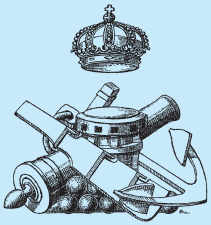
Prediction

By Wayne P. Hughes, Jr.



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Prediction[1]

This paper is an adaptation of a presentation to an American audience that has been edited for the Swedish armed forces. It describes the value and limitations of prediction for military applications, first by giving examples of failures when prediction is based solely on information, followed by a demonstration that even a modest amount of quantitative analysis with incomplete information can help to execute a military campaign. Analyses cannot eliminate wartime surprises but they can help to avoid the worst mistakes and steer military leaders toward better decisions.

The predictive power of models, the role of operations analysis, and the value of information are three big, interwoven subjects that are hard to winnow down to some essence. Prediction is a big subject, so I have limited these remarks to what I know best—the operational and tactical domains of conventional war. My purpose is as much to stimulate reflection in the Swedish armed forces as to impart new wisdom. I'm going to conclude by describing what is too rarely done: compare quantitative campaign analysis done before a war, three wars in fact, with what actually transpired to show that useful—even critical—advice can be formulated very quickly to help decision makers. On one hand intense thinking about the war at hand is as important as a quantitative assessment. On the other hand, it isn't expert judgment that augments professional experience but some transparent, timely—but incomplete—quantitative analysis.

1. Adapted and extended from an address to the Military Applications Society of INFORMS delivered in Monterey CA on 27 March 2012.

Prediction from information only

Black Swans

Surely the most drastic book on prediction is N. N. Taleb's *The Black Swan*, subtitled *The Impact of the Highly Improbable*. Taleb makes an entertaining case for the existence of unforeseeable events, but his advice is pretty trite: since by definition a black swan cannot be predicted, the most we can do is be ready for

surprises, and responsive and adaptive when they occur.[2]

Grey Swans

More interesting are what might be called Grey Swans: surprising events of great consequence for which there was *ex post facto* evidence that got lost in a clutter of misinformation. In the commercial sector the recent burst of the housing bubble is the latest of many collapses brought on by "the madness of crowds" whose herd instinct overcame many clues of excesses in plain sight.[3] Grey swans in the military domain are exemplified by the invasion of South Korea in 1950, the collapse of the Soviet Union in 1989, and the invasion of Kuwait by Iraq in 1990. All three illustrate "surprises" that Monday morning quarterbacks have decried. After Pearl Harbor was alternatively described as a nefarious plot or the careless handling of information, Roberta Wohlstetter wrote what is to me the definitive interpretation in *Pearl Harbor: Warning and Decision*.[4] At the intellectual level she shows that it was easy to miss the clues of what turned into a tactical disaster amidst information overload. At the emotional level one must see the need to hedge against human shortcomings in anticipating future conflict while being careful not to cry wolf too often.

Grey swans are complicated by the fact that an enemy frequently will use deception to help create them. The successful Japanese attack was partly due to the employment of deception to achieve surprise. I will refer later to Barton Whaley's study of strategic deception, but here I will mention another good source of understanding, which is a recent book by Erik Dahl, *Intelligence and Surprise Attack: Failure and Success from Pearl Harbor to 9/11 and Beyond*.[5] Dahl is particularly insightful because he goes beyond deception in big wars and includes deception that terrorists have used to achieve surprise.

Expert Political Judgment

What, then, about predictions by experts? There is a marvelous book by Philip E. Tetlock who found 284 self-proclaimed authorities who made a living commenting on political, international, or economic trends and were willing to participate in his study. The questions were the kind that could be answered "Better," "Worse," or "About the Same." Over several years in the 1990's Tetlock accumulated 82 361 answers in his data base. In 2003 Tetlock compared the predictions with actual results. Two years later he published his conclusions in a book entitled *Expert Political Judgment*.[6]

And the envelope, please. Well, it's a fat envelope because Tetlock's findings give all the interested parties a nuanced hearing. To keep this brief, I quote from a *New Yorker* book review: "[t]he experts performed worse than they would have if they had simply assigned an equal probability to all three outcomes. . . Human beings who spend their lives studying the state of the world are poorer forecasters

2. Nassim Nicholas Taleb, Penguin Books, 2007.

3. I am referring, of course, to the classic book on economic bubbles, written by the Scotsman Charles Mackay and published in 1841: *Extraordinary Popular Delusions and the Madness of Crowds*.

4. Wohlsetter, Stanford U. Press, 1962.

5. Dahl, Georgetown U. Press, 2013.

6. Philip E. Tetlock, *Expert Political Judgment: How good is it? How can we know?* Princeton NJ, Princeton U. Press, 1995.

than a dart-throwing monkey."[7] Worse still, the experts tried to defend their wrong predictions with excuses like, "My timing was off," or "An unforeseeable event interfered with what should have happened." Tetlock also shows that non-experts who answered the same questions did better than the dart-throwing monkey. Not a lot better, but significantly more so than the experts.

Information supplemented by analysis

These are examples of predictability based on information only. Tetlock's Expert Judgment is full of statistics measuring the performance of experts, but as far as I know the experts did not do any analysis to supplement their opinions. So let us next make a distinction between information-based prediction alone and decision making that is assisted by quantitative assessment.

Why Military Analysis Cannot Predict

As we begin the shift to military operations analysis, I refer to an essay by the late, great Air Force analyst, Clayton Thomas.[8] In effect, he described model-based analysis as an IF-THEN statement. Two things, the model and its inputs, are on the IF side. Model and inputs together are processed to yield a result, the THEN side. If the model represented reality—which in campaign analysis it cannot—and if the data was precise—and in warfare the data is always "dirty" with errors—then the result would be an accurate prediction. We military analysts make no such claims. We say no more for the results than that when operations analysts use quantitative methods wisely insightful conclusions can be reached and better decisions made.

Prediction is sometimes unavoidable

Although generally we don't claim to predict combat outcomes, sometimes a decision maker must, in effect, do just that, and we operations analysts must help him. A prominent example is procurement of warships that are anticipated to have 30 or even 40-year service lives. To see the impossibility of getting the designs right, however detailed and comprehensive the research may be, reflect on the state of the world in 1979 and all that has changed since then to affect their prospective performance today.

Space permitting I could write at length, first, about how U. S. Navy ships completed before 1979 were designed much earlier with yet earlier technologies; second, that expensive, multi-purpose ships are a poor way to hedge against future grey swans; and third, that both the American and Swedish navies are fortunate not to have been in a sea battle since 1945. Happily all our learning about war at sea in the missile age has been vicarious, except for a handful of embarrassing single ship attacks my navy has suffered. The degree of success at prediction is in part measured by the decision maker's expectations. Analytical methods and predictive power vary with tactics, technologies, tests, and whether the predictions concern policy, operations, logistics, procurements, or strategies. A fine book on the subject is MORS' *Military Modeling for Decision Making* because it is comprehensive in

7. Louis Menand, "Everybody's An Expert," *New Yorker*, December 5, 2005, pp. 98-100.
8. Chapter 13, "Verification Revisited" in *Military Modeling for Decision Making*, MORS 1997, edited by Wayne P. Hughes, Jr.

distinguishing modeling and techniques for different defense-related purposes.[9]

Strategic Planning and Force Procurement

An accurate, recent, 38-page appraisal of predictive power when aided by extensive, even exhaustive, analysis was published in October 2011 by the American statesman, Richard Dantzig.[10] Quoting liberally from both Taleb and Tetlock, Dantzig shows the limits of model-assisted planning and why the limits have been inevitable when programming weapon systems for the future. His cure is difficult to implement, however, arguing Black Swan fashion for more nimble DoD and Congressional processes and acceptance of something less than the perfection demanded by those in government.

In one respect Mr Dantzig's advice seems implementable. It is to work on simpler systems that can be designed and produced more quickly and be discarded after shorter lifetimes when geopolitical circumstances change, or when new technologies serve up either threats or opportunities. Although Dantzig does not say it this way, the clear implication is that top down solutions are unavoidable when expensive, long-lived systems must fill capability niches and predictably will endure for the long haul, for example multi-function orbiting satellites, or ballistic- missile-carrying submarines with well-designed, "failure proof" nuclear weapons for strategic deterrence. Otherwise bottom up, quickly implementable, relatively inexpensive systems that fill immediate needs by short-circuiting the procurement bureaucracy are the best way to recover from failures of prediction in national strategic planning. An example is the recent, rapid development of unmanned vehicles in both quantity and design variations. Falling somewhere in between were the successes at Kelly Johnson's Lockheed Skunk Works, which responded quickly to fill a need for long range manned aircraft perceived at the highest levels of the CIA.

Strategic Analysis in Wartime

Barton Whaley's *Strategem* is a good, quantitative book on methods of deception to achieve strategic surprise, how many false clues it takes to achieve it, how to enhance your chances of success, and why attempts to deceive haven't cost much in resources.[11] He gives historical examples, like the strategic surprise the Germans achieved in 1941 when they invaded the Soviet Union, and the Allies achieved in the Normandy Invasion. Whaley tells the deceiver how to succeed and the rewards that ensue. He shows that the victims of strategic deception behave much like Tetlock's experts, who were blinded by their own overconfidence.

Tactics, Technology, and Testing

Carefully measured performance of weapons in peacetime exercises get caught up in the fog of war when the shooting starts. Jon Sumida observes that the Royal Navy

9. Wayne P. Hughes, Jr., ed., Third Edition, Alexandria, VA, Military Operations Research Society, 1997. Now 20 years old, the book has stood the test of time.

10. Richard Dantzig, *Driving in the Dark: Ten Propositions About Prediction and National Security,* Center for a New American Security, 2011.

11. Barton Whaley, *Strategem: Surprise in War*, Norwood MA, Artech House, originally written 1969, published 2007. A valuable companion also rich in quantitative and qualitative analysis is Donald Daniel and Katherine Herbig, *Strategic Military Deception*, New York, Pergamon Press, 1982.

expected a hitting rate of 30 % with the fleet's big guns.[12] At the Battle of Jutland the Germans achieved a rate of about 4% and the British 3,5 %.[13] There were perfectly good reasons for this, but that is the point about prediction: there are always going to be *ex post facto* reasons your peacetime expectations will be wrong. The Englishman David Rowland has devoted much of his career to comparing ground combat exercise data with wartime data from similar battles. In one of his early papers he compared laser-instrumented, non-lethal experimental results with deadly combat results for similar environments in World War II. The experimental results overestimated the casualty production rate for tanks by a factor of two; for artillery duels by a factor of three; and for pure infantry actions by a factor of seven. Yes, a sevenfold overestimation of soldier performance. In effect, Rowland confirms S. L. A. Marshall's highly controversial conclusions about the small number of American soldiers who fired their weapons when under fire in World War II.

One of the most famous model-based predictions—I think predictions is the appropriate word—was by Frederick W. Lanchester who claimed the square law phenomenon would apply to air-to-air combat.[14] He wished to show the advantage of numbers over quality in a new age of air warfare. But Lanchester was wrong. From evidence reported by Philip Morse and George Kimball in their famous *Methods of Operations Research* and in more detailed recent analysis by Niall Mackay,[15] we know that through World War II the linear law applied in the air.[16] What Lanchester failed to see was that air combat is essentially dogfights and ambushes. The square law assumptions aren't met. This was no theoretical matter. As Mackay shows, the top air campaign leaders in the Battle of Britain argued between massing defending fighters Lanchester square law fashion, or getting the fighters in the air early to be in the best position to win duels between single aircraft. As far as I can tell, the linear law still applies today.

But I also tell our students of campaign analysis that the greatest number of kills doesn't come from air-to-air combat. If they want to anticipate—predict as it were—which side will achieve air superiority then they must make a difficult estimate of how successful each side's attempt will be to attack aircraft on the ground, the way the Japanese surprised and destroyed MacArthur's air force in the Philippines immediately after Pearl Harbor.

12. Jon T. Sumida, "A Matter of Timing: The Royal Navy and the Tactics of Decisive Battle, 1912-1916," in *The Journal of Military History*, Vol. 67, No. 1, January 2003, pp. 106-107.

13. John Campbell, *Jutland: An Analysis of the Fighting,* Annapolis MD, Naval Institute Press, 1986, pp. 346-355.

14. Frederick W. Lanchester introduced his theory in 1916 in *Aircraft in Warfare*. A more respectable and durable essay on the power of concentration treated more broadly is his "Mathematics in Warfare." See James R. Newman, *The World of Mathematics*, New York, Simon and Schuster, 1956, pp. 2138-2157.

15. For example, see Mackay, "Is Air Combat Lanchestrian?" in MORS' *Phalanx*, Vol. 44, No. 4, December 2011.

16. My Navy heritage demands that I say the square law was appropriate for battles at sea in the battleship era because square law conditions were met. Also, that two U. S. Navy officers, J. V. Chase and Bradley Fiske, invented the square law to describe the advantage of numbers quantitatively and they did so a decade before Lanchester, who never saw their work.

Lest you think we are better off now with modern computers and powerful algorithms built into our best models, here is a more recent example. The U. S. Navy depends mightily for defense of the fleet on the Aegis missile system. Using data from controlled experiments at sea, one may calculate that if you shoot two missiles at an incoming missile and they are operationally and statistically independent of each other, and if you also add some point defense, you can expect to shoot down 90 % or more of the attacking anti-ship cruise missiles (ASCMs). What is the combat record? In battles at sea, warships of other states have averaged around 75 % success in defending themselves from ASCMs. On the other hand, all of their success must be attributed to soft kill and point defense weapons, not to surface-to-air missiles (SAMs). There are also several instances of warships that might have defended themselves but did not, illustrated by the recent successful missile attack on the Israeli warship *Hanit*. Navy analysts will also remember the Exocet hits on the defendable USS *Stark* and HMS *Sheffield*. In the entire record of more than 220 missiles fired on ships at sea starting in 1967, only one anti-ship missile has been shot down by a SAM. The record of U. S. Navy missile ships in combat is zero for two, if one counts the action of USS *Vincennes* in shooting down an Iranian airliner as a failure. As at Jutland, a careful examination of these missile era events shows there were reasons for the wartime results—pretty good reasons, too—but the important conclusion is that the fog of war almost always makes peacetime predictions too optimistic. Wartime surprises, though not exactly Black Swans, will always be present.

Our Product is Useful Insights

Now I am going to focus the lens on the domain of military grey swans when our tools are used for operational and tactical predictions. I hope to show that even though the predictive power of our analyses is less than we would wish, if we are suitably modest, do our work with the right objectives, and use appropriate measures of effectiveness, then our results and recommendations will be a powerful aid to decision makers. Indeed, I am going to arrive at conclusions so cheerful they may surprise you.

Campaign Analysis

Campaign analysis is hard to do, and its predictive power is very much a matter of how demanding you want to be. For example, between the World Wars, the Naval War College played over 300 games, most at the campaign level and most against Japan. They were highly valuable by sobering our early optimism about its most important elements. After the war, Admiral Chester Nimitz wrote a famous letter saying except for kamikazes the games had accurately anticipated its major events, meaning I suppose, what happened in the drive through the Central Pacific that he oversaw. On the other hand, the Guadalcanal campaign, the shift from a battleship centric force to a carrier centric force, the vital contribution of American code breaking, and the drive by MacArthur up the New Guinea coast were vital aspects about which the games afforded no clues. In fact, after Pearl Harbor *every* class of warship changed its function—every class except mine sweepers.

At the tactical level even the *post mortems* do not do justice to two factors that

modern operations analysis might have revealed. After the Battle of Midway in June 1942, historians recognized four things that were necessary for the Americans to overcome a numerical inferiority of 75 ships to 25: code breaking; brilliant leadership by Nimitz, Fletcher, and Spruance; great courage in our naval aviators; and just plain good luck. But they missed two others. Until recently no historian had picked up on the value of radar. If the Japanese ships had had our air search radar then our surprise dive bomber attack could not have succeeded.[17] Nor has *any* historian I have read identified the key role of Midway Island, which served as an immobile fourth American aircraft carrier, drawing away Admiral Nagumo's attention and firepower at the critical time.[18]

And yet, and yet . . . war games and fleet exercises schooled our carrier commanders before the Pacific war to know the best way to win—and the only way to win when outnumbered—was to detect the enemy first and get off a decisive first strike with every aircraft you had. Simple but elegant salvo equations, not yet invented in 1942, would later match the results and "predict" with sufficient quantitative accuracy the outcomes of all five of the big carrier battles in the Pacific e*x post facto*.[19]

Having in mind, then, that both Admiral Nimitz and the Midway historians ought to be given some slack, I will now describe three remarkable examples of the power and utility of our methods applied to campaigns, to show how analyses can help military leaders make better decisions and avoid the worst blunders. The examples are entertaining because they were performed by the young officer students in a course on joint campaign analysis at the U. S. Naval Postgraduate School. The students had to reach their conclusions very quickly, with maximum professional knowledge and minimum computation, because the class pretense—a realistic one—was that their decision maker needed their inputs within about 72 hours. In these ministudies, the students did not have time to construct a "realistic" detailed simulation.

Foresight and Hindsight in Wars

The Falkland War

In the first example, the students fought the Falklands war on paper in 1982 before it started in fact. They had no inkling the *General Belgrano* was about to be sunk, taking the Argentine surface navy out of the war; or that Exocet missiles would be highly effective in destroying British ships; or that the Argentine ground forces would be thoroughly outclassed. To do justice to their insightful work would take several paragraphs, but I can report

17. The Japanese had 41 fighters in the air to protect the carriers, but they were at low altitude after shooting down the American torpedo bombers that served as an inadvertent but critical decoy.

18. I believe the journalist Hector Bywater's book, *The Great Pacific War, 1931-33*, written in 1926, was nearly as good in predicting how the war would transpire as were all the Naval War College games. Bywater's description of the battles in a Pacific campaign had sound predictions that even our many NWC war games did not foresee.

19. See Hughes, *Fleet Tactics Theory and Practice*, Annapolis MD, Naval Institute Press, pp. 93-103 and also the descriptions in the second and third editions.

the bottom line very quickly. Neither side that fought had done such an analysis: early, fast, and basic. I believed then and still do that if they had, the Argentine Junta would have won the war, and Prime Minister Thatcher would have been more cautious about sailing 42 ships, essentially committing the U. K. to take back the Falklands. Why so? Because the students' fast, focused campaign analysis showed the Argentine air force, all 140 fighter attack-aircraft flown by capable pilots, could have with only a little foresight staged through Stanley airfield on East Falkland Island.

It doesn't take a detailed model or precise inputs to conclude that those aircraft, even when dropping iron bombs the old fashioned way, would have penetrated the mere 22 Harriers and other modest air defenses and put enough British ships out of action to force the fleet to abandon the invasion.

Desert Shield

While my class was meeting in the fall of 1990 a big debate was raging over whether the U. S. and our Middle East partners could force Saddam Hussein out of Kuwait without a ground campaign. At the time many American Congressmen and pundits were arguing that this was possible. The charge to my students was to do a fast-turn-around mini-study to determine whether there was a campaign other than an invasion of Kuwait that would persuade Saddam Hussein to leave. After doing as much quantitative assessment as time permitted, the students concluded that if we wanted him out we must attack on the ground. This seems obvious in hindsight now, but it was not so at the time when the students made their appraisal.

Operation Iraqi Freedom (OIF)

Lastly I report on OIF. This student appraisal was done even faster than a ministudy, over a single weekend. We asked the four student teams how long it would take to win the war. Astutely, they asked "What do you mean by win the war?" Together we agreed that getting to Baghdad and toppling Saddam Hussein would constitute victory. I still think that was a suitably specific analysis goal, because everything after that comprised peacemaking operations, long and difficult though they turned out to be! Each team arrived at an independent estimate. One team said it would take four weeks, one team said two to four weeks, and one team said two weeks to get there but we don't know how long the city fighting will last. And the fourth team said three weeks. As you know, it took three weeks and a day to overthrow the regime. What our students could not predict, of course, was a sand storm that slowed the advance, and the remarkable luck and courage by some elite soldiers operating inside Baghdad. But the students had some crib notes to help them. They knew that research, most notably by the Army analyst Bob Helmbold, had concluded that the rate of advance of an army unopposed or against light opposition has been and still is about 25 miles a day. The students could scale back the movement rate appropriately in making their estimates—predictions, as it were. In actuality, our soldiers and Marines advanced the 300 miles to Baghdad in three weeks against moderate Iraqi opposition, which is an advance of 15 miles per day.

Other Domains

I have emphasized the rewards and limitations of operational and tactical analysis to prepare for war. There is a lot

more to the story. Before summing up, here is a brief contrast with two other different domains of prediction.

Attenuating Terrorist Attacks

I am not well informed on what kind of analysis would best supplement experience in fighting the perpetual war against terrorists. But I have read a fine paper entitled "How Probabilistic Risk Assessment [PRA] Can Mislead Terrorism Risk Analysis." It is a warning against a methodology that cannot help and might hinder prediction and planning for homeland defense.[20] Authors Jerry Brown and Tony Cox see two problems with the methodology. One is the folly of putting confidence in the predictions of experts that are *inputs* to the analytical scheme. They are as suspicious of expert opinion as am I. The other problem is adapting a methodology—Probabilistic Risk Assessment (PRA) —that has been effective for engineering analysis but is essentially a decision theory way to design against natural events rather than a thinking opponent. The authors point out that when the enemy is not nature but a ruthless attacker who wants to outwit us and penetrate our defenses, then "PRA" can actually help the enemy. The proper mindset is game theory which says we must do the best we can against the best he can do. The PRA methodology comes no closer to examining enemy choices and capabilities than to ask an expert the "probability of an attack" without regard for what the enemy observes us doing.

In the U. S. there is general agreement that an attack against our homeland will come someday. Predicting where and against what is the hard part that the PRA method intends to illuminate, but it cannot because the enemy has his own standards of risk versus reward. Brown and Cox "recommend shifting the emphasis of risk management from using experts to guess where risk might be greatest . . . to calculating where targeted investments will most improve the resilience of critical infrastructures." This entails attention to two things: First, discerning where adding some "inefficient" redundancies have the biggest payoff, e.g., to our "efficient" but vulnerable electrical distribution system. Other vulnerable grids distribute trains, trucks, and petroleum. Second, establish and practice procedures to recover after an attack, e.g., on the large container port at Long Beach, or on the San Francisco-Oakland Bridge. Perhaps we have improved disaster recovery since the terrorist attack on the Twin Towers, but from the natural disasters I am aware of, such as the Indonesian tsunami, Hurricane Katrina, and an NPS-conducted experiment in inter-government cooperation in San Diego, more emphasis on preparing to act after an attack may be more productive than trying to prevent every attack. The two best ways to recover more quickly are probably by inexpensive drills to improve coordination among many agencies and levels of government, and readiness with emergency modes of communication. The general rule is "when there's a war on, study the war." That applies to war on terrorists, to the frequent use of unmanned vehicles in peacetime, and to the unending competition to safeguard and exploit cyber space.

Measuring Influence to *Avoid War*

The object of the cold war was to exert American influence without fighting the Soviet Union. We could never measure

20. Gerald G. Brown and Louis Anthony Cox, Jr., *Risk Analysis* Vol. 31, No. 2, 2011.

past success in predicting outcomes of our campaigns, including a highly predictable world disaster from a nuclear exchange, because there were none to study. The paradox is that the measurable prediction of successful analysis was that, year by year, deterrence held.

As far as I know, in the long Cold War there was only one attempt by an American analyst to measure the predictive power of the many campaign analyses of a hot war. It occurred because an inspired analyst at the Center for Naval Analyses (CNA) persuaded the CNA President to "refight" on paper a study his think tank had conducted for the Navy in 1965 of a war at sea conducted ten years later, after 1975. A study assumption was that the nuclear threshold would not be breached, in part because the American strategy was to confine the war to the oceans. The war was bloody enough among the combatants but massive civilian casualties were avoided. Around 1976 (I am speaking from memory) the analysis was repeated with the same military objectives but with the geopolitical environment updated, resulting in somewhat heavier demands on NATO forces, principally the U. S. Navy's. Soviet forces were slightly greater than projected, and our estimates of Soviet maritime combat capabilities were about the same. On the other hand, between 1965 and 1975 the American fleet had become much smaller, and future combat capabilities projected to be in the fleet in 1975 had not lived up to technical expectations when actually deployed. All inputs for the repeat campaign analysis seemed to indicate a worse outcome. Yet the amazing result of the campaign "fought" again on paper was about the same as for the 1965 study projection and perhaps a little bit better. The reason was that in two or three instances after the new systems were deployed new tactics were conceived and developed to fight with them more effectively. Better tactics more than offset technological disappointments and a smaller fleet.

That interesting finding is peripheral to the two main points. First, it is highly useful to test our tactical and campaign analyses when their inputs and assumptions can be tested, yet it is hardly ever done. Second, the study results—even in 1965 and despite their flaws—were decisively instructive. The purpose of the study was to test whether a war at sea strategy was attractive for NATO. The answer was no in 1965 and still so in 1975. As with the Falklands scenario, it did not take exquisitely detailed analysis to see why—a*fter the analysis had been done*. The Soviet Union was a continental power that did not depend fundamentally on the oceans, but NATO was a maritime alliance for which control of the Atlantic was essential. The Soviets had too little at risk and NATO too much to make a war at sea strategy an effective asymmetric deterrent. No more was heard of it, and NATO continued, wisely, to believe the central front in Europe was the critical region of interest.

There is a modern analogy to the war at sea, as we contemplate ways to influence China, keep faith with friendly states in Asia, and a*void a big and economically disastrous war.* Far from being unwise, a war at sea strategy against China looks attractive, because unlike the Soviet Union, Chinese influence and prosperity depend on the sea. Unlike the Soviet state, China has begun to build a fleet that can protect the movement of its shipping in the open ocean, shifting from a sea denial to a sea control navy.

A brilliant recent article by Naval War College professors Toshi Yoshihara and James Holmes points out that one cannot construct a strategy unless its ends, ways, and means are well defined. Hence the state, namely China, must be identified as the strategy's object.[21] The *ends* have almost been established, because the American Secretary of State and others have indicated our intention to put more emphasis on the Western Pacific. In effect, our policy experts have made a prediction about the future. Next must come an analysis of the best *ways* to sustain our influence there at an affordable cost. Yoshihara and Holmes have pointed out the limits of the new Air-Sea Battle concept and suggested other ways that can and should precede strikes on mainland China. U. S. and allied navy components would try to keep the war at sea, exploit American maritime strengths, and demonstrate that China has the most to lose at each level of escalation—from maritime interdiction short of a full blockade all the way up to sinking Chinese warships and commercial vessels by American submarines in their home seas. Having the ways in hand to constrain every kind of confrontation, next comes further campaign analysis, and negotiation with allies and partners in Asia. We must ascertain the *means*: the types and numbers of forces in such a flexible strategy to fit the desires of China's neighbors and worldwide commercial interests. No step is easy. For example, the same fleet must be suitable during times of cooperation, competition, confrontation, or conflict, and China has a say in what our ends must be. If all our ships are expected to have 30 and 40 year service lives, the challenge will be to construct one long-lived fleet for all circumstances. We don't yet know whether Yoshihara and Holmes are right about the ways and means, but analysis to meet various conditions, not a prediction of a single future, is the way to find out.

21 Toshi Yoshihara and James R. Holmes, "Asymmetric Warfare, American Style," *Naval Institute Proceedings*, April 2012.

In Summation, What to Believe about Prediction

Black swans exist. The more we know of nature and human behavior, the more certain we are that unpredictable surprises will continue. Black swans don't have to be deceptive because by definition their surprise cannot be predicted.

Grey swans in the military world are complicated because they are concealed by a perverse enemy who wants to surprise us. Pearl Harbor happened not just because it was an unlikely event and the clues about the attack were mishandled, but also because a clever enemy was doing his utmost to surprise us. Deception helps to create surprise.

Regrettably, grey swans are not likely to become rarer. The growth of knowledge, illustrated by the replacement of a written Encyclopedia Britannica with the electronic Wikipedia, exceeds our capacity to sort the information quickly. And in fast-moving military operations the enemy will constantly be trying to throw sand in our eyes with deceptions.

Expert judgment for national policy and military strategy is unreliable unless it is substantiated through the quantitative methods of operations research.

Critical decisions can be greatly—even decisively—enhanced by quantitative analysis, notwithstanding that the decision makers' prewar conclusions are well short of—and without any claim to be—a prediction of the future. Useful

insights come from wise application of dirty data processed in an appropriately simple model to yield results that are at once precise, inaccurate, and helpful.

The Falklands War "72 hour" analysis by our campaign analysis students illustrated how decisive macro insights can be discerned in a very short time. Despite limited knowledge of how a war will unfold, quantitative analysis is powerful in uncovering the essential features of good and bad choices.

The students' "overnight" analysis ahead of Operation Iraqi Freedom showed two different things. On one hand, that an amazingly accurate estimate could swiftly be made of how long it would take to topple the Hussein regime. On the other hand, the power of analysis did not help to anticipate that after defeating him there would be a long, long aftermath of difficult peacemaking. It is not new news that an enemy gets to choose, and sometimes his choices will seem not to be in his own best interests.

Accurate predictions are useless if they are too late to help the decision maker. If he must act in 72 hours we must help him within 72 hours. We teach our students to follow the 1/3, 1/3, 1/3 rule of analysis. Given three days to complete the work, spend the first day figuring out how quantitative analysis can help make a decision, do the analysis on the next day, and take the third day to recover from your mistakes, answer his questions, or enrich the work.

There are many variations of conflict in which military operations analysis can profitably supplement professional knowledge. One is when the battles goes on and on, so there is more time to gather "combat" data, assess it, and apply it without expecting perfection, all the time remembering that the enemy is also adapting. Another is when the object is not to prepare for war but to adapt new ends, ways, and means to retain influence over a prospective enemy in changing circumstances. The goal of analysis is to help decide what strategy and capabilities will be the best ones to contain the war or keep the peace.

A paradigm of all prediction is the IF-THEN statement with two parts to the IF side. To the extent that a model describes the circumstances and the data is accurate, the analysis process will give accurate results. When the model is a simplification, but hopefully an artful one, and the data is "dirty," but good enough, then the goal is not to predict the outcome but to help a wise decision maker do the best he can after adding his own wisdom to our quantitative analysis.

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