop. What follows is a summary of the high points from this workshop. In the opening session of the program the purpose was presented in a brief manner.

> The purpose of the workshop is to gain a richer understanding of those conditions which are appropriate for the use of certain general management tools and those conditions which render these tools inappropriate. Invariably the decisions of a research manager are based on personal experience - and the experience of others. How do managers bring this experience to bear in new situations? What do they look at - what dimensions do they measure - to tell them when their experience is applicable and when it isn't? What are the key variables to this process of bringing experience from one situation to bear upon another situation?

To accomplish this general purpose the workshop design was that of a rather free flowing discussion from which the important understandings would be abstracted at a later date. For this reason a verbatim report of the workshop was produced and this report is the basis of the following summary.

The first question put to the group was the overall question of whether or not research can be managed. This was restated in a most important way by a university laboratory research executive.

> I think a fairly meaningful question might be when, and how, and for what reason do you change the direction in which you are going at any given time. Most of us inherit a project or we have been on a project for a reasonable length of time - and it is very tempting to continue along the road you know. Then comes a time when you say, 'Is that the right thing to do?'

Here it was correctly pointed out that if a project went along in an unchanging manner and met its initial objectives, then the question of can research be managed becomes much less interesting. Thus the crucial question

is the one dealing with decision-making in a changing situation. When, how and on what bases are change decisions taken? Two general kinds of pressures for change were mentioned:

1. Pressure for change arises when progress is slow and interesting modifications become apparent.

2. Pressure for change arises when the development begins to swing away from an area of value to the laboratory, given its general objectives.

A research manager learns about the first pressure by simply pursuing the project. The second pressure can be highlighted by specific management action, or as one participant put it:

> In the real world, the best decision on what to do is made as soon as possible before you have any more research done than you absolutely have to have.

This is a strong belief in judgment and the need to identify project value in light of laboratory objectives early on. At this point a director of an industrial chemical research group suggested five bases for making decisions. Some of these bases could be used systematically and some are rather random.

> First of all, using the data that you have, there is the analytical basis - it can be economic or technical, scientific or ecological or what have you.
>
> Secondly, there is the basis of experience - what you know or what you have learned that you can document.
>
> Thirdly, there is the basis of "gut feel" - the intuition factor.
>
> The fourth basis is politics. It's politic to make a decision in a certain way. You can call it a frame of reference or company policy but it gets down to politics.
>
> The fifth basis is emotional. Joe's a good friend of yours; so you throw the project his way. Usually you do this without

realizing it.

The remainder of this introductory session focussed in one way or another upon the relationship between research and the objectives of whatever supra-organization the laboratory was embedded in. Here the question faced was not the management of research projects, but it was the management of research laboratories.

For instance, the following comment was made:

> You get to the point where research can't be divided from the total operation. In other words, the success of research depends on the success of a lot of other parts of the organization in carrying the thing through. If you have a weak link in development or production or marketing the research result can fall by the wayside. You can't look at research and development out of context with the whole organization. It is part and parcel of it, and its success depends on how it can interact with the rest of the organization.

Thus one of the keys to research success seemed to be the value the organization as a whole gains from the laboratory findings. This value can only be gained if the organization is willing to use research results. The participant continues by saying:

> Sometimes it is very difficult to determine what the management really wants because what they say and what they really want sometimes are two different things. It is only when they come up to the fence and they have to make a hard decision that the truth comes out. The research manager has to sense where his function fits into the total organization and the full success depends on how well he can sense the 'real objectives.'

At this point the meeting began to get heated as the classic tension between science and business raised its head. Referring to an example in which three research departments concurrently developed the same new product - one refusing to market it, one unsuccessfully marketing it, and one successfully marketed it: the following comment was made:

> You think that research management success is measured

> by the ultimate profit to the company, whereas I think a narrower definition is that the three research organizations (above) all came up with a successful development. Where it went from there is another story.

The rebuttal to this was:

> I think the research manager in the company that decided to drop it was wrong in carrying the development through to the point of showing success. If he didn't know enough about what his company wants, he is wasting resources to carry it out.

Another twist to this argument then arose:

> There <u>is</u> a frame of reference that the research man must be very alert to, but, it is also the obligation of the research man to help set the frame of reference. Let us not assume that management is master and research is subservient. Research has an obligation to lead, just as management has an obligation to set objectives.

The discussion then turned to the clarity of objectives with the understanding that industrial organizations had clearer objectives than basic government laboratories; but that even in industrial organizations that part of their resources devoted to basic or speculative research seemed to be as much at sea as the basic governmental laboratories. Thus one dimension of project success is wrapped up with the clarity of project objectives and the clarity of laboratory objectives.

It is becomming quite clear to this author from the verbatim typescript if not from the summary that a major area of confusion in the study field of research management is the relationship between project success and laboratory success. This is further compounded by the time dimension. Laboratory success is measured best over a period of years, whereas project success is measured in the present. Referring

to the typescript these comments are important to note. First:

> It is highly useful for us, in certain areas, to establish centers of excellence because that may even on a standby basis be highly important for us in order to maintain a competitive margin.

Then another participant:

> On this center of excellence that you mentioned, we established those about ten years ago and now you can sell them to management because you can point out the fruits of that research. For the first years of these centers you didn't have this performance data to sell management with.

These comments suggest the long term nature of the success measure when applied to basic research (centers of excellence). Now talking about development projects there is this comment of one of the authors:

> Part of the payoff from an unsuccessful project was learning so that next time they started a development program they were able to produce an order of magnitude improvement over the previous system.

Thus project failure leads to laboratory success!

The following morning the group reconvened and agreed to look at a particular case presented by the industrial chemical director. One strong flavor of the incident presented was the need for the research group to actively overcome corporation resistance to the marketing of the subject innovation. This was a good example of the earlier comment about being unable to understand the real intent of management until they had to make a hard decision. The ensuing discussion focussed on the question of should research management be responsible to "sell" their results to the corporation or should research management be responsible to pursue only the results which the corporation will buy without

any pressure from the research division.

Here the need to "sell" is presented with two justifications, one from the point of view of the laboratory and one from the point of view of the individual researcher.

> A research group justifies its existence by getting its product into the marketplace. I think that points out getting it to the marketplace is not proof of how effective the product is but how it is handled once it is innovated.

Continuing:

> The other part is the need that everyone has, particularly creative people, to achieve at the highest possible level, and inherent in achievement here is commercialization.

Supported by an ethical drug executive:

> I would agree - the idea is to develop something simply to achieve. And until the dollars come back, what have you done? You have only issued a couple of patents.

Thus it was argued that the "selling" of research results to the corporation is a responsibility the research manager has, not from the corporation, but toward the individual researcher. Here a contribution from one participant added a specific management strategy which makes the "sale" of research results quite a bit simpler.

> As soon as we do identify a product concept that we are interested in moving through to the marketplace, we set up a steering committee composed of top management people in the three functions that are involved (marketing, production and R & D) and a project manager, and a task force. This starts very early so that as the direction of the project has to change the production and marketing men are part of the decision. But, then, if you look at corporate research which isn't tied to an operating division, the problem is altogether different and much more complex because it is much more difficult to create this sort of an operating team.

This statement which offers a technique for making the "selling"

job easier and for making the transfer from R & D to production to marketing easier reinforces the concern about clarity of objectives. The inference is that in divisional research a product team can easily be identified, while in corporate research this is not so. The value of clear objectives in estimating research success and enabling research management was not universally accepted as the following interchange demonstrates. First a food products director:

> I think everyone in this room has a responsibility for the pursuit with management of where they are going to go in business. That has nothing to do with running the research facility or defining objectives, but simply trying to get your stuff sold.

A chemical executive:

> Essentially, you are dealing with management by exception. As long as what you are producing is being absorbed, you are satisfied. If for one reason or another, the flow of your products to top management stops, or it is not being accepted adequately, then you have to do something - something unusual, at times.

The rejoinder:

> I think you have to be sure that they have an understanding of what it is you are getting them into at an early stage. If you start them when you start and then keep playing to them, when the new product comes, it is not a surprise.

In understanding the reasons for needing to "sell" research results and some of the problems attendant to this, a quite important new twist was added to the discussion by a commercial electronics executive. His comment was directed to the core example under discussion.

> The product division managers accepted fully - that here is a radical new process with a marvelous market, but there is a big risk factor. These managers have to measure the risk against their profits <u>this</u> quarter, <u>this</u> fiscal year, and over the immediate future. If the divisional managers go into

> something long term, where profits aren't going to come up for a long period of time, they are not likely to be charitably measured in the present. So, on the scale of values of the product division manager, which is heavily influenced by the feedback and measurement system of the corporation, it is quite understandable that a long term project wouldn't be very interesting.

Thus the standard accounting procedures of most corporations is a demotivator in regard to accepting long term risk and this present orientation causes a major tension between research and operations. Minor product improvements will flow easily from research to production but major improvements, new products or breakthroughs meet a resistance which is intensified over and above the inherent risk by the financial control system used to measure the operating division. Later he returned to this subject at some length.

> I was thinking about this in our discussions last night, the very significant importance of the way the accountants - we sometimes call them the bean counters - arrange to put the beans. Its feedback effect on people, I think, is intensely critical. That is why I responded earlier to be charitable to the product division manager who might be measured on a short term basis. He does not have time to look far beyond. Under those conditions a central research function regardless of how glorious or productive it might be isn't likely to easily gain acceptance for its new products or new knowledge. But often the accounting person without perhaps too much experience may set up structures which the research organization is forced to fit within. The feedback loops are established and a major impact is felt on research effectiveness.

Still trying to grapple with the problem of selling the result of research to the rest of the organization, a new task was tried. In this new task several areas of uncertainty were identified. The original ideas of the authors about need or target uncertainty and the technical certainty was expanded by pointing out areas of resistance to the use of research results.

> I think about five different factors. One is technology; another is manufacturing capability; another is marketing capability - not just capability, but existence in the market of known activity; another is financial structure; and another is management.
>
> When looking at potential new products or new knowledge that might go into a product one must be aware that if more than one of the above concerns are new, then the barriers toward getting it through and being a profitable product for the corportation are extremely high.

Uncertainties that come from lack of experience can lead to many barriers to innovation. For instance, an electronic company of this author's experience considering going into leased computer peripheral equipment faced a market which they had never explored (industrial rather than government), a financial structure they were unfamiliar with (leasing rather than sales), a new technology (computer logic for ground-based equipment rather than airborne), manufacturing processes that required competitive approaches (low unit cost) rather than aerospace methods (high unit cost), and a new management team to head up the project. On all five counts they were in trouble according to the criterion posed above. They succeeded in selling only one systems worth of peripheral equipment at a great loss to the corporation and a great deal of embarrassment.

After a brief recess, in which three main groups tried to see when it was a research manager had to "stand up and be counted," the group began to wander. One of the criteria of importance that developed from this wandering is related to the organizational relationship between the research laboratory and the set of people who represent the user. Sometimes this set is marketing. Three general types were suggested. The first type of

organizational relationship is one in which the research laboratory has all the information upon which the project selection decision is made. Two examples of this were represented at the workshop. In one, marketing research was part of the research division, and there were no organizational members outside research who had good information in order to second guess. In the other, the laboratory was dealing in such advanced research that they played the role of adviser to the customer (government) as well as researcher. The second type of organization is typified by a company in an old technology. Here research and operations have worked together for so long in a slowly changing technology and market that disagreement is rare. The third type is typified by the normal government contracter. There the customer is a very complex, sometimes uncoordinated, government. Clarity and agreement is often tenuous and mercurial.

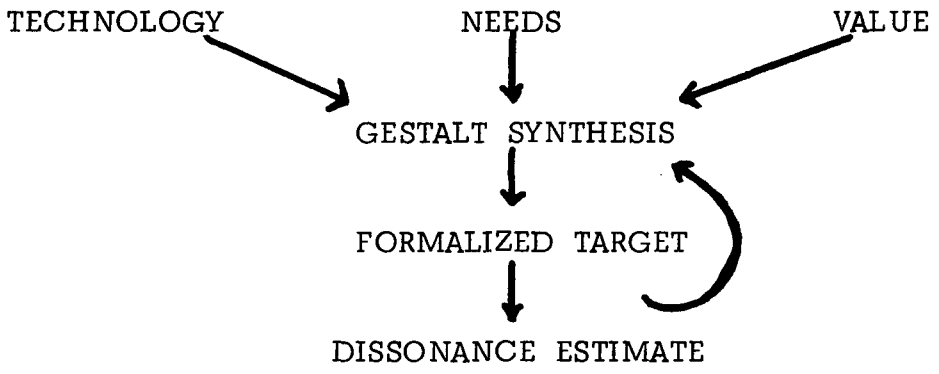
Following the luncheon recess the remainder of the small groups reported to the session. Another aspect of the feedback problem presented earlier was brought up.

> I think one reason your chief executive doesn't place the R and D man in a top management role is that by the time a product becomes a success and has generated a big pile of profit the chief executive associates its success with the marketing organization. There are very few who ever think back to the origin of the product. The top guy would rather look at something existing, something big with a lot of volume and profit. When I say can you remember this product when it was a prototype on the bench, he can't quite remember that. He just remembers that now it is a very successful business. Then when I say I can remember when it was a prototype on the bench and now I am showing you another prototype which I think has the same potential, he looks at you kind of funny and gives you this permissive, 'Yes,but'.

One of the inherent difficulties of managers involved in R and D is

the extensive time horizon between their work and noticeable value to the organization. Since executive mobility in any one role is so short, two to five years on average, very few executives have from personal experience seen research pay off. Thus the manager must invest in the future, knowing that the risk he takes even if right will not be to his personal benefit as he will have moved on to a new role before the investment begins to pay off.

Here the conference turned to the presentation of a paper entitled "A Speculative Framework for the Analysis of Research and Development". This paper itself is presented intact in the third section of this summary. To relieve the reader from immediately referring to the "Speculative Framework", a small section of the model is summarized below. It is this section which served as the focus of much of the ensuing discussion.



A Gestalt synthesis is said to occur when a researcher on a research organization can initially perceive how a combination of technology might satisfy a need or set of needs that are valued by some group of users. Over time the synthesis becomes clearer and finally it approximates the reality of the new product or service or process. A formalized target is

an attempt to spell out the synthesis (idea) so that others can help to work on it. In early days it is called a line of research, later a specification and finally a drawing set. A dissonance estimate is the researching part of the organization trying to estimate whether it can successfully invent or develop the line of research or product as represented by the formal target. As the research itself is pursued the dissonance estimate becomes sharper and the organization can more closely specify the differences between the possible and the target. This estimate or evaluation often serves to alter the synthesis or idea and eventually to alter the target. The full model bears inspection but is not needed in order to detail the workshop reaction to the delivery of the "Speculative Framework".

The group struggled with the "speculative framework" for quite a while. They needed to understand some of the unfamiliar words used in the model and to see if it fit different situations. To some, the model appeared to be useful for development of systems but not component development. To some, the model applied to product research not basic research. To some it seemed to have universal applicability as indeed the authors had hoped. One criticism that was dead on is reported here.

> "It seems to me that this model takes it from the top down. I perceive you think perhaps that a research manager or somebody up in the top of the organization gets these wonderful Gestalt Syntheses, and he perceives in his infinite wisdom what should be done, gets it done, gets the feedback and manipulates the whole thing to the project. How about the case when the Gestalt Synthesis comes up from the bottom? Have you looked at that? In our laboratory this happens very, very often. The idea comes from way, way down".

A further well taken criticism of the model was offered by another participant:

> "One concept you used - when you went from "formalized target" to "dissonance estimate" does not appear to be valid way to think about research. You infer the flow from "synthesis" to "target" to "dissonance estimate" is down the hierarchy and done sequentially. Those three things always have to be done by a closely-knit team of people. Normally in a big project you get 20 guys together, and they churn up and down the hall and scratch on each other's blackboard, and later certain things become obviously ridiculous and certain things aren't. Never do you hand a "formalized target" to another outfit who looks at it, does a "dissonance estimate" and then feeds it back to the Gestalters. It is always one group. In the first step you don't have formalized targets - you have parametric traits. You go through this thing parametrically, not freezing it too fast. It is sort of a big change as you are organizing chaos. Then you get to the preliminary design phase, after which the concept of formal target can be divorced from the concept of "dissonance estimate" (evaluation) and from that of doing the research itself".

The remainder of the session was devoted to the value of structural problem analysis and structured decision analysis, which is a way of making good decisions on limited information. The Kepner-Tregoe methodology was highly recommended with examples of its value. The specifics of the technique were not discussed. At this point the session adjourned for the evening recess.

In the morning a case was raised that involved the closure of a major laboratory and the transfer of some of its programs to other laboratories. Some of the personnel from the closed facility also were transferred, but the majority were detached. This laboratory had been operated on a contract basis and part of the closure decision turned around the need for an in-house capability in this field rather than the dependency on an external laboratory, even though it was under contract. In the discussion of this decision several

points were raised. The closed laboratory was involved in basic research and applications which were believed to be many years away from field use. Thus it was known that the transition period from external laboratory to internal laboratory would delay progress by two or three years, but the impact of this delay was impossible to estimate. This delay was to be tolerated in the light of an overriding policy to have positive control over research areas such that future heavy pressures would not be able to interfere with the response capability of the organization. Another point of this decision is the nature of consultation. The decision was taken with only a few members of the organization participation. The financial information relating to the decision was easily available, but the rest of the decision parameters were purely judgemental and the decision itself was, in a sense, considerably political. The decision was taken at a level in the organization where the judgement could best be made. It was taken at the first level that encompassed all the research units which were relevant to the organizational strategy relating to the value of in-house capability.

The wrap-up of this particular discussion raised the issue of research organization survival. Some organizations of the size of the closed laboratory would say that there is a lot of value to the continuance of our working together. Somehow we reinforce one another and we have reached a "critical mass" that makes our organization as a whole greater than the sum of its parts. Other organizations would not feel this way and each of its components would drift into new association. In this latter case the cross-stimulation between components has not been very high, and each component is relatively independent. In such a case the organization exists only as an

umbrella to handle administrative problems and not much else. The question raised but unanswered in the discussion is 'What conditions create critical mass or interdependency such that organizational survival becomes an important issue'.

The remainder of the workshop was devoted to evaluating the sessions and concluding how a future workshop might be made better. This concludes the summary of the typescript. The section following this presents an analysis of the workshop intent and the results of the dimensional analysis approach.

II. WHY A WORKSHOP ON DIMENSIONAL ANALYSIS ?

The stimulus for the workshop was the rather widespread agreement that much of our shared knowledge about the management of research has limited generality. The weekend focused on one important premise:

> The ability to usefully share knowledge about research management has been limited by the lack of explicit identification of important conditional dimensions.

Most reported experience has implicit limitations to the understanding of its general usefulness - limitations which are due to overly simplified descriptions of the conditions surrounding the experience being discussed. For instance, results are often reported thusly:

1. It was found that creativity was stimulated when controls were relaxed. (Data is from seven pharmaceutical laboratories.)

2. Project management forms of organization cause an increase in voluntary termination. (Data from six aerospace corporations over a three-year period.)

The objective of the workshop was to overcome such sparsity of description and to develop a rich set of project and environmental descriptors or "conditional dimensions".

Evidence shows that good research managers do, in fact, manage research such that there is a directive correlation[1] between what they do and the outcomes that they desire. A good manager is richly connected with the feel of the institution and the people with which he deals, and this rich

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[1] A directive correlation implies a purposive causal relationship between action and desired effect.

connection allows him to make decisions based on a great number of variables only a few of which are ever systematically articulated. In a large sense this is why length of experience pays off so handsomely in the field of research management. In essence this is saying that a good manager really does understand what he is doing, at least in the sense of his intuitative grasp of the process and of the hueristic or rule-of-thumb rules which seem to work within this specific context.

The difficulty then lies in the ability of a particular good manager to share this knowledge with people with whom he has very limited contact. Within his own staff he shares this knowledge by constant example and action, discussion, bull sessions, and the like. External to his organization - and by external I mean upward to the corporate level as well as outward to the research community in general - he has an extremely difficult task in describing what really went on in his own mind when certain types of decisions were taken.

The writer or speaker in the field of research management tends to report what may be termed conventional wisdom about the management of research. He knows what he does. He would like to tell other people what he does, but he has a little trouble imagining the context in which they might apply what he has to say and as a result of both time presures and limited capability to interact, the presentation focuses on one or two points which were developed in a very limited setting and the richness of the experience goes by the boards.

The problem that stands out now is the method of arriving at a set of dimensions. There seem to be three fairly interesting techniques which might

be used for such a task. In almost all cases, these techniques lean heavily on some form of disagreement. "It is when I explore why you disagree with me that we begin to see what are the dimensions of the problem over which we have this disagreement." In effect, it is often disagreement as to what the problem is that makes it so hard for individuals to agree on the solution.

One device is to have a general problem presented in a strange language, in other words to take away from the researchers and the people discussing the problem the stereotypes up which they lean for everyday communication. It is important to describe the situation in unusual words, words that stick in the mind which are not comprehended by the twinkle of an eye. From this point the discussion of a particular problem, once stated in this new language, tends to be around the elaboration of what do you consider a particular word to connote, what does it mean, and is it the understanding of what you mean by that word and what I mean by that word and what somebody else means by that word that begins to reveal the dimensions upon which the group has worked for a long time but never articulated.

A second technique involves the construction of an idealized model of the problem, the line of research for instance or the development objective. This idealized model is then placed before a number of relevant people who must contribute to the realization of this model. In the interchange back and forth among these relevant members over the possibility of realizing this ideal, they begin to understand what kinds of hidden unarticulated dimensions are of importance. To use an example from a large development effort, a

number of companies, when they begin a development effort, take the managers from all the related functional areas offsite for a two or three day conference. In this setting they hammer out the basic problems of execution in this particular project. Here they find out from the manufacturing man that when this project is scheduled to be released to production, there would be a production capacity restraint, but he will be able to handle it if it slips six months or is available three months earlier. It is these kinds of understandings or other kinds of parameters that really make the difference between the way you manage this particular project versus any other.

There is a third technique for developing dimensions and this seems to be the technique most relevant to the development of a so-called dimensional analysis for research and development broadly conceived. This is a technique of focusing on a particular set of issues and bringing a heterogeneous set of research managers together. The concept here is that a particular critical managerial intervention by one man in one setting may be viewed by other men as being irrelevant or it doesn't seem that it would really cause the effect that the man claims. Seasoned researchers would question other seasoned researchers to articulate a little more fully as to just exactly what was going on that made that particular managerial intervention a success or perhaps a failure. It is crucial to understand why a particular intervention in a particular setting does work and does not seem applicable in some other setting. It is these kinds of understandings that lead to a more explicit listing of dimensions that seem to be relevant to the comprehension and sharing of research management knowledge.

It is this particular approach that the workshop employed to explore the dimensions of Research and Development. A transcript of all comments made by the participants provided a record of the workshop and subject material for post workshop analysis. Unfortunately, in the dynamics of small group discussions it was not possible to directly explicate as rich a set of dimensions as anticipated, although much sharing of insight occurred in these sessions. An analysis of the transcripts does however provide new information about important dimensions.

From a preliminary point of view the theoretical model presented on the second day seemed to continue to hold some promise. Analysis of the other data developed dimensions that were not accounted for in the model. The immediately next section of the report presents that theoretical model while the later sections of the report focus on the post workshop data analysis and the conclusions regarding dimensionality.

III. A SPECULATIVE FRAMEWORK FOR THE ANALYSIS OF RESEARCH AND DEVELOPMENT

Introduction

Like all executives, the research and development executive is basically an "action taker". He takes action in a situation frought with uncertainty and complicated by the unusual behavioral[2] characteristics of his staff. He is faced with a broad assortment of issues upon which he must take action in order to move his organization forward. These issues involve his organization's ability to develop relevant technical capabilities and a continuing stream of progress in the long-run as well as his organization's ability to execute present projects and allocate present resources in the short-run. The nub of the executive's problem seems to revolve around the interaction of uncertainty and behavior and the trade-off between long-run and short-run. The speculative framework presented herein presents a set of dimensional questions which focuses on this critical issue.

This paper explicitly deals with design, development and research (DD&R). At a specific level there are many differences between the various activities that are characterized as design or development or research; but, the different types of DD&R hold in common the central theme of purposeful learning and the application of this learning. This paper intentionally focuses on the common dimensions of such activities. It offers a framework that is aimed at usefulness for discussion, comparative analysis and improved understanding among those who must manage DD&R activities. The

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[2] Highly educated, creative, independent, oriented to personal growth, resistant to structure, etc.

payoff from such a common framework is an ability to selectively use knowledge which others have acquired about the management of these activities.

Several common elements pervade all DD&R tasks. One set of such elements can be grouped under the heading of "sensing the developments external to the organization." The organization must be informed about technical or scientific possibilities (i.e. the state-of-the-art), the sort of results that are needed and the changing value of those needs.

In order to assess technical possibilities the organization must find ways of keeping abreast of new technical knowledge being developed in the scientific and technical community. This involves many diverse forms of information acquisition. This diversity arises from the diversity in the sources of information, the diversity in the methods be which the organization digests the information and, of course, whether the information is needed for a present problem or to increase the organization's basic store of knowledge. Such knowledge includes the question of what is our present state of knowledge about matrix isolation, full cell development, or ESP, for instance. A similar question is what is our likely future knowledge about these research areas in the short-term and in the long-term.

The organization must also sense the technical needs toward which it might be able to make a contribution. Such needs include contributions to basic knowledge as well as applications of basic knowledge. This, also, involves the use of diverse information acquisition methods. For example, will increasing pollution or decreasing fossil fuel supply present new demands requiring solutions arising from some combination of DD&R activity?

Interestingly, the relative prioity of needs continues to shift as the underlying value structure in society shifts. For instance, the advent of urban rioting in the

mid-sixties has seem to trigger a number of diverse shifts in emphasis within the research community. There has been some increasing interest in the "ruin problem" in actuarial mathematics due to the increasing size of insurance losses. Similarly, an increased emphasis on riot control has led to the development of technical devices to aid law enforcement departments. Whereas, a different thrust of concern, that of riot prevention, has stimulated interdisciplinary social science research. Relative need priorities are, also, affected by organizational value changes. Diversification, for instance, tends to broaden the range of applicable basic research while at the same time requiring more specialized applied research. The decision to emphasize mission oriented research and to lessen the resource allocation to central research is a reflection of a changing organizational value system.

Aside from the sensing of input an organization must, also, have a way of putting them together -- of connecting technological possibilities with needs. This process may generate the understanding of a potential theoretical breakthrough, or a possible innovational product, or a major improvement in the present way of doing things. The coupling of need and potential is crucial to the successful DD&R establishment. With the above as prologue this paper now describes a conceptual framework for analysis.

The Conceptual Diagram

The conceptual diagram presented here is a schematic version of our present focus regarding the research and development process.[3] It is presented in a rather simplified form for the sake of clarity. The diagram is the contextual setting for the following detailed description.

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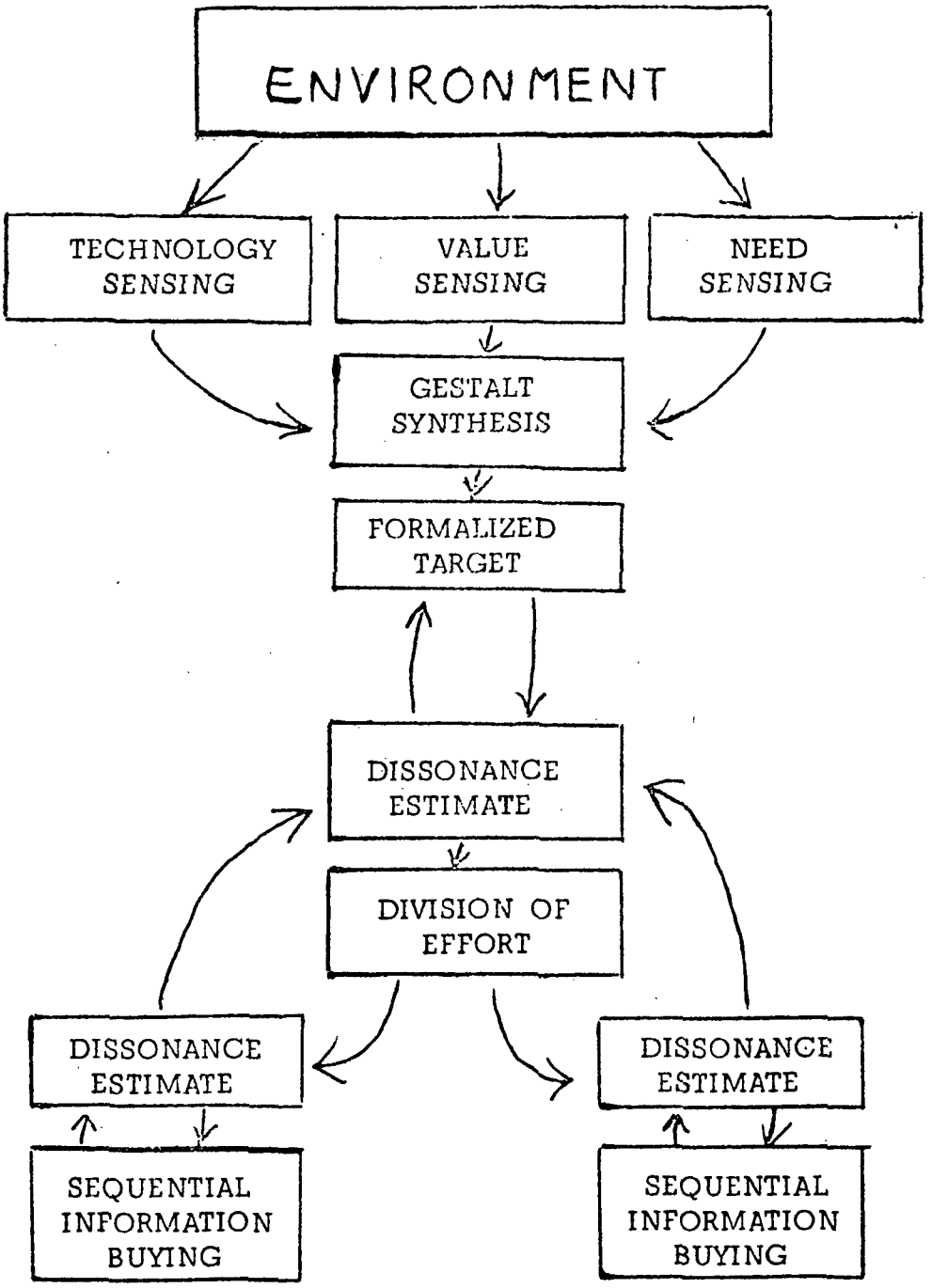
[3] Although our framework is considerably different from that of Roberts in his The Dynamics of Research and Development (New York: Harper & Row, 1964), we do subscribe to the process theory of research and development.



A SPECULATIVE FRAMEWORK FOR

THE ANALYSIS OF RESEARCH AND DEVELOPMENT

Briefly the diagram depicts an environment which is coupled to the research and development process by three sensing systems. The sensors obtain information regarding 1) the present scientific and technological state-of-the-art, 2) needs, both external and internal, and 3) values from society at large and from the organization itself. The information is combined into a gestalt synthesis which recognizes that a particular conbination of the sensed technology will probably meet some of the sensed needs in a manner which is consistent with the sensed values. This gestalt synthesis is then transformed into a formalized target (e.g. a line of investigation for basic research or a specification for a product improvement).

To continue, the formalized target becomes an input to the next stage in the process of research and development. The initial step in this stage is the construction of a dissonance estimate, that is, an evaluation of whether the target can be achieved. The estimate includes cost and schedule as well as the performance envelope. This dissonance estimate is then fed back to the gestalt synthesis. After the initial evaluation has been made a division of effort occurs among the individuals and/or groups of the research project/team. The individual groups now perform their own dissonance estimate and feed it back to the top level management. The research groups then engage in the process of reducing to practice the part of the problem assigned to them with periodic and/or continuous feedback vis-a-vis their current progress and their current dissonance estimate.

The following sections of this paper will concentrate on an elaboration of each step in the process. Emphasis will be placed on a description of the nature and the dynamics of each step in the process.

Value Sensing

The value system plays an important role in sensitizing an organization to other inputs. Because of this, the values held by the individuals and the groups which determine the overall objectives of an organization are an important conditional variable in the research and development process. The traditional, professional value system of scientists -- the quest for "pure" knowledge -- is becoming laden with a sense of social consciousness.

The value systems of many large aerospace or systems corporations have recently gone through a phase of significant change. Social problems are now included on the menu of needs considered as possible targets. This shift in values has caused a secondary shift in their traditional information couples with answer producing sciences. For example, behavioral sciences are now included where only physical or natural sciences may have been previously considered relevant.

Major policies and actions are formed in such a way as to satisfy the preferences of those who are important to the organization's continuation. Cyert and March[4] have conceptualized organizations as viable coalitions which determine their objectives by negotiation among the members of the coalition. The organization's value system then changes by changing the preferences of individual members or by changing the membership of the organization. This first process is relatively slow.[5] The second process is relatively constant with a complete change in

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[4]R. M. Cyert and J. G. March, "A Behavioral Theory of Organizational Objectives" in Modern Organizational Theory, Mason Haire, ed., (New York: Wiley, 1959).

[5]This is inferentially derivable from the work of E. H. Schein, I. Scheiner and C. A. Baker, Coercive Persuasion (New York: Norton, 1961).

membership estimated at 10 year intervals.[6] As presently conceived, the value system of the organizational coalition changes relatively slowly but is subject to exogenous events such as urban disorders and other seemingly remote considerations.

Need Sensing

The need sensor is crudely coupled to the environment and provides a list of needs of the relevant society and estimates of the benefits of these needs. The list of needs that are consciously or even subconsciously available to a research organization is very incomplete when compared to total societal needs. The major area of need sensing of organizational concentration is somewhat determined by the organization's conception of what "business" it is in. The need sensing mechanism is traditionally considered to include marketing research, long range planning and technological forecasting. As such it is done by many members of the organization who have widely varying positions and influence. The accuracy and effectiveness of sensing need and estimating their benefits require abilities which are in existence in a broad range in the economy. The Du Pont organization seems extremely able to do this whereas the Studebaker Corporation seems less effective.

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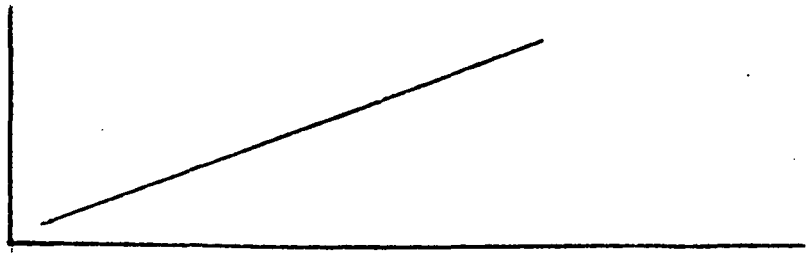
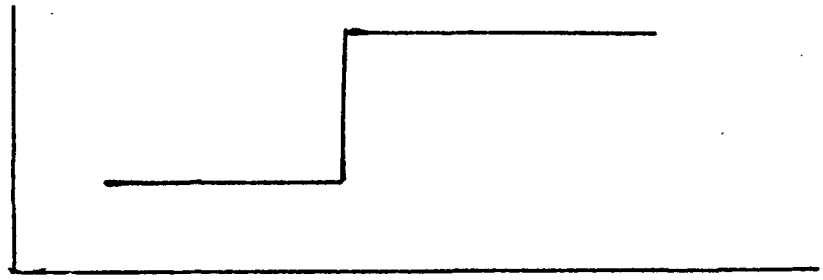
[6] Unpublished data of Richard Alan Goodman

The dynamics of needs sensing are of several modes:



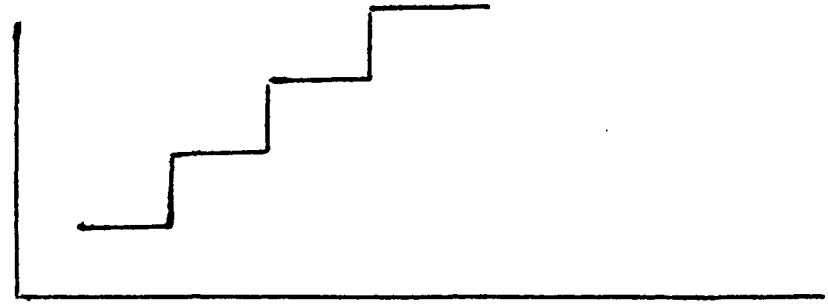
MODE A

Mode A is a continuous process best characterized by the Volkswagen Corporation's steady and continual improvement of the product. This might be characterized as a continual tension between the research department and constantly changing needs.



MODE B

Mode B is a large discontinuity caused by a major environment change (or the sudden recognition of such a change that itself may have occurred slowly). This can be exemplified by Pearl Harbor or the creation of an Ad Hoc Committee on the Urban Crises by the UCLA Graduate School of Business Administration.



MODE C

Mode C is a continual step increase in need awareness which comes from a continuous questioning of the environment.

There is no necessity for any organization to exhibit all the above modes, but it is hypothesized that large organizations probably do. The style and rapidity of need sensing has a distinctive impact upon the research and development process.

Technology Sensing

Technology sensing is a classic and obvious problem facing all decision makers in the R & D environment. Recently, under the sibboleth of technology transfer, interest has been focused more intensively on the problem of achieving an effective couple between those generating technical and scientific knowledge and the potential users of this knowledge.

The organization must design its technology sensing capability, its window into technology, to satisfy several criteria, frequently needed technical information must be available within the organization, the individuals who hold this information must keep their store of knowledge fresh, and efficient means of identifying those who are knowledgeable in new problem areas must be established. The organization must filter against irrelevance but guard against obsolesance.

Emerging results from studies of innovation suggests that in most instances it is the recognition of needs or better articulation of prior needs that usually stimulates innovations. This was the pattern revealed by a study of 550 breakthroughs in DOD weapon systems[7] and a similar pattern prevails in Sumner Myers' study of 2,000

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[7] C. W. Sherwin and R. Isenson, Project Hindsight .

commercial innovations.[8] The implications of this pattern makes the task of management more difficult. The relevant solution technology or science will be defined by the need (or problem) which is being addressed. The manager will consequently find that he must anticipate and change the organizations menu of available knowledge as areas of application are changed (i.e., market specialization).

Some organizations have taken the approach of defining an area of interest by technology. This eases the problem of technology sensing but leads to more serious problems. Such was the case with Western Union at the turn of the century. This company envisioned telegraphy as its specialty, but the advent of new communication technologies soon gave AT&T the dominant role.[9]

The ambitious organization must be willing to change and adjust its information sensing capabilities. The degree of change will be influenced by its policies of entering new use areas, but even with a relatively stable use area some change is essential if Schumpeter's gale of technological change[10] is not to destroy the organization. Even in a static environment the problem of coupling and sending is a difficult one, and one that has technical as well as behavioral implications. The most predominant role of effective transfer has been noted to be a people-to-people one, within as well as without the organization. The manager must be concerned with establishing effective people-to-people paths within a firm, change these as required, or better still, create an environment in which they will change themselves.

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[8] Summer Myers, Unpublished data.

[9] W. R. Maclaurin "The Process of Technological Innovation" in James Bright, ed., Research Development and Technological Innovation (Homewood, Illinois: Irwin 1964).

[10] Joseph Schumpeter, Capitalism, Socialism & Democracy (New York: Harper & Row, 1950).

Gestalt Synthesis

In this part of the process the organization conceptually juxtaposes the needs, the benefits of satisfying these needs, the technological possibilities and their implied cost and the extent value system. All are required, but any one or two may be latent in an organization, with the innovative spark being provided by the addition of the third element. With varying levels of clarity and creativity a gestalt synthesis envisions the combination of several technological possibliities which will produce an outcome that satisfies one or more needs either partially or completely. The synthesis also includes a conceptualization of the process by which the technology can be combined to produce the desired outcome, and therefore, the cost and time as well as the technical implications of the envisioned process.

For any one project or line of investigation the gestalt synthesis continues to change over time due to changes in the three inputs and the characteristics of feedback to the synthesis. The need, value and technology information which determine the original gestalt synthesis continue to change. The output of the actual research and development process provides feedback which increases the understanding of what really can be accomplished by the proper combination of technology and resources. As the research and development proceeds, it tends to clarify an originally fuzzy synthesis as more and more real pieces of the outcome come into existence.

The idea providing innovative spark required to form a gestalt synthesis and the effectiveness with which it is elaborated can be affected by organization form (Lorsch and Lawrence)[11] as well as organizational climate. A significant change along

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[11] Jay Lorsch and Paul Lawrence, "Organizing for Product Innovation", Harvard Business Review, January - February, 1965.

the input or feedback links to the gestalt synthesis will cause changes. One of the significant characteristics of the feedback process is the fact that an outcome which matches the formalized target may not in fact match the gestalt synthesis. This usually triggers a change in the synthesis or the target.

Formalized Targets

As a particular project progresses a more formal articulation of the gestalt synthesis is required. This articulation, called the formal target, is necessary for communication of the idea from the synthesizing organizational unit to other units which are to help bring this idea to fruition. The formalized target can assume any form from a one paragraph statement of project purpose or a pencil sketch to a 500 page detailed specification. In general the target becomes more detailed and specific over time.

It is important to realize that the target is only a surrogate measure of the gestalt synthesis and when taken by itself often does not communicate the full intent of the synthesis. In this light, an initial and continuing problem with the target is how well it communicates the intent of the synthesis. Further, the target has some form of dynamic response to the changing synthesis. This can be varied from a real time response to a lagged response of various dimensions to no response.

As development progresses, the characteristic trend toward greater detail, documentary proliferation, and specificity in the formalized target is an adaptive response to complementing changes in the program. The number of individuals involved in the program increases, as does increased tasks/specialization on their part and a requisite need for communication and control. The task must be factored into work packages which

are consistent with the specialized skill and knowledge that must be brought to bear in completing various elements of a program. The target must also be factored in a similar way to provide for the communication of objectives and goals for subtasks. The factored targets take the form of subcontracts, purchase orders, specifications, etc.

While formalized targets serve the purpose of communication, they also serve other needs, as points of reference for control and as tangible evidence and milestones for analytical and design progress. The process of elaborating the formalized target is largely paced by the need for communication and control. As more resources are added, subgroups designated, etc., there is a corresponding exigent need to provide targets for such subgroups.

A major function of management is to control decision making and resource programs to insure that target elaboration does not preceed the gathering of relevant knowledge. If the process of specifying and elaborating targets exceeds the available knowledge level, the sub target elements may deviate significantly from the choices which would otherwise have been selected.

Researcher Estimate of Process Outcome (Dissonance)

The formalized target is transmitted to a research and development organizational unit for reduction to practice. Upon receipt of the formalized target and with some communications with the gestalt synthesizing function, the research and development chief begins to expand an initial appreciation of the proposed project using techinques such as critical experiment control, systems analysis, PERT, simulative modeling, technological forecasting, operations research and the like. He estimates the possibility of producing an outcome which will satisfy the requirements of the

formalized target. The estimate, obviously containing some degree of uncertainty, provides the R&D chief with a notion of the dissonance between expected outcome and the previously articulated target requirements.

During the estimating period those involved may be in frequent communication with those directly making up the gestalt synthesis function trying to establish more complete articulation and understanding of what is wanted. This is an attempt to develop a shared cognitive mapping of the project. Varying techniques are used which improves or degrades the ability of various units in an R&D organization to develop an accurate shared cognitive map. The formality of the multiple bid technique, in which an early specifications sent to a number of bidders, leads to a poorer shared mapping than a more people-to-people process. For instance, project IHAS (Integrated Helicopter Avionic System) drew from one company a proposal for a tightly integrated multi-purpose radar subsystem, whereas the cognitive map held by the customer, envisioned a number of separate radar functions all using a single computer. The company's proposal was considered non-responsive, even though the company thought it was reacting to the customer request. Conversely, the often recommended technique of working with the customer's engineers closely during their search for a gestalt synthesis often leads to a clearer and more faithful mapping between parties. Robert's work shows that this appears to be the most effective technique for success in dealing with the government.

The estimate of the dissonance between the target and the expected outcome, as they both evolve over time, is continually changing as a result of changing input about the target. This may come about as the target per se changes or as the target becomes elaborated. The dissonance estimate, also, changes due to feedback

concerning emerging outcomes or information concerning likely outcomes from lower participants in the research and development process.

Feedback to Gestalt Synthesis

As a result of the dissonance estimate, the research and development chief then faces the decision as to what, how much, how often, and by what medium should the dissonance estimate be fed back to the gestalt function. Robert's simulation of the R&D process suggests that the higher the integrity of the R&D chief the more likely the expectation of a fruitful outcome. Contrary to this is the implication of Dubin's works,[12] which suggests that real time information may overload the channel and cause a breakdown in understanding. This suggests that the "more information the better" is not the best answer. That is, a policy of integrity smoothed over time may be a more appropriate feedback strategy.

Another phenomena relating to the feedback questions comes from the ambiguity inherent in the early targets and the poor understanding and/or ambiguity related to an early estimate of the outcome. A major dissonance is not perceptually admitted until the outcome begins to shape up to reality (e.g., until a breadboard model falls far short of desired performance).

A related feedback strategy is what information should be requested and/or furnished to develop an appreciation of the dissonance estimate. Remembering that the formalized target is only a surrogate measure of the gestalt synthesis, one can

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12 Robert Dubin, "Stability of Human Organizations" in Mason Haire, ed., Modern Organizational Theory (New York: Wiley, 1959).

easily see that a reporting and/or control system must necessarily be only a surrogate measure of a surrogate measure. This is compounded by the problem that any estimate of where you are in the R&D process is highly uncertain and due to the nature of innovation, it is hard to find measures of how far ther is to go.

The feedback of a dissonance report also, has implications for continuing support of the research and development unit. Thus, a strategy that is related to organizational viability is usually considered to avoid either early termination of a project or the development of competing approaches.

Division of Effort

The research and development chief must divide the effort required among the various, relevant, organizational sub-units (sub-units can be individual researchers or laboratories, etc.). One possible general methodology for establishing a division of effort is clearly spelled out by Koontz and O'Donnell. Their solution of assigning authority and responsibility to larger independent units, though, is often frustrated in R&D by the inability to conceive of blocs of effort or tasks which are sufficiently independent. Hall[13] suggests that the principles applicable to such a division of effort is to either minimize the number of interfaces between the sub-tasks or to maximize the predictability of interfaces which are chosen -- to select interfaces which are well understood and have a low variability associated with them (Perrow[14] agrees basically with this). For a research problem, this involves complete independence, perhaps.

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[13] A. Hall, A Methodology for Systems Engineering (New York: VanNostrand, 1960).

[14] Charles Perrow, "A Framework for Comparative Analysis of Organization", American Sociological Review, April, 1967.

Concurrently with the problem of appropriately dividing the task is the problem of maintaining a useful interrelationship and communication system between the sub-tasks. These bring to the fore problems of organizational behavior. Each organizational sub-unit will perceive its own character and behave in a way functional to its own preservation. This will add to the inherent technological difficulties, pure organizational conflicts. The management of system trade-off will be contaminated with the need to manage organizational trade-offs.

Subdivision Dissonance Estimate

The research and development unit's various subdivision receive sub-targets as their work statements. The sub-unit chief compiles a dissonance estimate in the same fashion as the research and development chief does. This dissonance estimate has one particular new dimension and that is interdependency. The sub-unit chief must, also, develop information regarding the dissonance estimates of other sub-units and their impact upon his estimate.

Ideally, continuing information on the dissonance estimate may be available through the control system. The effect of a control system, however, is to inhibit a rich dissonance estimate by requiring the presentation of information upward in a specific way. The merging of information systems with control purposes tends to turn the dissonance estimator's attention to the surrogate measures inherent in the control system rather than to the search for relevant cues from the process.

The process described above relates to the feedback to the gestalt synthesizer. Part of this feedback is formally contrained by the control system. Here the motivation toward real time feedback is a function of the organization unit security and capability.

Here the feedback is contaminated by political considerations.

R&D Process

The conduct of Research and Development may be characterized as a purposeful quest for and application of knowledge. Information is sought and decisions are made in steps or stages. The information gathering aspects are as important as decisions and actions leading toward a final product. A given project or task is typically started with only a general image of the final product.

The decision maker must guide his search for information and gauge choices so that he learns about appropriate choices as he goes along. At the same time, he must avoid the risk of commitment to a particular area which will prove to be inappropriate.

Early actions are largely information gathering in nature. These may open options or opportunities for subsequent choice. These options are finally closed as progress occurs, directions are set and product choices made. Such closure is a necessary consequence of bringing the task or project to a successful completion. The timing of commitment is, however, a variable which may be controlled by the decision maker.

While the manager's criteria is typically one of efficiency which may seem to call for early completion to conserve resources, the need for effective results and broader measures of efficiency may not be served by making early commitments. Klein, for instance, has argued that in development the most effective policy is one which considers but avoids early fixed commitments to new and promising areas. The actions which he recommends be expedited are those required to get prototypes into tests before decisions are made. He notes that development projects very frequently

fail to result in the product as planned. Scherer, on the other hand, has pointed out the problems which have resulted because the development process is not sufficiently controlled to insure the desired result.

In still another discussion of the development problem, the present authors have noted that achieved outcome falls short of the desirable outcomes because frequently targets are incorrectly set.

The manager who is responsible for conducting development must deal with all three possibilities.

1. Technological options must be developed and effective choices from the set of available options chosen.

2. As new options and possibilities are made available and the implications of previous ones better understood, it will frequently be desirable to redefine the target. This will require input from development into the gestalt synthesis process.

3. The development process must be controlled so that stray value systems, incorrect targets or plain inefficiency does not result in inappropriate or costly outcomes.

These three requirements mean that good communication with the gestalt synthesis process is needed during development.

The development process is not one in which the target is given to a developer who in turn produces a product. It is one which requires feedback and effective communication. The limits of effective communication are yet to be defined.

IV. DIMENSIONAL ANALYSIS IN ACTION

The post conference work then has focussed on locating common dimensions which are useful to distinguish the variety of research situations. This analysis was accomplished in three steps. Initially the verbatim transcript was carefully read and then rearranged for each participant. A "script" was developed that contained every remark made by a participant with enough other commentary to put the remark into context. Thereby, nineteen scripts were available for analysis, each script reflecting the thoughts and questions of a single participant.

The next step in the analysis was based on the following assumptions:

> It was assumed that the scripts of each participant, while not exhaustive, represented his conceptual model of the R & D process.

Because of the many sources of variation the dimensional differences which can be discerned may not be directly attributed to any one cause. It is the strong opinion of the authors from dealing with the data that most of the differences are reflections of differences in types of organization and industry rather than individual circumstances.

Given this assumption the scripts were scanned and the statements made were used to deduce "assertion sets." That is, a set of assumptions about the R & D process were constructed such that the scripts would appear to be the logical outcome of a person who held such assumptions. If a participant held the assumptions deduced then he would have responded as noted in the

scripts. These assumptions, and assertion sets, were then used for comparison and analysis.

For this analysis the assertion sets of six participants were used. They were selected to represent a broad cross section of the conference and to represent a significant portion of the transcript.

The six participants whose assertion sets were separated out and analyzed are identified by: 1) The type of R & D performed by the organization they represent (i.e., industrial, governmental, etc.), and 2) Whether the R & D is centralized or affiliated with an division. The specific assertion sets of each participant follow. Pharmaceuticals have been included as a separate category since they seem to defy direct categorization as either consumer or industrial.

Participant	CA	GB	GC	CD	PE	CF
Type of R & D (I-Industrial, C-Consumer, G-Governmental, P-Pharmaceuticals)	I&C	G	G	C	P	C
Present R & D organization (centralized - decentralized)	C	C	C	D	C	C

ASSERTION SETS

Participant CA - Industrial Chemical

1. An important expertise of an R & D manager is the ability to reach decisions on the basis of meager information.

2. Differences in the expertise of research management explains order of magnitude differences in the success ratios of research organizations. For instance, differences of 1/10 to 1/200 in the ratio of successful products to the total number initiated can be explained by differences in managerial capability.

3. The R & D organization's only justification (in the long run) is the organization's contribution to the introduction of new products or services.

4. Part of the R & D group's job responsibility is to encourage the utilization of their own output within the larger organization.

5. R & D people are often more creative than others in the organization and have an obligation to extend the horizon or perspective of others in respect to new product potential of R & D results.

6. To get acceptance and support for an R & D project it is necessary for the R & D man to go further than to forecast the "key property" that will be achieved. He must also demonstrate the difference that property will make. It is the latter step that is the most difficult to get across.

7. The R & D group seldom gets credit for its contribution to a new product.

8. The R & D group must collect its own "marketing research" (sense its own needs). Those in marketing have difficulty in seeing beyond immediate obstacles.

Participant GB - Government Laboratory

1. The R & D organization operating in the public sector with no economic product and no profit and loss statement as a gage faces a much different decision making environment than the R & D laboratory of a corporation.

2. Considerations of major importance in closing (or opening) a given government laboratory are the effect of this action upon:

   (a) The balance of R & D capability throughout the military service involved,

   (b) The need to have "in house" capability in those technical areas which tend to be main stream for the military service, and

   (c) The need to retain high quality R & D personnel in important areas.

3. It is important to periodically review the justification for an entire laboratory since there are no built-in review mechanisms. A laboratory will continue in operation perpetuating its function without change unless a special review is forced.

4. The interaction of R & D personnel within a university environment is a very desirable objective. Location of an R & D laboratory with a university does not guarantee such interchange however.

5. There is a significant risk in allowing the continuity of coverage, in an important R & D area, to lapse for as little as a year.

Participant GC - Government Laboratory

1. The R & D manager should intervene in the normal process when plans begin to fail.

2. An R & D manager must accept the overall goals of the organization as a framework for decisions and not be influenced by his own need for gratification.

3. Our organization is not concerned with specific R & D products but rather with the task of advancing science or technology in specific areas. The output of our laboratory is not the specific product but an improved capability or reliability of products across a broad field.

4. A long time lag (two to four years) will be involved within an organization in developing a competence near the "state of the art."

5. There tend to be long term cyclical fluctuations in centralizing and decentralizing R & D functions. During some periods they are located in association with operating elements, (i.e., marketing production, etc.) and at others they tend to be more autonomous.

Participant CD - Consumer Product Laboratory

1. The R & D manager's long term success depends upon his success in sensing the real objectives of other members of management in the organization. This is very difficult but the R & D manager is in a position to do so by crystalizing these in terms of an R & D choice and obtaining a commitment from others.

2. The R & D group can't live within the larger organization unless the objectives of all are nearly coincidental. Success in R & D can't be divorced from success of the total organization.

3. In sensing values of other members of management (not in R & D) there is often a lag in feedback that influences the prospects for rapid action.

4. Long term fluctuations in centralizing and decentralizing the research organizations tend to occur because management "forgets" the disadvantages of the strategy not in use.

5. R & D groups tend to assume the criteria or values of the other organizational elements with which they are most closely affiliated.

6. Accounting systems cause problems because they emphasize this year's costs and payoff -- an emphasis antithetical to good R & D decision making.

7. A team or committee, made up of members from different elements of an organization can be very effective in moving a given R & D product through to success. The team must be set up early so that all may feel a party to the changes that will be brought about.

8. The operating divisions ideas about needs and objectives can only be changed slowly.

9. R & D decisions must be made in consensus with others in the corporation outside of R & D.

10. A good manager in R & D will be a good manager in other areas. Management ability is more important than R & D ability.

Participant PE - Pharmaceutical Laboratory

1. Profits are important to the achiever in the R & D group because they measure success for the man who wants feedback on his performance.

2. Establishing plans for R & D is part of the corporate goal setting process and, therefore, cannot provide space. Interpreting corporate goals for decision making in R & D consequently is not an appropriate way to characterize the problem.

3. If all the informational sources involved in evaluating new products are located in R & D then resistance to new product decisions is largely eliminated. There is no basis for disagreement.

4. R & D decision making is strongly influenced by visibility of the control system, i.e., accounting records. Accounting systems, by focusing attention on this year's profits, force an inappropriate de-emphasis from long term R & D decisions.

5. Good researchers want to publish their results to share them with others.

Participant CF - Consumer Products Laboratory

1. In the absence of good criteria concerning payoff we supported more R & D centers than could now be justified.

2. The payoff afforded by operating an R & D center can be evaluated (at least subjectively) on the basis of about 10 years of results.

3. Other elements must be convinced of the benefits of any given research result irrespective of their participation in the setting of R & D objectives.

4. Other elements of organization must be prepared and kept aware of product implications from the very early R & D stages.

5. Management's joint utility for risk governs decisions in R & D. The R & D decisions that are taken on individual products depend upon the willingness of others in management (outside of R & D) to take risk.

6. An R & D group which is accommodated to the consumer market has difficulty in obtaining payoff from an industrial market spinoff that may accrue from R & D.

7. The assessment of an R & D product or outcome, that is made in undertaking the supporting R & D, is a "subjective commitment" and is very difficult to transfer to others.

8. Other management functions fail to properly attribute success origins to the R & D group who initiated a product.

9. R & D project selection cannot be buffered from the effects of the R & D groups affiliation with other organizational elements.

10. R & D decisions are ultimately based upon consumer need. The path of relating such an assessment to a product may be diffuse but this is the true criteria.

11. In supporting university research you have to have a definite beginning and end to a project understood from the beginning, or you make more enemies than friends.

V. AN ANALYSIS OF THE ASSERTATION SETS

A cross comparison of the assertation sets highlight the areas of difference and the areas of consensus. Differences were found in regard 1) to the generic nature of R & D; 2) to the question of who establishes criteria and goals; 3) to the question of criteria for project selection; 4) to the location of risk; 5) to the sort of information required to justify specific projects and 6) to the type of expertise required of R & D managers. On the other hand consensus was found around issues such as the requirement for R & D group to "sell" their output to the larger institution and the poor link between R & D accomplishment and institutional control systems. The differences are treated first in the paragraphs that follow.

Perception of R & D Management. One difference among the views presented by those participating concerns the way R & D is considered or envisioned. On one hand the statements made refer to R & D as a process that can be objectively characterized and defined; modeled and improved. An alternative characterization defines R & D as more of an interpersonal and group process. The way individual participants refer to the R & D process suggest some fundamental differences among participants in this respect.

This sort of difference is apparent in the transcript but can be demonstrated by a comparison of the individual assertions which were developed from the participant's comments. Each assertion in the set offered by an individual was classified in accordance with the way it referred to R & D.

The following classifications were used:

i. The statement concerned either a description or recommendation concerning the role, or behavior, of an individual.

ii. The statement concerned the behavior of groups or organizations involved in R & D.

iii. The statement concerned objective and non personally oriented observations about R & D as a process.

The breakdown of assertion sets into these three categories suggests a major difference among governmental participants and commercial participants. (As a reminder government means in-house government laboratory not a government contracter.)

Table 2

	Participants	
Assertion Category	(CA,CD PE,CF) Commercial Establishments	(GB&GC) Government Establishments
i (Role Behavior)	10	1
ii (Group Behavior)	12	1
iii (Objective)	11	10

A chi square test shows that at a 5% confidence level, the assertion sets of government and commercial participants are not drawn from the same population.

This very striking distinction in the conceptual model of R & D seems related to the distinctions noted in the section below regarding the criteria for selection of specific projects. It is a rather puzzling finding and may be

explainable as a coping mechanism. The government in-house laboratories engaged in fundamental research, have few objective measures of performance and they continue to struggle with the objective issues. On the other hand the development laboratories have an abundance of objective measures, (though most seem inappropriately short term) and are struggling with behavioral questions regarding the actualization of these objectives.

Precedence in Establishing R & D Goals and Criteria. A critical point of difference that emerges concerning R & D goals revolves around the subservience or the jointness of R & D to the corporate goal setting process. Does the corporation set goals and then ask the R & D organization to help meet these goals? Or does the R & D organization and the corporation jointly set goals in light of the existing technology? Most participants, both government and commercial, projected an assertion set implying that the R & D goal setting process is subordinate to the goal setting process of the larger organization.

Assertions GB2, GC2, CD1, CD2, CD5, CD9, CF5, CF9 imply that R & D decisions are subordinate or at least strongly influenced and constrained by conditions and preferences previously established in other areas of the organization. For instance, GC states "An R & D manager must accept the overall goals of the organization . . ." CD says, "The R & D manager's long success depends upon his success in sensing the real objectives of other members of management in the organization."

Participants CA and PE on the other hand strongly imply, and in the case of PE directly claim, that the R & D goal setting process is in fact not subordinate to the process at higher organizational levels.

"I say if a corporation is going to base its success on research then research management ought to be a part of corporate management." --"You -- (participant CD) -- were implying that somehow research management was talking to them instead of being a part of them."

This observation on the part of participants CA and CD is consistent with other comments which suggest that the R & D group in CD's organization has achieved a significant level of formal leadership in respect to the rest of the organization. The aspect of dominance in goal setting seems to be symptomatic of an important dimensional difference among R & D organizations as it should affect risk taking and the inherent probabilities of success.

Criteria for Specific Projects or Programs. As might be inferred from above, there tends to be a major difference among participants as reflected by assertion sets, as to whether the criteria for investing in specific R & D projects should be group oriented or objective and goal oriented. More specifically on the basis of the assertion sets criteria seemed to fall into three broad categories.

i. The criteria is definite, concise and product related. -- R & D can only be justified to the extent that it promises (in the long run) to contribute to specific product lines.

ii. The criteria is a group consensus. -- The type of R & D undertaken must be acceptable to the management group (within and without R & D). The criteria is the consensus of management.

iii. The criteria is remote from specific applications. R & D can be justified on the basis of its contribution in maintaining the

technological competence of the organization in the specific area at some acceptable level.

Various participants can be segmented in accordance with which of the three criteria statements noted above they assert as important. Participant CA strongly asserts criteria i (see CA-3). At the same time the need to convince the management group, criteria ii, is recognized (CA4 & CA6). Participant CA is therefore designated i, ii. It should be noted that the existence of objective criteria make group consensus less important. So it is internally consistent that participants should tend to recognize either iii or both i and ii. The criteria emphasized by each participant is listed in Table 3.

Table 3

Categories of Criteria for Selecting New Projects

Criteria	Participant
i (Specific)	PE Pharmaceutical
i (Specific) ii (Consensus)	CA Industrial Chemical
i (Specific) ii (Consensus)	CF Consumer Products
ii (Consensus) i (Specific)	CD Consumer Products
iii (Maintenance of Competence)	GB Government Laboratory
iii (Maintenance of Competence)	GC Government Laboratory

There is apparently much heavier reliance upon proxy criteria in justifying expenditures in Government R & D than in Commercial R & D. As speculation we might imagine that this is the result of greater decentralization or that the government is undertaking more fundamental work.

The important point is that there is a difference among participants as to criteria and this difference is consistent with the differences in other dimensions of R & D.

Another difference is noted between pharmaceuticals and other organizations. Participant PE states that a consensus with a non R & D management group is unnecessary in gaining support for R & D commitment, while participant CD considers this consensus critical. The differences are assumed to be, in no small way, attributable to industry and organization -- pharmaceuticals vs. a consumer and industrial based R & D organization. In the particular pharmaceutical corporation discussed all members of the corporation who could exercise R & D judgment were formally part of the R & D organization.

Here it is interesting to reflect upon the analysis reported above. The government laboratory executives tended to talk objectively about R & D process rather than about motivating and directing the researchers. This seems to coincide with the relative lack of specificity exhibited in their goal structures. The commercial laboratories' concern for specific product related outcomes seem to require more emphasis upon personal control than upon general process control.

Individual versus Group Utility for Risk. A fourth area in which there is a consistent difference among participants arises concerning group versus individual assumption of risk. Decision theorists have noted that groups differ from individuals in their propensity to assume risk, with groups tending

to assume greater risk. This in turn will have implication for the organization propensity for risk in undertaking R & D projects. As foreshadowed by the different criteria for R & D investments, significant differences are apparent in risk sharing -- whether the risk of a typical R & D activity is shared broadly or assumed within the R & D group.

Participants GB, PE, and CA (and GC tentatively) imply narrow risk assumption, that the R & D group itself must assume the risk inherent in a new undertaking (see assertions CA8, GC3, and the transcript for GB and PE). On the other had CD and CF clearly state that an R & D manager cannot make commitment decisions in a vacuum. It is the willingness of the larger management group to share the risk of a new area or project that enables a go ahead (see assertions CD1, CD2, CD5, and CF5).

The way that organizations configure themselves to accept the risk of project development will influence the profile of investment in R & D. The writers consider this to be an important dimensional difference the way organizations carry out R & D.

Informational Analysis for R & D Decision. Some difference of opinion was apparent concerning appropriate sources of information about new directions for R & D. Several participants held that market and market research information from the marketing area of the organization could be relied upon to point out new directions. Other participants held strongly that marketing was unsuited to this task due to their short time perspective. Views concerning the proper role of marketing information lay along a spectrum from complete distrust of the marketing groups ability (this had little meaning to government participants GB & GC) to an acknowledgement that the marketing group does point the way

tended to be very influential in R & D groups closely associated with marketing organizations. This sort of distinction is of considerable importance in terms of organizational structure.

Specialization of Management. The final aspect noted here, along which difference of opinion arose, concerned the appropriate capability of management. Several participants expressed the opinion that the individual who is a good manager is a good manager in R & D as well as other areas. The trait of managerial ability was considered more important than specialized ability in the R & D area.

On the other hand, other participants stressed the unique and necessarily special capabilities of those who perform well in R & D areas. The strong implication from the latter category is that special abilities are required in managing R & D.

The most outspoken advocate of special ability was participant CA who claimed important expertise for the effective technologist and decision maker (assertions CA1 and 5). On the other hand participant CD asserted a common management skill that was of great importance to all managers (CD-10). The other participants seemed to lie along the polar extremes established by CA and CD.

Areas of Consensus. The group seemed to agree in several respects. These areas concerned 1) the R & D groups obligation to advocate and promote the product yielded by the organization itself, 2) the fact that the R & D group perform their valuable services at the beginning of the long chain of new product introduction leads to a condition where they received improper credit for their achievement, 3) accounting practices and other annual management

control systems tend to lead to inappropriate decisions in the R & D area since they are both highly visible and completely myopic in automatically emphasizing immediate return to the detriment of long run payoff, 4) there tend to be long term "fad" cyclical fluctuations in the way R & D groups are organized, managed and associated within the larger organization. These result because 1) there is no explicit criteria that can be immediately assessed and 2) management tends to forget what was wrong with previous approaches. Organizations therefore oscillate between organizational modes with a very slow frequency of oscillation.

VI. CONCLUSION

A summary of the issues raised in the abstract, in the "speculative framework," and in the analysis of assertion sets shows major disparity in respect to the important dimensions of research and development. The "speculative framework" identifies critical dimensions unique to project management, while the results from the Workshop discussions reflect dimensions relating to laboratory management.

For instance, in the Workshop sessions much concern was voiced about the match between research objectives and organizational objectives. This was evidenced in the discussion regarding the role of the research director - is he a top manager creating policy or a second level manager executing policy? Should research objectives be set by the parent organization, or should they be set by the research group within general guidelines of the parent organization or should they be set jointly? Clearly, such an issue is important, but not so centrally in the process of project management as in the process of laboratory management.

In retrospect, the disparity between the concerns of managers in the two situations was extremely apparent in the tensions of the workshop itself. Since the focus of the authors was on research project management they attempted to get "seasoned" research managers as workshop participants. This emphasis on seasoning led to a set of participants who were so seasoned that they had become laboratory managers. Then the use of an open structure to the discussions allowed the participants to continue to shift toward

laboratory management issues. Thus, the post Workshop analysis showed interesting results mainly on issues of central concern to laboratory management.

The scope of this shift can be best shown by summarizing the issues dealt with in the conference. These critical issues included:

1. How to relate research to the objectives of the parent organization?
2. What essential steps were required of the research organization to follow innovation past the laboratory and into production, marketing and commercialization so as to assure effective outcomes and due realization of the R & D product?
3. What are the different types of value that arise from individual projects (be they successes or failures) in regard to later projects and how can the laboratory organization improve this transfer?
4. What actions are essential to maintain the creativity and effectiveness of R & D groups within the larger organizational setting?
5. How does the relative clarity of objectives influence performance?
6. What is the effect of organizational reward and control systems which are normally short term upon the performance of R & D groups which have inherent long term time horizons.

The fundamental differences in the "areas of concern" in project and laboratory management settings suggest that this dichotimization is itself

an important situational partition. In many circumstances it may be sufficient to distinguish which situation you are using as your unit of analysis and explore the dimensions of importance within the unit rather than to develop a complete set of dimensions applicable to either. Thus, as the authors pursue further studies of laboratory and/or project management they will be forced to carefully consider whether there is a major interaction between the two levels in the problem area of interest or whether the interactions can be ignored for the problem at hand.

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13. ABSTRACT

This research was aimed at developing a rich set of dimensions for use in the analysis of research and development projects. The comments of twenty (20) research managers were analyzed, and some modest progress was made toward the goal. The research methodology was the content analysis of a semi-structured two-day workshop and proved to be less than adequate to the task. Further use of the method is not recommended.

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