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2

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WHAT GOOD ARE WARFARE MODELS?

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If in the following pages I seem to express myself dogmatically, it is only because I find it very boring to qualify every phrase with an "I think" or "to my mind." Everything I say is merely an opinion of my own. The reader can take it or leave it. If he has the patience to read what follows he will see that there is only one thing about which I am certain, and that is that there is very little about which one can be certain.

W. Somerset Maugham
The Summing Up

If, as Whitehead said, "the certainty of mathematics depends on its complete abstract generality," the attraction of mathematical models of warfare is their apparent certainty.

Aside from a natural urge for certainty, faith in quantitative models of warfare springs from the experience of World War II, when they seemed to lead to more effective tactics and equipment. But the foundation of this success was not the quantitative methods themselves. Rather, it was the fact that the methods were applied in wartime. Morse and Kimball put it well:

> Operations research done separately from an administrator in charge of operations becomes an empty exercise. To be valuable it must be toughened by the repeated impact of hard operational facts and pressing day-by-day demands, and its scale of values must be repeatedly tested in the acid of use. Otherwise it may be philosophy, but it is hardly science.*

Contrast this attitude with the attempts of analysts for the past twenty years to evaluate weapons, forces, and strategies with abstract models of combat. However elegant and internally consistent the models, they have remained as untested and untestable as the postulates of theology.

There is, of course, no valid test to apply to a warfare model. In peacetime, there is no enemy; in wartime, the enemy's actions cannot be controlled. Morse and Kimball, accordingly, urge "hemibel thinking":

> Having obtained the constants of the operations under study... we compare the value of the constants obtained in actual operations with the optimum theoretical value, if this can

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* Philip M. Morse and George E. Kimball, Methods of Operations Research (Washington, D.C.: Operations Evaluation Group, 1946), p. 10.

> be computed. If the actual value is within a hemibel (...a factor of 3) of the theoretical value, then it is extremely unlikely that any improvement in the details of the operation will result in significant improvement. [When] there is a wide gap between the actual and theoretical results...a hint as to the possible means of improvement can usually be obtained by a crude sorting of the operational data to see whether changes in personnel, equipment, or tactics produce a significant change in the constants.*

In other words, combat is not a mathematical process, F.W. Lanchester notwithstanding. One may describe the outcome of combat mathematically, but it is difficult, even after the fact, to determine the variables that made a difference in the outcome.

Much as we would like to fold the many different parameters of a weapon, a force, or a strategy into a single number, we can not. An analyst's notion of which variables matter and how they interact is no substitute for data. Such data as exist, of course, represent observations of discrete events -- usually peacetime events. It remains for the analyst to calibrate the observations, but without a benchmark to go by. Calibration by past battles is a method of reconstruction -- of cutting one of several coats to fit a single form -- but not a method of validation.

Lacking pertinent data, an analyst is likely to resort to models of great complexity. Thus, if useful estimates of detection probabilities are unavailable, the detection process is modeled; if estimates of the outcomes of dogfights are unavailable, aerial combat is reduced to minutiae. Spurious accuracy replaces obvious inaccuracy; untestable hypotheses and unchecked calibrations multiply apace. Yet the analyst claims relative if not absolute accuracy, certifying that he has identified, measured,

*Morse and Kimball, <u>op. cit.</u>, p. 38.

and properly linked, a priori, the parameters that differentiate weapons, forces, and strategies.

In the end, "reasonableness" is the only defense of warfare models of any stripe.

It is ironic that analysts must fall back upon the appeal to intuition that has been denied to military men -- whose intuition at least flows from a life-or-death incentive to make good guesses when choosing weapons, forces, or strategies.

It is not surprising, however, that analysts must finally appeal to intuition. A warfare model, like any system of formal logic, is ineluctably incomplete: It cannot prove itself or define the truths lying outside the reach of its axioms and rules of inference. One must go outside a warfare model for its proof, which, given the impossibility of validation, throws the analyst back on intuition. Nor does sensitivity analysis save the day; at best, it illuminates faintly the boundaries of a model, without establishing the truth or falsity of what lies on either side of the boundaries. A political observer, in another connection, put it more elegantly:

> The power of analysis, of course, has its limits. Intense assessment of the shots on the board misses the shots that are not on the board -- the truly revolutionary departures that bend the course of history.*

In short, it is impossible to quantify warfare without having a war to quantify. Only then are there data embodying the true complexities of combat. No number of iterations of a warfare model can create such data. Zero a thousand times is still zero.

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*Joseph Kraft, "Lippmann: Yesterday, Today, and Tomorrow," Washington Post September 11, 1980.

What good, then, are our models of warfare? If there is no salvation in complexity, must we retreat into solipsism? Has the work of two decades been for nothing?

If the power of quantitative analysis had not been overrated and oversold by Mr. McNamara, it might be unnecessary to answer -- or even ask -- such questions. The "McNamara revolution" was a victory not for quantitative analysis, but for the intuitions of civilian analysts over those of professional warriors. One result of this upheaval was a blurring of the line between quantitative analysis and policy-making. Policies appeared to spring full-blown from quantitative analysis, demonstrating once more that analysis done with a conclusion in mind is bound to seem conclusive -- at least at first glance. It is no wonder that some thought civilian analysts "could invoke the contemporary resources of mathematics...to perfect... managerial magic."*

It is improbable that such naivete infected the military leaders of the day. They, no doubt, understood that Mr. McNamara was using quantitative analysis as a means of establishing his dominance by finding their proposals wanting for reasons military, not budgetary.** Eventually, however, the military was forced to back its qualitative arguments with quantitative "evidence," which the civilians -- given the last word in nearly every dispute -- could pick apart at will.

The civilians, of course, chose to view their ability to pick apart the services' studies as evidence of the inferiority of the services' brand of analysis. What the civilians had, in fact, demonstrated was the wisdom

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* Arthur M. Schlesinger, Jr., A Thousand Days (Boston: Houghton Mifflin Company, 1965), p. 127.

** See Arnold Kanter, Defense Politics (Chicago: The University of Chicago Press, 1979), pp. 87-91.

of "hemibel thinking" -- of relying on quantitative analysis for only a "crude sorting" of alternatives, not a judgment of which was "best."

Should we really attach little significance to differences of less than a hemibel? Consider a five-parameter model, involving the conditional probabilities of detecting, shooting at, hitting, and killing an opponent -- and surviving, in the first place, to do any of these things. Such a model might easily yield a cumulative error of a hemibel, given a twenty-five percent error in each parameter. My intuition is that one would be lucky if relative errors in the probabilities assigned to alternative weapons and forces were as low as twenty-five percent.

Some may protest that quantitative analyses of warfare alternatives are not helpful if we cannot claim for them greater accuracy than a hemibel. On the contrary, only by admitting the inaccuracy of such analysis can we help to turn decision-makers from the futile and fitful search of two decades for the "best" defense posture to the deliberate achievement of a "good" one -- and give the lie to decision-makers who would evade responsibility for their choices by hiding behind analytical half-truths.

If our clients are disappointed that we do not always have answers, we should remind them that there are no answers -- only difficult choices. We do not help them to make better choices by pretending to know what is beyond our ken.

Newton said it best:

> I do not know what I may appear to the world, but to myself I seem to have been only like a boy playing on the seashore, and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me.*

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*Quoted in Horace Freeland Judson, The Search for Solutions (New York: Holt, Rinehart and Winston, 1980), p. 5.

As Newton's self-doubt was not an attack on science, neither have I essayed an attack on quantitative analysis -- only on the abuses of it that are too often found in the company of warfare models. It is too easily forgotten, by analysts and decision-makers alike, that warfare modeling is an art, not a science. With this art we may portray vividly the few pebbles and shells of truth that we have grasped; we can but vaguely sketch the ocean of warfare whose horizons are beyond our reach.