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THE ROLE OF OPERATIONS RESEARCH IN PLANNING FOR LIMITED WAR

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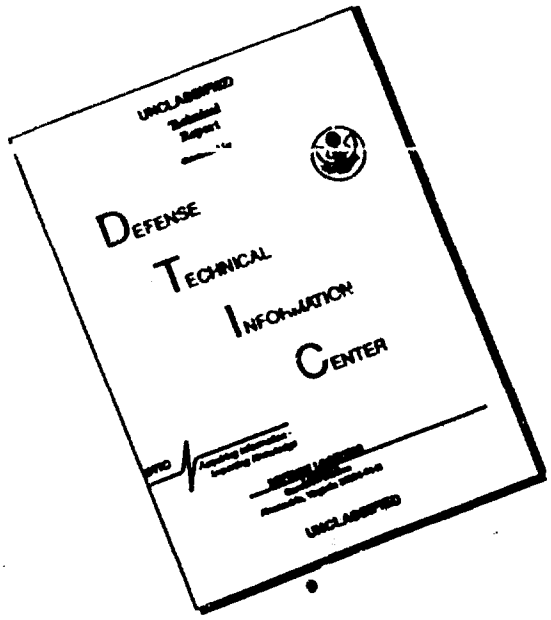

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The title of my talk is "The Role of Operations Research in Planning for Limited War." It is a masterful title. I wish I had selected it myself. I must admit, at the outset, that I doubt whether many of us would be in agreement on what we mean by "operations research," on what we mean by "planning" or on what we mean by "Limited War." Such a lack of agreement has many virtues. It provides me with an opportunity to peddle my own definitions and then use these definitions to lead to personal conclusions about the subject.

Unfortunately such an approach also has several difficulties. It would require more time than I have available, and it would most likely end up as one man's views and speculations. Therefore, I have chosen a related approach. I would like to talk about limited war and some of the problem areas involved. Then I would like to provide a subjective evaluation of where operations research has been somewhat successful in assisting the military planner. Finally I would like to speculate about where we might go next, and what some of the difficulties might be.

First, then, to what limited war is, and what problems it presents. In the years since the war in Korea some new words have come into prominence in the vocabulary of international relations. Among these words are "limited war, small war, brushfire war, insurgency," and others. If the first time you heard one of these words you went looking for a definition, you probably were not successful. Some were not defined at all and others were not defined precisely. The lack of specific definitions seems to be based on the fact that the thing we call limited war comes in a great many different shapes and

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sizes, and not on any lack of examples. It is possible to list more than 40 examples of armed conflict in the last decade that could be called limited war, under a broad definition of the term. They range from sporadic actions of a band of guerrillas to the crossing of national borders by large, well-organized armies. Limited war may be seen or anticipated in actions from Laos to Latin America, from Cuba to the Congo.

It is not completely clear why limited war has become such an important part of the mechanisms of international relations. It may be that, as one author has observed, "Two basic, historically unique conditions encourage limited war; a deep conflict of aims and interests between the nations that hold a predominance of world power, and a terrifying capability of the two antagonists to destroy each other."(5) Whatever the reasons, the emergence of this type of warfare as an important weapon of international struggle has produced a growing concern with understanding some of the problems involved.

What are some of the problems? Without attempting any kind of detailed listing let me indicate four topic areas of interest connected with such warfare.

First on the list are the problems of <u>What causes limited war?</u> Everyone is aware of the Clausewitz principle that "War is nothing but a continuation of political intercourse with an admixture of other means . . .," but the translation of the principle into specific military events is not well understood. Questions like <u>How does limited war fit into the politico-military aims of Communism?</u> <u>Under what circumstances and for what reason will the political intercourse erupt into war?</u> and <u>What political and military indicators presage such an eruption?</u> are just a few of the questions that can be raised.

A second topic is <u>How to keep limited war limited</u>. Most of the studies of such wars emphasize the critical point that the nations engaging in the war observe some type of restraints. They may observe restraints or limitations in the kinds of weapons they use, the kinds of targets they attack, the geographical areas that will be involved, etc. Many problems regarding these restraints are of interest. These

include questions like How do the restraints get established? Under what conditions will they be maintained? Under what conditions will they be violated? and What are the consequences of violating them? Some understanding of the machinery of checks and balances that is operating in limited war is of great importance for controlling the course of such wars, or preventing them from expanding uncontrollably or escalating into general war.

A third topic of interest includes the kinds of military capabilities needed to fight limited war. The fact that these wars come in such a variety of sizes and shapes raises questions of How do we develop and use the right forces and weapons in the right place at the right time? And, we might add, on the right enemy. These are questions that interact with our strategic capabilities and posture, and cut across our defense budgets, and push against the limits of our technical knowledge.

A fourth topic area is based on the fact that limited war is an international event. As such, it is influenced by international commitments, support agreements, and assistance pacts. We are thus faced with questions of the role and utility of treaties and treaty organizations, of military aid and assistance programs, and of technical aid and assistance programs in both deterring and fighting limited war.

In addition to these four topic areas of interest, there are many others that could be mentioned. My object has not been to attempt a catalog of all of them but rather to indicate something of the scope and variety of questions that can be asked. There is, evidently, little difficulty in finding problems to study.

At this point you may well argue that the questions I have raised are of some interest, but that most of them are not appropriate to operations research. They belong in the domains of the policy-maker, the political scientist, or the national planner. They refer to problems that at best are poorly defined and inadequately formulated. While such a response would be gratifying because of the humility it suggests, it is not clear that it is justified.

First, operations research has a long tradition of inserting itself into areas where it has been considered inappropriate and, in some cases, unwanted. It has even been successful in many of these areas. Second, we should not confuse complexity with intractability. Many of the questions I have mentioned involve problems that are extraordinarily complex. At least on first approach they do not take shape as clearly formulated studies with evident pay-off criteria, obvious alternatives to be examined, and opportunities for objective measurement. But then few problems do, initially. The major art of operations research, which some people would like to make a science, still consists of the difficult, creative effort of adequate formulation of the problem. We are not incompetent in the face of complexity, but we are often slow.

There is no doubt that limited war is a complex problem area. It is evident that a major part of the difficulty lies in the variety of forms and sizes that it can take. It is not a single phenomenon, but a range of phenomena involving political, ideological, economic, and logistic components as well as military and technical ones. It is not a single "design point," but a field of design points. It is not like general war which, very crudely and with apologies to my "general-war colleagues," I would characterize as having a relatively small number of design points. At least the number is small enough so that researchers can establish reasonable performance criteria and carry out detailed studies of alternative forces, force mixes, allocations, costing, etc. with some confidence. This does not imply that there are not major problems involved. It only implies that there is considerable agreement about the phenomenon to which the results apply.

But try the same approach to limited war. Start with the definition of limited war as "armed conflict short of general war, exclusive of incidents, involving the overt engagement of the military forces of two or more nations," a definition that occurs in the Dictionary of United States Military Terms for Joint Usage. Compare this with a definition of general war as "armed conflict between the

major powers of the communist and free world in which the total resources of the belligerents are employed, and the national survival of a major belligerent is in jeopardy." In the latter case the enemy is identified, the capabilities can be generally prescribed, and the objectives are reasonably clear - a usable set of design points. For limited war, by contrast, the immediate enemy may be unidentifiable-- in fact an enemy may not exist in the usual sense of the word - the magnitude of the military and political commitments of the protogonists is uncertain, the war objectives may be poorly defined, and the freedom to pursue these objectives may be subject to undefined constraints.

All of these characteristics add up to complexity--in the variety of forms that the war can take, the variety of forces that can be involved, the variety of outcomes that are possible. It is an awesome spectrum from the OR point of view. In spite of this, operations research has played a significant role in one of the four topic areas mentioned. In the area of developing effective military capabilities, operations research has a tradition of success. Particularly in the area of weapons and weapon system selection and development, we have been able to carry out analyses and to provide useful results to our military clients.

I am not tempted, to quote Flood, "to speak to you about the great and glorious past achievements of our profession: how we helped win World War II, how we teamed with military professionals and helped to insure wise choices of weapons and defense programs since World War II . . ."(3) A review of the Journal of the Operations Research Society of America, or of the Proceedings of the Military Operations Research Symposia, or of any of the excellent books available on operations research would illustrate the magnitude and value of our efforts. They are sizable on both counts.

The role of operations research in the weapon area will continue to be an important one. Quantitative evaluation of alternatives, improvement of force structure, and cost effectiveness comparisons are the stock and trade of military operations research, and in the weapons area we will find important applications for these and other methods.

Although the weapon area has been the major area in which operations research has contributed to wartime planning, including planning for limited war, there are several related areas in which substantial contributions have been made. These are logistics, tactics and military organization, and command and control--all of which are part of our efforts to improve military capability. I would like to touch on each of these areas briefly, with due apologies to my colleagues who will undoubtedly find that my remarks do not do full justice to their efforts.

Logistics is a critical part of limited war. In fact, some people regard limited war as a battle of logistics. Logistics is defined--to paraphrase the military definition--as the science of planning and carrying out the movement and maintenance of forces. It deals with materiel, personnel, facilities, and services. In the broadest sense it is not surprising that operations research has been of value in these areas since military logistics is related to many industrial problems of production, transportation, distribution, storage, scheduling, etc., permitting the application of non military research to military problems. However, the applications are not as simple and straightforward as one might expect, since military logistics involves some unique characteristics.

To indicate just a few of the important aspects of military logistics operations: The military services live in two worlds. In the peacetime world their operating and training requirements present fairly predictable demands on their logistic systems. At the same time they must be prepared to react rapidly and effectively to meet contingencies that may arise. These contingencies may be very different. On the one hand they may require the movement of a limited number of military forces to some country halfway around the world to prevent a local crisis for developing into a full scale war. On the other hand they may require massive participation in a major conflict. These possibilities as well as all the variations and shadings in-between may place very different demands for logistic support on the military services. It is the uncertainty in the size and nature of the logistic demands that might develop, that has no equivalent in non-military

operations. It has sometimes been compared to a large department store that must be prepared to meet a possible continuing Christmas rush every day of the year, without changing its routine operating procedures.

A related characteristic is the importance of response time in limited war logistics When national circumstances dictate the movement of major forces from one area of the world to another, it is too late to start planning what military support will be required, how and where it will be obtained, assembled, transported, stocked, and issued. Not only does planning have to precede such moves, but it has to cover the tremendous variety of contingencies that might develop. And once a military move has been initiated, the logistic systems have to keep pace with the military operations The lack of a spare part in a combat situation, unlike its civilian equivalent, may not represent loss of a sale, but loss of a battle.

Thus, limited war logistics requires a capability to respond to any one or combinations of a variety of military situations in a rapid manner and with sustained effectiveness. In helping the services acquire this capability, operations research has made sizeable contributions. Some indications of the variety of problems and types of OR efforts in the field can be found in publications such as the Naval Research Logistics Quarterly.

Another area of OR effort related to improving limited war capabilities is the area of military tactics and organization. At one time, military operations consisted basically of conflict between the organized regular forces of two belligerents. The weapons consisted of those designed primarily for destruction of point targets, i e., the individual soldier, bunker, or aircraft. Strategic warfare was more a political concept than a unique weapon capability. The development of atomic and thermonuclear weapons changed this state of affairs. Economical mass destruction became feasible

This change had enormous and terrifying consequences, particularly for strategic warfare. But it also has significant consequences for conflicts short of general war. The nuclear battlefield has had to

be viewed in a different way than the non-nuclear battlefield. On the nuclear battlefield, massed or concentrated forces, a limited number of high capacity lines of communication, or large supply complexes close to the front line, present the enemy with important and vulnerable targets.

It is evident that a new dimension exists in warfare. The overall missions and purposes of military force remain, but the procedures and conduct of military operations have had to change. Operations research has played an important role in creating the changes. As concentration of military forces gave way to dispersion into smaller units, as mobility of forces became more and more necessary, and as supply, communication, and coordination of smaller, more mobile units became more critical, the opportunities for operations research contributions was evident. The researcher, working more closely with the military in this field than in any other, participated in analysis and evaluation of military organization and of the impact on tactics. For army forces as well as for naval and air forces, the consequences of nuclear warfare have been studied and the techniques of operations research have become an important adjunct to military planning in an area where both civilian and military analysts could only dimly visualize the phenomenon with which they might be faced. Of the various forms that limited war might take, the type called limited nuclear war is one of the most complex and difficult to understand.

However, a rival to limited nuclear war has developed. Just as limited nuclear war represented an increase of violence in an otherwise limited conflict, an older type of warfare appeared in modern dress to extend the scope of limited war in what is sometimes called "the low end of the spectrum." This new, old phenomenon is currently called "counterinsurgency." Counterinsurgency operations, abbreviated "COIN operations" are by definition and in practice more than military problems. They include significant political, economic, civil, and psychological components. At the present time it would be premature to talk about the contributions of operations research to COIN operations. It is probably sufficient to point out that OR is actively involved in the area, and the expectation is that OR will contribute

to developments and improvements in concepts, tactics, equipment, and weapons for such operations.

A last item that deserves mention in the area of improving military capabilities for limited war is the one called "command and control." In recent years the term, command and control, has become widely used. It is used in essentially two ways: to define a group of functions involved in military operations, and to indicate a group of problems connected with carrying out these functions. The first use is, of course, not new. From the beginning of organized forces military commanders have had to carry out such diverse activities as determining the objective or mission of military operations, specifying the concepts, preparing the plans, directing and guiding the forces, evaluating the outcomes of combat, and conducting related activities. These are some of the basic functions of command and control.

However, the thermonuclear age has created problems in regard to these functions. These "problems of command and control" have been as diverse as how to insure rapid and effective target coverage under all wartime conditions. how to establish and maintain limitations on the use of weapons, how to protect the decision-maker and the decision-making centers, how to improve the reliability and adequacy of communications, how to increase the coordination between military forces, etc.

In trying to find solutions for some of these problems, considerable emphasis has been placed on modern technology in the form of improved communication techniques or equipment, automated or semi-automated data processing equipment, protective construction, safety devices on weapons, and other developments. In addition, command and control has faced operations research with many familiar topics in a new guise. To cite just a few: the definition, purpose, mission, function, and effectiveness of existing and proposed systems and procedures, the interaction of human and machine operations, decision-making functions, as well as information handling, priorities, scheduling, display, network organization, and a host of others. While much of the emphasis on command and control has been in terms of general war operations and air defense, increasing attention is being

given to the limited war aspects. The indications are that operations research may find itself in a unique position in the command and control area. On the one hand its long and fruitful association with the military can be of immense value in helping to identify, analyze and evaluate some of the critical aspects of the problems that are arising. On the other hand operations research has always been close to system designers and system engineers in approach, analytic techniques, and evaluation of results. This convenient relation with both groups could play a major part for the role of OR in command and control.

Thus far I have attempted a very simplified survey of the role and some of the achievements and prospects for operations research in limited war military operations. Our achievements have been of great value in this area. Not only has our general analytic approach to military problems been productive, but our continual emphasis on improving research methods and theory bodes well for the future. Developments in mathematical analysis, computer techniques, as well as in simulation, game theory and war gaming, decision theory, control processes, etc., promise to provide greater capabilities for the future.

But we should be aware of some of the pitfalls that exist for our efforts to contribute to limited war planning. I would like to mention just two that I consider significant. The first is over-simplification or over-idealization of the complexities of limited war.

Simplification or idealization are usually necessary and often desirable in operation research efforts. If, however, they are used as retreats from the real complexities of a situation, we may only succeed in creating "pseudoproblems" and coming up with "pseudosolutions." How to attain the appropriate level of simplicity or detail in our formulation of problems is, of course, a difficult question. As most of you are aware, articles and publications frequently admonish the reader to choose the right level of detail and the appropriate parameters for his studies. These kindly directives are usually supplemented be a description of the author's choice of parameters and values as they apply to his problem. It is my experience that

they can seldom be applied to one's own research efforts. As far as I am aware no theory or guide book exists for selecting the right technique and level of detail for OR problems in general. Nor does it appear that such an effort is merited. Our efforts are still concerned with questions that are so nearly unique that a "methodological cookbook" is of dubious utility.

A somewhat more significant pitfall is an inadequate awareness of the rate of developments in our military establishments. We have come a long way from the time when operations research was only a step away from actual military operations, and when the test laboratory for our recommendations was a conflict actually taking place. World War II and the Korean War are becoming increasingly inadequate as models for our studies, or as the basis for evaluating the effectiveness of our results. Major changes are occurring in at least three aspects of military activity. As indicated earlier, after hundreds of years of acceptance, the values of "mass" and "concentration" are being reinterpreted in light of the development of nuclear weapons. Similarly, concepts and techniques of combat are being reexamined on the basis of the importance of developing new capabilities for the new forms of counterinsurgency operations.

A second aspect of the military in which changes are occurring is the command and organization of forces. The emergence of the Unified and Specified Commands, the creation of the Composite Air Strike Force, (CASF) of the Strategic Army Command (STRAC), of Strike Command (STRICOM), and the repeated reorganizations of the Army division as ROCID (reorganization of the Army division), the pentomic division, and the recent ROAD are all illustrations of the changing nature of military organization.

The final aspect is one that is better known. It is the great change in weapons and equipment that have occurred in recent years. Among the weapons, the intercontinental ballistic missile, the thermonuclear bomb, the nuclear submarine are widely known. Less widely known are V/STOL, Redeye, Cobra, Mauler, ASROC and a host of others.

With these changes in concepts and doctrine, in organization, and in weapons and equipment, the role of the military operations

researcher becomes more demanding. He can no longer be confident that the use of World War II or the Korean war as a model, as a source of data, or as a basis for effectiveness comparisons is justified. He may find the analyses of today being made invalid or obsolete by the changes of today. An excellent cost effectiveness comparison of two obsolescent weapons would be of limited value to the military planner.

These two pitfalls, oversimplifying the phenomena with which we must deal, and developments outdating our studies are not new to operations research, nor are they unique to the limited war researcher. What makes them of particular significance in limited war studies is that the spectrum of possible military conflicts is so broad that we may find ourselves using scarce resources working on the wrong problem for the wrong place at the wrong time.

So far I have been dealing with the contributions of OR to improving our military capabilities for limited war. This was only one of the four topics which are of interest in limited war. The other three dealt with causative factors, with the establishment, maintenance, or violation of constraints, and with the influence of international policies, military and economic assistance, technical aid, etc. For these topics, which we might broadly call the political, psychological, and economic areas as opposed to the military and technical areas, the OR situation is less encouraging. While some attempts have been made, they have only stirred the waters. Our investment has not been at all commensurate with the importance of understanding the complex relations between the military and the political, economic, and psychological aspects of limited war.

To identify, select, and analyze problems of causitive factors, of the "limiting" process, or of politico-military interrelations is a major task. It has, in fact, led many individuals to question whether OR can or should consider these as legitimate areas of study. The answer to whether it "can" obviously depends on making the attempts and evaluating our success. The answer to whether we "should" is clear.

As long as we consider OR as being concerned with interactions we should be willing to attempt to work in these areas. To paraphrase one of our colleagues, the characteristic of operations research that distinguishes it from the more familiar sciences such as physics, chemistry, biology, psychology, astronomy, and geology is perhaps the fact that its explanation of phenomena asserts little about the exact physical and bio-social nature of the elements involved, but deals rather with the interplay between elements of the phenomena.(3) For the phenomenon of limited war, the interplay of the military, political, economic, and psychological elements is paramount. Understanding, even in a modest way, this interplay would be a major contribution.

There is a second reason why we "should" attempt to apply ourselves in these other areas. From our earliest days the application of operations research to broader policy questions has been one of our objectives. One can trace in our literature over the years a series of challenges to extend our efforts to broader problems of national importance.(1,3,4) For those who accept the challenge it is clear that success represents a major contribution to limited war research. But it is not a challenge which we should accept without some caution. To indicate just a few difficulties that are important:

1. To assist the planner, whether military, political, or economic, in these areas demands a broader understanding of the problems than has been characteristic of our efforts in the fields of weapons, logistics, tactics, etc. We will have to educate ourselves--and it may be a costly and difficult education, so much so in fact, that the alternative of staying within the areas of our demonstrated competence may be both more appealing and more fruitful. If we do undertake the self-education necessary--as some of you know--we will find ourselves in some "never-never" lands of politico-military-economic complexity which, if they do not defy description, certainly defy quantification at present. For years we have been unable to define in its broadest terms the "military worth" of a weapon, a piece of equipment, or a military force. To try to define its "politico-military worth" may be completely beyond us.

2. In these areas we will face difficulties in data availability, in defining measures of effectiveness, in costing, and in application of our results. Although many of our concepts and methods would be appropriate,[2,6] I suspect that some of our research techniques and tools will turn out to be blunt or useless instruments. We will have to be ingenious in improving our present tools and in inventing new ones.

3. We may not be welcome. The early development of operations research in the military field was not without suspicion and rejection. The later introduction of OR to industry was not effortless. It would be unreasonable to assume that its introduction into the politico-military-economic field would be without resistance that could, in many cases, be justified.

In spite of these difficulties it may still be worth the effort. Just as OR has and will continue to fill a role in assisting the military planner in problems of weaponry, logistics, tactics, and organization, it can render a great service if it is able to expand this role into assisting the limited war planner with some of his related political and economic problems. One can, with an earlier author, be optimistic about the fact that, "As our concepts and methods have improved in effectiveness and generality, we have ventured into problems of increased scope and social significance."[1] The complexities of limited war fit the bill well.

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