



ROLE OF OPERATIONS RESEARCH IN MANAGEMENT

Dr. Harold O. Davidson

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Reviewed by Col E. J. Ingmire, USA on 10 September 1963.

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28 August 1963

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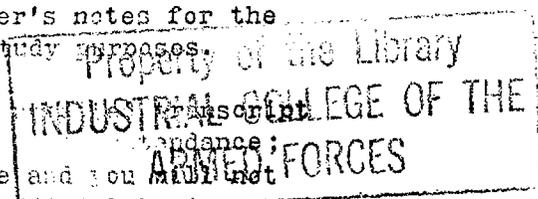
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Reviewed by: Col E. J. Ingmire, USA Date: 10 September 1963.
Reporter--Grace R. O'Loone



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MR. BARAN: First of all I want to introduce some members of the Wash-
ington Operations Research Council who have been so gracious^{as}/to come over this
morning and participate in our seminars. Will you please stand. Thank you.

One of the techniques used in the subject of today's lecture is the
queuing theory, or, as some call it, the theory of bottlenecks. Before the day
is over--I am speaking of the activities going on now in the District--some of us
may find that the bottleneck theory is more than a theory itself. However, before
we may run across some traffic bottlenecks, we will get a a broader view of
operations research from our speaker this morning.

It is a pleasure to introduce Dr. Harold O. Davidson, whose subject is
the "Role of Operations Research in Management."

Dr. Davidson.

DR. DAVIDSON: I believe that we can direct our attention most effectively
today to getting an overall perspective and appreciation of the scope, the capa-
bilities, and the limitations, and restrain ourselves on some of the details which
the technicians in the field are sometimes delighted to talk about at great length.
I thought you might not be interested in hearing them at great length.

I would like to start out with something that may seem a long way from our
subject, a simple demonstration. I have here a piece of stone and a feather. We
will conduct an experiment by dropping them from the same height. Our observation
proves that the gravitational force is proportionate to the density of the object.
least,
At/this was the result of thousands of years of human experience. It wasn't until

we applied careful observation, measurement, and analysis that we got a better insight into the nature of gravity.

The point I am trying to make is that experience, even a lot of it, can sometimes be a very poor guide to understand the true nature of the phenomenon or the system or the process we are trying to manage.

Most of the progress that has created the new problems of management that you heard about yesterday, I believe, in advancing technology has come about because of our increasing skill to utilize the techniques of the scientific method-- observation, measurement, and analysis.

I could speak on this subject not only with pleasure but with a great deal of enthusiasm because I believe there is much that operations research can do for management. But in establishing our perspective let me also say that it has some limitations. There are problems that OR can't solve. There are other problems that it is foolish to use OR to solve.

Let me give you an example. Considering the present state of the Nation's railroad, rail transportation industry, and its problems, one that has attracted a great deal of attention is featherbedding. We don't need operations research to find out that a fireman isn't necessary on a Diesel locomotive, and that it costs us many millions of dollars to indulge in this extravagance.

The problem here is one of implementation of a solution that we know, and this is the case with many of the problems of management. I have seen managements, however, that don't want to face up to that. It's much easier to continue to do studies and build simulators. This, I think, is a mistake in the use of operations research.

Since we are talking about the role of operations research in management,

it might be a good idea to begin with a general view of management functions. I don't want to get into a great deal of detail on this subject, but, broadly speaking, I think we can classify management functions into three major areas.

(Slide) Management has a function of promoting the external interests of the enterprise. Going back to our railroad example, one of the reasons that railroad management hasn't been able to solve the problem--let me say--all the reasons are primarily external. The same is true in another problem that occurs in the railroad industry. The discriminatory minimum rate legislation puts it at an economic advantage. This also is an external problem. You can do all the research you want to about the internal structure of the organization or how you are going to do your business, and it won't help.

Another major class of management functions--I probably ought to call these classes of management functions because the specialists in this area can detail a lot of specific functions--is directing the internal operations, running the business you've got now, and this is an area also in which operations research is not primarily applicable.

Thirdly, we have the function of improving the internal operations. This, in an era of accelerated technological change and increased economic competition, is becoming an increasingly important function of management. This is the area of management functions in which operations research has its principal role.

(Slide) The principal role of operations research, or the operations research function, then, is to support management in improving internal operations.

(Slide) Improvement implies change. There is no point in using operations research unless you are willing and able to change something. These changes may be in operating, equipment, doctrine, procedures. They may be changes in command and control, equipment, doctrine, procedures. They can be changes in organization,

changes in policy, changes in objectives, or even changes in personnel.

Thus far operations research, or the techniques that we will talk about, have had differing degrees of success, depending upon the nature of the change that is most needed in order to gain an improvement in operations. If it involves replacing the general manager of a subsidiary of a corporation, operations research techniques are not especially applicable. You don't need simulation to find out that a man isn't running his job effectively. This is a management problem that lies pretty much outside the scope of operations research.

So let's bear this in mind, too, in getting our overall perspective, that what we are after overall is improvement. Improvement implies changes, and there are many types of changes which may improve the effectiveness of the internal operations of an organization.

Operations research is most effective with respect to only a part of them.

The next thing I think we should look at is the improvement process, or at least the logical or rational improvement process. (Slide) It has a number of steps. The first step--people don't always go about it by taking all these steps, but these are the ones they ought to take, at least--ought to be to articulate our objectives. That seems like something we might take for granted, but I would like to show later that it is something that we shouldn't take for granted, that we should know what the objectives are and that we know they are properly articulated.

Secondly, we have to define the alternative ways of achieving the objective--existing alternatives, potential new alternatives--that might be made available by research and development. We then have to develop information concerning the characteristics of these alternatives--how they would behave under various

operating circumstances. Only then are we ready to evaluate the alternatives.

Having done this and management having made a decision, the next step is implementation of an improved solution. We don't improve the world by filling up library shelves with operations research reports. We may improve the library profession or the job opportunities in the profession, but we certainly aren't improving the world.

Finally, confirmation. We need to verify that we have in fact accomplished pretty much what we thought we were going to achieve when we made the decision on the alternative implement.

On this point I think you might say this is very good, but that you always heard that operations research was something new, and this doesn't look very new to you. Rational, intelligent managers were doing something like this 100 years ago, I suppose. So, what's different? Well, let's get at this question of change of development. Let's go away back for an example, to a very simple kind of improvement problem.

(Slide) Here we have, insofar as we know the history of the development of the axe, an example of a very straightforward way of getting improvements. That's called trial-and-error development. It is customary today to scoff at trial and error and say that anyone who uses the trial-and-error approach is a darn fool and behind the times.

But this I don't think is so. Trial and error, so far as we know, was a very effective process. It got us to a design for the axe that has not changed substantially in several hundred years, despite all the physics and mathematics we have since discovered. I say the design has not changed. We have, of course, improved the materials.

The reason that we were able to arrive, that the society was able to arrive, at an effective design by trial and error was that there were few variables to consider. Later they had the length of the handle, basically. Mistakes were cheap. If you got an idea that building an axe a little different way with a longer handle was better, you just went out and built one. You tried it out, and if it didn't work you threw it away. There was no great loss of resources then in trying alternatives full scale.

Finally, the linear time. We had lots of time to try them out. We don't know how long the development took, but it was a rather extended period.

So let's recognize the fact, then, that for simple problems with few variables, cheap mistakes, and adequate time, the trial and error is an effective way of going about the process of improvement. And I think there are still some applications of it.

Some time ago I ran across a statement of a problem, some research that was desired to investigate the development of a mathematical formula for selecting soles for combat boots. They wanted a simulation or an analytical model built so that they could develop the soles and choose between alternative designs. Now, here was a case where I would have said, "Heck, don't bother with that. Just go make them. It's cheap enough to try them out, and you'll probably be a lot more certain of the answer you get by that process than you will by a lot of abstract research."

So I think there are still some areas of application in developing simple components. But obviously we can't apply this approach to the design of large-scale systems.

Well, let's see what happened over a period of time in the evolution from this trial and error, which was the only method of development that we had in the early history of mankind.

(Slide) For this let's take an example of a bridge design. Starting at the bottom, the first bridges weren't designed, probably, they were just discovered--a tree had fallen across a ravine, and somebody's rather obvious idea was to exploit this easy way of crossing an obstacle, by just walking across the fallen tree. Then, having this kind of accidental discovery, it would become fairly obvious that you could create a bridge just by chopping down a tree and letting it fall across the ravine. Then you could elaborate on that idea by dropping a couple trees and putting cross planks on them. This possibly is the way that the initial technique of a bridge design was developed. This is about the state of development of bridge design that had been reached, for example, by the Incas in Peru, except that they had substituted stone for timber. But the mechanical approach to the problem was still essentially that of laying a couple timbers and putting surface onto them.

Now, these bridges did not serve the purposes of the Roman Empire. In order to maintain contact with their far-flung activities and to support them logistically, they had to be able to move heavier loads, and they had to have a more effective highway system. So the Romans addressed themselves to the problem of improving bridges. Now, despite what one reads in some of the histories of technology, the Romans didn't learn much about bridge design. What the Romans did learn was bridge construction. From the standpoint of design the Roman bridges are just fantastically inefficient. They used an enormously greater quantity of material than was necessary from an efficient-design standpoint, and they were inefficient in the use of manpower. But, since they used largely slave labor, and since the material was quarried locally in most cases, it didn't make any difference.

So the Romans solved the right problem for the time, the problem of constructing heavier bridges. So the fact that some of these bridges endure today is not

attributable to the excellence of their design. It's a commentary on the massivity of their construction. In fact, many of the Roman bridges were swept away for the very reason that the central pillars were so wide and took up so much of the width of the river that the velocity of flow between them was highly increased and would cut away the foundations underneath. So, only when they were built on a rock foundation underneath did these bridges last.

All right. Running up nearer to the future, during the Renaissance, the labor supply became a problem, as you remember. The plague and the increasing economic activity made it very important to use labor more efficiently. Also the problems of financing public projects had become more difficult in this period. So there was now incentive to really improve the solution of this problem. There was also a basic change in the approach that occurred.

Prior to about 1732, the way they went about trying to get these bridges was the old trial-and-error method. They'd build a bridge and then wait and see if the bridge stood.. Going back through some records--I pursued this as kind of a hobby at one time--I found that in many locations it was not uncommon for four bridges to be built within 20 years before they would get one that would stand.

So this meant, then, that if you were going to try a new idea it took some period of time to construct your experimental model and then a further period of time of use before you knew whether this particular idea was good or not.

We just couldn't wait that long for progress, and a French engineer by the name of Denici addressed himself to the problem of trying to build a better bridge. Here was the important idea that was embodied in Denici's approach. He did not try to simulate or to analyze the entire bridge. He recognized that the essence of the problem was a light-weight, strong, masonry arch. Therefore, what he did

was to build scale models of arches--not complete bridges but just a study of the behavior of arches with a scale model, until he discovered the principles of a light-weight, long-span, masonry arch. He solved his problem in about 1732. These principles were adopted in the construction of bridges from the period from 1735 to 1790. This is when it was introduced into practice, and you will still see in Paris and in many of the European cities bridges of this type still standing, built in that period. The general configuration is shown there.

To go beyond that, though, to the modern long-span, suspension bridge, we had to have far more advanced analytical tools. All that we are doing here, though, is trying to find a substitute for trial and error, some cheaper way of trying out and discarding the ineffective alternatives and sorting out from among those the better designs. This is what essentially we get either with our scale model test, a cheaper way of testing designs, or our analytic models, which are still more powerful.

Through the development of some of our analytic tools, such as Hook's Law, Young's Modulus, Mechanics and Material, Stress Analysis, Vibration Analysis, we have the means at hand for developing efficiently more complex systems.

So the point I am making is that the improvement process essentially has always had the same basic steps involved. What has happened is that the problems that we now are faced with and the systems that we are now managing are vastly larger, vastly more complex, more powerful tools to carry us effectively through this improvement process.

(Slide) One of the tools that are considered to be quite modern and getting a lot of attention at the present time is computer simulation. I think that will be discussed to a greater extent by Mr. Hare on September 3. All I want to point

out here is that this technique that we are now just discovering had a rather older history than we had imagined. In 1929 the AC network analyzer, which is an analogue computer, was put into operation for solving problems of electric power distribution system design. Essentially it was a way in which you could simulate a large-scale system quite quickly on a small scale and test the consequences, imposing various loads on it, and shutting down parts of it during an emergency and seeing the effect on the system. This was in use in 1929. Before the beginning of World War II I think there were something like two dozen in use, and they are now used throughout the world. There isn't very much fanfare about them, but they are effective devices. They're just an awful lot cheaper than building full-scale systems to find out your mistakes.

Let's come back then. We've seen why it is that, although the improvement process itself is not new, the implementation of the improvement process is a far different matter today than it was several hundred years ago.

(Slide) Let's take the first problem, the articulation of objectives. This is one that you don't very often find mentioned in the literature on operations research. I think it is unfortunate, because many of our difficulties arise because either the objectives are wrong or the articulation of objectives is not clear. People don't understand them.

Now, it is management's job to select and articulate the objectives. Let's make it clear. I am not saying that this is a role of operations research. However it is OR's responsibility, I think, to question the objectives in a constructive way, and surely it is necessary for the OR people to understand them.

The objective, I would say, was very simple when you were trying to improve an axe. Objectives today are still comparatively simple for some individual people.

For instance, the individual salesman for a company may have a very clear objective, to make his quota. The quota may be good or bad, but he knows what his objective is. It's pretty clear cut, to make a quota. When we get to the level of the corporation itself, it is maybe not so clear as to what the sales activity objectives ought to be.

Let me give you an example of that. Most consumer products companies are interested in something called market coverage. One way of figuring market coverage is to determine the number of dealers carrying your product per thousand of population. For example, if there were 80 dealers carrying your product in an area that had a population of, say, 10,000, then your market coverage would be 8 dealers per thousand. That's a nice, handy number, like the deadline rate on vehicles. You can even calculate it. And it does have some bearing on the things you are interested in.

So this company had for years been keeping track of the dealer covering, and the dealer coverage had dropped from about, in this particular case, around 8 per thousand down to about 6.6 per thousand. One of the sales executives, who was extremely excited and perturbed about this, felt that a major investment ought to be made by this company to restore this dealer coverage.

What OR did at this point--it could have been anyone else--I am talking about the OR approach, if you will, the scientific method--was to ask the question: Well, suppose you got 100 percent dealer coverage, what would you have? We were just questioning the objectives. Well, that's easy enough to figure. You just know the number of all the dealers in the United States, and you know the total population, so you divide one by the other, and you get 7.3. You couldn't get 8 per thousand in dealer coverage. So this raised another question: What was happening here?

It was pretty simple. In the part of the market through which the product was distributed, the trend over the last 10 years has been for many of the small dealers to go bankrupt and for larger and larger establishments to take over. Therefore there had been a big shift over this period because of this trend in the basic market characteristic of the marketing. So that even if you had all of the dealers you couldn't possibly have a market coverage of 8 per thousand.

In other words, here was where the failure was, of not going deeply into the measure that was being used as an objective. It was meaningless. As a matter of fact, the dealer coverage held by this company at 6.6 was a larger percentage of the potential dealer coverage than they had held previously with 8.

So articulating our objectives in a meaningful way and still in an easily understandable way for the members of the organization and for the OR people, who, after all, have to use the objectives as a way of deciding and evaluating alternatives, has become quite a difficult task.

Another thing that is difficult in the matter of objectives is that in large systems we typically have conflicting objectives. Not too many years ago there were problems in the Seventh Army in Europe which could be very clearly traced to a conflict between two objectives. On one hand the Seventh Army said, "You must be combat ready. That's your mission." On the other hand, they said, "You've got to run it economically. You've got to save money." Another thing we said, too, was "You've got to keep the troops happy." So we had dependents. Now, having dependents living overseas with a combat-ready force is kind of an interesting concept to contemplate.

So many of our problems that we find in large enterprises result from the fact that we have conflicts in several sets of objectives. I am not proposing that OR can eliminate those. That's the nature of life, I'm afraid, that we do have

conflicting objectives. I think it is important, though, that we have to articulate all the objectives and recognize where the conflicts are, and then the manager, the executive, has to decide which way he can compromise. OR cannot tell him that answer. The answer often depends upon a political climate--the external world that I was talking about earlier.

So it may often depend on not what the manager believes is right but on what the manager believes he can sell. And unless the OR people understand that, they are not going to be able to do a very effective job in supporting management in its attempt to improve the operations.

So the matter of objectives, then, I think, is all important. Since I am speaking, I believe, primarily to managers, I might as well warn you that operations research people have a predisposition to select objectives or measures that they can calculate. They love to calculate. This is one reason, of course, that these people can be helpful, because they will attempt to quantify the problem. But sometimes this urge to quantify can carry one too far, because, since he doesn't have the realistic numerical measure of the objectives he may create an unrealistic one that he can calculate.

Let's take an example of this. Although OR people sometimes have twinges of conscience in working on military problems associated with destruction, killing people, and all that sort, when it really comes down to it, the OR people are the most bloodthirsty lot that I have ever seen in my life, because they will tell you that the measure of effectiveness of many systems is casualties.

Now, of all the military people I have talked to and discussed this question of objectives, none would ever say that he could measure his success by casualties. And history doesn't say this, either. History will say that success in military campaigns is often inversely correlated with casualties. Rommel's breakthrough to

the Channel Coast, for example, was a tremendously successful military operation with very low casualties per day and per thousands involved.

We have this kind of situation arising, then, when the OR people who look at a weapon system will say, "Well, this one is better than the other, because it produces more casualties per pound of ammunition."

There was a study done, I believe, if I can recollect it, in which it said that one artillery piece was better than another because it had a greater lethal area per pound shell. Now, the assumption here, of course, was that the purpose of the weapon was to kill people, and the more people we killed with it the better it was. But, one only has to calculate the lethal areas in the number of shells fired in World War II to figure out that if this had been done effectively there wouldn't be anyone alive today.

So basically these systems aren't very effective by that criterion, and fundamentally they are not very effective, or else their effect must be something else. And combat soldiers will tell you that, indeed, if it couldn't kill anybody, it wouldn't be effective. But, once given this capability, its main effect may be to keep heads down and to gain the freedom to move.

Well, frankly, we don't know quite how to handle that problem adequately, the treatment of that objective, that measure of effectiveness, in operations research.

So this is one of the main points I want to bring out regarding objectives, that they are, objectives of large-scale systems, difficult to articulate. They are complex. They are typically conflicting. And our best articulations of these, the best measures of effectiveness that we can devise for all our purposes, are still imperfect.

So the first thing the manager needs to do in making effective use of the OR people in the management function is to make sure that they take a look at the objectives, that they understand them, and invite them to criticize them, to ask questions. You have to make sure that when the study starts they at least have an understanding and that there is an agreement on what the objectives are. Otherwise you can do a beautiful study on the wrong problem.

The second point--the definition of alternatives. In the case of the axe that we were looking at a while back, once you settled on the basic idea of a handle and a head, the alternatives were to change the weight of the head or the length of the handle, and maybe the configuration of the head somewhat. So it wasn't too difficult to define them.

One of the consequences of a richer technology today is that we have a far greater number of alternative ways, just in terms of equipment, of tackling a particular problem. Now, complicate this with alternative procedures, alternative doctrines, and you see that our riches are partly the cause of our headaches. We have just too many different ways of doing things.

Sometimes we are inclined to have a bias toward doing things differently, toward seeking a radically different way of doing things. Therefore, we are inclined to overlook the potential that may be available by improving the old way--not making a basic change but improving it.

Let me give you an example of this. You have been reading, I think, some of you, at least, in the Washington papers, articles concerning the subway planning studies for Washington. The newspapers have taken a considerable interest in what has been done in Paris in developing an improved subway system, specifically in the fact that they are using pneumatic-tired subway vehicles which, according to the

newspapers, and Time Magazine, and particularly the developers of the system, are far quieter than the steel-wheel system.

Now, the French went about the problem, of course, of developing an improved subway vehicle because, after all, standards of comfort in vehicles have changed. We are not willing to accept the same standards that we did when the New York subway was designed and installed. So a number of engineers in Paris and officials of the Paris Metro set about the job of getting an improved subway vehicle. One of the alternatives, of course, was just to improve the basic steel wheel concept. The other alternatives were to abandon it to some other basic concept, such as the pneumatic tires, or even ground-effect vehicles, and so on and so forth. They did not bother really to pursue the alternative of improving the technology they had.

On the other hand, other people have. The Swedes have done it. The Germans in Hamburg have investigated what you could do with an improved steel wheel vehicle. They have done it in Berlin. So, earlier this year we had a field team that went out with instruments--coming back to this old question of measuring. Let's get the facts. So this team went out to see just the fact that this many people--the Swedes and a number of different German groups--were following the alternative of improving the old system which in itself ought to be a caution that maybe there is something there.

So we were not willing to accept the claim that the pneumatic-tired vehicles were far quieter. When we got back with the magnetic tapes and ran them through the analyzer, we found that not only was it not far quieter but that it wasn't as quiet as the best of the steel-wheeled vehicles.

So here is a case of a management which undertook, at considerable development cost, to pursue a new technological alternative when they had not in fact

considered as one of their basic alternatives the improvement of the existing technology. There should have been some reason for suspecting that you might be able to do better with steel wheels, because, when you stop to think of it, there is a steel wheel inside every one of the tires on the wheels of your automobile. They call it roller bearing. It runs the steel wheels on steel surfaces, and these are not very noisy.

So that, if the problem had been adequately analyzed, first from a technological viewpoint--this is the thing I want to bring in here, that technological analysis must be tied in to OR in the modern world, because our alternatives are so deeply involved with technology--it should have been clear that there was a very real possibility of significantly improving the operational characteristics of the existing technological approach.

I want to skip over the next two--the development of information and the evaluation of alternatives--because then we will go on to several slides in which I will take each one of those up in detail. I want to go on to the last two items there, again because these are items that are perhaps not so much stressed in the literature of operations research, and particularly in military operations research.

It is purely a guess, but I would say that ⁱⁿ something like 90 percent, or certainly over 50 percent, of the military OR studies from my experience, at least, the OR people have had no experience of assisting in an implementation. Of course in some of these studies there has been no implementation, except through a decision. They were for management information.

In cases where there were implementations there tends to be too low an involvement and too little emphasis on implementation. Conversely, in the commercial work, at least that that I am acquainted with, it is just about 100 percent of the cases in which the OR people are involved in the implementation. Their

responsibility does not end until they have turned over to the operating people the deep-bugged operating system solution. So they are there to be confronted with the consequence of their mistakes. I think that is awfully important.

Confirmation I think is also necessary. We are undertaking this process of change, not for its own sake, I hope. We are undertaking change in order to make improvement. So we ought to have some feed-back from this process. I am quite sure that there are cases in which we have accomplished change without improvement, where we haven't learned much from it, because we never set out as part of the project how we were going to confirm whether or not we had succeeded in what we set out to do. That I will frankly admit is just as difficult as defining objectives, or even more so. In some cases it is not even economic to confirm the amount of data that you need to take. Let's put it this way: In some cases, in order to get a base from which you could measure improvement, you would have to take several years of data on an existing system--data that you are not now collecting--before you could introduce the change. Well, now, if the change is any good at all, you want to introduce it now.

So, in order to obtain really objective confirmation you have to delay an implementation for 3 or 4 years while you collected a base of information, then made the change, and then evaluated it. So in many cases confirmation is not accomplished in as precise a way as one would like, simply because it is not economic to do so.

On the other hand, the point I wish to make is that, even when you cannot confirm in an analytical, objective way, with as many measurements as you want, the manager should ask himself the question, and if he has to do it by subjective judgment, he still should do it: Does the experience confirm the predictions that

were made and has it been a success? If not, why not?

All right. Now let's step to the two areas in which the techniques of operations research are the richest, in which we have the greatest capability of assisting managers.

(Slide) The existing analytical tools are most effective in the areas of information development and evaluation. Of these two, greater stress has in the past been placed probably on evaluation. If you put lousy information into a good evaluation scheme you could still get lousy answers. So that the choice and development of adequate information are also part of the obligation of the OR people in fulfilling their role in the management function.

The basic problem that we face here is rather a considerable one. We need information that is oriented toward design and decision, whereas in most organizations the vast bulk of information we have available is oriented toward fiscal control or operational information, like customer orders and things of that sort.

So, although we may have a wealth of information, the fact is that just in terms of quantity we actually may be information poor in the kind of information we need--design and decision oriented. I have yet to find a study in which it was not necessary to set out to develop some additional information that wasn't available. Or, at least, when you looked it over, you decided you would be better off if you had the opportunity to develop some better information. Sometimes you had to do a quick and dirty study and approximate this in a crude way, which is still the same thing. You had to get information that wasn't available in the existing records.

So the first step, obviously, then is to define what your information requirements are in terms of the evaluation you are going to make, in terms of the

objectives that you are interested in. What information do you need?

Now, in acquiring information there are at least three broad approaches. First is the analysis of operating experience. Some of the techniques that may be used here are correlation, analysis, or cost-effect models. You are just analyzing what is happening in the past in order to gain an insight into the phenomena and to get some basic measurements and some basic data that you could apply in forecasting the effects of change or the effects of operating this system under a new set of circumstances.

So here we come down now to specific analytical tools or the kinds of tools that OR can apply, has available, or will be developed for assisting management. I am putting in this same category as part of the OR information problem research and development testing. Now, that isn't always done, but my reason for putting it in here is that, although there is obviously a great deal of research and development testing that has to be done only for purposes of research and development engineers, there is also a need that some of this research and development testing produce information that can be used in making management decisions on the development project.

Here is where I think the function of OR comes in to determine what is the information requirement from a management decision standpoint as distinct from purely the technical information requirement for fixing bugs in the equipment.

The development and scheduling of a test program to produce information for management decision is, I would consider, part of the OR function. ✓

Secondly, the design of tests to get this information in an efficient way is also an area in which I think there is considerable opportunity.

The third area is field experimentation. Now, there could have been or

should have been another line in there, too, saying that the design on the field experimentation program in a particular task needs to be done, and then the design of experiments within that program is another area in which tremendous progress can be made.

To give you two examples of this: In one of them, in conducting an operations research study on chemical weapon systems, we dug quite deeply into the field test literature that was existing, because we had to get basic information to plug into this model. As we got into this we had an idea of what kind of information we needed because of the structure of the analytical model. In the way it was set it had to have certain information inputs. We found that the field experimentation was getting only one out of four kinds of important information that was needed. In fact, there was some indication that the phenomenon that was being measured in the field as far as the particular effectiveness was concerned was not the dominant phenomenon.

So, only when you can relate field measurement to analysis of the operational system are you able to tell what data you ought to be collecting in field experimentation.

Let me mention another example of field experimentation which I think represents a substantial advance in management thinking toward the use of OR and the methods of OR. About a year and a half ago, following some initial studies in which we found it extremely difficult to control conditions so that we could make accurate measurements, the Coca Cola Company management was convinced of the desirability of taking an operating enterprise--one of their bottling plants, a complete facility--and converting it essentially into an experimental facility. They were still going to sell a product, of course. We weren't going to experiment with the beverage. But it would be experimental in the sense that we would change routes around, we would

change many of the techniques of distribution and marketing, and we would do this deliberately. You can imagine the consternation of the local plant manager when we arrived on the scene and described the first experiment. We said, "Now, these are the things we want to do." He said, "Well, look. If you are going to do that, you'll lose money." We said, "Oh, yes, we know that, but we want to measure how much." This is completely alien to the thinking of most operating people. Certainly, to do something that your experience tells you and which these research people agree with is bound to hurt just doesn't make any sense, except for the fact that this was one plant out of over a thousand in the country, and that you could afford to hurt in one place to gain information that could be exploited profitably in many other cases. Let me also say that this was not done on a basis in which the total profit picture was impaired, but selectively, within the area, we would match up outlets and conduct experiments through which, in some of them, we very definitely did hurt the business from that part of the activity for a specific and deliberate, predetermined objective of measuring a relationship.

Thus far this has gone on for one year and now we are in the second year of doing this sort of thing. Last year their profit was still higher than the year before. It could have been higher than that if we hadn't fiddled with the system in certain ways with the deliberate expectation of losing some money.

So here is a concept, then, of field experimentation on a full-scale basis, conducted, though, not in an accidental way but in a preplanned way, with a stated objective and knowledge of the consequences that could possibly happen, and what you were going to get and what you were going to pay for it.

Now, this in a way is really not such a radical thought, I believe, because I think that every business is an experiment, anyway. Managers don't look at it that

way, but many of the things that are done in business are really experiments. The only difference is that we haven't defined what the experiment is in a scientific way and we haven't set up a way to measure it so that we can learn from it.

So the principal difference here is that, although we did, of course, in this case, do some things that a businessman wouldn't do, by and large, the main thing is that we just put some flight-test instrumentation on a business. This is really quite comparable to the idea of putting flight-test instrumentation on aircraft and then running it through some rolls and other maneuvers that stress it, things you wouldn't ever do to a 707 in commercial service, but you do it to find out how the beast behaves. Then that information enables you to operate it in a normal mode far more efficiently.

There is a quite close analogy, I think, to the idea of flight testing an aircraft or shakedown testing on a vessel with this idea of taking a business enterprise or an organization process and instrumenting it to measure the effects and then deliberately putting it through some maneuvers that you wouldn't normally do in regular operations--the objective being to gain information that you can then exploit to advantage.

(Slide) The next topic is the area which, in terms of the wealth of analytical materials and the models and the past experience in OR, is the richest area in the improvement process, the evaluation of alternatives.

To start out with, let's say, "What is our concept of evaluation? What is a valid concept?" You will in some literature, I am sure, find the suggestion that the objective of all this is to find an optimum solution. That is one concept of evaluation that I don't agree with. I believe it is unsound. In fact, I believe it is purely mythical. There is in the real world no such thing as an

optimum solution, because, even if it existed, we couldn't measure it. Getting back to our information problem, our inadequacies of information are so great that we couldn't recognize an optimum solution if we stumbled on it and fell flat on our face in the middle of it. So I think this is an unrealistic concept of evaluation.

I propose alternatively that the objective is to predict the cost-effect consequences of alternatives for executive appraisal. You don't tell the boss, "Look, here's the best solution. That's what you needed." You say, "Look, here are the alternatives that we discussed with you a couple months ago, if you recall. There they were. We have rejected several of them. This is the set we looked at. Now, if you do this, this is what we think it will cost, plus or minus so much. There is the uncertainty, and this is what will happen." We go through the same with B, C, and D.

This is the way, I think, in which OR supports the executive decision. It presents the predictions of cost and consequence of the alternatives for appraisal.

Let me stress the iterative nature of the evaluation process. It is simply not efficient to take all possible alternatives and exhaustively analyze each one. The nature of an efficient evaluation process is that we go through a number of steps, successively narrowing the alternatives, and in each step going into greater detail on those that remain. So, if anyone proposes that in a very short project you can really do a thorough job of evaluating alternatives in a complex system, it is not so. Just because of its iterative nature, it is going to take us some time to do the job.

All right--techniques for evaluation. Of course, the simplest and most direct is the one we mentioned a while ago--full-scale trial and error. I propose

that for some limited, small problems it is still the most efficient method we have available. Now, stepping back from that and becoming a little abstract, we have pilot-plant tests. Proceeding to a higher degree of abstraction, a qualitative logical model is a further degree of abstraction from the pilot-plant test. Then we may have analytic models. By analytic I mean to imply primarily quantitative, the qualitative as against the quantitative model. And we have various versions of that. The computer simulation is actually a kind of analytic model, but we have come to give it a special name. Then there is war gaming. These indicate the spectrum of techniques we have available. Mr. Hare is going to talk about those later in detail.

The final point I want to stress is the choice of techniques. Occasionally one finds a manager who wants some operations research work done, who will say, "I have been looking into the literature and I've decided that I need computer simulation." Well, he might be right. He might also be right if he read a little bit in the medical journals and went in to his doctor and said, "Look, I need some penicillin." He also might be wrong. I think he is basically wrong, though, in attempting to make a decision. Whether that particular decision happens to be right or not, I don't think the manager should set out to make one of the crucial decisions of the operations research activity, which is the choice of techniques. If he is going to choose the technique, then he might as well then absolve these people of all responsibility for the consequences of its application.

On the other hand, he should be concerned that the choice is made carefully, that the choice of the validity of the technique for the application is carefully appraised, and the technique is efficient.

There are many problems. I'll grant you that almost any problem can be solved on a computer, if it can be solved any other mathematical way, because we

can put any kind of mathematical formulation, practically, on a computer. That doesn't mean that it is efficient to do that. Once you've got your formulation, it may be the kind of problem that a few days' work on the back of an envelope will give you the answer. It may not be. So that the efficiency is a consideration, too. It's not a matter just of getting the answer. It's a matter of getting it with the least expenditure of resources, because, it appears to me, at least, we've got more problems in the world right/^{now} than we have time to work on. So the sooner we can get off one and get on to another, the better off we are. Therefore, we want to be efficient in the choice of our techniques.

Then, finally, there is the problem of understandability. There may be techniques--and I want to point this out--that may be valid analytically, but, if they aren't understandable to the man who wants to use them, their utility is definitely decreased. One of the problems that we get into, I know, because I have seen it, is that, if we get into very large applications of such things as computer simulations or war gaming, we get answers but we don't know what they mean. I know of one case where this has happened, and where a special project was set up on top of the original project to try to discover what the answer meant.

Now, along this road, gentlemen, I think there lies no profit. So, if we once get to the point where we've got the formulation of a problem that has become so complex and interacting that we don't even understand what it means any longer, it is quite questionable whether we are even prepared to judge its validity, much less to gain insight from the results it produces for us.

This about winds up the presentation I prepared. The objective was to give you at least my overall perspective of operations research, its scope and its limitations. My objective was principally to emphasize that we are trying to

achieve improvements, improvements which imply change, and that OR is concerned, then, with the improvement process. In my judgment OR cannot be effective unless it looks at all steps in the improvement process. It must begin with the articulation of objectives. We should question the articulation. We should above all things make sure that they are understood before the study is begun. It is up to management, of course, to determine what they are.

Secondly, it is the responsibility of Operations Research to make sure, as sure as people can be, that all reasonable alternatives have been suggested, and present it to the executive for preliminary screening. If the executive finds that some of these alternatives are, say, politically infeasible, or timewise infeasible, or economically infeasible, he can cut them out and say, "I don't want them analyzed. I can't use them." It is the responsibility of the OR people to give him as comprehensive a picture of the potential alternatives that we can.

It is the responsibility of OR to make sure that the information that goes into the analysis is valid and to be prepared to develop improved information. I am not saying that we don't sometimes have to do analysis with inadequate information. This is usually the case. In that case we are responsible for assessing the limitations of the information and showing how these can influence the results.

In the evaluation of alternatives I have indicated that there are many techniques that are available, almost too many at times, and we have difficulty in making our choices. But, when the choice is made, it should be made with an eye to the validity, the efficiency, and, finally, the understandability of the product to the manager we are trying to support.

Next, I believe that Operations Research should consider its responsibility to participate in the implementation, to make the task of the operating people

who are trying to put in a new system or a new solution as easy as possible, and they should stick with it until they have proved it out.

Finally, I think that, to the extent that it is possible, the method for confirmation should also be developed as part of the improvement process.

Thank you, gentlemen.

MR. BARAN: Dr. Davidson is ready for your questions.

DR. DAVIDSON: May I first say that on this problem of objectives I have an illustration of it. I was firmly in mind that my objective was to finish at 9:45 sharp, but I had the wrong objective, so you see I successfully accomplished the wrong objective. I am just aware that you were deprived of some of your question opportunities for that reason. I apologize.

QUESTION: To what extent does OR delve into the psychological and human-relation aspects of a case?

DR. DAVIDSON: I don't know to what extent we delve into it scientifically, but I can assure you that we run headlong into it. We have to contend with it. Thus far I would say that the successful contensions that I have seen with this problem have come about more through executive skill guiding the OR team and the rest of the people concerned with implementation than we have through application of scientific knowledge. But we have sure run headlong into it.

I can tell you one example. In the Coca Cola Company the first project we did was to develop some concepts and some techniques for modifying the distribution system. We were told by most of the people in the industry that 50 years of experience proved that this wouldn't work. Top management said, "Well, we don't know," and we said, "No, we're not positive either, but we think there is a lot of

merit and shouldn't you make a test?" Top management agreed. We went into the test plant and one of the problems we were immediately confronted with was what the union was going to do about this, because, if we were right, they were going to have 25 percent fewer people when we got through. So all we could do in this case was to point out, "Here are the implications. If this thing works we are going to get rid of 25 percent of this particular part of your labor force. What implications does this have for you, the executive?" We didn't tell them how to solve the problem but we told them that this problem was sure to arise. They had to deal with it. If they hadn't been willing to deal with it they shouldn't have started the project in the first place.

Is that an adequate answer to your question? We think we have to face the problem. And again I say, improvement applies change, and change often, if it is going to be a more efficient operation, means reducing labor cost and getting rid of people. At the very least it means displacing people and causing a retraining problem.

On another job we did once with an airline, in the approach there we saw that a man was going to become obsolete for his job because he didn't understand anything about the use of the computer, and the computer was going to be at the core of this center. This was an operating computer, not a simulation. So very early, before we got anywhere near that, we sat down with the manager and we said, "This man has ability. You'd better send him to computer school." We didn't have to have any critical psychologist to sort out that problem.

QUESTION: Doctor, in reference to your dissertation on the bottling company, I understand why you would try things that might or might not make you run out of gas, but I can't get clear how you would use the data which you had gained from

testing things that affected the business.

DR. DAVIDSON: Well, in this way: We were interested in some of the particular cases in finding the boundary at which we should begin to change one of the characteristics of the distribution. You would actually start to lose. One way of fighting a problem is to step over it. It's that simple. We wanted to find out exactly how far we could go. The only way we knew was to be sure that we stepped well over it.

Did I explain the logic of that?

STUDENT: Then you weren't really intending to lose. You were just going over a loss. You really marked time.

DR. DAVIDSON: We knew that the further we went in this direction the more we'd go. We had to go far enough in the direction to where we got a definite loss to make sure we knew the point at which we got that loss. So I wasn't just moving over a point. We were moving into an area where the further we went the worse things were going to get.

Now, we had to be fairly judicious, and we sat down and used the experience of management to thresh this problem out. All we wanted was to take enough of a step to make sure we were there, the smaller the better. And it worked out all right. Once we got them to understand what our logic was and what we were trying to do, we could work together effectively.

We had an equitable experiment. The effect was so small that, after all, they made more money during the year in spite of these losses in some of the small areas. But we were not going over the point, very definitely. The further on we had gone the more money they would have lost. If we had gone too far they would not only have lost money but they would have lost the business permanently.

It was kind of a delicate situation.

QUESTION: Occasionally in problems of this type I am sure you counter variants whose magnitude either is unknown or has upper and lower limits which tend to give you a variety of answers. How do you take care of this?

DR. DAVIDSON: Well, the first thing you do, and I am sure this was understood in asking the question, if you have identified the variables and you know their limits, is to plug them in and find out whether this makes any difference. Sometimes you are lucky and even within the range of variations an uncertain number still doesn't change the decision. In other cases it does. So that puts it back in the executive's lap very much.

Now, one thing you can say is, "All right, you have to prepare for either alternative." In other words, you can cover uncertainty with money. If you don't know whether the enemy may use this kind of weapon system for the attack or the other, you defend against both. If you can't, then you go back to the old technique of basing your decisions on estimates of intent rather than on estimates of capability.

This is rather abhorrent to us because of our past history. We've had enough money so that we could base it on capability. But some of the European countries for years have done some of their basic military planning on the basis of estimates of intent. If you are shrewd and lucky, you win. If you are not, you lose. I mean, this is the nature of the world, you know. People sometimes have lost.

So that's the only answer. You see, a civil engineer, if he doesn't know exactly what the stresses are, covers his ignorance by putting in his uncertainty, or his ignorance, whichever way he wants to put it, by putting in more material

than he theoretically thinks he needs. In other words, he buys insurance with extra resources.

So that is one answer. Going down the logic, if you don't have the resources to buy insurance against the uncertainty, you face it. You flip your coin, and make the best guesses you can. That's it.

That's the only kind of answer I can give you. Someone may have a better one, but I don't.

QUESTION: Doctor, I am sure your projects have application to a number of firms in industry. When you complete the project, does it become the property of industry or are you free to use it for application in other firms?

DR. DAVIDSON: It's the customer's option to determine that.

STUDENT: When he makes the makes the initial agreement?

DR. DAVIDSON: Yes. I might say that our work with the Coca Cola Company is a top, cosmic secret. It was laid out this way. They said, "You can go to work for Pepsi Cola any time you want to stop working for us." Other clients, like the Southern Railroad, for example, on certain projects that they feel give them a competitive advantage they don't want us to talk about. On others, where they feel it will help the industry as a whole, they say, "Fine, the more people you can get behind this, the better."

So it varies. It's the customer's option.

QUESTION: Dr. Davidson, to come back to the point of military science, will you discuss the validity of war gaming for affecting or determining the ultimate value of two opposed weapon systems?

DR. DAVIDSON: That's a good question. I would say that in my judgment war gaming has two values: (1) In training people. Now, in order to train people

you've got to know what it is you are going to train them in. You've already satisfied yourself what the war games show is valid. That doesn't help us on the political. You may end up training them very well in the wrong things. So for training we have to determine in advance that what the war game shows--the relationships in the game, the structure of the game, and the consequences that come out of it--is valid.

The second thing it is useful for is as a tool for helping an analyst to formulate the problem. In order to build a war game, you've got to make decisions on an awful lot of relationships. By the time you've got the war game, if you've got a valid one, you probably don't need it in the evaluation. You can make a much more restricted analysis to get the answer you are looking for.

I've tried to answer your question. It's a difficult one to answer. To the particular question I would say a very weak yes. By that I don't intend to imply a very weak answer about the value of war games. I think they are quite valuable for those two purposes.

QUESTION: To what extent is the competition which is involved in OR service to industry having an effect on the trend?

DR. DAVIDSON: In competition among the OR groups for business? Well, there is competition. The competition is a much different kind for the military business than it is for the commercial, partly because it's a different world. Congress says you will go out and get competitive bids for things. In other words, in general, the procurement of operations research in the Government tends to be governed by the same rules that you use for buying nails. You write out specifications and then you buy from the cheapest bidder.

None of us personally would go about selecting a doctor that way or an architect or a lawyer. I am not saying the Government does it precisely this way,

but there is a tendency for the rules and the system that have been developed to handle this very large material procurement problem to be applied over here. Industry doesn't do it that way. By and large they choose in one way or another. Sometimes they will get very short competitive bids in the sense of approaches. They'll talk to a number of people. Very often they will just pick the people that they think can do the job and then they'll discuss the resources that are available, what the job is worth, and what it will cost. Then they will decide to do it or not to do it. There's still competition, even though for the particular job you may not get underneath the same kind of competitive mechanism.

Does that answer your question adequately?

QUESTION: Scientific approaches to solving management problems by expert organizations such as Operations Research certainly do not include the elements of the approach you spoke about, of obtaining information, setting the objectives from the information, analyzing the alternatives. My question is simply this: What is unique about operations research?

DR. DAVIDSON: I tried to avoid saying that it was unique. Even if it were, I don't know that that would make any difference. It's either useful or it isn't useful. Whether it is unique or not is kind of immaterial. What I tried to point out is that we have had a development process, with more elaborate, more complex, and more powerful tools for carrying out the basically known functions. We've had this development for a very good reason, because the problem is getting more complicated.

Now, if you take a particular technique and look back at the literature and say "That wasn't available before 1957, and therefore it is unique," that's all right.

OR today, as a means of supporting the improvement process, is not the same in terms of the detailed mechanism and tools we have as what we were doing 20 years ago. But the basic process and the objectives are the same.

Does that illuminate my view a little better for you?

STUDENT: Yes, sir. Primarily it is the use of tools that are available.

DR. DAVIDSON: It's the use of the basic scientific approach. Science, after all, is expanding. Technology in this area is growing just as it is in electronics. We have tools today which we didn't have before. To that extent there is something unique about it. But I prefer not to call attention to the uniqueness of what we do today but rather to call attention to the basic process. Let's make it an effective process.

In that connection there is a comment that I would like to make, too. I have observed this difference between OR for military establishments and OR for commercial plants. I can, I guess, count on one hand the occasions in which I have been able to brief a general and talk about problems we are working on for more than, say, an hour and a half. I have spent all day with the top executive officer of the Coca Cola Company. I have spent all day with top executive officers of the Southern Railroad.

Part of this, of course, is because of the kind of agenda that gets set up for people in the Pentagon and other headquarters. Maybe they wouldn't let them spend all day if they wanted to. But I think it is worth the effort of the military executive to try to spend more time not just to get delivered to him the answers but to get into the structure of a study, because, if the study is well done, it should give him some improved insight into the problem that he is trying to manage.

Let's face it--a lot of decisions still have to be made by the seat of the

pants. What we are doing here is making the seat a little more sensitive. If you've got a study that really gets down to some fundamentals, then what the executive gets out of it is not just the answer to a particular problem but an insight into the system that he is managing that he can then use in other ways. You can't get that in an hour or an hour and a half. You can get it if you sit down all day and pound away at a man, and challenge his ideas.

Of course, if after you get through you find out that there isn't a valid structure, then you've learned something important, too.

I have seen this difference with the military. I am not saying that all commercial people do it this way. I am saying the successful commercial shops, the really highly successful shops, usually have--and in my experience the university has it--this kind of top management interaction.

I think it's worth the consideration of those of you who attempt to explore to try to find enough time where you get yourself deeply involved and you force the people to reveal to you what their thinking is, what their structure is. If they've got something useful, then you will have your money's worth back out of the time. If they haven't, then it was probably worth your time to find that out, too.

MR. BARAN: Gentlemen, the seminar will begin 20 minutes from now. In the meantime, Dr. Davidson will be available for further questions down in the cafeteria. Dr. Davidson, we express our appreciation to you for taking time out of your busy schedule to come here to be with us and to give us your views on Operations Research.