

War Gaming and its Role in Examining the Future

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THE FIELD OF WAR GAMES IS a complex and rich discipline that is subject to multiple interpretations. To accommodate the vastness of this issue and narrow the scope of inquiry let us then regard war games as analogous to theater. A play is an abstract slice of life which can never be a perfect representation of the subject or behavior it is intended to represent. In reality, the art of playwriting is, above all, to choose a focus and provide verisimilitude only to particular details. A play may have a variety of purposes. It can serve a pedagogical role, as a means for the audience to vicariously practice a particular belief, relationship, or course of action, and it can be a laboratory in which experiments are conducted to test the consequences of a particular idea, value, emotion, or human interaction.

Much the same can be said about war games. They, too, are abstracted slices of life though their chosen focus is usually the military rather than familiar aspects of human existence. The art of devising a war game is deciding what to portray in detail, what to represent in a more schematic or impressionistic fashion, and what to omit. Every war game is characterized by a problem, issue, or question embedded in a depiction of a realistic series of events and resulting situation. Participants in a war game must tacitly accept the inevitable artificialities of simulated war. If participants spend their time complaining that they cannot smell gunpowder or hear the sounds of battle, the war game will fail.

War games vary in their elaborateness, and here too, an analogy with the various stages of a theater production is useful. Early in a theater production, the actors sit

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together with the text and read their parts. The purpose is to familiarize themselves with the script and perhaps to develop insights into the character and plot. The next stage involves the actors walking through the play without costumes, scenery, or blocking. The most elaborate stage, of course, is the dress rehearsal and then the performance itself. Similarly, war games can be as informal as a group of players talking through different ways of approaching a particular problem. Next in formality might be a so-called map exercise in which players move forces in the relevant air, sea, or land area. The most elaborate are staff exercises in which large groups rehearse or experiment with carrying out their roles in a hypothetical conflict. The final significant association between the two is that just as most plays have an antagonist, war games involve an adversary, the so-called Red Team. The Red Team is charged with responding to the moves of the players, often called the Blue Team. The play of a war game then consists of repeated action-reaction cycles. It is this iterative quality that distinguishes war games from simply an organized discussion, seminar, or workshop.

WAR GAMES: WHAT ARE THEY GOOD FOR?

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Now let us turn to the question of when war games are useful and, conversely, when they are not useful. This is a subject of some confusion because war games are such powerful tools. Anyone who has engaged in organized debate knows that the simple act of presenting a good argument can, in time, cause the debater to convince himself. Returning to theater, actors can grow to believe they are the character they play, after repeatedly playing him. Put most broadly, the art of behaving as if one believes something can cause one to believe the argument or scenario as fact. The danger, therefore, is that if done carelessly, war games can seem to the players to produce insights or conclusions that have a basis no more solid than that of a debate or play.

For this reason, I have come to believe that war games are most often not an effective method for the analysis of military problems. Suffice it to say that good military analysis is not fundamentally different from good analysis in other settings. Repetition and control over the variables are needed if one is to reach sound conclusions. Yet war games meet neither of these requirements very well. War games, even if informal, are time-consuming for players. The ability to repeat war games, therefore, is quite limited. Experimental control is similarly difficult as a practical matter. War games are sensitive to who is playing. Unless the cast of characteristics is the same throughout, that critical source of variation remains uncontrolled. And, even if the players can be kept constant, their behavior, moods, and perspectives cannot be. Therefore, it is very dangerous for players or analysts to emerge from a game with the view that they have concluded anything analytically solid.

War games can be a powerful way of developing questions, issues, and provisional insights that must then be analyzed more vigorously with different methods. In this sense, war games can be an essential precursor to the process of analysis, if not part of the analysis itself. The critical role of war games in analysis is most clear when the composition of the United States defense community is considered. This community is composed of both war fighters and military analysts, which tend to be separate and distinct populations. Indeed, military analysts are often not military personnel and never have been. This is important because results reached by lengthy analysis may contain critical departures from what military professionals might regard as war-fighting common sense. Such departures do not mean that the underlying analysis is wrong. War-fighting common sense may not be all that sensible—making differences in professional military judgment versus analysis a stimulus for further exploration. War games are a superb tool for identifying these departures in the following way: at the end of the analytical process when draft conclusions have been reached, a war game incorporating those draft conclusions is conducted with uniformed war fighters as the players. The game is a powerful vehicle for communicating the analytical results and an equally powerful way of eliciting professional military judgments about those results. Differences of opinion become obvious in the process and serve as the starting point for further exploration.

There are two other areas in which war games are invaluable. The first is education, the second is what is called inspired practice. With respect to the research on education in war games, anecdotes, and personal experiences support the view that applying classroom lessons in the field is as potent a pedagogical method as classroom teaching. War games are an excellent way to provide that sort of learning. They are widely used in all of the U.S. military institutions, providing undergraduate and graduate-level education. An associated benefit of learning in this way is socialization as war-gaming permits individuals, who may have little contact or who may be bureaucratic competitors, to come together in a common endeavor. Invaluable bonds between individuals and organizations are thereby established. Similarly, war games are often an occasion when even colleagues who see one another every day can take time for focused conversations.

Finally, war games, especially simple ones, are an important tool for providing military decision makers with opportunities to practice those decisions and evaluate their consequences. The point here is not to predict the future *per se*, though the sce-

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narios used and dynamics of the games must reflect a general sense of the broad contours of the future. The purpose rather, is to allow players to develop a deep familiarity with military problems and the choices they contain so that when players encounter the “real thing” they have a collection of experiences on which to draw. This type of inspired practice is, perhaps, the most important role war gaming can play, at least for uniformed war fighters.

WAR GAMES AND FUTURE WARFARE

Utilizing war games as a tool to explore the future of warfare has both its strengths and weaknesses. The current war with Iraq is likely to be a watershed event for the United States for several reasons. Among the substantial issues being played out is the United States’ future way of war, which in turn can be expected to influence strongly how the military is used in the future by the civilian leaders of the country.

In brief, there are two competing theories of future warfare under consideration today. The first can be called evolutionary, the second revolutionary. The first envisions steady improvements in precision weapons, aircraft, ships, armored vehicles, and command and control systems. The fundamental force structure of the United States would remain more-or-less the same, though its capabilities would substantially grow. With these improvements the United States would not drastically alter the way it conducts war today. By contrast, the revolutionary view envisions radical changes based primarily on the exploitation of advanced information technology. This involves the netting of military forces far more densely than today and is expected to produce a new way of war for the United States. Such a war would focus on the views and motivations of the adversary’s civilian and military leadership. A fledgling version of this approach apparently was attempted early in the war with Iraq.

The problem with revolutionary theories for warfare is testing them adequately. One would be hesitant to utilize revolutionary developments in warfare without being certain that a radical departure from current practices would be effective. Customarily, military planners rely on experiments and analyses, war games, and exercises of various kinds to test new tools for effective implementation. However, continuing the use of traditional approaches to military force and concept development for the examination of revolutionary theories for warfare could fall dangerously short of understanding the utility of revolutionary technology in combat. To understand the risks associated with tests that lag behind new tools of warfare a closer look at what underpins traditional approaches for concept development must be undertaken.

A NEW GAME IN TOWN

War gaming and the other approaches can be viewed as forms of modeling. A model is a representation of reality simplified to permit examination of the portions of the real world deemed to be of interest to the question at hand. The primary reason for developing models is cost-effectiveness. Done properly, they permit experimentation with a phenomenon for far less time and expense than if all experiments had to be done with the material phenomenon itself. Obviously this is especially important with military experimentation, since using real battles as experiments is unacceptable—despite what movie script writers might imagine. The objective of experimentation is to understand the phenomenon at issue so well as to be able to predict its behavior in the real world. In military analysis, the ability to predict the behavior of alternative military forces, at least to some limited degree, is essential to force and campaign planning and acquisition decisions. Being approximations of reality, models produce predictions which themselves can only be approximations of reality. The adequacy of a model is judged primarily by the quality of its output: does it consistently produce predictions that are sufficiently good approximations of reality as to productively address the problem? Judged against this criterion, two conclusions emerge. First, the models of war used by the U.S. defense community as the basis for war games and similar activities are and have always been inadequate, and that problem is becoming worse as the nature of war changes. Second, the remedy to this problem is not and probably has never been simply to create new military models (e.g., computer simulations, war games, and exercises).

Models of warfare, like war games, begin by making some categorical distinctions with consequences. Models of warfare can portray conflict on land, air, sea, or space, and between individuals, single systems or weapons, small collections or units, large collections or units, and so on. Using the criterion of predictive accuracy, the best models of warfare depict isolated engagements involving a single weapon and a target (e.g. a projectile striking a piece of armor plate or a missile warhead exploding within a given distance of part of an aircraft). Such engagements can be modeled with relative accuracy because they can be based on extensive laboratory and firing range experiments, which are reasonably reliable.

All would be well if warfare between collections of systems and individuals (i.e. units) could be portrayed accurately simply by aggregating or scaling up the results of the models of individual engagements. Attempts have been made to do this, but they have failed primarily because the outcome of warfare between collections of units depends on factors not present in individual engagements of weapons or individuals: the effects of combined arms, maneuvers, the role of information, command and control,

cohesion and morale, and so on. It seems clear, therefore, that to model future warfare involving units with gaming or any other technique, data must be gathered and models developed that include those factors expected to dominate future warfare.

Unit modeling is where the trouble begins, and this trouble is not new. Nor is trouble solely the product of the changes anticipated in the character of warfare, though these changes probably amplify the severity of the predicament. Briefly put, no one understands or has ever understood warfare between large aggregations well enough to

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develop particularly good models. Care should be taken to define what is meant by particularly good. We have developed models good enough to predict whether the addition or subtraction of a capability or a unit will upgrade or degrade performance of the force as a whole. This can probably be extended to such questions as whether changing the time of a unit's entry into the battle improves or diminishes

performance of the force. Using warfare models to predict more demanding matters such as casualties, ground gained or lost, or time related events is well beyond their capability. Moreover, the models are not and never have been adequate to support predictions of how much performance may be improved or diminished by adding or subtracting an asset or unit. These limitations have not stopped the continued use of these models, like war games, to grapple with the problems in concepts of future warfare. After all, something is needed to help us think carefully about these going concerns, and war games, even with their limitations, have been better than nothing. But better than nothing is a low standard, hence the conclusion that these models are not and have never been particularly good. The problem deepens considerably when we not only use these methods for tasks they cannot perform, but when we also forget or are unaware of that fact.

The evidence for the inadequacy of all models of large-scale warfare is to be found in the literature documenting the defense community's efforts to validate models like war games and computer simulations by comparing them with real warfare. For example, underpinning many war games and computer simulations are variants of the famous Lanchester equations. These equations represent warfare as based on the differing effects of quality and quantity of forces. They are simple, elegant, parsimonious, and provocative. The problem is that they have never consistently succeeded in meeting the fundamental test a model must meet: prediction of the real world. In this case, the Lanchester equations have been tested many times by comparing the results of real battles with the predicted results produced by the model. In almost every case, the

differences have been pronounced. The Lanchester equations can be made to fit real world battles, but only to the battle for which they have been specifically tuned. Put another way, the equations with coefficients tuned to fit Iwo Jima are *sui generis* for that battle. So, even with these occasional successes, it cannot be said that the Lanchester equations have ever constituted a generalizable model of warfare.

Nevertheless the successful results have been alluring enough to suggest that the original Lanchester equations, though deficient, may contain the roots of an adequate model if modified. This prospect has led to decades of research to produce the variants that redeem the apparent promise of the Lanchester equations by adding new coefficients and terms. In general, these have been intended to account for the variables missing in the original equations; e.g., morale, information, logistics, competence, weather, air power, combined arms effects, and the like. Unfortunately, the increased complexity has not resulted in improved performance. As before, it is possible to fit the output of the variants to some real battles. But this still requires a tuning of the equations until the output reaches the desired convergence with the real world. Again, note that the coefficient values are not assigned *a priori* based on the battle data, as would be needed for a strong test of the model. Instead the fits are obtained by tuning the equations more-or-less blindly until the fit is produced, which is a much weaker process in terms of what conclusions can be drawn. Again, the simple act of adding variables can itself be expected to tighten the fit, but that does not constitute an improvement per se or signal an increased understanding of warfare. Unfortunately, even with these problems, the modified Lanchester equations have lain at the heart of almost all computer-driven, higher-level warfare simulations for four decades. Today they lie at the heart of all war games intended to help in the assessment of the competing theories of future warfare.

In other fields of research, the response to a poorly performing model is to either discard the model or to improve the adequacy of the data underpinning the model. Unfortunately, in the field of military research, this process is malfunctioning, and it is important to diagnose the reason why no progress can be expected. The original Lanchester equations were based on air combat data, and it was not until after World War Two that attempts began in earnest to empirically test them against other types of warfare, primarily land combat supported by air. Enormous amounts of data from that conflict and the Korean War have been collected and analyzed with the unsatisfactory results noted above. The difficulties in producing good fits stimulated the modifications and the development of more data to support them. Yet, this too, has failed to produce good fits consistently. Perhaps more data could be collected. Perhaps the existing data can be further refined. However, the probable reason for these repeated failures to produce a powerful model of warfare is that the existing models embodying the Lanchester equations are substantially wrong or incomplete, and the games and exercises based on

those models can be no better. If Lanchester and its variants have been generally unsuccessful at predicting the outcomes of battles for which rich data exists, we should have even less confidence in the model and its descendents for wars that differ substantially from its database of World War II, Korea, and the 1973 war in the Middle East.

GAMING THE PROMISE OF A REVOLUTION IN MILITARY AFFAIRS

Many people today are struck by the promise of a revolution in military affairs to radically change the nature of warfare. Much argument has been devoted to the pros and cons of its hypothetical innovations. In particular, the possibility that future wars could be decided with a minimum of physical destruction of targets is very attractive. Ideas such as network-centric operations and effects-based operations are anticipated to give the United States a reliable capability to conduct warfare primarily through actions intended to influence, confuse, and deceive the thinking of the adversary at every level. Such operations are thought by many to obviate war's historic focus on winning wars by physically denying the adversary the means to wage it. There futuristic concepts merit that promise, but getting beyond the realm of essay writing, briefings, and rather shaky war games has proven difficult. This is because, as poor as they are at predicting the results of warfare fought with current concepts, the existing models of warfare are virtually of no value at all in exploring such futuristic notions.

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What is to be done? The critical point to convey is that the problem cannot be remedied simply by building improved models of warfare. That remedy would be satisfactory if the problems we suffer from were results only of inadequate models, but that is to mistake the symptom for the cause. The models can only be as good as the data behind them. As discussed earlier, the models using the given data have never been very accurate, and attempts to improve both the data and the models have led to no greater ability to predict the course of warfare in its current incarnation. Those data and models are even less adequate now that warfare seems to be changing at an accelerating rate. Therefore, building a new model cannot solve the root of the problem, yet that is precisely the course of action taken so far. A new generation of warfare models to be used by the defense community is being developed by the Defense Department. Research institutions and individuals are developing other new models. Certainly these can be regarded as improvements in the technical sense: they are more sophisticated, easier to use in some ways, faster, more flexible, more user-friendly, and more transparent. However, a model's output, which in the end is all that matters, can never be better than the currently defective model of warfare it embodies.

The problem designers face is to develop a model of warfare that is adequate to represent the way future military forces can be expected to behave, while basing it on

data relevant to the developments projected for the future. We are far enough from warfare embodied by the data of World War Two, Korea, and the 1973 Middle East war that we must start over again in some respects. This can be seen as an opportunity to do better this time what we never did all that well. We do not possess broad, systematic, and reliable data on the performance of military forces equipped and organized to use powerful new technologies, especially information technology. With the possible exception of the Army's attempt to transform itself, experiments with futuristic forces and concepts have been piecemeal and unreliable. Nevertheless the results have been tantalizing, hinting, as they do, that major increases in combat power can be obtained by netting assets together. Some business analogies suggest the same conclusion. But these kinds of data are hardly a sufficient basis for models of warfare, much less force-planning decisions.

There seems to be no escape from the need of all three Armed Services, the joint community, and the defense community as a whole to regard the next decade as an unparalleled opportunity for the most searching, dispassionate, and intense campaign of experimentation ever undertaken by this country. We would be properly outraged if medical science undertook to enter a new era without exhaustive experimentation. We would also be outraged if medical science disregarded great opportunities to improve therapies simply because they did not wish to undertake the proper experiments. It is hard to justify taking military analysis less seriously. The objective of this campaign of experimentation is to understand empirically what the nature of war in this new era will be.

CONCLUSION

To be useful, a program of experimentation must involve the equipping of real forces with futuristic capabilities and the exercise of those forces under realistic conditions permitting observation and measurement. The United States has a huge advantage in this area that is rarely cited when net assessments are performed—instrumented test areas. There is no escaping the need for experiments with real forces, because much of our knowledge about future war has been built literally from the ground up with questions such as: Are netted forces more effective, and, if so, by how much? At what level of organization are the advantages and disadvantages most evident? Are such forces fragile in some ways? How dangerous is the prospect of too much information? Under what conditions is that likely to occur? Can command and control be flattened and

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decentralized? At what levels? Do netted military forces become self-synchronizing and self-organizing? When? Is that always a good thing? Experiments with real forces, of course, suffer from a great problem, which is their expense in money, time, and personnel. These resources are in particularly short supply in the U.S. military, at least in relation to current commitments. This is one reason why experimentation to date has not received a high priority.

War gaming can greatly reduce the costs of the experimentation program. Indeed, properly designed, war games can constitute the bulk of the experiments themselves. To do so, these games have to be carefully conceived and tightly coupled to the experiments in the field. Like the experiments with real forces, the war games will have

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to begin by exploring future war at the metaphoric atomic level: the behavior of a small number of assets or units with advanced capabilities, particularly those concerning the col-

lection, distribution, and use of information. The tactical problems to be assessed will be uncomplicated at the beginning and grow in complexity with our growth in reliable knowledge. The results of the war games will be carefully compared to the results of field experiments, allowing for many war gaming experiments to be evaluated against experiments with real forces. By this process of iteration and accretion, we can steadily expand our area of sound information and raise the level of the experiments to increasingly larger numbers of units and higher-level command echelons. The results can then be used to inform computer-driven model building, which will make experimentation all the more cost-effective. If this course is pursued intensely, two or three years should be sufficient to obtain a basic, fundamental idea of what future war will look like, and, in particular, whether the hypothesized advantages of network-centric operations can be realized.

To accomplish this goal, the military services and the joint community must allocate resources for experimentation to include creation of experimental units and procurement of experimental equipment (or the ability to simulate it). Again, these need not involve large numbers, at least not initially, but even small investments can be expected to be painful in this resource environment. For war gaming specifically, resources will be needed to provide facilities that can simulate the environment of future warfare, particularly the information environment. This requires computing power, bandwidth, and information processing and display capabilities. Compared to field experiments, the resources required for sophisticated war gaming are small.

Finally, a cautionary note is in order. Even in war gaming, it is tempting to con-

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strue the central problem as technological. That is, if we buy the technology, we answer our questions. This is fundamentally confused. Our central task is to design and evaluate strong war gaming experiments with intellectual clarity and objectivity. If we succeed at that, the technology, though important, is secondary. ●