

**Commentary: *Strategy as a Profession  
in the Future Security Environment***

**Andrew W. Marshall**

Revised and updated version of Marshall's essay, "Strategy as a Profession for Future Generations," in Marshall, J. J. Martin and Henry S. Rowen, eds., *On Not Confusing Ourselves: Essays on National Security Strategy in Honor of Albert and Roberta Wohlstetter*, Boulder, CO: Westview Press, 1991, pp. 302-311.

The future is always full of uncertainties. A common error is to underestimate the scale and multiplicity of the uncertainties. This is a general failing that Nassim Taleb in his book, *The Black Swan*, explores in detail.<sup>1</sup> Here we are concerned with the national security area. In this case, as elsewhere, some aspects of the future are more predictable than others, and good assessments and strategies take whatever advantage they can of this. Demographic trends, relative rates of economic growth are some examples of relatively more predictable aspects of the future. Also cultural beliefs in different societies are more stable than other aspects of the future.

But big changes are also common, indeed major shocks can occur, and tend to be under-represented in forecasts of the future not only for the reasons that psychologists tell us about, but in the national security area because of the pressures of political correctness. Some topics, some future scenarios, may tend to be avoided, almost as taboo for a variety of reasons.

We need a strategy, or strategies, that both takes account of our best assessment of the competition we are involved in, now and in the future, and in some way takes account of the uncertainties of the future situation. As I will address below, Albert Wohlstetter was especially adept in his strategic thinking, particularly on this score. And Roberta Wohlstetter in her book on Pearl Harbor stresses the inevitable uncertainty of the future. We will never know, ahead of time, the future. I have found it useful to think in terms of the following model: there are the players, all with their individual goals, resources, distinctive culture, and strategies; and there is the context, which none of the players controls, for example, technology, climate, etc. There are long-term trends in many of these variables, and enduring asymmetries between the

players. A good strategy would have to accommodate in some way all of this, reflect the trends that are changing the situation, as well as exploit some asymmetry that provides the basis for advantages he has in achieving his goals. Strategies can involve coalitions, and obviously they must address adversaries. And, in some way, they must aim to limit the risks that the uncertainties pose.

Richard Rumelt in his forthcoming book has an excellent characterization of strategies as solutions to solve complex problems. One of the virtues of Rumelt's discussion is that it provides real clarity about how the word *strategy* should be used. In practice, the word strategy tends to be used in too many ways. In particular I would note that in the national security area, which is the main focus here, there is a constant tendency to think of military strategy as related principally to the application of resources in a possible future war and the general guidance for more detailed planning for specific contingencies. The result is that there is relatively little discussion of strategies for the peacetime management of our military organizations and for the allocation of resources over time so as to develop more efficient, effective, competitive military forces with appropriate doctrines and concepts of operations. Most statements of national security strategy tend to be just long lists of desirable goals with little to say about how these goals might be achieved. Good examples of fully developed national security strategies are thus very few. There is, then, a special problem in the national security area.

Given the existence of nuclear weapons, the highest priority objective for the United States has been deterrence of large-scale war. In this we have been largely successful. Therefore, the strategic management problem in our national security establishment was for a long time the peacetime competition to preserve and indeed enhance in the future our ability to deter the Soviet Union from actions adverse to our interests. Now this definition of our priority objective may need serious amendment as we move into a different world. The discernible aspects of this world are: the rise of Asia and decline of Europe, a long, extended struggle with Islamic extremists, wider proliferation of weapons, including nuclear weapons, and continued rapid scientific and technological changes.

With new problems, new thinking will be required. It is not that the uncertainty is higher. There were lots of uncertainties in the late 1940s and the 1950s, indeed throughout the Cold War.

But there are new players, new options, and the natures of the competitions are different. We will need to be as serious about strategy as we were in the early stages of the Cold War. Finding the right people and organizing the right sorts of teams will be important.

It is clear that some people among us seem more readily able to address issues of strategy, in particular the strategic management of our national security efforts. They have a willingness and a self-confidence to address the larger issues than do others. They appear to bring a very different perspective to the discussion of what our strategy ought to be. How do they get this way? What sort of training is useful? This is what I want to address in the next two sections.

### *What Environments Produce Strategists?*

This is a question that deserves extensive study. The best I can do is to draw upon my experience in and observations of the environment at the RAND Corporation in the 1950s and early 1960s and my later experience in government in the period 1972 to the present. One disadvantage of focusing on RAND as a producer of strategists is that it clearly biases the discussion toward an analysis of the development of people whose role has been "advising," in the sense that Herb Goldhamer used in his book, *The Adviser*.<sup>2</sup> There are other routes to becoming a strategist, including those who reach high positions in the military services or enter government service from other career lines such as the law or investment banking. But the case of RAND is perhaps of special interest because it did provide in the 1950s and early 1960s an environment that produced a number of people who are now acknowledged as major strategic thinkers.

### *The RAND Experience*

There was something special about the RAND environment from the late 1940s through most of the 1960s. For one thing, especially in the late 1940s and the 1950s, there was a sense of being on the leading edge, of dealing with the centrally important problems. The invention of nuclear weapons and several other technology developments at the end of World War II produced a situation that was quite new, one in which the issue of what our strategy should be was extremely important. Another aspect of

this situation, given the large increase in destructive power nuclear weapons introduced, was that there were no experts. Two small weapons had been used at the very end of World War II; what larger numbers of weapons and more powerful weapons might do to change the nature of war was unclear. Nobel prizewinners were no better than graduate students in thinking about the relevant issues, and at meetings and working groups at RAND in the early days there was no hierarchy. This was an ideal situation for younger people (the average age of the professional staff at RAND in 1950 was about twenty-eight), who were immediately treated as equals and valued for what they could contribute to the discussions. This is a rare situation, certainly not characteristic of academia or normal organizations, and it led to the rapid development of individuals who were willing to address the broadest issues of national security. There was also a sense of having a preferred position with respect to access to information on the new developments taking place in weaponry, in particular in the design of nuclear weapons, their delivery systems, and other relevant technology.

Two other things favored the development of strategic thinking and innovation at RAND, and the willingness of the people there to address the highest level national strategy issues. One was the freedom RAND had to select the problems and the issues on which it worked. This is very different from the environment in contract studies organizations, especially now. The other was the presence of several remarkable men who set the intellectual tone and style of much of the broader strategies analysis that began in the early 1950s. Two I would name are Charles Hitch and John Williams, the heads respectively of the Economics and the Mathematics Divisions. Apart from their own intellectual contributions, their cultivation of full-ranging discussion, their intellectual fairness, and their interest in the development of younger people and of new methods of analysis all favored the fullest examination of all issues of U.S. national security.

One of the interesting things that happened at RAND was the success of the economists in assuming a leading role in the direction of a number of important studies and, more generally, in shaping the way in which RAND addressed national security issues. Initially the economists were brought into what had been largely a technological organization to deal with what was called the military worth issue. It had become clear to the technical people that they needed some assistance in thinking about the

objectives that military weapon systems were to achieve. There was also some interest in the economics of defense, especially as it dealt with issues of mobilization, and in the targeting of an opponent's industrial capacity and assessing damage to industrial societies from strategic bombing. The economists soon played a much larger and more central role in managing and directing a number of the successful studies. Why was this?

Herman Kahn and I used to discuss this puzzle. We had a number of hypotheses. For one thing the economics of the situation, broadly conceived, were important. What things cost, the level of resources that nations are able to devote to defense over an extended period—these all shape one's views as to the kinds of weapon systems and forces that are desirable and feasible. But another advantage the economists had was that they knew from their own experience that experts could be wrong. Indeed, they also knew that much discussion of economic problems is foolish and that many widely-held views, even among responsible people, are faulty. The experience of engineers and physicists is different. In those fields there are real experts who are much more likely to be right than are others. Economists, therefore, were more intellectually comfortable in the situation that existed with respect to nuclear warfare, in which there were no experts.

One of the people in the economics department who was the first to lead and manage a large RAND study was Albert Wohlstetter. Beginning in the early 1950s, he examined a set of issues connected with the basing of long-range bombers. I want to note what seems to me one of the major innovations or inventions Albert made in the conduct of that study. In previous large RAND studies, the practice had been to lay out a number of alternative systems or programs at the very beginning of the study. The study itself focused on evaluating which of the alternative systems was the most cost-effective.

Albert's approach was different. He started with a few alternatives to the existing plan or program, but as the study went on he evolved improved alternatives. He was also less rigid than had been reflected in the earlier practice in setting down the criteria, the objective functions, the measures of effectiveness at the beginning of the study, and then simply sticking with them. His evolutionary approach developed additional criteria and tests of performance as more understanding of the problems and the issues emerged. And a wider range of situations within which the alternative possible solutions could be tested grew as the study

went on. This was, in my judgment, a crucial invention for doing these kinds of studies, because one would learn much more about the nature of the issues and the problems, how one ought to look at them, and what criteria were relevant as one went further along in the studies. Also, this way of conducting the analysis had the advantage of inventing additional and better alternative solutions to examine as one went along. Albert's study was in many ways emblematic of the kind of good strategic analysis I wrote about at the beginning of this essay: it accepted certain structural elements of the situations, and then sought measures to both limit and mitigate effects of the uncertainty about the future.<sup>3</sup>

Another aspect of the situation at RAND that was exceptionally favorable to strategic thinking and innovation during the early period was the practice of inviting first-rate people to come and spend the summer. This created an environment in which the important thing was to try to tap into the very best talent in the whole country. The objective was not to do the best that RAND could do with its existing staff, but in a sense to do an analysis that was the best that the country as a whole could accomplish. By its very nature, any organization is limited in the amount and variety of talent, backgrounds, and insights that it can include among its staff. This attitude of searching for the very best people and drawing on the best talent is a key to excellence in broad thinking about any problem or issue. Unfortunately, most organizations do not operate this way.

Another way in which Albert was especially good was in reaching outside Rand to get the best technical advice. In the mid-1950s the experts, at Rand and a DoD advisory group on physical vulnerability, believed that no structures could be built to withstand blast overpressures exceeding something like 25 psi. Albert recruited Paul Weidlinger, an innovative structural engineer, to design hardened structures for protecting aircraft and missiles to withstand overpressures far beyond this limit. Herman Kahn was also involved because of his knowledge of the physics of nuclear weapons effects. This led, after a long argument and tests, to a major shift in views of what was possible.

The RAND of the 1950s and early 1960s was a remarkable place, both for the talent it recruited and for its atmosphere and intellectual dynamic. It was also remarkable for its boldness in addressing broader questions of strategy. It is, therefore, not surprising that some interesting and influential people developed there.

## *The U.S. Government*

The next experience that is perhaps relevant comes from my time in government. Beginning in the middle 1970s, I was involved in attempts to initiate strategic planning activities in the Department of Defense including some strategic planning experiments. In particular, James Roche, then a U.S. Navy Commander, and I wrote several papers during 1975-1976 to promote strategic thinking in the Defense Department. We also sponsored contractor research on some aspects of strategic planning. This experience led me to believe that, while systems analysis had been a liberating force during its early development, by the middle 1970s it had become a constraint on thinking strategically. People who were systems analysts found it difficult to address the sorts of questions that we felt needed to be considered in strategic planning. People with a business background or a combination of business school and military service seemed to be among the best at taking up and addressing the questions we wanted dealt with.

We saw it as a vaccination problem: some backgrounds promoted strategic thinking and others seemed to inoculate people against it. Why is that? To some extent, the systems analysts had by that time developed routine approaches to analysis and perhaps had ceased paying sufficient attention to the complex consequences of acquiring the systems they dealt with. James Schlesinger commented to me a number of years ago that systems analysis proceeds by trivializing the measurement of effectiveness while perfecting the analysis and estimate of costs. Programmatic actions, the acquisition of particular weapon systems, the adoption of a new concept of operations, and the setting of new objectives for military forces have complex consequences, including their effects upon the beliefs, actions, and resource allocation patterns of potential opponents. Most of these consequences are not usually considered in the standard kinds of analysis. One result is that the top leaders of the Department of Defense often get remarkably little assistance from their staffs when truly strategic decisions are addressed. This is because the focus of the work of the staffs, the criteria they use, and their measures of effectiveness are too narrow to account for the considerations that top-level decisionmakers in fact want to consider, are concerned with, and take into account as best they can.

Some decisions have larger and different consequences than others. For example, a decision to pursue or create a major strategic defense capability is different from a choice among several alternative programs for the next generation of fighter aircraft. The former involves going into a new business for the U.S. military (although it is a business we once were in), the latter the continuation of an existing business. Different issues are involved, different forms of analysis seem needed, but existing analysis methods tend to treat the two types of decisions the same way. Part of the problem may be that much if not all of the existing analysis methodology was developed to assist in procurement or operational planning decisions. Other methods of analysis are necessary when the questions are more like: What businesses should I be in? What are my competitive advantages? One advantage people from the business world or business schools may have is that they are used to addressing these kinds of questions, though often with analysis methods that are less systematic.

*What Backgrounds and Experiences Are Conducive to Strategic Thinking?*

There is no specific set of disciplines that must be mastered to be a strategist. People who think strategically come from a number of different backgrounds. Among those whom I have met, and feel that I know personally, the best academic backgrounds seem to be economics, business school, applied technology (especially for those who have been in the business world), and in some cases political science. But what seems to be central is a cast of mind that is questioning, eclectic, able to address the broadest kinds of issues and goals, and able to formulate appropriate ways of achieving these goals. A high tolerance for the uncertainty that necessarily accompanies any effort to think forward five, ten, or twenty years is required. For many people, some period of intense involvement in an important, large-scale project or enterprise has proved to be crucial.

World War II was such an experience for a number of people and, indeed, there may be a generational factor at work: living in interesting times may contribute to being a good strategist. People who were involved—even if only in staff positions or on the peripheries—in some major decisionmaking body connected with that war had a special quality about them. Experiences in

World War II clearly had a significant impact on a number of the people who were at RAND during the 1950s. Because they contained many people with World War II experience the Truman and Eisenhower administrations had a character to them that favored strategic thinking. This characteristic of administrations has gradually eroded since the late 1950s.

The changes that we now see in the security environment of the United States are forcing another major effort of rethinking our situation, our goals, and our strategies. It might, therefore, be a period in which a new generation of strategic thinkers will emerge as a result of the critical experiences they will go through in the next decade.

Turning to the question of what kind of academic study or professional training might be useful, I would start with economics and business school training, especially business schools that have strong programs in business policy and strategy. My recommendation about economics is, however, a guarded one. Since the 1940s and 1950s, economics training has become too mathematical, too focused on the acquisition of particular analytic tools that are not, in fact, of much use in the national security area. Something like the first courses in graduate school may be enough. They are important, however, because people who do not have a sense of macroeconomics and the fundamental tradeoffs that societies have to make, find it difficult to think clearly about the long-term implications of devoting large, possibly excessive, percentages of gross national product (GNP) to military uses.

In the early 1980s, when the first initiatives were taken within the Defense Department to encourage application of a set of ideas that later were labeled as competitive strategies, I had a discussion with the chief of one of the military services. His reaction to the idea of designing some military programs so as to impose increased costs upon the Soviets was negative, or at least cautious. He had two arguments against focusing on increasing Soviet costs or expenditures.

The first was that the Soviets would simply spend the extra money, there being no reasons for them not to do so; the second was that our own budgets fluctuate so much that it was unwise to stimulate a competition which we ourselves might not sustain. The second of these arguments has real merit to it. The first shows an unawareness of the long-term consequences for the Soviets of high levels of military expenditures or of possible tradeoffs between individual programs the Soviets might be compelled to make, since resources always are limited.

Another virtue of economics training, or for that matter business school training, is that a modest amount of mathematics is acquired, as is some sense of the importance of technology and an ability to interact more effectively with technologists and hard scientists. This was one of the advantages the economists had over the political scientists at RAND in the early 1950s: quantitative analysis was something the economists were used to, and their interest in or ability to discuss and understand what the technologists were up to was somewhat better than that of the political scientists.

Demography is another area that deserves much more attention than it has had in the past in the development of strategy. The relationship of demography to political and military behavior is likely to be an area of increased importance and attention. Demography is often brought into discussions of strategy and broad national policy, but only in the most obvious and limited ways. William McNeill a few years ago wrote a small volume addressing some of the broader relationships of demography to political behavior.<sup>4</sup> As in other of his works, he provides a number of hypotheses and sketches out areas that deserve considerably more attention.

Additional fields of interest are cultural anthropology, ethnology, and some areas of psychology. In some ways a new understanding of man is emerging, based on study of the evolution of man and human society and on new analyses of the biology of man, in particular the functioning of the brain. How men process information, make decisions, and behave are central issues on which much new knowledge exists and more will be available in the future.

But above all, if I had a suggestion to make, it would be that people study, in any case at least read, history of all kinds: military history, of course, but also economic and technological history. The history or analysis of past wars is a major antidote to the narrow focus of many existing methods of analysis of defense issues. Most discussion of strategy and defense programs is, if anything, too focused on technology and weaponry and not enough on the other factors that often dominate actual warfare. Also, if one considers the extended competition between states such as Rome and Carthage, the issue of why the Romans won in the end may shed interesting light on the key variables that need to be considered in our conceptions of strategy.

Another factor of great importance is to understand the differences in the ways in which other nations are likely to perceive situations and react to them. Specialized studies of the strategic cultures of Russia, China, India, Japan, Iran, and the European nations and many others are of great use. Some of this can be gained by reading the history of these nations, especially the development of their military and other national security organizations. Other aspects relate to the particular cultural characteristics of these societies.

### *The Future of Strategy*

We are at a major turning point in history. Uncertainty about what the future competitive environment will be like is especially pronounced. There are at least three major issues that our defense or national security strategy must deal with. There is the problem of radical Islam, which both poses an immediate threat and has the potential to be a long-running problem. Any serious strategy dealing with this problem will have to have a substantial nonmilitary component. A second issue is the potential emergence of a strong hostile China. A major problem of strategy here is setting and articulating in some definitive way the goals for the U.S., or a picture of what, ideally, we would like to see Asia as a region look like in 20 or 30 years. The third major strategic issue, I believe, is the likely proliferation of WMD (particularly nuclear weapons) and long-range strike systems. We can of course try to prevent proliferation, but any realistic strategy must take account of the possibility that these efforts will fail and that the future world will have many more nuclear powers, some of whom would employ weapons in ways very different from how we have tended to focus on.

Of course, a defense or national security strategy for the long term must deal with all of these problems. It must attempt to shape the future security environment where possible, and develop hedges against the emergence of particular threats or problems. There is also pronounced uncertainty about the character of future warfare: new kinds of weapons systems are being developed, which in turn will require the development of new doctrines, new concepts of operations, and new kinds of military organizations to exploit fully the new technologies. What our strategy should be for the more complex competition that is emerging will require consideration of many aspects of the changing security

environment and changing technology. We will need to know much more than we now do about the emerging regional powers, as well as about the likely major actors, their strategic orientation, their strengths, and their weaknesses.

It is hoped that new centers of strategic thought and innovation will arise and a new generation of strategists and military innovators will develop to deal with these problems.

#### **ENDNOTES - Marshall**

1. Nassim Nicholas Taleb, *The Black Swan: The Impact of the Highly Improbable*, New York: Random House, 2007.

2. Herbert Goldhamer, *The Advisers*, New York, NY: Elsevier, 1978.

3. "Theory and Opposed-Systems Design" (1968), Wohlstetter's essay on the theory and design of competitive systems in an earlier part of the present volume, reflects this approach.

4. William H. McNeill, *Population and Politics Since 1750*, Charlottesville, VA: University Press of Virginia, 1990.