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AN ELECTRONICS ENGINEER'S VIEW
OF OPERATIONS RESEARCH*

James F. Digby

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AN ELECTRONICS ENGINEER'S VIEW OF OPERATIONS RESEARCH

James F. Digby

Let me begin by asking you to explore with me the reasons why operations research has emerged as an identifiable skill.

Most of us here are connected with electronics, or with aviation, or with both. Suppose that it had somehow been possible to build airplanes and radio transmitters and receivers in the year 1700. I feel that I can be fairly confident in guessing that the growth of the aviation and electronics industries would have been much, much less than what we have seen in the past 50 years. Society of the 1700's just did not demand much transportation or much communication.

There are a great many more people in the world today and we humans have spent the last two centuries increasing our interactions with each other. This is indicated in the following table.

<u>Year</u>	<u>World Population</u>	<u>U.S. Urban Pop. %</u>	<u>Ascendant Political Forms</u>	<u>Ascendant Commercial Form in U.S.</u>
1750	0.7 Billion	Less than 5%	Monarchy	Small shop
1850	1.1 Billion	15%	Democracy	Company
1950	2.4 Billion	64%	Democracy & Dictatorship	Corporation

This growth of population and increasing tendency to interact has, of course, been enhanced by aviation and electronics, and is one of the dominant trends of the first half of this century. This recent trend is manifest in other areas, too, as shown by increasing numbers of the symbols of

interaction: automobiles, social scientists, telephones, psychoanalysts, and cocktail parties, to name just a few.

No one should be surprised to see operations research emerge from this teeming mass of interacting humanity. While the practitioners of operations research have never agreed on a single definition, it is usual to apply the term to the use of scientific methods to study the operations of a complex organization or process; this normally goes beyond the bounds of single scientific disciplines, and it normally is expected to result in recommendations for change. Operations research (OR) is thus much like management, but with emphasis on scientific methods and investigation.

The kinds of activities encompassed by OR have been practiced in some degree for centuries. The completely conscious use of scientific methods to evaluate operations became widespread during World War II; this was brought about primarily by injecting a few rather enthusiastic and diligent scientists into several headquarters as advisors. Most made good names for themselves, somewhat to the surprise of the more conservative generals and admirals. Good work was done, for example, in planning anti-submarine tactics, in designing bomber formations and tactics, and in making the most of airborne search procedures.

After the war the military organizations in both the U.K. and U.S. organized their OR on a permanent basis, while some wartime practitioners began using related methods in industry. First in England, and then, in 1952, in the United States, societies were formed and journals begun. This might be said to represent the birth of OR as a profession. It should be noted that the English, along with their other odd customs, deplore the use of nouns as adjectives, and have stuck to the term "operational research."

The profession has flourished. There were probably less than 500 members of the profession in 1952 in the Western World, and there are probably more than 4000 today, with the proportion engaged in civilian pursuits rising each year. Along with this expansion there have naturally appeared a number of energetic promoters. Some of these promoters, in the good American tradition, have undoubtedly contributed much to the profession and to the efficiency of their clients' operations. Others have over-sold OR and have been guilty of poor judgment in some ways which I will mention later. The outright charlatans have not been as evident as some of us had feared, perhaps because they use less specific labels than "operations researcher."

Before getting to the pitfalls, however, let us consider some case histories where OR had done good work.

First, a wartime example that you may have heard about. (Incidentally, I doubt if the men doing this work consciously thought of it as OR.) A great deal of effort was expended during the war for each decibel that would make our search radar have better range performance. It was difficult, or expensive, or both, to wring this out of noise figure, magnetron power, or antenna size. But a group from Radiation Laboratory interested in test equipment made surveys of radar performance on field units during the war, and found a disquieting condition.¹ The average set tested was 14 or 15 db below the rated value for the radar. These men were not labelled operations researchers, but the OR-type recommendation stands out very clearly: Effort has been inefficiently allocated; too much attention

¹R. D. O'Neal and J. M. Wolf, The Need for System Testing, sec. 15.2 of Radar System Engineering (L. Ridenour, ed.), McGraw-Hill Book Company Inc., New York and London, 1947.

to theoretical performance and not enough to improving actual field performance through better maintenance.

My second example deals with planting and harvesting peas, and is something of a classic OR case history by now. It was presented by C. W. Thornthwaite at the First National Meeting of ORSA in 1952.² A climatologist, he was engaged to advise Seabrook Farms on irrigation problems. After spending the first season watching the operations of this huge farm he saw that irrigation was a small problem, compared to the imposition of huge peak work loads on crews and machines as thousands of acres of peas ripened simultaneously. He developed a scale for measuring growth and found how many "growth units" were forthcoming each month, this being a function of warmth and rainfall. Later, he rated various kinds of peas in terms of growth units required between planting and harvesting and verified the predictability of their growth. The next step was to work backward from the capacity of the factory to process peas, then to ask how many acres would have to be harvested to get that amount. It was then necessary to see that the needed machinery and crews would be on hand to harvest the given number of acres -- presumably an easy requirement compared to that imposed by the big peak loads of the existing system. From all this he developed a planting schedule that came close to producing a steady output of peas to be harvested and processed each day.

Finally, I will cite an example which is not strictly operations research, in that it deals with planning for the future. This example shows the need for a comprehensive systems approach quite clearly, however.

²C. W. Thornthwaite, Operations Research in Agriculture, JORSA, Vol. 1, No. 2, (Feb. 1955), pp. 33 - 38.

I will use Charles Hitch's description.³

Let me give you an example that cropped up in a RAND defense study of several years ago. It was at that time operational doctrine for certain interceptors to carry armament that, according to Air Defense Command estimates, gave each plane a 50 per cent probability of killing an intercepted bomber. Well, 50 per cent looked mighty good to most experienced Air Force hands. It was lots better than anything achieved in World War II.

What did we find when we examined this doctrine in a systems context? Essentially:

1. As was not the case in World War II, we were really preparing for defense against one (or at most a very few) massive atomic strokes.
2. The total systems cost of procuring and operating the interceptors to get them into position prepared to fire a rocket at an incoming bomber was extremely high, so high that the most lavish expenditure on armament scarcely affected the total.
3. It was therefore obvious nonsense to economize on armament.
4. The single-pass kill probability and the kill potential of the defense system could be increased by nearly 50 per cent simply by increasing the armament.

³Charles Hitch, An Appreciation of Systems Analysis, JORSA, Vol. 3, No. 4 (Nov. 1955), pp. 477 - 478.

The performance degradation of the interceptor resulting from increased weight was of the order of 5 per cent -- which, at least in the period of interest -- had a negligible impact on kill potential.

Now this again was a tremendously important result of looking at a problem in a systems context. A systems analysis was not really necessary. I am sure that some Air Force officers, using a broad systems context, thought their way through this one and reached the right conclusion without so much as using the back of an envelope. But many apparently did not, and the doctrine was not changed until the systems analysis was produced and presented. Systems analysis forces both the systems analyst and his audience to think the problem through in a systems context.

Now I have cited several examples of OR-type work which seem to have come up with useful results. There are dozens more that have been recorded in the literature and folk-lore of OR. Perhaps it would be more useful, though, if I could alert you to some typical mistakes.

First, there is a strong temptation for the more academic practitioners of OR to fit complex problems to mathematically or analytically tractable frameworks. While this sort of thing can be useful, it is essential that factors that apply in real life be retained in the analysis if significant, even if it means an approximate solution. Seeking an exact solution to an artificial problem is an academic exercise, not a basis for decision.

Secondly, there sometimes is a tendency to manipulate inaccurate or

dubious data in a very complicated and accurate series of steps. This can be very impressive while at the same time quite irrelevant to the real problem at hand. This sort of thing might be called "OR mystique."

Thirdly, there are the dangers of solving a problem within too narrow a scope. The OR man should always, as a minimum, verify that his solution does not make more acute the problems of someone who looks at things from a higher level of optimization.

Fourthly, there are many temptations to be over-ambitious. There are several cases that I know of wherein operations researchers of considerable technical proficiency used OR methods in addressing governmental, social, or economic problems with demonstrably bad results. They had overestimated the simplicity of these classes of problems. I might add that much research needs doing in these areas, but most of the output should be clearly labelled as research reports and not as action recommendations.

Unfortunately, there are a few practitioners who play up the mystical aspects of OR. They may be those who tend toward pure solutions of impure problems. They may be hiding some unrealistic pieces of reasoning under a big pile of IBM cards. Whether they know it or not, they may be obscuring numerical or logical errors. It is hard to tell you how to recognize all the bad solutions, but it is usually a characteristic of the good solutions that their logic shows through in simple terms after the labor has been done.

What techniques of OR are likely to be most useful to an electronics engineer? The following partial enumeration of techniques often used includes several that are already very familiar in electronic engineering.

Probability and statistics have long ago settled into their niche in electronic engineering, so no comment is needed there. Mr. Johnson's paper will provide a good example of their use in OR. Their offshoots, queuing theory and Monte Carlo analysis, have not received as much attention in electronics circles as they deserve. Queuing theory should be especially helpful in solving certain problems of large scale equipment maintenance, some production line problems, and in getting the maximum utilization from communications channels and devices. Its value is well known to the telephone companies, where the theory was developed, but seems to have been overlooked by other communications specialists.

You will hear later this morning about a case in which Monte Carlo techniques were used in a straightforward way in a problem that involved a great deal of simulation of a complicated situation. This type of use of Monte Carlo is typical of operations researchers, who are often faced with real-life situations much too complex for analytic solutions, and should be of interest to electronic system designers.

On the other hand, there is a class of Monte Carlo applications in which simulation plays a much less important role. There have been applications which were considerably more complicated from a mathematical point of view, but in which the real-life situations were simpler. Subtle ways of reducing sample size and getting the most information from the sample are used. Neutron transport analyses using Monte Carlo are typical of this class. I have heard that it is being used for an analysis of shot effect in tubes. This is the class of application that is likely to be of most interest to the electronics engineer who is concerned with the physics of transistors or the design of vacuum tubes.

Linear programming⁴ and dynamic programming⁵ are relatively new techniques which permit the solution of problems involving large numbers of variables (perhaps thousands) and they do this in a way which is feasible with present computing machinery. Typically, linear programming is applied to an input-output analysis of some process which consumes a large number of items such as labor or raw materials and which turns out a number of completed products. One minimizes some measure such as cost, subject to certain constraints. Linear programming probably will find its best industrial uses in solving problems in well-established processes where the rules are quite stable, and particularly where small increases in efficiency are significant. Dynamic programming treats multi-stage processes wherein the state of a system is changed from time to time by decisions or transformations. The outcome of earlier decisions affects the choice of later decisions.

Some interesting new applications of linear and dynamic programming have been made in the communications field.⁶ Optimal message routing, the most economic plant extension programs, and certain equipment replacement problems can be solved. The dynamic programming techniques can be applied to various propagation problems involving propagation through inhomogeneous media, to some problems in information theory, and to various control system problems.

⁴George B. Dantzig, Formulating a Linear Programming Model, RAND Corp. Paper P-893, 1956.

⁵R. E. Bellman, Dynamic Programming, Princeton Univ. Press, (June 1957).

⁶R. E. Kalaba, M. L. Juncosa, General Systems Approaches to Telecommunication Optimization Problems, RAND Corp. Paper P-964, 1957.

Information theory and search theory are the next tools of OR on my list, and I shall just mention them by name, since they are more familiar to electronics engineers than they are to operations researchers.

Organizational behavior theory, threats problems of human group interaction: how information is passed, how leaders are picked, what communication patterns work best, etc. Dr. Biel's talk will mention some experiments in this technique which have had practical results.

Decision theory and measures of value are of interest to toilers in the more recondite reaches of OR. Along with game theory they promise intellectual stimulation to the electronics engineer, but, as of today, are not likely to be used directly on his problems. Their current role is to orient the researcher for a proper attack on some of his toughest problems.

Operational gaming is a newly emerging technique which is more of a descendant of the old war games of Prussia than of game theory. Gaming involves the use of live opponents and holds much promise, both as an aid to learning and as a kind of OR laboratory. So far, I have not heard of any applications of gaming that bear on electronics problems except those of the military communicator.

Finally, simulation is a technique of considerable interest to the electronics engineer as well as the aeronaut. This, of course, is the main theme of Dr. Biel's talk, so I will say no more about it.

Let me conclude this talk with some remarks for those of you whose interest is to join OR, rather than adapting it. Job openings in OR, while not myriad, are almost decimyrriad. (I am using the word in its more precise sence.) Formal training in most of the fields I have just mentioned is so rare that it is not a prerequisite, although an active interest in

several of them is a good omen. Most of my friends seem to feel that electronics engineers make excellent operations researchers (but I should mention that most of my friends are operations researchers who were electronics engineers). The system oriented engineer is likely to be much happier at it than the component designer or deep specialist. Salaries are about the same, but the true OR man gets his reward from his chance to solve very complex problems, albeit usually superficial ones. He enjoys working on a team with an astronomer, a biophysicist, and two topologists. He has his frustrations, too, because his problems are often not solvable in the usual sense. His frustrations, we may say, are the frustrations of a complex, furiously interacting world, and he feels both frustrated and rewarded because he is involved in what is going on more than most people are.