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CHARACTERISTICS OF A SUCCESSFUL STUDY

The scope of naval studies ranges from defining a naval mission to evaluating how well a naval weapon system performs under specified environmental conditions. Studies are examined from both general and specific viewpoints. Observations are made concerning why some studies are successful, whereas others fail. Guidelines are offered for conducting future naval studies in our dynamic environment.

INTRODUCTION

Planning is difficult. As an intellectual process that often requires boldness, planning involves the future with all its uncertainties. Volumes have been written on how to do planning right. It is practiced universally. Yet, I will dare one more essay on the subject because, in the words of Dickens, "It was the best of times, it was the worst of times." The introduction of Perestroika and Glasnost in the USSR under Mikhail Gorbachev, the conclusion of the Intermediate-Range Nuclear Forces Treaty, and the revolutions of 1989–90 in Eastern Europe and the Soviet republics have cumulatively closed the curtain on the Cold War. The result is an eroding framework for national defense that is undergoing a metamorphosis that will continue for some time.

The Laboratory has nurtured the seed of many naval technologies for fifty years. It has been a partner with the Navy in the planning that has taken these technologies to weapon, combat, and force systems and successfully deployed them at sea. I believe that many of the planning efforts that have been the forerunners of these developments can be characterized as "the innovation that changes the probabilities."[1] The nurtured innovations have been engineered, manufactured, and maintained in service. Each ability or attribute can be associated with some unique innovation for each system development. The Laboratory has been adept at determining which developments can be carried through to successful operation. It has correctly predicted the success of several complex developments as well as the failure of many seemingly simple ones.

In this article, I would like to focus on some characteristics of successful studies. I will examine the successes and failures of the past with the intent of inferring some principles for the future. The subject is timely because of the changing nature of the world. We should adapt the rich heritage of APL's involvement in Navy planning toward studies in the future. I will draw only on my own experience, limited principally to weapon, combat, and force systems for surface combatants. An examination of the Laboratory's broad array of programs would strengthen and enrich the general conclusions.

The dominant theme in this article is that a successful study requires a confluence of engineers knowledgeable in the system(s) of the study, analysts, operators, and experts in intelligence. Too often we see useless or wasteful attempts at planning without the knowledgeable engineer being integrally involved. In the 1980s, the Navy set some long-term broad objectives and purposes through the Maritime Strategy.[2] More recently, the Maritime Strategy has been replaced by the Navy policy, as discussed in Ref. 3.

At the other end of the spectrum, the processes of budgeting, program approval, procurement, and the like are implemented in unending detail, mostly with only a few years of lead time. Studies intended to provide technical planning seem to have reached a low ebb. It is increasingly important to have a planning process for steering the fleet ten to fifteen years ahead in all the specifics that must guide actions while leading to the general objectives. The question is how to make such activities productive.

WHAT MAKES A STUDY SUCCESSFUL?

Studies started to become an intrinsic part of every facet of defense programs in the McNamara era of the mid-1960s. Decision making was heavily influenced by a new, huge wave of numerical analysis. In the Department of Defense, and thus by derivation in the armed forces, every major new system or purchase, or change in policy or objective, was mainly decided by studies. The scope and degree of this study dependence reached levels far beyond previous practice.

The trust in studies has since waxed and waned in multiple cycles. Studies continue to be made in ever-widening fields and in ever-increasing detail, and they are established as necessary or useful in most defense (and other) management. Studies have become a major industry, and the techniques for conducting studies have been refined and sorted by long experience. Likewise, the proper role of studies in management and decisions has become better appreciated. Nevertheless, not all studies have been successful, and many authorities have developed an antipathy toward them. The criticisms are so strong that they must be recognized in reshaping the total study effort, and certainly in planning any new study.

The initial reaction to the McNamara approach was often that experience and judgment were set aside in favor of seemingly more quantitative facts. Recent studies sometimes try to merge these elements. Other weaknesses now command more attention.

Some studies have been successful and well received, and a continuing contribution to weapon system development is expected. What is needed is an understanding of what has gone wrong, such as the choice of issue examined, timing, scope, sponsorship, attitude, or analytical technique. The intent is to generalize the lessons learned to help in new studies.

The discussion here will proceed from the negative to the positive. We start with some general criticisms received from decision makers or analysts, then discuss some specific issues that studies have not been able to resolve over the years, and then provide some examples of completed studies that turned out to have no useful results; finally, we describe the successes.

At this point, a careful definition of success is not needed. A successful outcome generally involves some useful result, some appreciable degree of acceptance, some contribution to decision or understanding not otherwise provided, and perhaps a framework or measure useful in further pursuit of the subject. In most instances, a consensus on study results is reached, especially after some months or a few years have passed.

TYPES OF CRITICISM

One criticism is that studies lead to, or recommend, complex systems that are not practical or that are too theoretical. The system is designed to handle extreme situations, which is not necessarily sufficiently important, but which leads to equipment that does not perform well. The high-technology system may not fill the wider general need.

Many criticisms concern the inadequate allowance for realistic factors, such as cost, operational constraints, the role of human judgment, logistics, training, or personnel. Sometimes a lack of these considerations leads to "goldplating"—overdesigning the system with unnecessary features.

Some studies are too simple. A common fault is to base general conclusions on a single situation. Models may not include all the important elements (e.g., weapon system environment). The measures of performance or quality may be too limited. The study may examine a set situation and not explore the actions of a reactive enemy, for example.

Studies involving a specific system design issue or tactical issue sometimes try to address wider issues not examined. If such studies attempt to draw general conclusions, then their legitimate narrower conclusions may be questioned. Studies commonly involve a view of future situations; both operators and technologists have estimates, not necessarily in harmony, and the analyst conducting the study may not do well at reconciling or providing a view.

In some instances, studies have been biased to sell an idea or a product, intentionally or otherwise. If not objective or credible, the critics attack.

Some studies are not performed well in the sense of modeling, data collection, analysis, or exposition. The main issue may never be well defined. Much data may be gathered, but no conclusions are drawn. Details may obscure issues. The exposition may be too extended or obscure. Such studies are unlikely to have an effect, if indeed they reach any significant audience.

Such criticisms may lead to an impression that studies are not useful, and hence that the effort is wasteful. More importantly, some critics find that the studies are wrong or misleading, and hence that they are harmful or even dangerous to programs. Recent Vice Chiefs of Naval Operations, Chiefs of Naval Operations, and Secretaries of the Navy have expressed such views in particular program areas. Studies continue to be supported as essential to planning and management. The challenge is to ensure that the studies are properly framed and executed.

ISSUES UNRESOLVED BY STUDIES

In seeking to understand the requirements for a successful study, it could be instructive to examine some topics or issues for which studies are needed, but for which adequate and decisive studies have not been performed, and for which programs or a general outlook has not crystallized. Identification of such issues involves subjective judgments, and some studies may have been done. The impression is, however, that important decisions are needed.

The following issues appear to be amenable to study but have not been able to be resolved: multi-warfare coordination, big ship versus small ship, the role of electronic warfare in a warfare area, and the role of missiles and manned aircraft in strike. Why are studies not decisive in these areas? Are they perhaps all separate issues, each with its own difficulties, or can some general constraint be discerned?

Multi-warfare coordination studies have rarely been attempted, and some notable interested parties do not consider that a problem exists. Advances in microelectronics and computers as shown in communications, combat information centers, and naval tactical data systems, coupled with long operational experience, attest to the ability of using a multipurpose navy. The current problems of using the same platforms, or interdependent platforms, for several warfare areas nearly simultaneously have not been sharply defined. Thus, a strong pressure to study this area has not been felt.

Many who work in defense-related areas fully appreciate the need to study multi-warfare coordination, but the problem is so large and complex that it is not yet clear how to analyze it. A guiding conceptual structure has not yet been formulated. Perhaps this structure cannot mature until some of the component warfare modes can be described separately in mutually compatible frameworks.

Force composition, in the sense of favorable mixes of platforms and systems for combined warfare missions, has always received attention. The problem seems to have been addressed mainly by tactical planners. Nevertheless, no widely recognized analytical framework exists. The spectrum of possible situations is broad, and all situations must be addressed. Force positioning for one warfare area is often not optimal for another, especially when the same ship or aircraft is involved in two warfare areas. In that situation, no "good" disposition or mix exists, and compromises are required. This problem has not been addressed in any depth. It has become important to develop an approach.

The big ship versus small ship issue regards the Navy's need for attributes of both, depending on the situation. Since the quantity of ships, regardless of size mix, is below the level that could be well employed, some uncertainty will always be involved in how to allocate the ships with respect to procurement and mission. Practical limitations have dictated specific programs for some big and some small ships, but the ratio of big to small is not necessarily ideal.

Studies could well address the uses of each ship type and the desired ratio of ship size, rather than the unresolvable issue of what absolute quantity to seek. The desired ratio depends on the evolving missions and threats. The Navy may not be able to fill all conceivable roles well, and some choices may have to be made in advance for ship-type planning.

U.S. electronic warfare on ships has been studied and tested extensively. The contribution of these systems to defense is generally accepted, but trusting them in specific combat situations causes some ill-defined uneasiness. These doubts tend to relegate electronic warfare to a secondary or backup insurance role. Proponents consider that it warrants more confidence and a larger role, but studies and tests have not yet overcome the doubts. Something is missing from the studies and tests, which prevents them from being more decisive. It should be timely to open this subject again, perhaps with a plan for studies and tests in a complementary program.

Historically, electronic warfare studies have often seemed to be separate from studies of other aspects of combat, with a consequent reduced impact on assessment or planning of whole defense programs involving many other kinds of systems. Hard kill–soft kill studies are mostly recent and not yet very comprehensive or definitive. The subject needs expansion, looked at from the total defense perspective and not from the technician's perspective.

The future roles of cruise missiles and carrier-based strike aircraft in offense depend on Navy missions. If the future missions are sufficiently defined or bounded, the respective uses of missiles and aircraft can probably be well defined. One popular view now concerning land attack is that missiles, which are accurate and able to penetrate defenses, could be used to knock out defenses, and the aircraft that can carry heavier loads more efficiently but are vulnerable to defenses could then deliver the more massive attacks. Other missions could exploit other characteristics, and the favored uses could change as technology changes. The difficulty is to define the missions, which must necessarily be of great variety and not entirely predictable in type.

Thus, some topics have not yet been studied for a number of reasons: lack of pressure or motivation, lack of a conceptual base, sheer complexity, immaturity in the subject, the wrong questions being asked, inadequate technical data input, and inadequate understanding of the naval mission setting. It is sometimes alleged that a subject is too hard to analyze, which is a poor reason. The hard problems may most need resolution, and studies should address them in phases and pieces, if necessary.

EXAMPLES OF STUDIES

The following examples of studies are suggestive of, but have not yet clarified, the general principles we seek concerning how to make a study successful. I have been associated with the studies over the years in varying roles ranging from leader to analyst, reviewer to interested observer. I will not critique each study in detail; my point is not to reopen old wounds but rather to try to profit from the successes and failures. I will not address ongoing or recently completed studies.

The Countering Air to Surface Missiles Study examined the suitability of Aegis (then just defined and in early development) for anti-air warfare. The thrust was to find cheaper, simpler defenses, based on super point defenses. The assumptions were challenged.

Sea Plan 2000 attempted to deduce the size and composition of the Navy needed in the year 2000 by examining potential geographic mission areas and likely levels of conflict. Naval community consensus concerning the future and the study assumptions were at issue.

The Major Fleet Escort Study was an early study of the number of escort ships (mostly anti-air warfare) needed in the fleet. It was based on a limited set of scenarios and had a limited perspective regarding changing threats and technologies. It was accepted and significantly influenced the makeup of our current fleet.

The Responsive Threat Study (re Aegis) explored potential responses to Aegis that could modify the threat presented by a knowledgeable enemy. Interesting particulars were uncovered, but no general message was delivered.

The Ship Missile "Pilot" Study (see R. J. Hunt, *Harpoon Ship Missile "Pilot" Study,* unpublished memorandum, 1968) was an early exploration of the U.S. need for a surface-to-surface missile system, carried out after the Soviets introduced such systems. It was in effect the first look at the need for Harpoon, which was not yet defined. The findings were in the affirmative, but the actual influence of that conclusion was minor.

The Cruiser Study examined the role of cruisers and the possible need to develop a strike cruiser. The problem had many dimensions and was not well defined. No strong leader emerged, and no receiving audience had a framework to assess or act on the study. No general principle emerged, and the impact was minimal.

Project 80 attempted to estimate the consequences of evolving technology for anti-air warfare of the future. Specific technological areas were surveyed. The effort was not tightly organized and was not finished. The completed pieces were not widely disseminated.

The Advanced Naval Vehicle Study assessed the potential for the Navy of several possible new ship types, such as hydrofoils, surface effect ships, and SWATH (small water plane area twin hull). The study was very prolonged and produced valuable engineering information. It did not, however, make a convincing case for bringing any of the ship types into actual naval use, and very little follow-up ensued.

The Battle Group Composition–Disposition Study explored the choice of ship mix in a battle group, as an aid in deciding on the acquisition of new ship types and their numbers. Several "equal cost" combinations of ships were examined. The payoff results for the selected forces were simple and not widely accepted.

The Standard Missile Nuclear Warhead Utility Study (see R. J. Hunt, *Standard Missile Nuclear Warhead Utility Study,* unpublished memorandum, 1981) assessed the tactical usefulness of having a nuclear anti-air warfare warhead in defending against a nuclear attack. The analysis was straightforward in the narrow framework of kill and penetration probabilities; it did not address the consequences of engaging in nuclear actions. The study was completed as national policy was shifting away from building up nuclear capabilities.

The Outer Air Battle Study chose programs for systems to counter enemy missile-launching aircraft before they reach their missile-release position—the "kill the platforms" aspect of anti-air warfare. Many possibilities were examined, and system procurements, developments, tests, and further studies were recommended. The recommendations were not consistently accepted for action. Nevertheless, the issues raised in the study were then widely debated, and the topic became a priority and was further examined. The thoroughness and breadth of the study enabled the subsequent examinations to prevail in some program decisions.

The DDX (new destroyer class) Study, made in detail and wide scope, was intended to design and justify programmatically a new destroyer class. The recommendations provided the programmatic ammunition to proceed, and further studies and designs were undertaken to shape a program. Although the final ship design differed from the original study results, the study set forth most of the main factors and options, provided an approach for judging the value of various capability levels, and generally supplied a basis for the more detailed designs and assessments that followed.

The next three studies, often cited as successful among a great many more, are particularly noteworthy.

Technical Plan I provided developments, system choices, standard product lines, and schedules that subjected the Terrier, Tartar, and Talos (3T) surface-to-air missile systems to a coherent study status over several years. This was the "get well" plan. Many technical assessments were involved, as well as programmatic decisions. An example was the recommendation to rework the AN/SPS-48 radar and then use it as the long-term choice for three-dimensional radar. The plan was accepted and implemented almost *in toto.*

Technical Plan II was a modernization plan for the 3T systems, primarily to extend their capabilities from defending against aircraft to defending against missiles. Several separate technical extensions were defined and centered on achieving more rapid system reactions to targets. The recommendations were implemented rather slowly, but in the long run, the plan essentially guided program developments.

The Advanced Surface Missile System Assessment (Withington Study) established the technical requirements for a new anti-air missile system that eventually became Aegis. The study, beginning with numerous system proposals from industry and laboratories, examined the threats and missions expected in the future. A concept of the role of shipboard area defense was defined, and the performance specifications of a system judged to be feasible at acceptable developmental risk were developed. Trade-offs and optimization were examined in depth. The resulting system definition became the basis for Aegis development, and the study concept prevailed throughout the Aegis implementation.

The success of the foregoing studies rests on the fact that the recommendations did guide the ensuing programs and that the decisions stood the test of time. The studies were well defined for those areas where action would be taken and had strong sponsorship.

LESSONS LEARNED

The key elements of a successful technical planning study, as learned from the foregoing studies as well as others, are the following:

1. The study must have a driving and compelling motivation. The driving force may be a new or unforeseen threat challenge or a new or changed mission requirement, such as the current emphasis on naval forces operating in close proximity to land in the face of a significant threat. The motivation may also be a glaring deficiency in operational or developing systems—the basis for Technical Plan I, as discussed earlier—or other reasons such as cost, schedule, or risk concerns for new systems.

2. The sponsorship must be strong and appropriate. The intended audience must want and be able to understand the study. The sponsor must be willing and able to take the study forward through all potential adversaries. The sponsor must also be at an appropriate level of command to lead the charge.

3. The members of the study group must be able to identify and screen alternatives, not just on the basis of cost-effectiveness, but most importantly on the basis of feasible engineering. The requirements that drive the study, or that are to be derived from the study, must be stated in engineering terms easily translatable by the engineer.

4. Because of the need for sound engineering input, the membership of the study team must include operators, intelligence personnel, analysts, and knowledgeable engineers.

5. The system definition must be stable and circumscribed. This requirement is a primer of system engineering, but it is often ignored in a study. Too often the system is unbounded or elastic.

6. Threat, mission, and scenario concurrence are essential. Every study starts with a threat/scenario approved by intelligence personnel. What is often missed, however, is the need to construct models of a future world to serve as tests of the circumscribed system. Thus, concurrence

is needed between the broad array of decision makers who will affect or be affected by the study.

7. Issues must be well defined, and assumptions related to the issues must be credible. Creating a forum at the beginning of a study to define issues and agree on assumptions can be very time-consuming and difficult. But these considerations and threat/mission/scenario concurrence may be the most important parts of any study. Together, they address the nature of the problem.

8. The metrics or numbers associated with system effectiveness are critical to understanding and illustrating system utility and mission need, but experience, wisdom, and judgment are equal partners in the decision process.

SUMMARY

If the success of a naval study is measured by its effect or influence, then most studies are unsuccessful. Key elements of a successful study include a compelling need to conduct the study; strong sponsorship; expression of the study in understandable engineering terms; balanced and dedicated team whose members are skilled and experienced in operations, intelligence, analysis, and engineering; bounded system definition; concurrence of threat, mission, and scenario information; well-defined issues and assumptions; and use of metrics along with individual judgment to measure system effectiveness.

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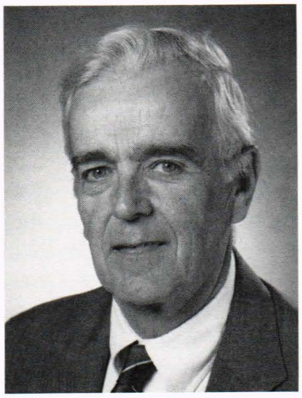

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