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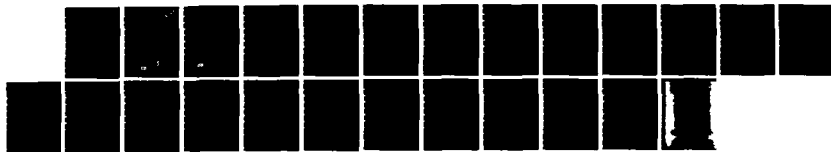
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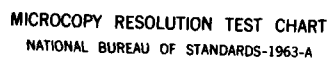
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Thomas E. Anger

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THE OUTLOOK FOR MILITARY OPERATIONS RESEARCH*

Thomas E. Anger
Director of Review, Center for Naval Analyses

Before getting on with my topic, I want to distinguish between "operations research" and "operations analysis."

To me, "operations research" (O.R.) is the broader term; it includes such black arts as "systems analysis" and "net assessment." In my lexicon, the branch of operations research called "operations analysis" involves the study of current forces, for the purpose of evaluating them and improving their tactical employment. I make these distinctions now because I will use both terms a lot.

What is the outlook for military operations research? I can only give you a personal view, one that is fraught with biases which will become apparent to you.

To lay the foundation for my views, I will begin where military operations research began--with its genesis in World War II as operations analysis.

Next, I will survey subsequent developments in warfare and military operations research, to see what lessons they may hold for the future.

Specifically, I will assess two possibilities:

*This paper was given at a general session of the 50th Symposium of the Military Operations Research Society, which was held at the U.S. Naval Academy in March 1983. The paper reflects the author's personal views; it does not necessarily represent the opinion of the Center for Naval Analyses.

- 1) that we need to reform military O.R. to purge it of accumulated sins, and
- 2) that military O.R. must be redirected, if it is to ride the wave of the future.

After weighing these possibilities, in specific terms, I will end by stating my vision of the future of military operations research.

THE PAST AS PROLOGUE

Genesis of Military O.R. in World War II

Turning to the genesis of military O.R., we come inevitably to Morse and Kimball, who were there at the creation. In Methods of Operations Research, they describe the scope of O.R. in World War II, by quoting Fleet Admiral Ernest J. King. In urging the continuation of the Navy's Operations Research Group (ORG) into peacetime, Admiral King called out these wartime activities:

- (a) The evaluation of new equipment to meet military requirements.
- (b) The evaluation of specific phases of operations (e.g., gun support, AA fire) from studies of action reports.
- (c) The evaluation and analysis of tactical problems to measure the operational behavior of new material.
- (d) The development of new tactical doctrine to meet specific requirements (e.g., antisubmarine screens and screens for slow-moving damaged ships).*

These are, of course, activities I would classify as operations analysis.

*From a letter to Secretary of the Navy James V. Forrestal, quoted by Philip M. Morse and George E. Kimball in Methods of Operations Research (Washington, D.C.: Operations Evaluation Group, 1946), p. 1.

Why was this analysis considered successful? What characterized its practice? Borrowing from a history of ORG's descendant, the Operations Evaluation Group, I come up with five attributes.*

First, the members of ORG brought suitable academic disciplines to the operational problems of the day. Those disciplines were, mainly, the physical sciences and mathematics.

Second, as civilians, they operated outside the Naval hierarchy, and could bring a detached perspective to their study of the operational problems.

Third, the value of this perspective fostered a relationship of mutual trust between the analysts and their Naval clients. The Navy divulged whatever information the analysts needed to get the job done. The analysts, in turn, protected the information.

Fourth, in this atmosphere of mutual trust, the analysts could readily get their findings and recommendations to Navy decision-makers, who--in turn--were quick to tell the analysts what problems were most pressing. In other words, there was open communication.

*Keith R. Tidman, The Operations Evaluation Group: A History of Naval Operations Analysis (scheduled for publication in 1983 by the U.S. Naval Institute), manuscript pages 144-149, passim.

Finally, reinforcing all of these characteristics of a successful venture, we find the field program. A large fraction of the Operations Research Group's analysts were assigned to operating bases where they could "more readily learn of operational problems and pass along proposed solutions directly to those in command." The practical knowledge gained in the field was essential to group members in Washington, "who needed to know whether their suggestions concerning tactics or equipment squared with the real world ..."

The most important point about operations research in World War II seems almost too obvious to mention--so it must be mentioned. As Morse and Kimball said, it was "toughened by the repeated impact of hard operational facts and pressing day-to-day demands," and "repeatedly tested in the acid of use."*

In short, it was done in wartime, to do nothing less than help win an actual war. I will return to this point in assessing the evolution of military operations research since World War II, and its future.

Evolution

How has O.R. evolved since 1945? First, its scope has expanded widely beyond operations analysis. Aspects of defense that were once embedded in the routines of defense management, or ignored altogether, have developed into formal, analytical enclaves--each with their own jargon, techniques, and mystique. These newer branches of O.R. deal with such by now familiar pursuits as:

*Morse and Kimball, op. cit., p. 10.

- o The assessment and projection of enemy strategies and forces
- o Formulation and evaluation of alternative U.S. military strategies
- o Assessment of our ability to execute the approved strategy and its derivative missions
- o Evaluation of alternative future forces and systems, for specific missions and tasks
- o Development of efficient manpower policies and logistics operations, to support the forces
- o Integration of all these elements into coherent, multi-year spending programs.

The actual problems studied in these newer branches of O.R.--and in operations analysis, as well--have been shaped by changes in the instruments of warfare. Without stretching things too far, I can say that in naval warfare, for example, we have gone from guns and bombs to missiles, from radios to satellites, from eyeballs to imaging radars, from chaff and underwater eggbeaters to a spectrum of electromagnetic tricks, and from the finger on the trigger to the onboard computer. (It is interesting to note that among the constants of naval warfare are the displacement hull and conventional carrier-based aircraft.)

Prevailing opinion seems to be that the new gadgets add up to this:

- o Combat can take place over much longer ranges; there are fewer places to hide.
- o Combat can be observed and controlled more directly by higher authority.
- o Success in combat depends more heavily on things than on people.

I will come back to the role of things and people--analytical people, in particular.

Now, I want to consider how the practice of operations research has evolved. Are the practices of World War II--for example, putting the analysts where the operations were--still observed, and how?

The first practice I cited earlier was the application of appropriate academic disciplines. The physical scientists and mathematicians of World War II have been joined by economists, psychologists, engineers, computer analysts, political scientists, and so on. Though some may disagree, this development is essentially neutral.

What about the detached perspective of O.R.? The techniques of O.R. have been adopted by advocates--the producers and military proponents of systems. Thus the only unique value O.R. has left to offer may be detachment. But a high proportion of its practitioners are part of the organizations they are supposed to advise--holding civil or Naval rank--rather than advising at arm's length.

With more analysts serving in the government, the old client-lawyer relationship has given way mainly to an employer-employee relationship. Those in government, at least, hold a position of privilege--a position that a lot of private contractors may not enjoy.

With the burgeoning of military O.R., its findings and recommendations have reached decision-makers by more and more rigidly-controlled routes. Why? First, there is an absorption problem: The military establishment today is smaller than it was in World War II, yet the numbers of analysts who are pouring out paper has increased by orders of magnitude. Then, there is no war, so it is harder to tell what problems are really urgent and what ideas hold real promise.

The absence of war, of course, means that today's military operations research is removed from reality. There is no "acid test" for our views of the threat and our recommendations for strategy, forces, and tactics. Even our manpower, logistics, and management studies may point to policies that are good for peacetime efficiency and disastrous for combat effectiveness.

Enough reminiscing and breast-beating. Where do we go from here? Must military O.R. be reformed or redirected? Is the coming age of technological wizardry going to render military O.R. useless, no matter what we do?

WEIGHING THE FUTURE

Reform?

First, let us ask what problems military O.R. has today, and how they can be fixed. I think all the fundamental problems can be traced to the lack of urgency and feedback that I just mentioned. This leads to a lot of uncertainty about the purpose of military power, about the proper selection of forces and other resources, about the capabilities of current forces--and even about the future of military O.R. The uncertainty manifests itself in what we do and how we do it.

Activities like threat assessment and campaign analysis, for example, take a lot of heat for being pure guesswork. A closely related problem, which affects most branches of military O.R., is that the lack of feedback allows analysts' biases to creep in, unchecked. Thus, even in traditional operations analysis, results often have to be taken with large doses of salt. The "lessons learned" from peacetime exercises, for instance, must always be--or should always be--preceded by warnings about exercise artificialities.

The uncertainty also leads to problems in the way O.R. is done; for example:

- o If problems are unclear, it is easy to let them be defined by researchers' interests and techniques.
- o Inferior work is more difficult to spot, because results are harder to test. Thus bad O.R. can be "sold by the yard"--which hurts the whole profession.

Are there solutions to these problems? Obviously, we don't want to relieve the uncertainty that underlies them by having a war. But there are some things that we in the O.R. profession can do to get more respect for our endeavors from military leaders and operators. Basically, it means accepting the limitations of our art and cutting out much of the window dressing that lends the appearance of science to what is often no more than guesswork. Here are a few maxims that I try to keep in mind when reviewing CNA studies:

- o Our purpose is to help decision-makers find roughly right courses of action; there may be many roughly right options--there is seldom a "best" one.
- o Perhaps the proper way to help decision-makers is by a negative approach--use heibel thinking to weed out the non-starters, and tell decision-makers what special conditions must prevail if any option is to be clearly preferred.

- o Avoid judgments--implicit or explicit--about which conditions will prevail; leave the decisions to decision-makers.

When you think about it, these ideas apply to all types of O.R., not just the much abused cost-effectiveness branch.

Reorientation?

So much for what we can do to clean up our act. What about the new challenges we may have to meet, as the range over which warfare takes place is extended, even into space; as tactical decisions are made farther from where the weapons explode; and as electronic gadgets do more of the "seeing," "hearing," and even "thinking"? Will the machinery of warfare so dominate the outcome of war that its planning and conduct will become primarily an engineering problem?

Let us consider the recent conflicts in the South Atlantic and Middle East, which involved many weapons that were far-removed from those of World War II.

First, the Israeli experience in Lebanon--and in earlier wars--shows that there is nothing like knowing who and where you're going to fight to make it easier to decide how, when, and with what.

According to a recent magazine article, for instance, the Israelis have designed their main battle tank--the Merkava--according to the view that speed, range, and fire power should be sacrificed to tactical mobility and crew protection.* This approach seems to have paid off.

*Maj. Richard Gabriel, USAR, and Prof. John Moriarty, "Israel's Main Battle Tank Pounds Its Message Home," Military Electronics, January 1983, pp. 124-32.

Then, in the air war, the Israelis demonstrated tactical readiness, by their coordinated use of RPVs, electronic countermeasures, chaff, and missiles, to take out Syrian SAM sites. The Syrian air threat was evidently defeated by jamming the Syrians' C³ and attacking their MIGs from the "blind side."*

Such results suggest not only that the Israelis had excellent intelligence, but also that they used analysis--whether they called it that, or not--to develop nearly foolproof tactics. (It also helps, of course, to place a lot of emphasis on training in these tactics, and to keep the enemy from knowing what you know and knowing what you are going to do.)

In other words, the clearer your pre-war objectives, the more decisive pre-war analysis may be.

On the other hand, the battle for the Falkland Islands--which also involved modern weapons--showed clearly that war can still be an uncertain and messy business. As the British undoubtedly learned, unless you have been fighting the same enemy for a long time, and are sure to keep on fighting him, the next war will never be the one you have planned for.

*These impressions are gleaned from two other articles in the January 1983 issue of Military Electronics: "EW Won the Bekaa Valley Air Battle," (p. 106) and "ELTA Plays a Decisive Role in the EOB Scenario" (pp. 135-7), both by Paul S. Cutler.

The situation of the U.S., vis-a-vis the Soviet Union, is more like that of the British than like that of the Israelis. In this situation, it is critical that strategies, forces, and tactics be robust—not tailored to specified planning scenarios. The need for analysis to explore the future uncertainty and compare the options for coping with it remains compelling.

The battle for the Falklands also showed that technological advances do not lead necessarily to "walkovers." The British needed O.R. during their conflict, to design and carry out vital logistics operations, to assess the seriousness of the Argentine mine threat, and to develop tactics to deal with the Exocet missile threat. (And I understand that the British would not go do it again without some on-scene O.R. help.)

In the event of a major U.S.-Soviet conflict—involving, as it would, rather evenly-matched opponents—this record of improvisation, aided by analysis of actual operations, would likely be repeated.

CONCLUSION

As is obvious by now, I have taken two approaches to the future of military operations research. One is prescriptive—how we must act to preserve and enhance our profession. The other is predictive.

First, the prescriptive part.

O.R. (by various names) has taken a lot of flak in recent years. A lot of that is due to the relative peace in which we live.

Take those who criticize U.S. strategy, forces, and tactics, and who blame quantitative analysis for the supposed failings of our defense posture. I would remind these critics--who usually have their own strategies, forces, and tactics to promote--that they are free to be so critical and prescient mainly because their own pet theories cannot be subjected to the acid test of combat.

But neither can ours. Yet, we have invited much of the criticism by being pseudo-scientific and comically precise in offering assessments of strategies, forces, and tactics, on the basis of a priori models, brochure values, exercise data, and plausible-sounding intuition.

We can help decision-makers weed out the unworkable courses of action and understand the conditions under which one workable option or another may be preferred. But the analyst who presumes to know the answer--or to have the technique that reveals answers--does his profession and his client a disservice.

In the predictive vein, I submit that O.R. analysts may have to know a lot more about technical matters--but that technology will not do away with O.R., no matter how "smart" and adaptive our weapons and their ancillary systems may become.

The choices among technologies will remain difficult, and fraught with implications for strategy options, manpower policies, and so on. Even if analysis cannot say which technologies are best, it can help to sort them

out, according to which strategies they may serve. And, given a strategy--
if we're given a strategy--there will always be the need to strive for a
rough consistency between such things as manpower and logistics policies,
on the one hand, and the force structure, on the other hand.

Perhaps the idea of consistency--rather than optimality--will animate the
future-oriented branches of military operations research in the 1980s.

On the operational side, there will always be the need to test the
technology and see how well it actually performs. The more dependent we
become on technology, the more important it is that we know its physical
limitations, and fix them or adapt to them.

Beyond that, however, technology actually opens new horizons for
analysis. The number of tactical possibilities increases with the
geographic range over which operations can be conducted and the speed with
which they can be controlled and carried out. The challenge for analysis
will be to sort out the possibilities and help operational commanders
develop a manageable repertoire of tactical tricks.

Finally--as Morse and Kimball observed in Methods of Operations Research--
there will be opportunities to exploit the predictability of technology.
For until the millenium of artificial intelligence arrives, machines will
always be more predictable than people.

These opportunities may be the most promising and critical for the future of operations research. For one thing, we may expect the enemy to become more dependent on technology, too. Thus we can figure out, in advance, more and better ways to thwart him--if we know enough about his technology.

We can also figure out more readily how the enemy may thwart us, through our technology--and prepare for that event by developing tactics that do not rely on the technology.

Perhaps the growth industry of military O.R. in the next few decades will be the systematic evaluation of technological failure modes, and analysis of their tactical implications.

Should a war come for this nation, there is no doubt that O.R.--of all types, but especially operations analysis--will prove as valuable as it did in World War II.

Whatever happens, I believe that military operations research--for all its faults--will prove more valuable than ever, for operations research can be an instrument of guile, as well as an instrument of brute force. And one of the overlooked lessons of all wars is that guile is usually decisive.

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74

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