Naval War College Review

Volume 21
Number 9 *December*

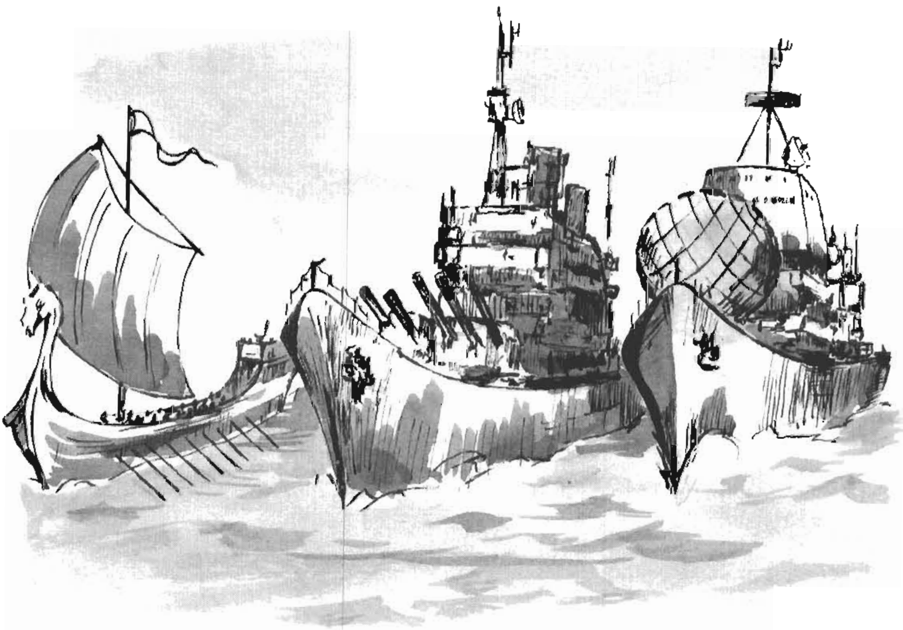
Article 2

1968

Navy Research and Development

Robert A. Frosch

NAVY RESEARCH AND DEVELOPMENT



An address delivered at the Naval War College

on 13 June 1968

by

The Honorable Robert A. Frosch

Assistant Secretary of the Navy for Research and Development

I am not quite sure what the title of my talk really is. It started with a title like: "The Future of the Fleet," aimed more or less at the beginning of the 21st century. It might also be called: "What Does Make a Difference and What Does Not Make a Difference."

I can express one aspect of my central theme best in a statement that Vice Adm. Tommy Booth tells me was made by Sir John Carroll (then Chief Scientist to the British Admiralty) some time around 1963: "The ingenuity of your innovation nearly blinds me to its utter uselessness." I am afraid that this is an overwhelming point about a good deal of military research and development. I have tried to avoid this flaw in what follows. As you will hear, I find concepts more significant than their hardware realizations.

My question is: "How can we make predictions of what we are likely to need, in time to carry out the implications of those predictions in such a way that we can have the Navy that we require, at the time that it is required?"

The history of prediction of any kind

is fairly dismal, and the longer ahead one has tried to make a prediction, the more dismal predictive history really is.

Inasmuch as the time it takes to develop and produce military systems has come to be long, compared with the time in which political and military change in the world is possible, we are now in an extraordinarily difficult situation.

I have tried to get at this problem by a process of extrapolation; that is, I have tried to look back over the past 30 years and ask myself, "What are the few things that I think have happened in technology in the past generation, the past 30 or 35 years, that have made a difference?" and then, looking at these, ask myself, "What are the things that are likely to happen between now and the year 2000 that are likely to make a difference?"

First I must say what I mean by a "difference." I am using the idea of difference at a rather extreme and high level. By difference, I mean things that make a radical change; things that change the character of warfare, particularly of naval warfare, in some qualitative way. I am looking for changes that are as radical as the change from sail to steam.

I think one needs to be careful about what it is that changes. For example, there is frequently a temptation, particularly among people who have not really dealt with naval warfare problems, to think that the change from sail to steam was principally significant because of a change in speed, and, of course, that is not particularly true. I believe the time for a trans-Pacific crossing actually increased with the shift from sail to steam, certainly initially. The radical nature of the change had little to do with speed. It consisted almost entirely of the fact that one became relatively independent of the weather for the first time so that the necessity for following the winds was greatly decreased.

This attempt to look for qualitative, rather than merely quantitative, changes is a difficult one. It is sometimes difficult to distinguish between things that make major differences and things that just make small differences, since the superficialities of what one sees frequently overwhelm the important inner characteristics. However, I have made my choice. Such a choice is partly a matter of taste, and we could argue about these choices rather profitably, or perhaps they will at least be some food for thought.

I have tried to pick the things that are in the first category of what I would call "three categories of change." The first category contains the things that make qualitative differences; that change the world in some intrinsic way, so that warfare or human activity is somehow different in a major way from what it was before.

There is a second category of change in which the change is not in itself intrinsically important, unless it occurs asymmetrically, so that one side makes the change but not the other. Then there is an imbalance. All that happens when both sides change symmetrically is that warfare is just as it was before, except balanced on a new quantitative level.

Changes in speed are usually like that. For example, I would say that the supersonic aircraft has no intrinsic significance whatever in warfare. It has a significance only on imbalance. If one side has it and the other side does not have it, then it may have a significance; but, if both sides have it, all that happens is that you have air warfare that is twice the speed you had before. I assert that there is really no important qualitative change there.

The third category are changes where the imbalance does not mean anything whatever even if it is asymmetric. These are imbalances that are offset by other things. That is, a particular weapon may be of some special use to one side or another in a war for geographic reasons, but may quite naturally be offset by some totally different factor without leading to any basic change. For example, the use of silo-based ICBM's and Submarine Launched Ballistic Missiles in different mixes by two countries can be the result of geographic asymmetries and signify nothing as a strength asymmetry.

I will make one other comment which bears very strongly on the difficulty of making predictions at all. It is a statement about the nature of research and development. The best way I can make it is to make it by analogy, by making a comment which is mathematical in form: Theorems and the proof of theorems in mathematics are never found by systematic means; but after you have a completed collection of theorems and proofs, you always have a systematic arrangement of things. That is, the process of research and development, the process of getting ideas, particularly at the research end, is unsystematic; it is untidy; it is essentially unpredictable; and, in fact, in some ways, it is almost unmanageable. However, the process of taking things that have already been invented and turning them into specific engineering objects can be a very systematic kind of operation. We have two kinds of things, which are very different intellectually, going on inside the research and development community. At the research end, at the end of getting ideas, we have an untidy, difficult, ill-understood process in which what we do is try to get lots of people thinking and hope for the best. Once we have ideas and something that we want to go on and build, we then have a fairly systematic process even though there are many unsystematic and difficult things imbedded in the process. There are always unanticipated difficulties that feed back into further research and applied research.

For the reason that the research end of things is so unpredictable, before I go on to making my predictions I will make a disclaiming statement: the most important technological developments for the next 30 years will turn out to be the fruit of things that have not yet been invented. It was that way if one made a prediction at the beginning of the last 30 years; I think it is that way now; it will always be that way. Thirty years is a long time for technological change. It is a human generation, but it is something between five and seven development generations; we could have technological changes several times in the course of the next human generation.

It is asserted from time to time that the time from the invention or discovery of an idea until its actual use in hardware is getting shorter and shorter. What is getting shorter and shorter is the time from the invention of an idea until somebody attempts to apply it. The time from the beginning of the attempt to apply the idea until it is actual hardware that can be used in the Fleet has stayed constant, perhaps gotten slightly longer, over the past couple of generations. The reason lies in the fact that the two processes are different. To get from the fundamental idea to the idea of an application--that is, to the beginning of development--one only needs communication and ideas, and the communication has been getting faster and better. To get from the beginning of the development to the end of the development takes a long sequence of detailed and specific steps. In particular, the beginning of development of the system is the point where one begins to have a complicated administrative problem, so that the process is governed by more than the availability of ideas and the availability of communications. If we get to a point in history such that the time from fundamental idea generation to the beginnings of application becomes zero, we will not necessarily continue to cut the time from the beginning of idea generation to actually having completed weapons and devices. I'll come back to this point a little later.

At the present time I think we are in the position that the time to go from development into initial production is shorter than the time it takes for the development, and that is an important point for the next 30 years.

Now I will give you my list, with some comments of the dozen or so things developed over the past 30 years that I think have really made major differences in warfare. I think I would not get a very different list if I tried to make a list of the dozen things that have probably made the major difference in all of technology over the past 30 years.

The first thing that I would list is, of course, nuclear explosives, not because of the properties of the hardware, but because of the mere existence of the hardware. What has happened is that the existence of nuclear capability has introduced the formal idea of deterrence, the capability of deterrence on a worldwide scale, and that has probably changed warfare more than anything else, because it has introduced a peculiar level of doubt and difficulty that previously did not exist. I will later come back to what I think that means for the future.

The second major event was the invention and development of radar and communications. I put these together as the general use of the electromagnetic spectrum, because this has made it possible to see at great distances, to see through the weather, to communicate at great distances, and to communicate without regard to weather or to distance. It is no longer necessary for a detached unit to be out of communications, or for a detached unit to be unable to see.

The third change is related to the use of the electromagnetic spectrum, at least conceptually, and that is the use of space because it means that we can look at the whole earth; we can look at the weather; we can do surveillance; and perhaps most important, it has forced us to think of the world as the world in a much more direct sense than we previously did. I think that there has been enforced a kind of psychological difference between looking at a globe and saying, "Oh, yes, the world is round, and at the present time around on the other side, it's nighttime," and looking at photographs from space. One sees it in a different way from the geography classroom than one observed it from photographs taken by astronauts from TV transmitted pictures of global weather, and so on. I think this concept of looking at the whole earth as the whole earth simultaneously has made a major difference.

In a direct naval warfare sense, I think the invention of nuclear power for submarines has made a qualitative difference because it has made the true submersible possible, and I think this has changed the nature of potential sea warfare greatly, even though we have not yet had to use it in warfare. It has changed the antisubmarine warfare problem profoundly, as well as making a special class of nuclear deterrent systems possible.

Nuclear power for surface ships I would class as a quantitative improvement, but not a qualitative improvement that changes everything completely as it does for submarines. That does not mean it is unimportant, but it does mean that I would not class it as one of the revolutionary and overriding changes.

The next thing I list is what I insist upon calling the revival of rocketry. The new rocketry, both tactically and strategically, has changed the world. The reason why rocketry could be revived is connected with one of my later points, which is probably of more importance.

Solid-state electronics, and its capability to pack a tremendous amount of information handling into a small space and weight, has of course been extremely important, and will continue to be so.

Perhaps as important, or more important, than the hardware capabilities that come with solid-state electronics, has been the philosophical change from analog thinking to digital thinking, from using continuous voltages and currents, as the computing elements themselves, to manipulating with numbers. This has meant an entire change in computing capability, in reliability, and, in fact, in whole new modes of thought about accuracy and about how to attack problems. This is making, and will make, a major change.

Another idea which is more important than its hardware, and is also connected in some ways to radar and communications, is the idea of the separation of the operator and the sensor; the idea that a man's eyes and ears, or what he uses for eyes and ears, do not have to be attached to him. There was always a rudimentary version of this in warfare in the sense of the forward scout who could go and look and come back and report, but we are in the beginnings of a revolution which is far beyond that, because we have begun to put the eyes and ears far away from the man who operates with the information. A man on the ground can now dig on the moon while he watches.

I would now like to mention four ideas which are more fundamental than any of the things I have already mentioned. They are ideas and not hardware. I think they are part of the really important intellectual inventions of the 20th century, and perhaps among the important intellectual technological inventions of all time. They are seldom or too infrequently mentioned.

The first is the idea of feedback: just the very idea that it is useful to take a portion of the output of a device and put it back into the input in order to change the operation of the system. This was invented originally as a technique for dealing with the stabilization of amplifiers in the early days of radio reception. It has become an idea which has a life of its own far beyond its use in hardware. It has changed the way in which computations are done; it has changed the way in which we look at physical, social, military, and economic processes entirely, and I think it has changed the way in which all warfare is done, at least because of the way it has changed hardware. It is the key idea that has made the revival of rocketry sensible, because it is the essential element in all modern guidance and control systems: take the output of the system and put it back in to correct the input so that the next output will be better.

The second idea that is extremely important, and I'm not sure these last four are in any particular order, is the idea of "system": the idea that lots of things working together are an entity that has to be thought of as an entity for a total purpose: it makes no sense to deal completely properly with each of the pieces unless you look at the overall entity and tie it together to be a total object to do a particular job.

The third is the systematic treatment of signals and noise, and signals in noise, along with the systematic ideas that flow from the signal-to-noise concept: the ideas of detection, detection rate, missed detection, and, most important, the idea of the false-alarm rate. This last is the idea that the ability to detect is intrinsically tied to the number of false detections--detections of targets that are not there. Previous to having this concept we went questing after impossible systems--for example, systems with perfect detection probabilities--because we did not understand the connection between our ability to detect things and the fact that we would automatically be fooled by detections of what is not there. This is a very general idea that goes beyond specific sensor equipment systems. It is an important general idea in thinking about how we deal with the world.

Finally, the idea of systematic economic choice--which is the real idea of systems analysis--has had an obvious impact on warfare and technology. This has had a very direct effect which has been obvious to all of us. To some extent many people feel it has had a directly deleterious effect. I think that problem is the result only of the birth-pangs of the idea and its use. In the long run, when we get better methodology and a better understanding of law to use study results in context, it will prove to be one of our most important intellectual tools.

All of these things that have happened contain the seeds of what will happen, so that the things I will now talk about are built directly on those past events.

I come to the things that will make a difference. Perhaps I should call them things that would make a difference, in the sense that they require work; we must work on these problems if they are really to make a difference.

The first is one to which I have already referred. The change that might make the greatest difference in future warfare, and in fact in future technology, would be to shorten the time it takes to develop something new. It now takes anywhere from 5 to 10 years to take a development from the idea to production and into the Fleet. Whether you think it's 5 years or 10 years depends in part on when you define the process as starting and ending; there is quite a lot of variance from development to development.

This change would make a difference because it would make it possible for us to respond more rapidly to change in the world. The time that we have to take to predict, develop and produce a Navy is now so long that we have a concern that we may, at the end of the process, have the wrong Navy and the wrong weapons for the true technological and political-military circumstances in being by the time we actually have that Navy and those weapons. This is frequently expressed by saying that everything is obsolete by the time it is produced, and is a natural consequence of the time it takes to develop. Any piece of electronics that goes into the Fleet uses technology which is at least 5 years old the day it goes into the Fleet. If we try to update that technology in the course of the development, we never complete the development, and/or we have terrible logistics problems with the weapons system. If we could make a radical decrease in the time it takes to do development, say, from 5 to 10 years down to 2 years, then we would be much more responsive and much more able to change the Navy to track the way in which the world changes. This could have the consequence that, at any given time, we would actually need to have a smaller inventory of actual hardware without the smaller inventory being a danger to us, should we have to use it. I mean this in the following sense: currently we have to maintain a collection of navies aimed at a collection of possible wars and possible circumstances, and at possible technologies that a potential enemy might develop. If we actually were more responsive, we could diminish the necessity to keep in being some things--the trouble is we do not know which things--that we will never use or which, because circumstances have changed, would be useless if we had to use them. We would have to build fewer interim systems; systems we know are not right, but had better build, or patch together, because we might need them before we can get around to developing what we really ought to have. We do a large number of developments on an accelerated, makeshift, 2-year scale, because what we really want takes 5 to 10 years but we cannot wait. With a valid 2-year schedule, for example, we would be able to adjust our weapons better to the precise circumstances for which they would be required.

The second area which will grow in

importance--it has already begun to do so--is the methodology for doing analyses of hypothetical situations, for analyzing warfare situations, and for understanding what they mean in terms of hardware which we should develop and tactics which we should use. We are just really at the beginning of the methodology for looking at war and at various scenarios for war. Better analytical methodology will make an important difference.

The third problem has very much more general application than warfare, and that is the development of the ability to think about nonlinear situations. Almost all of our analysis, almost all of our thinking about warfare, economics, hardware situations, and tactical situations is linear. We look at a large problem, and we are appalled at the size of the problem; we rapidly cut it up into pieces, each of which we can solve independently; and we then make the blithe assumption, because we must, that having put the problem into subproblems we can put the subproblems back together again, add up all the solutions, and thus have the correct solution to the overall grand problem.

This assumption is true for almost no real-world problems. One of the most important problems we are on the verge of solving, that will make a major difference in warfare, will be the ability to think about large nonlinear problems. If we are able to do this, then we will be able to measurably reduce development time by improving our capability to predict the results of large nonlinear situations. Improvement in analytical methodology and in nonlinear techniques together might change the whole time scale of our ability to change the Navy and to make predictions about how the Navy should be changed. If we can lengthen the time into the future for which we can make a good prediction and shorten the time it takes to respond to that prediction by changing the Navy, we would be in a much better position for future warfare.

The next area which is most important--and more important than any of the hardware--is a change in how we can learn about using people. By this I am referring to the vast expansion of solid knowledge in physiology, psychology, and in behavioral and social sciences in general. We are just at the very beginning, on the shadowy edges of knowing how to fit people and machines together; how to build the machines so that the people can really use them; and we have scarcely begun to work on how to fit people together, in a systematic, scientific, technological way.

People have been tackling these problems more or less by intuition for a long time. We usually encapsulate existing knowledge in this field under the name of leadership, and we do fairly well without knowing anything technologically and systematically sensible about it. A systematic understanding in this area could help us in the detailed matching of people together, in groups, to their jobs and to the machinery they use to do those jobs.

I mentioned the idea of deterrence earlier and will now return to that subject to say what I think is happening to the idea of deterrence, what it has done to warfare, and what it is likely to mean in the future. The first thing the idea did was to lead to the invention of the escalation ladder, a systematic recognition of the fact that two boys in a schoolyard start by yelling at each other and end up pummelling each other by a simple, straightforward series of steps. Once nuclear deterrence actually came into being it had an effect which was not originally anticipated--that is, it decapitated the escalation ladder. Instead of having an escalation ladder that goes all the way up to an ultimate conflagration in smooth stages, what has now happened is that we have an escalation ladder truncated somewhere about the middle. It is a little fuzzy there. Everybody starts getting frightened that instead of going up the next couple of steps smoothly, there will be a sudden jump up the ladder to a global war. This has meant that it gets harder and harder, and there is more and more reluctance to engage in warfare above a certain level. Without arguing the question of whether this is a good or a bad thing, I think it is psychologically real, and something with which we will have to go on dealing. I think there is a research problem here which could change the whole nature of the problem.

We understand almost nothing systematically about the psychology and social forces that operate in deterrence. We really do not know why deterrence works, except in the general sense that people are afraid of what might happen and therefore they stop. We simply have no quantitative understanding of the problem. I am not sure it is possible to have one, but I think it is very important to look for one. For example, when we work out force levels for strategic war, we start out with assumptions that always read something like: "To deter an enemy nuclear weapons attack, we must have a force that is surely capable of destroying one-third of the enemy population and 50 percent of their industrial technology." These numbers are quite literally pulled out of the air, and nobody has the faintest idea of whether one-third is 10 times the necessary deterrence level or one-tenth the necessary deterrence level.

For example, how much deterrence must the Soviet Union provide to deter the United States? From the way they seem to have gone about it, they seem to think they need 90 percent destruction. I assert that we must consider--I am perfectly willing to treat this merely as a hypothesis for discussion--that the suitable level for destruction by the Soviets on the United States, to provide quite complete political deterrence, might be as low as 1 to 10 percent. That is, the absolute capability to destroy one reasonably major U.S. city may be perfectly good political deterrence against the United States, because in our present political state and frame of mind, one which I think will continue, it may be that no President could be other than deterred by such a threat. In fact, since we have not planned and are not likely to plan an attack on the Soviet Union under foreseeable circumstances, the required level of Soviet deterrence force is really zero, but they do not believe this.

I don't think we fully understand the situation; I think it is most important to try to understand it better, and I think that such understanding might change many aspects of future foreign policy in general, as well as future warfare.

The problem of political restraint on tactics and doctrine is also related to deterrence in relationship to the decapitated escalation ladder. I mean the kind of political restraint we have witnessed in Southeast Asia and which many of you, I imagine, have experienced in actual tactical operations there. There is a tendency to regard this as an imposed irrelevance, an error, as something that we just simply should not be doing to ourselves. I do not agree with this view. I regard these restrictions as intrinsic to the situation, as restrictions and doctrine that are not going to vanish. These changes are going to change the nature of naval warfare. I think this because there is reason to believe that a large portion of naval warfare in the next 30 or 40 years is likely to be relatively small action--small action in the sense that it will not be global, it will not involve all the maritime powers, and, in fact, it need not even involve all of the countries in a local area.

Just consider fighting a naval war in the Mediterranean in which the Soviet Union, for example, was not an overt participant; although some of her satellites, or even countries that nobody was quite comfortable about calling satellites of the Soviet Union, were involved with Soviet assistance. We are not fighting hand-to-hand or within sight, we are fighting with radar; we are fighting with long distance systems, with systems that don't always identify targets perfectly; we are fighting in situations where we never have known absolutely which submarine fired the torpedo; we have no way of knowing which submarine will fire the torpedo or who owns it. We cannot even be sure who owns the airplanes which fire at us, even though we may identify them as *Badgers*, because countries have *Badgers*. In fact, we have come to a state of confusion whereby we cannot even be sure if an F-4 fires on us, who it may be. We are in a state in which we may not know precisely the politico-military scenario; where naval battles are likely to be fought in a situation in which there are neutrals who want to be regarded as neutrals, but become involved in a naval battle, as they have been occasionally in Vietnam; in which some of the adjacent countries are participants, some are not; where we want to restrict our actions because we do not want these countries to become participants; where we do not want to kill the innocent, and yet we do not want to take action to keep the innocent out by force, because this might adversely change the character of the war. I think this is intrinsic to this type of situation.

Furthermore, in peacetime maneuvers with navies we are doing what amounts to inadvertent combined exercises with the Soviets and with other navies, so that we do not even have the freedom to manipulate a naval force in absence of other naval forces and to hold completely independent operations. These difficulties are intrinsic and will perhaps have the greatest effect on what we can or what we must do with technology.

Partly for this reason I think we must and will move in the direction of a completely integrated Navy-wide and national global command control and communication system in which there will exist a complete surveillance system. In order to make any sense out of the kind of political-military situation outlined above, the fleet commander or the unit commander has to have full knowledge of his political and operational environment. He must know where all the other forces are, and not only the purposeful forces, but the inadvertent actors, the merchant ships and fishermen. He must know where they are and who they are. His actions must either be directed by central authority, based on information available globally to the central authority, or, possibly more sensibly, he must at least have available to him, on the local scene, the information which can only be available through central authority because it comes in from global information sources. The commander on the scene really needs to know the worldwide political and military situation before he can decide whether he wants to take the risk of firing at the unidentified submarine, because there may be information elsewhere in the world that would help him with the identification of that submarine. Not only is 'hard' sensor generated information, like surveillance information, important, but also 'soft' information, such as political intelligence that says that Party C, who is not involved in the battle between Parties A and B, probably sent a submarine there to keep track of what was going on, and the last thing in the world we want to do now is to involve Party C; therefore, prudence is required. The world has gotten complicated militarily in a way which we are just beginning to comprehend. This requires massive changes in technological requirements.

One obvious increasingly important technological requirement is IFF—the ability to identify at least your friends completely, unambiguously, and continuously. IFF has always been a hard problem; it has not gotten any easier with time. I think we are beginning to have some handles on it, at least for aircraft, possibly for ships.

That brings me to the question of the use of the electromagnetic environment, which I mentioned earlier as one of the great things that has happened in the past 30 years--the ability to see and to talk continuously. I calculate that we are now in the third era of the use of the electromagnetic environment; the future holds the fourth era. I count the zero era as being the time in which we had no radar and no electromagnetic communications. Then there was a period in which there was a massive qualitative change. There was radar and we could talk on the radio, but there were no artificial countermeasures. The second stage was the halting invention of countermeasures. The third stage has been the halting invention of counter-countermeasures--the things that will make you immune to what the other guys do. I think that what will have to happen next amounts to complete management of what happens electromagnetically, at least locally and possibly more than locally. We can no longer distinguish between the measures, the countermeasures, and the counter-countermeasures. We will have to be able to play the whole electromagnetic environment continuously as a single systematic operation.

I think we will have to move into an era where ships and aircraft do not just radiate when convenient or as they please, but they will radiate only as part of a symphony of radiation which is thought out and operated in a sensible and coherent manner.

This is more than just the ability to turn everything off. There are lots of situations in which (a) you cannot turn everything off and in which (b) you don't want to turn everything off; and you really want to use the electromagnetic spectrum. We may get into an era where, instead of being quiet except when you want to talk, you are always noisy, except sometimes you are noisy in a significant way, and sometimes you are noisy in a way which is not significant. We may also need to go to an era in which those who are noisy are not always in the same place, in which the use of electromagnetic radiation jumps around the task group, around the fleet, in a way which is confusing. We may have to fake, to make sure there are plenty of things that are radiating and look perfectly sensible, that have nothing to do with the fleet or are even someplace else. This is going to be complicated.

I want to comment a little more on the system idea. I think we have come into an era in which the system idea, which I earlier classified as one of the great inventions, has now gone mad in the sense that we have optimized and systematized and tied everything together so well that if the system is not quite right for the operational situation, we are faced with serious trouble. We cannot change it because it is all tied together so well we cannot pull one piece out without disrupting the whole thing. I said this to a group of technologists the other day, and they said, "Well, that's just bad system design," to which my reaction is that we have not designed any other kind.

The difficulty is that we are not designing systems to include the time dimension. We are designing a system to solve a problem at a particular time, whereas we should be designing a system with the idea of change with time built into it intrinsically. We should not be physically building the avionics into the airplanes, wiring it in and riveting it in, so that we must take the aircraft to a rework facility in order to change anything in the airplane. We should be able to change the avionics of the aircraft just as we change the weapon loading of the aircraft to suit the next day's tactics, and just about as easily. If the airframe is made for it, we should be able to change it from fighter-configured avionics and weapons, to attack-configured avionics and weapons. We should be able to change to any attack avionics system that the airframe can carry just as easily as we can hang the weapons on it. One of the significances of solid-state electronics and digital techniques is that they will permit us to do this.

I am afraid, however, that we will not do this. I am afraid that what we will do is take advantage of the space and weight improvements of solid-state electronics by packing in more electronics rather than making it interchangeable. We will again fill the airplanes with systems that will be so well tied together that we cannot change them. However, building avionics that can be changed easily could mean flexibility, the ability to make modular changes in tactics and weapons. The use of more unified solid-state components to accomplish this could mean a major improvement in reliability--up several orders of magnitude. The electronics can get to the point where, from the point of view of the people who have to operate with it, it is effectively as inert as the metal in the airframe. We would then have to maintain the electronics not at 35 flight hours, but on whatever cycle we use to worry about wing spars.

There is an important additional point about weapons--the air-to-ground weapons business, particularly, is now splitting. It used to be that all weapons were dropped using sensors in the airplane. They were all what I call "area weapons," that is, you can hit a football field with them. We have continued improving that accuracy, and we are still improving it, and I think that is a waste of time. We can go down that line for another decade, and, instead of being able to hit a football field, we will be able to hit half a football field, and the cost will have gone up fantastically. Fortunately we have split, and we have now gone to putting the brains into some of the weapons using this idea of extensors; that is separating the sensor from the operator so that the eyes or the ears or the radar are in the weapon. The weapon tells the operator where it is and what it is doing, and the operator controls it so that the weapon is worked by the man all the way into the target. By this means one can get terminal accuracy to split the goalposts instead of just hitting the football field.

This changes air attack, and it changes it particularly for situations like insurgencies. This was best expressed to me by a colleague when he said, "The ideal for air attack in a counterinsurgency situation is to be able to kill the mortar operator in the schoolyard without breaking the windows in the school," and I think we may be on the edge of doing this. It would make a great difference between just crushing at random and being able to destroy selectively.

I would like to make one last point which I think is of general importance--that being the idea of mobile basing on a vastly larger scale. It has become clear

BIOGRAPHIC SUMMARY

The Honorable Robert A. Frosch, Assistant Secretary of the Navy for Research and Development, holds a B.A., an M.A., and a Ph.D. from Columbia University. From 1951 to 1963 he was a Research Physicist, Assistant Director (Theoretical Division), Associate Director and Director at Hudson Laboratories. He was Director for Nuclear Test Detection, Advanced Research Projects Agency, Office of the Secretary of Defense, from 1965 to 1966. In 1966 he became Assistant Secretary of the Navy for Research and Development and Chairman, Interagency Committee on Oceanography of the Federal Council for Science and Technology.

Mr. Frosch is the author of numerous articles which have appeared in many scientific publications.

that the politics and economics of overseas bases are beginning to be very difficult. Their expenses are politically difficult, even if we count just the direct costs. When we count the indirect costs, such as the related economic and military assistance program aid, then overseas bases become even more expensive. There is further evidence that the mere establishment of a base in an area for the purpose of being responsive to an insurgency has had the effect of increasing the probability of the insurgency for which the base was established to cope with. That is the kind of self-defeating result that we can only control by lessening the use of foreign land bases. There are implications here worth noting. First is the ability to move forces rapidly from the United States. Since we cannot do the total logistics for that by air, it further suggests the development of major mobile sea bases. I think that is another important step that will change the Navy in a very profound way in the next 30 years.

I am sure that the lists I have discussed have somewhat the sound of a random selection, but I think they are all tied together, in my mind at least, by the idea of integration and coordination. We have moved into an era of warfare situations where, whether we like it or not, everything affects everything else. The only way in which we can cope with that development effectively is to join it and to integrate our response so that our abilities to fight as a Navy are integrated in the same sense that our problems seem to have integrated themselves.

ψ

The air fleet of an enemy will never get within striking distance of our coast as long as our aircraft carriers are able to carry the preponderance of air power to sea.

Rear Admiral W.A. Moffett, USN:
While Chief of Naval Bureau of Aeronautics,
1922