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I grew increasingly uneasy as I sat listening to senior officers around a conference table in the Pentagon making references to the "airtight" case they had assembled to buttress the next annual submission of the Army program to the Department of Defense and Congress. The airtight case, I knew, was based upon war-game simulations which, from my perspective, were as perforated with logic holes as a sieve.

In retrospect, I might have done better to have avoided interrupting the proceedings and to have let events take their course, but newcomer that I was to the force development business, I thought it important at the time to make sure that everyone understood the complex network

In the supposed dichotomy between systems analysis and professional military judgment, there is plenty of intellectual room for models derived from comprehensive analysis of historical battle data, rules that would aid battlefield decision-making, not just force development and logistics.

f assumptions and selective procedures which had been used in the analytical process. My explanation of the leakier portions of our reasoning fell upon unwelcoming ears. Bad news is seldom happily received.

My dismay over the enthusiasm with which the council had been embracing each glossy step of our briefer's presentation turned to a defensiveness concerning the analytic art as the members around the table began to question me about just what it was they were being told. I countered that whatever their shortcomings the procedures were not substantially different from those which had been used each year over the last decade for developing Army programs.

It became increasingly clear that no one had had the time to probe behind the numbers to see whether the foundation was on bedrock or on sand. My personal opinion was that it had the consistency of squishy mud, but no one would have understood that if I had said it.

The episode has stuck in my mind over the years as symptomatic of some basic problems we have with our analytical process. It was not, in my view, a simple case of the ascendancy of quantified decision-making over professional military judgment. Both methods figured to some extent in the presentation; the matter was more complex than that. It struck me as an example of how we have come to place great stock in some marvelous techniques, the limitations of which we either do not fully understand or tend to forget.

Somewhat daunted by the arcane nature of the art, we have been slow at times to ask blunt questions, and have allowed our lack of understanding to float by unchallenged, dulling our basic reasoning processes. In the early days when systems analysis was first thrust upon the services, one thing was certain: whatever their shortcomings, the analyses got attention at the highest levels. The secretary of defense made clear that quantitative work would influence his decisions.

However reluctantly, the service secretaries and service chiefs in turn made sure the models and analyses were clear enough to understand, that studies were led by high-quality professionals, usually in uniform, and that every flag officer with a stake in the matter grappled with the appropriateness of the underlying assumptions, scenarios, data and, if possible, the model itself.

As the limitations—and they are many—of systems analysis became apparent, and as later defense staffs seemed themselves to go soft on data-laden service analyses, and in the familiar, old-fashioned way, let politics, vested interests and budget ceilings intrude on their decisions, busy executives came to relegate calculations of force effectiveness to "technicians" and "number-crunchers" and to accept with fewer questions the products as the best answers available, disregarding the analysts' caveats, reservations and characterizations of uncertainties. We knew there were boundaries to our various techniques for situational forecasting, but we never got around to looking for other approaches.

Precision through quantifications of measurable factors is all too often an illusion. At times it seems that we have forgotten that sand retains its unstable qualities while we build attractive castles of the stuff on the beach. We have become accustomed to the neat results of purportedly "exhaustive studies," using trendy analytical techniques, and have come to accept too many of them as ground truth (models of reality), without subjecting them adequately to comparisons with historical experience or common sense.

It is not so much that baloney is passing for scientific reasoning. It is more that we have become too dependent upon a narrow range of procedures, some now enshrined in regulations, for shaping the Army of the future. Worse still, we have tended to focus the application of analytical techniques to rationalize resource-related questions and neglected the power of these tools for the assistance they might offer in dealing with operational questions.

Almost since World War II, and well before systems analysis came in vogue, the Army has endeavored to develop an intellectually appealing analytical process to determine its likely wartime requirements for resources and efficient peacetime structure, strength and deployments. Nothing as complicated or as ambitious as this could ever be easy, but there have been reasonable successes over the years.

There are those today who will argue, with some justification, that of the three principal services, the Army has been in the lead in the comprehensiveness and rigor of its total force calculations. The observations and criticisms which follow notwithstanding, we should recognize at the outset the merits of the Army's approach as far as it has gone. Fault appears to lie more in the limitations of the analytical techniques it has adopted and its limited range of applications than in misdirected effort.

Salvation, if there is to be such in the austere business of force planning and deployments, lies in an awakening to the advantages of alternative and multiple paths to problem addressal and in opportunities which may exist for additional applications of known techniques.

The Army assumes that it will fight its wars beyond the borders of the United States—overseas. This is the most basic tenet of our structure of assumptions, one that specifies that the cutting edge of our land force will be expeditionary in nature, whatever else it might be. This prospect for remote operational deployment in time of emergency means that the force composition must be a balanced one among com-

bat, combat support and service support elements.

We do not know where on the globe the force might next be employed, but we assume that it must stand on its own wherever it finds itself. We expect that it will fight with allies in whatever theater it is deployed, but there is little assurance that many allies will be able to provide much more than certain basic facilities and some labor support. The Army must plan, therefore, on carrying its own "tail" with it, wherever it goes.

The expeditionary nature of our deploying contingents and the indeterminate locus of the next acute threat also mean that the Army must have a balanced force between "heavy" and "light" components. A portion must consist of large armored forces, suitable for operations on the plains of Europe and for the desert wastes of Africa or the Middle East. Other elements must include agile foot infantry for jungle and mountain terrain. Whatever the outer dimension of the total force, there is no specialty which can be disregarded. There must be a mix of capabilities.

Similarly, there must be balance in deployability. For those theaters where the threat and risks are readily apparent and measurable, forward deployments of tailored forces and storage of equipment for reinforcing units make sense. In peacetime these serve as visible expressions of U.S. commitment and deterrent to aggression. In time of crisis they greatly simplify the overall deployment problem, cutting movement times to fractions of those encountered in the great European wars earlier in the century.

Another major assumption which the Army makes is that the central region of Europe constitutes the area of primary national concern outside the United States and is the theater in which the forces of the potential opponents pose the most taxing threat to the interests of the United States and its allies. As a logical consequence of this, the European contingency is construed as a template for establishing rough outer boundaries for the Army wartime objective force.

An annual analytical exercise, "Total Army Analysis," has followed this reasoning for over a decade. To be sure, other contingencies have been examined on an excursionary basis from time to time, but the *pièce de résistance* for Army force development remains the Warsaw Pact threat to NATO in Central Europe.

The basic techniques employed in this process are analytical modeling and simulation. Computerized models portray the movement of Army forces overseas, simulate combat and determine requirements for combat service support, based on combat consumption and attrition rates determined in the combat simulation. Of course, the magnitude of the support requirements affects the amount of air- and sealift available for the combat forces, so, as support requirements are identified, the movement model must be rerun with a different mix of "tooth" to "tail."

'Inconvenient realities of combat tend to be scarce in quantitative analysis.'

This, in turn, means a different force mix for combat, and the battle simulation, too, must be rerun based upon a new schedule of troop-arrival times. Then, as we alter the battle simulation, we must expect to generate different support requirements, and so on. We find we are on a circular course. Fortunately, experience indicates that after two or three iterations of the overall process the figures begin to converge.

Eventually, a picture emerges of a reasonably balanced force representing an optimum organization for coping with the threat in the situation posited. That picture becomes the basic rationale for future Army posture.

What is wrong so far? Conceptually, very little. If we could produce a better force posture with confidence in the validity of the myriad assumptions necessary for such an effort, together with faith in the quality of basic data and the many subroutines imbedded in the process, we would indeed have an analytical machine worthy of respect. But, of course, we cannot. Why not? Let us examine some of the pitfalls we have.

A common deficiency we find in quantitative analysis today is a tendency toward the exercise of selectivity among variables for our models. Speed, armament and survivability of tanks, for example, are relatively easy to measure and are usually readily accounted for in our calculations. Quality of leadership, troop esprit and national purpose, on the other hand, are much more difficult to deal with, and are usually omitted from the calculus.

Yet, who is to say that the quality of equipment is of greater importance than the qualities of the soldiery and of their leaders? Not quite so difficult, but nonetheless often omitted, are such factors as the quality and celerity of intelligence acquisition and the exercise of troop command and control. With such narrow focus on hardware characteristics, can we really say that we are now capturing the essentials of the action with our models?

There are other problems. Often we encounter differences among reasonable men over the relative values that should be attached to the variables which we can model. How much more of a contribution, for example, does a tank make on the battlefield than a rifle, or a tactical fighter bomber than a tank? Are these things proportional to their differences in cost? How do we aggregate and compare weapon systems?

In practice we make attempts at identifying "weapons effectiveness indices" and "weighted unit values" (WEI-WUV), but even the authors of these ingenious devices are quick to identify their limitations as mere shorthand indicators of combat capability. Most frequently heard are legitimate complaints about the subjectivity of the Delphi methodology (iterative judgment technique) upon which they depend.

Another problem: basic facts. Where do they come from? How valid are they? Often we hear senior officials and respected authorities cite such events as the 1973 Arab-Israeli War or the Falklands War to buttress arguments for or against one notion or another. But how representative are the campaigns cited? How can we be sure that these are not anomalies awash in a sea of countervailing evidence if we will but look for it?

Data selected from historical analogies allow one to prove almost anything one cares to. Virtually everything has happened at one time or another in a combat action someplace. One has only to imitate an attorney in court citing prominent precedents on behalf of his client's interests to sound profound in a debate over military doctrinal issues. But this is hardly the objective process we seek.

And what about military folklore and how it colors our models? "Everybody

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knows" that a defense can "service" an attacking force three times its size. (One can even find Clausewitzian language hinting at support for such a thesis.) Do we really believe it? If we can depict an ambush by a platoon of a larger force on a sand table in which we achieve a highly favorable loss-exchange ratio, does that mean we can aggregate up a number of platoon and company actions and portray World War III as the sum of a thousand ambushes?

That is what a computer will do for us if we simply feed it raw data from a high resolution model. With this approach we might venture a guess that NATO should rapidly emerge as the consistent victor in all of our simulations, always killing three or four enemy tanks for every one it lost. (There may be people who will believe results like this if they emerge from the analytical process via the bowels of a computer.)

Inconvenient realities of combat tend to be scarce in quantitative analysis. Where, for example, do the great masses of prisoners and discarded equipment show up in the calculations? If an offensive is successful and a breakthrough or double envelopment is achieved, how should we estimate the size of the bag? How much should we award or penalize the attacking commander for exposing his force to the risks of leaving the safety of dug-in positions for open combat?

Should we expect that the numbers of prisoners would occasionally run into the hundreds of thousands as they did at times in previous wars? If we could count the bag, should we justify the size of our force based in part upon the numbers of our troops we expect to have surrounded and to surrender to the attacker?

'Precision through quantification of measurable factors is all too often an illusion.'

The discreet analyst often avoids direct addressal of many questions of this sort and simply does the best he can with what he's got, hoping that his audience will understand the awkwardness of certain aspects of the problem and subconsciously factor them into the results he provides. He is simply working at the outer fringe of the art, whether his consumers know it or not. Too often they do not. Yet another weakness springs from the undisciplined nature of the overall analytical community. Every systems analyst or operations researcher with a problem to solve or policy position to justify is free to devise his own models and to choose and weight his own variables. It is little wonder that while one study will conclude that an airborne weapon system is most effective for a particular mission, an equally rational second study will conclude that a ground-based system would be more cost-effective.

As complex as the studies usually are, it is often difficult to identify equivalencies among them, or to assess what factors led to one conclusion instead of another, or to gauge the relative importance of the factors. Precise as the numerics may appear, underpinning assumptions often render one process virtually incomparable with another.

Finally, we have mounted no discernible effort to develop models with much utility beyond the clinical environments of weapon design and force development. Some models lend themselves to adaptation by staffs for tactical war-gaming, but the focus here has been primarily on analyses of set-piece engagements and the educational aspects of the exercise rather than on direct staff support. We do not see, for instance, the development of computerized aids with a capability for quickly scanning the various annexes of operations orders and identifying inconsistencies and gaps.

Nor is there a reliable tool in our computing arsenal for quickly checking the essentials of an "op order" to make sure that it keeps the flow of events in the right ball park. We have no standard methods for doing quick "what if?" analyses for commanders who do not have full knowledge of the enemy's dispositions. It might be helpful, for example, to have a capability at division or corps level for a very rapid analysis of what components of the command are likely to be most vulnerable to various hostile actions at specific points in an operation.

At present, we must rely primarily on intuition and the "feeling" of the situation, coupled with a hope that we can somehow muddle through if things get tight. Because of the emphasis on analysis for weapon procurement, we have not fully examined possibilities for weapon employment—for supporting commanders and staffs in the field with the benefits of modern decision-assistance techniques.

No, we are a long way from the "analytical machine worthy of respect." Lacking a real understanding of the elemental nature of what it is we are dealing with —the very complex process we call war —we are still guessing, trying to narrow the field of inconvenient facts and seemingly dimensionless variables. We continue to try to sharpen our analytical tools and to find ways to strengthen the relationships among the models we use. So much is to our credit.

Our heart is in the right place in this respect, but we are working at the high end of a curve of diminishing returns on problems about which we already know a great deal, while an unexplored world of other possibilities lurks over the horizon. The real question we might ask is whether we are exhausting all promising avenues to progress. Again, the answer must be "no."

Central to the theme of this discussion is that a significant share of our current shortcoming is due to our own oversight (at best) or (at worst) to a peculiar form of disdain (arrogance?) for existing alternative approaches, particularly to the organized study of recorded experience and comprehensive historical analogy.

In the decades since the McNamara Revolution in the Pentagon, quantitative analysis and professional military judgment have often been treated as opposite poles in a two-dimensional world. Quantitative analysis has been depicted as objective, scientifically sound and reproducible by anyone skilled in the art; military judgment as highly subjective, based primarily on experience and peculiar to the official offering it.

Partisans of the judgment approach have usually been quick to point out that their way is inherently no less reliable than quantitative analysis. On the other hand, most will admit that judgmental quality is dependent upon the experience and internal thought processes of an individual, while analysis is conceptually open to audit by anyone interested in the track followed.

Unfortunately, overlooked in this bipolar construction is any allowance for the existence of other techniques or aids in decision-making. A prominent candidate for comparison with our current approach is analysis by comprehensive historical analogy. There is very little under the sun, historians often remind us, which has not been encountered somewhere before in one form or another. The comprehensive and systematic review of historical precedent is viewed by

no less worthy authorities than our principal adversaries, the Soviets, as a primary technique for all types of military decision-making.

Because this process is so little known in the West, it is important that we take the time to examine it in some detail. First, it is not to be confused with the sorts of experiences associated with the judgmental technique. The experiences of an individual, or of a small group, are narrow and largely confined to recent events. In addition, they tend to be more anecdotal than typical of most events resembling circumstances surrounding the problem at hand.

In many cases, judgments are attempted with only the most rudimentary connections between the experiences of the judge and the problem, if, indeed, there is any relationship at all. The more remote the connection, the less "professional" or "expert" the judgment (but not, necessarily, less valid).

In analysis by systematic historical analogy, on the other hand, the practitioner requires no special experience. Instead, he draws upon carefully defined "norms" which have been painstakenly developed by researchers from examination of scores—even hundreds—of battle actions in modern history. Drawing primarily from the "Great Patriotic War," the Soviets have dissected and synthesized the details of engagements in such a way that like actions may be assembled, averaged and quantitatively expressed as coefficients suitable to determination of likely outcomes under given circumstances.

The total process of static and potential dynamic force comparison is summarized at the various levels of command as the "correlation of forces." The norms and expected values are carefully cataloged and computerized to afford rapid manipulation as the factors of the problem situation vary. The results are highly objective, empirically provable and readily adaptable to modern data processing. The research and developmental costs may be high, but the Soviets would probably argue that the stakes are even higher.

The Soviets do not view the quantitative analysis and historical analogical processes as tools solely or even primarily for force design or for marginal benefit types of decision-making. To the Soviets these devices are every bit as applicable to operational staff support as they are to support of staffs and decision-makers in the resource business. Aspiring operations officers in the Soviet Army are steeped in the techniques of quantitative assessment of the situation and the determination of mission accomplishment through rigorous computer-assisted analysis of the likely impact of factors at play on the prospective battlefield.

Of course, we should note here certain peculiarities of these techniques which make them particularly suitable for the Soviet circumstances. The Soviets believe in science, if they believe in anything at all. Their brand of scientific materialism teaches them that everything is knowable, if one will but persist in its investigation. There is no spiritual world into which verities can escape.

They believe that war can be studied as any other science to determine its basic nature and the fundamental, underpinning laws can be deciphered through careful scrutiny. Outcomes of battles are caused, the Soviets would argue; they do not just happen. The Soviets seek an understanding of the essence of war through analysis of historical events and the identification of contributory factors toward battle outcomes.

'The Soviet Defense Ministry calls for historical research into about 100 different fields each month.'

They believe that careful association of factors with the influences they had on the outcomes and the melding of these factors with similar ones from other actions can provide insights of substance for assessment of both operational and force development problems.

"Armed conflict," one of their leading theoreticians has written, "is subordinate to statistical laws . . . . The action of statistical laws in armed conflict arises as a result of a mass employment of personnel, combat equipment and weaponry under approximately identical conditions, as well as with the manifold repetition of events, when certain attributes common to all of them are discovered." (V. YE. Sdvkin, *The Basic Principles of Operational Art and Tactics*, Moscow, 1972.)

The Soviets believe that such discovery can unlock barriers to understanding of the process of war.

We know that Soviet military structure tends toward greater rigidities and conservatism than its counterparts in the West. Decisions at the lower echelons must be made in close accordance with the policies of the leadership, but the Soviets recognize that, unless there is adequate guidance for subordinate commanders and staffs, too many problems would necessarily be referred back to Moscow for resolution.

The existence of norms, derived from objective assessments of multiple actions and approved by the highest authority, provides just such guidance. The Soviets believe that there should be little need at operational levels for debate over whether this or that historical event has relevance to the problem at hand.

The characteristics of the current problem should be able to be fed into a computer already stacked with approved historical norms. Working perfectly, the computer should search its files, pick out a match and issue the essentials of operations orders or other action documents for the staff. The process should be very rapid, and lend itself to equally rapid examination of ancillary "what if?" questions and sensitivity analyses.

Most of us would say the Soviet methods are too mechanistic. Yet, can we say that their approach is less valid than ours? At least they can argue that theirs is built substantially on fact—or a compendium of historical facts. Their results are wholistic while ours tend to be limited by the selective nature of the variables in our models. Their opportunities for systemic error, they would argue, are largely confined to the process of combat action dissection, synthesis and cataloging, and errors may be reduced in subsequent refinements of their models.

We should recognize, of course, that while they rely, as we do, partially on professional judgment, the Soviets focus far more than we upon quantitative analysis and historical analogy. For them, each serves the other. History provides the facts. Quantification renders them manipulable. Objectivity comes from comprehensiveness rather than eclecticism in the historical research. "Norms" are derived for all manner of questions that the process must satisfy: ammunition expenditure rates, casualty rates, rates of advance and withdrawal, coherence in force behavior and so on.

All these factors are carefully reviewed at higher echelons because of the important role they play in Soviet military decision-making. Once approved, the norms enjoy widespread acceptance—until proved inaccurate. Then they may be recalculated as new experience provides

new raw data. The key to credibility lies in avoidance of drawing too much from any single historical event and in continuous updating of the data banks through research.

But we should also recognize that to the Soviets their approach is much more than a historically based technique for data acquisition and manipulation. In their view, they are working at fulfilling their concept of acquisition of knowledge through systematic scientific exploration. They see their efforts as a quest into the very nature of conflict—the essence of war.

In their eyes, the data they derive are the practical products of scientific research. They treat them as the valuable fruits of a proved process. They have full confidence in the basic scientific method and in their military decisions which they believe stem from it.

The magnitude of effort in their pursuit of the goal is impressive. They school their officers far more thoroughly than we do in the scientific method. Quantitative analytical techniques are the warp and woof of their military educational system—not just a specialty of a few officers. They have scores of officers with doctoral degrees in military science conducting research and publishing treatises on all aspects of war. The Soviet Defense Ministry calls for historical research into about 100 different fields each month.

Their demand for their research products is insatiable. Unlike U.S. tacticians, Soviet planners apply the data derived from historical research to their operational plans. Typically, they will draw up accompanying PERT (program evaluation and review technique) charts to determine critical paths through their schemes of maneuver. They press hard to avoid leaving anything to chance. They do not see themselves as practitioners of an art, as so many American G3s do.

At the tactical and operational levels, they see themselves more as engineers, applying the fruits of scientific process. They believe their approach is in concert with the laws of nature and their results are as reliable as man can make them.

The weaknesses of our current suite of models and procedures have not gone unnoticed, but the search for escape from the confines of our approach has not enjoyed widespread support. Busy executives tend to follow straight-line projections of their busy schedules.

Nowhere within the Defense Department do we hear a coherent voice for experimentation and change. Perhaps we should not be too surprised at this. A change might make it difficult to compare this year's analyses with those of last year, and often the principal purpose of the exercise is to justify incremental changes in existing programs. In these cases the current approach may be the line of least resistance.

On the other hand, if we are concerned with broader questions—and one would hope that on occasion we would seek to deal with something larger than programmatic issues in the pursuit of our national security—then a determination of some sort of "ground truth" (or absolute values) may have relevance. Certainly a glimpse at the intent behind our contingency requirements studies would lead us in this direction.

'The discreet analyst . . . is simply working at the outer fringe of the art, whether his consumers know it or not. Too often they do not.'

We really need to have some idea of how well a given force might do in a given situation—not just how much better or worse it might do than another force. Also, sometimes we need to know total requirements for various types of equipment for an anticipated operation—not just variations from base figures established (somehow) years ago.

We must believe that ground truth counts in what we are about. And we should recognize that analytical techniques can be of assistance in dealing with operational as well as logistical problems.

If we are correct in this assessment, then we may not be as far from the Soviet view as we think. We may have the same essential needs for an understanding of combat as they profess—we simply have not recognized it with the commitment of intellectual resources that they have. And, if their techniques are sound, we trail them by a great distance in the exploratory effort.

To the great credit of our (undisciplined) community of military analysts, there is a group within it which has taken upon itself a sense of responsibility for attempting to do what the Army thus far has not: to examine the phenomena of war and combat action in their essentials, and to organize the knowledge gained for practical application to military problems. Since 1979, this independent group, operating under the improbable title of "Committee to Develop a Theory of Combat and Philosophy of War," has been grappling with the problems inherent in its name.

It seeks answers to the basic questions of how military forces fight and win or lose, and why battles take place where they do and have the outcomes they do. Unfortunately, unattached as it is to the regular plumbing of sponsorship and support in government, the group has been obliged to look to its own resources for carrying on the work designed for the public good.

Undaunted, the committee argues that in our selective approach to battle analysis we in the Army have missed the essentials of what it is that we are endeavoring to reproduce in our models. The members suggest that we have been unable to find a way out of our intellectual *cul-de-sac* largely because of our inflexibility and parochial thinking. While they prefer to point to new concepts than to focus on criticism, they do not shy away from a judgment that our current approach has limited value for the discovery of reality.

Their argument is reminiscent of Plato's parable of the cave people who never saw the surface of the earth. They assumed that all the world was a tunnel. The committee, like Plato, would have us crawl to the surface and take a look around.

Independent as it is, the committee has been obliged to work outside the context of the official military analytic community which its ideas must ultimately serve. While interested military and civilian government analysts are connected with the committee, no official affiliation has developed between the committee and the Department of Defense. Committee symposia have been sponsored on occasion by various military educational centers, such as the war colleges, but, thus far, the essential linkup has not occurred.

The Army and other entities within DoD, which ought to have a close and direct interest in the effort, have so far missed the point. We can only hope that the work will somehow continue so that when the importance of the effort is finally recognized, the Army will be aided in grasping the essentials of its intellectual foundation in a form it can assimilate and put into practice.