The Sometimes You Win and Sometimes You Lose Hypothesis: Some Comments on the Use of Models in Force Comparisons

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What is the purpose of force comparisons? In a security policy context the purpose is to assess or predict the probability of a successful national or alliance defense (or conversely for an aggressor the probability of a successsful attack). While the drawbacks of relying on simple numerical counts of forces (bean counts) are obvious, and while models that incorporate some of the effects of the battlefield appear attractive in arriving at a more sophisticated analysis, it is not at all clear what model, if any, can give reliable results from which policy judgements can be made. If deterrence is an objective then we are looking for a reliable high-probability result from the model. Furthermore, the validity of the model must be accepted by the opponent, who is, afterall, the one to be deterred.

As Barry O'Neill has pointed out there are other uses of models. These are to gain insight into the dynamics of warfare and to compare weapons, force structures and tactics. It is one thing for a field commander to run some numbers to confirm a tactical hunch or for a weapons procurer to use a model to measure the cost-effectiveness of purchasing a more rapid fire gun. It is quite another to use a model to arrive at attrition rates, and by

implication war results, along the entire Central Front of Europe. The method may simply not be up to the task. From my rather quick review of the attempts I would conclude that this is the case.

The original Lanchester equation was a method of quantifying the dynamics of direct fire on the modern battlefield. It is no doubt a valuable insight into this one battlefield factor, however it hardly begins to model the complexity of battlefield phenomenon. Acknowledging that warfare uses indirect fire as well as direct fire, Lanchester also produced an indirect fire equation. The quality of C3I and the effective use of fortifications, smoke and terrain cover can make the difference between direct and indirect fire opportunities which will have considerable effect on the differential casualty ratio. While a case could be made that the direct fire equation ("square law") applies under ideal conditions and is the dominant factor in such engagements, modern warfare rarely occurs with good visibility on faceless terrain in head-on battle. The Lanchester square law assumes perfect (or, at least, equivalent) targeting data on both sides, a condition which is certainly the exception rather than the rule.

Lanchester models assume a new target can be acquired after each kill. This would not be the case at the beginning of a defensive engagement. Defensive forces, by in large, would not have to present themselves until they fired their first shot; thus

targeting information at the start of battle is likely to be much better for the defense than for the offense. While the square law incorporates the advantages of concentration and directed fire, it does not incorporate the effects of range limitations and force-to-space limitations (Lipingwell).

Epstein has presented a model that incorporates battlefield factors not considered by Lanchester. Epstein models the ability of the defender to limit his rate of attrition by retreating, trading space for time. There are several dubious assumptions in the Epstein model. One is that attrition rates remain constant during battles and from one engagement to the next. Another is that engagements can be broken off cleanly and battle joined again on terms equivalent to the prior engagement. The outcome of efforts to withdraw and fight from new positions is not predictable; in the right circumstances it can be a successful tactic, at other times it can be a disaster. Any reading of the history of warfare reveals that retreats can turn into routes as well as good defensive regroupments and that many factors other than simple force size will effect the result. To model a long series of smooth withdrawals to new positions as in Epstein's model is not credible.

The above criticism does not mean that Epstein's model is not useful in testing the tactic of serial retreats in the face of larger forces. Under the optimistic withdrawal assumptions employed by Epstein there does seem to be a tactical benefit to

trading space for time as compared to fighting it out in place. However, illustrating a tactical benefit in a special situation is very different from predicting success across a wide front with many different battles.

Epstein's model succeeds in making the point that we must consider battlefield behavior (derived from doctrine and tactics) as well as the exchange of fire equations offered by Lanchester. But fire equations and battlefield behavior do not begin to touch on the complexity of the real battlefield and our model must approach this full complexity if it is to have any predictive capacity.

Some models attempt to incorporate a large number of factors and approach the required complexity. With modern computers such systems complexity is possible. However, Barry O'Neill has made the point that the outputs of highly complex models are of little use to those who are not expert, because non-experts have no way to make independent judgement on the validity of the inputs and equations. Be this as it may, it is hard to see what alternative there is to complex analysis. Either we aim for the best simulation possible or we acquiesce to the judgement of old soldiers who can say: "I fought three armored battles with the Germans in World War II and I can tell you what we need to beat the Russians."

Complex systems rarely yield simple answers. In factoring-in the full complexity of battlefield it is not adequate to use an average for individual factors such as probability of kills, survivability and equipment reliability. The aggregation of numerous mean probabilities does not yield a valid overall probability. Many factors will not have a simple discrete set of probable values. In conditons of dynamic variability and complex probability sets for numerous battlefield factors outputs in terms of attrition rates, etc. are likely to be unstable and show patterns of oscillation from one simulation to the next. Especially as antagonists approach force equivalency the sensitivity of the battle to the probability variation in factors will become dominant. Outcomes that approximate conditions of chaos are likely. Chaos has its own patterns (attractors) but no stable or predictable outcomes. Thus the high level of assurance required by a non-nuclear conventional deterrence can not be achieved in chaotic battlefield conditions.

Biddle has noted that: "Many combat models display occassional oscillation, whether as a result of varying reinforcement schedules, the dynamics of withdrawal and re-engagement (such as in the Adaptive Dynamic model), or purely stochastic variation in Monte Carlo models"(p. 18, note 33) Biddle goes on to "exclude the theoretically interesting ... possibility of an 'oscillating' combat process" because "it is not clear that useful policy guidance could be obtained at all." But this possibility should not be dismissed in a footnote. If oscillations are frequent in

good complex models then there are very serious implications for policy. The issue for policy-makers is that a stable conventional deterrent can not obtain in the modeled conditions which produce oscillating outputs. Consequently complex models could be very useful in distinguishing the conditions (i.e. force structures, tactics, and armaments) that produce chaotic, oscillating or unstable outcomes from those that produce stable outcomes.

We can surmise that not all battlefield conditions will create oscillating outcomes. We would expect oscillation to be most frequent in the battlefield with highly mobile forces of rough equivalency. One the other hand, if one side's force level is clearly superior it will overwhelm all the probabilistic variability of individual factors and arrive at the stable outcome of victory in each simulation. Of course such an assumption ignores the dynamic of the arms race which makes such a security solution highly unstable over time. The other approach to stability is when both sides specialize in or optimize for defense, producing a large disadvantage for offensive action. Such force structures would create what von Muller has called "mutual defensive superiority" while undoing the dynamic of the arms race.

If my hypothesis proves true and my reasoning is correct then the objective of arriving at conventional force balance must be challenged. The outcome of conflict between balanced forces as

currently structured are unknowable and unpredictable. This would seem to support the position that no stable conventional deterrent to war can be built, short of overwhelming superiority. If that was as far as our analyis went, we would end up giving support to flexible response doctrine with its fundamentally unstable nuclear deterrent component. Of course the favored option is to model restructured conventional forces that can take us out of the chaotic conventional battlefield and give us stable outcomes.