Paradigm-Level Issues in M&S: Historical Lessons and Current Challenges

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ABSTRACT

The paper discusses alternative ways to think about the modeling endeavor; the importance of including qualitative factors (i.e., "soft factors") despite critics who think doing so reduces rigor; the fundamental necessity of worrying seriously about uncertainty from the outset, including the kinds of uncertainty present in complex adaptive systems; and about implications for design of models. The paper's admonitions would be straightforward except that they fly in the face of common organizational practice, which is to avoid soft factors, ignore uncertainty by obsessing on standard cases, and use models ill-designed for serious uncertainty analysis.

ABOUT THE AUTHOR

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INTRODUCTION

This paper comments on some paradigm-level issues that have: appeared recurrently over the years; been affected by historical developments; have caused me to ponder and adapt; and remain salient today. The issues are

- How should we think about models? What are models supposed to be?
- What do we do about the soft and squishy aspects of the world? About the world of complex adaptive systems? And, most generally, about uncertainty when it's more than just an annoyance on the margin?
- What about designing models? Should we proceed bottom-up, top-down, or how?

Let me address each in turn.

HOW SHOULD WE THINK ABOUT MODELS?

The very conception of "model" varies drastically within and across communities (e.g., those concerned with force planning, acquisition, operations, and training). Let me contrast two views.

Models as Tools

To some, models are essentially tools. The application (e.g., analysis) is central and models should be assessed for their usefulness in accomplishing what is needed for the problem at hand. They need not be more accurate, precise, instructive, or comprehensive than necessary for the context. A procedural checklist is a kind of model; an empirical plot showing the cost of an aircraft as a function of its weight and technological generation is another. So also is an attrition-based warfare model used to assess the adequacy of a future force structure by calculating whether, in the mechanistic attrition battle envisioned by the model, the force in question would fare poorly or well against some defined adversary force. I use this last example because related models have played a big role in DoD's work even though the more thoughtful users of the models have often made clear that they did not see a close link to reality, often quoting George Box to the effect that (Box, 1979)

All models are wrong but some are useful.

This models-as-tools viewpoint arises also in exercises. One of the important "aha" moments for those involved in relatively high-level exercises is sometimes "Oh, the models don't have to be right; they merely need to assure that the consequences of actions are in the right direction so as to keep the flow going usefully and achieve the intent of the training."

Another version of the models-as-tools theme comes when economists, statisticians, and operations researchers among others see models as data-driven prediction-generators, i.e., as black boxes that generate predictions based on available inputs. They may not care whether the structural character of the models relates well to the phenomena of interest; rather, they care about predictive value. The model itself may be nothing more than some regression structure. When a statistician uses the term "explain," as in "X explains 25% of the effect observed," he is saying nothing about the physical world. Rather, he is saying merely that the X term in a linear regression accounts for 25% of the overall variance. Despite their dubious basis in phenomenology, empirical models can be very, very useful (Davis, 2009). Indeed, the model-as-tool paradigm has a strong base in experience.

Models as Representations of Knowledge

Scientists often look at models differently. They see many models as idealizations that record and organize insight about the actual world. A classic example is the ideal gas law, $PV=NkT$. This law is not general, since actual gas molecules have volume, which means that the exact law has correction terms except in limiting conditions. However, the ideal gas law expresses relationships among pressure, temperature, and volume meaningfully. Further, it can be derived coherently from fundamental laws of physics. All of this means that we "understand" the law. The model, then, may be a tool, but it's surely not a mere tool. It conveys knowledge.

Are models necessary to convey knowledge? When dealing with simple phenomena, a well-written essay can suffice. For somewhat more complicated knowledge we need mathematics, taxonomies, courses, and text books. These rely heavily on models that are idealizations such as homogeneous systems, linear systems, and point masses. Essays don't suffice.

This, however, is not where the story ends. Scientists, after all, aspire to understand everything knowable about our world. What happens when we are dealing with even more complicated systems? There are at least three paths worth distinguishing:

- Representing our knowledge in combinations of equations, diagrams, and even pseudo code, treating computer programs as technical implementations.
- Building computer models that represent our various types of knowledge, treating these models to some degree as surrogates for reality.
- Building computational models that reflect concepts that can be described relatively simply, but that manifest themselves in ways perhaps best understood by viewing results of computational experiments (simulations).

The first of these paths appeals to theorists. A computer program is seen rather like a machine doing the nitty gritty work of calculation, but the essence of knowledge is in the higher level representations. Consider Maxwell's equations, Liouville equations, or Feynman diagrams in physics, or the diagrams and interface specifications used by system engineers.

To illustrate the second path, consider someone using M&S to represent a real-world physical system as closely as possible. This might be someone involved in both command and control and, e.g., fleet exercises or mission rehearsals. Such a person uses the same displays to show outputs from simulation on the one hand, or from sensor data from real ongoing operations on the other. In an exercise, identical moving objects on a display may be real while others are simulated; it may or may not be possible to tell the difference. As an different, early example, consider that the mechanized forces going into battle in Desert Storm were seeing what they had "seen" and experienced in their live, virtual, and constructive simulations in peacetime (see the Battle of 77 Easting discussed in Neyland, 1997).

The third path is being pursued by many who are using agent-based modeling, some with their intentions candidly expressed in titles such as artificial life or artificial societies (Epstein and Axtell, 1996). It has also been pursued in remarkable and provocative ways by some, such as Stephen Wolfram, in his New Kind of Science (Wolfram, 2002).

All three of these paths, and others I have not thought to list, are clearly different from paths in which models are treated as mere tools.

Tools or Knowledge Base?

The reason for this section of the paper is that it seems to me useful to be explicitly aware of the tension between ways of thinking about models. We are wise to avoid joining one tribe or another, and to instead recognize that models can be seen in different ways and approached in different ways. Nonetheless, I would like to reinforce the following points before proceeding with the rest of the paper:

- Models are the primary means by which we record and communicate our deepest knowledge of complicated and complex systems.
- Robot-building and computational experiments with generative models are crucial new mechanisms for learning about complex systems, both artificial systems and, quite likely, real systems.

Yes, some models are just tools, but others are something more. They are not reality, but the distinctions are shrinking. This can be disquieting, as when we use describe model processing in human terms: the robot must "assess the situation, consider options, and choose among them, having learned from experience." Or consider the comments of biologist Richard Dawkins in his book about evolution science (Dawkins, 1996, p.59)

> *Nothing in my biologist's intuition, nothing in my 20 years' experience of programming computers, and nothing in my wildest dreams prepared me for what emerged on the screen.... With a wild surmise, I began to breed, generation after generation, from whichever child looked most like an insect. My incredulity grew in parallel with the evolving resemblance.... I still cannot conceal from you my feeling of exultation as I first watched these exquisite creatures emerging before my eyes. I distinctly heard the triumphal opening chords of Also sprach Zarathustra (the '2001 theme') in my mind.*

SOFT FACTORS, UNCERTAINTY, AND COMPLEXITY

Soft Factors

I would like next to talk about three challenges that make modeling of the real world very difficult. First, we have the problem of soft (or "squishy") factors (variables). It might be nice if all the variables that mattered could be precisely defined and measured, but what do we do with such variables as: team quality; leadership quality; determination; loyalty (to family, friends, group, nation, religion,...); respect; trust...; distrust; and love/hate,...If models are intended to describe aspects of the real world, then these must be part of the models.

This might seem unexceptionable, but a classic view of both analysts and modelers in operations research, systems analysis, physics, and engineering has long been that the variables of the model should be observable and quantitative so as to be rigorous. It has often been claimed that if we can't measure a variable quantitatively, we don't know what we're talking about. For this and other reasons, the Department of Defense favored a style of M&S throughout the cold war. The resulting models were good at dealing with "hard," quantitative matters such as the number of tanks, their range and firing rate, and their vulnerability. For the most part, however, they ignored soft factors despite their criticality in actual wars. This could be rationalized for strategic planning because using such models imposed a kind of discipline and built in certain desirable aspects of conservatism. There are arguments on both sides of this matter, but if we turn to asking not about force-structure planning but rather at-the-time military balances or war prospects, no one except mathematicians would ignore the qualitative considerations. Did the British give up the Falklands when challenged? Did the Israelis preemptively surrender to the Arabs in the early 1970s? Is not the hearts-and-minds theme of modern insurgency doctrine quintessentially "soft?" and therefore not ignorable?

My point here is that the omission of qualitative considerations from DoD's modeling and simulation has long been an outrage to those of us who believe that models should be "serious," not mere tools. A colleague and I expressed our views on this matter two decades ago in a paper that caused remarkably negative reactions from some, but was applauded by others (Davis and Blumenthal, 1991). Our observations mirrored the empirical observations of the late military historian Trevor Dupuy (Dupuy, 1987).

A chronic bugaboo stemmed from the myth that soft variables should and could be ignored, in part for the sake of conservatism. The reality is that to ignore such a variable is equivalent to assuming in the model that it has zero value (or a value of 1 if it appears as a multiplier) (Forrester, 1969). That, of course, is patently ridiculous for many of the factors. Nor is the assumption "conservative;" it's just wrong. As soon as one realizes the folly of ignoring "soft" variables, one can quickly accommodate to the necessity of estimating their effects from history and other experience, or of exploring the consequences of their having different values. Often, such factors can be game-changers, such as factor-of-two effects in an otherwise close-call situation. They are ignored at our peril.

Complexity

Let us distinguish here between "complicated" and "complex." System engineers routinely deal with highly complicated systems such as aircraft that have numerous components and subcomponents. "Complexity," however, is different. I am using the term here in the sense of complex adaptive systems (CAS), which is still a relatively new domain of study even though much discussed or nearly two decades, particularly since an exciting era of work at the Santa Fe Institute (Waldrop, 1992; Holland and Mimaugh, 1996; for a text, Bar-Yam, 2005). The need to embrace the basic concepts of CAS theory in defense M&S was highlighted in a recent national-academy report (National Research Council, 2006).

The usual concept of a CAS is of a system with a large number of interacting entities (usually called agents) that change behaviors as their environment changes. The changes may be governed by remarkably simple rules or by something richer. A defining characteristic of CAS is that they exhibit so-called emergent properties, properties of the aggregate that are not obvious from an understanding of the elements. If a crowd turns into a mob, the "mob" has emerged. All nontrivial social systems are complex adaptive systems. Think of social movements, rebellions, or even the phenomenon of a coherent theme "emerging" in the final week of an intensive summer study, after weeks of cacophony.

Such phenomena matter greatly in today's national-security work dealing with subjects such as terrorism, counterterrorism, and stability operations. These subjects are inherently human-centric and we find ourselves talking about the hearts and minds of the people, or cases in which "the people turn" against the violent extremists. The Jihadis, in contrast, believe that they are the vanguard of a movement.

Uncertainty

The existence of soft factors and the need to embrace the CAS paradigm further enhance the need to see uncertainty as at the very core of good modeling and analysis. This has been the most bitter pill of all for many to accept. Life would be so much simpler, or so it would seem, if we could build models of this and that, make predictions, make good choices, and carry on. In truth, we might not like that world better than the one we're in, but in any case we live in a world in which a great deal is uncertain, predictions are often not worth the electrons expended to represent them, and choices are often impossible to make with confidence.

A serious and especially enjoyable rendition of this view is that of Nassim Talib's book, the Black Swan (Taleb, 2007), but authors (including me) have struggled with the same issues for many years (Davis, 1994, 2002). Our diagnoses have been similar, but our prescriptions different. My own approach has been to champion various ways of planning under uncertainty, such as so-called capabilities-based planning, or what I describe in long form as planning for flexibility, adaptiveness, and robustness (Davis, 2002). Flexibility, in this context, means the ability to take on other-than-expected missions and tasks, or to drastically adjust objectives. Adaptiveness refers to the ability to cope with other-than-expected circumstances, which may require changing tactics, mechanisms, and so on. Robustness refers to the ability to withstand and recover from disruptive events (usually adverse, but opportunity-presenting as well). The forces that invaded Iraq in 2003 were highly adaptive, as evidenced by the numerous adjustments made before and during the combat phase. However, neither the plans nor the forces were flexible enough to deal with the insurgency when it erupted. Shifting to a non-military example, the oil industry displayed an absence of robustness when the Gulf oil-spill disaster occurred earlier this year. The disaster was not merely the result of an erroneous calculation somewhere, or bad luck, but a failure to assure robustness.

Suppose that we accept the ubiquity and dominating significance of uncertainty. What implications does this have for modeling and simulation?

In my own work I have sought to structure the issues by distinguishing between structural and parametric uncertainty. That is, we may be unsure about how to build (i.e., to structure) a model so that it will correctly represent the phenomenon. Even if we do, there may be numerous uncertain parameters in the resulting model. Let me discuss each of these, but in reverse order.

Parametric Uncertainty

Given a reasonably correct structure of a model, we may still find ourselves faced with uncertainty that is both broad and deep. This is reflected in "input data" (parameters) of the model. We can proceed with "best estimate" values of the parameters, but that can be an analytic atrocity if uncertainties are profound (as they usually are in strategic planning).

To illustrate, consider that for many years, the canonical approach to defense planning was based on specific point scenarios. Despite policymakers referring to them as merely illustrative, their organizations interpreted the scenarios as expressions of necessary and sufficient conditions, i.e., of "requirements." Complicated models were constructed, massive data bases assembled and blessed, and cases run so as to inform decisions about what to buy and how much was enough—based on those point scenarios. Even a cursory look at the analysis, however, would quickly demonstrate that the single planning scenario was neither a meaningful best estimate nor a prudent worse-than-expected case. It was just a case decided upon by a well-meaning committee. Such planning systematically buried numerous profound uncertainties about how conflicts would arise, the warning time that would be available, the adversary's strategy, operational objectives, and so on.

So what? The following illustrate some follies over the decades, all of which resulted from failing to consider a broader range of possibilities. All of the examples remain vivid from my personal experiences both inside and outside of government.

1. 1978-1980. Many analysts using normal scenarios and models recommended eliminating light divisions because mechanized divisions were more cost-effective in simulated Central Region conflicts.
2. 1980s. Analysts (and organizations) dealing with the Persian Gulf focused so exclusively on the Soviet threat as to be unprepared for the far more credible threat from Iraq. The United States was still in the relatively early stages of reorienting its thinking to the Iraqi problem when Saddam invaded Kuwait in 1990.
3. 1980s. Planning focused so much on major regional conflicts as to leave the U.S. and NATO with very little capability for crises such as arose in the Balkans in the 1990s. What could we do to "stop the killing?" The only answer, it seemed, was massive bombing—not exactly an advertisement for U.S. inventiveness and subtlety.
4. Mid 1990s. Despite the breakthroughs in technology and demonstrations of capability in both field tests and certain battles of the 1991 war, there was great resistance to adjusting scenarios or models to exploit the revolutionary lethality of air power.

5. Late1990s. Despite what might have been the lessons of 1990, planning scenarios in the 1990s continued to assume substantial actionable warning before the start of conflict in the event of a second war with Iraq. Further, the focus was almost exclusively on defense.
6. Turn of the Century. The table turned, air power enthusiasts focused attention on scenarios in which air power could indeed be decisive, but downplayed other cases in which adversaries did not cooperate.

In the late 1990s thinking about planning began to change and in 2001 Secretary of Defense Donald Rumsfield introduced capabilities-based planing. Resistance continued, however. It was claimed that it was too difficult to build multiple data bases and models for diverse kinds of conflict in diverse circumstances. Thus, much of the canonical work continued to focus on the convenient big-war point scenarios. .

After 9/11, of course, everything changed. Except that it didn't. Modeling and analysis continued to focus unduly on big wars of a standard variety. By the mid 1990s (i.e., the 2006 Quadrennial Review), senior policymakers had lost patience. They dramatized this with a famous "quad chart" contrasting traditional, irregular, disruptive, and catastrophic warfare.

In recent years the DoD analytic community has done much to rethink its work and diversify its approaches. Much of this has occurred within the activity referred to as the Analytic Agenda, which now includes a rich set of challenge cases and numerous analytic approaches that range from traditional campaign modeling to human war gaming and use of simple conceptual models to represent social science, as discussed below. There has been distinct progress, but uncertainty analysis is still resisted—especially by organizational behavior. The most recent Quadrennial Defense Review (2010) does better, examining what it hoped was a "spanning set" of scenarios to test proposed force structures in different ways (see also Davis, Shaver, and Beck, 2008, which illustrates this in the context of global-strike options).

Structural Uncertainty

The above discussion imagined that we had a correct model, just uncertainty about input values. Structural uncertainty is worse. It can be paralyzing and it most cases it is simply essential to buckle down and do the research necessary to understand the phenomenon (or, failing that, to collect so much data as to be able to put together a reliable empirical model). In some cases, however, such as imagining future military conflicts, future terrorist attacks, or even future natural disasters, there are limits to what can be accomplished. What then? Further, when dealing with the challenges of irregular warfare, it quickly becomes clear that mathematical models are not a natural way to proceed when attempting to inform people-centric strategy. What, then, does one do?

Some suggestions for this difficult challenge are as follows:

1. Possibility Space. Use human gaming and comparably open-minded versions of M&S to understand the possibility space. I mention human gaming because humans are still better than machines alone for finding the plausible tactics of an intelligent adversary, imagining the ways things could go wrong, and conceiving creative solutions (including "cheating"). This is no longer a clear-cut matter, however, and well-conceived computational experiments can also help greatly in opening minds. Merely as one example, my colleagues and I did a study in which we built on results of a human brain-storming exercise, constructed a simulation to generate possible scenarios reflecting notions of that exercise, and then allowed for influences to have a random component so that certain events and actions would sometimes have opposite-than-expected effects (sound familiar from the real world)? We then generated a massive number of possible futures, not for the sake of the numbers, but to explore more comprehensively so as to recognize possibility patterns that might be both important for planning but ordinarily ignored. Thus, this work was in the realm of divergent, imagination-improving thinking (Davis, Bankes, and Egner, 2007). My own conclusion at the end was that most of the substantive possibilities had been foreseen by creative analysts, but that a valuable few had become visible as the result of the computational experiments. Similarly, in a more hard-core analytic study of options for global strike my colleagues and I developed a computational approach for generating options "mindlessly" and discovered, to our chagrin, that we had missed some important options when relying only on our allegedly sound but creative thinking (Davis, Shave, Gvineria, and Beck, 2008; Davis et al, 2008).
2. Alternative Models. A very different example of dealing with structural uncertainty is constructing alternative models of the same phenomenon so as to open our minds. The example I will use here involves alternative models of the adversary.

Historically, major failures in foreign policy have come about because of a failure to understand the adversary's mindset. Sometimes the adversary has been perceived as far more evil, reckless and determined than was the case; other times, intentions and determination have been underestimated. The problem, in my view, is the tyranny of the best estimate. In 1990, for example, U.S. intelligence was so confident about its perception of Saddam as to altogether discount the possibility of his invading Kuwait. What can be done? In the case of Saddam, a colleague and I constructed alternative "Saddam" models, one of which turned out to be prescient, not only with respect to the invasion, but also to Saddam's subsequent behavior. Our purpose was not to get it "right," but rather to encourage well-hedged strategy formulation. That basic concept continues to be sound and, in my view, should become part of doctrine. This is very different, by the way, than what comes out of minimax thinking in game theory. It is a matter of identifying different mindsets, patterns of reasoning, and consequences for hedging (Davis, Kulick, and Egner, 2005; Davis, 2010). A core concept here is that by opening the mind to more than one possibility, the floodgates open to imagination and recognizing the need for hedging and potential adaptation.

3. Conceptual Models. Over the last two years, my colleagues and I have done a good deal of work on constructing relatively coherent integrations of the social science knowledge relevant to counterterrorism and, more recently, stability operations. Here we have assiduously avoided building sophisticated computer models. Instead, we have put the most emphasis on developing simple diagrammatic representations of the factors at work. Called "factor trees," these can be regarded as simplified influence diagrams in disguise (see Figure 1). Their nature, however, is such as to maximize the ability to communicate and debate issues, and to identify major strands of causality that can be used to inform everything from intelligence collection to operational planning (Davis and Cragin, 2009). We did not attempt to specify the structure, but only to coherently describe the factors at work.

Hedging, not Narrowing. Why does all this matter? Well, suppose that we are studying irregular warfare. The intent should not be to look at the vast array of notions, speculations, and parochial "theories," and to then down-select to the most favored (a suggestion that I have heard offered up seriously). If the model is to represent our knowledge, then it will need to reflect a diversity of cause-effect relationships rather than merely those of some committee's "best estimate." Why? Because, depending on circumstances, different factors and processes will dominate what happens; in any one circumstance, there might well be a great simplification possible, but that simplification will be quite different for another circumstance. To be less abstract, consider the role of religion. Sometimes, religion is a causal factor in terrorism; sometimes it is a result of becoming an extremist, a part of indoctrination; sometimes, it plays no role at all. Similarly, financial incentives may be dominant when urban youths are paid to lay IEDs, but they are often not at all relevant to religiously committed Jihadis. Strategy, then, must allow local tailoring of tactics to fit the situation.

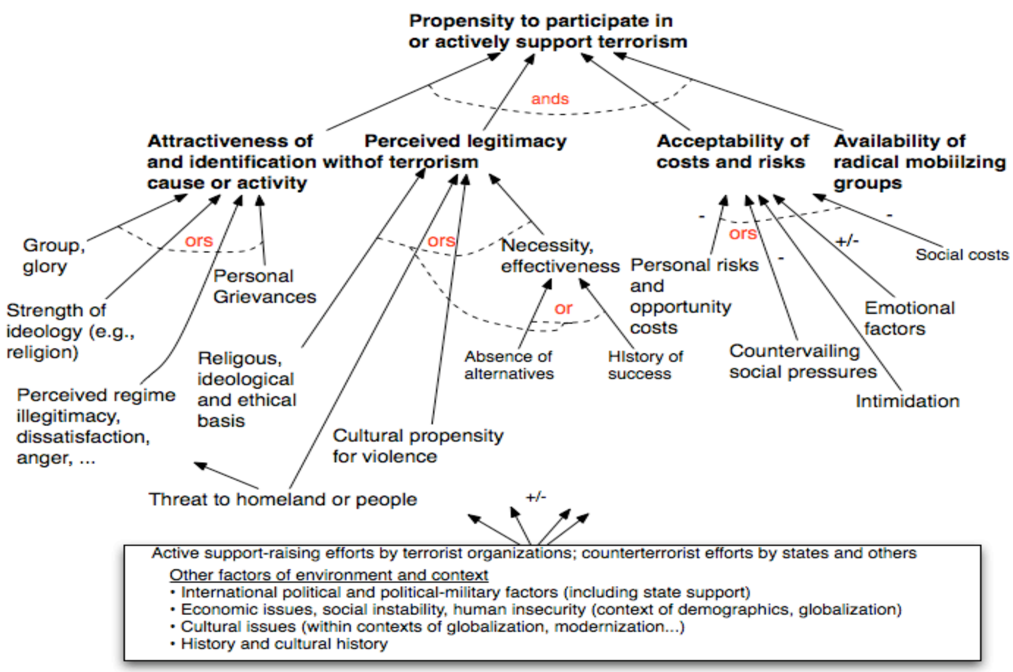
DESIGN IN M&S

The last topic I intend to discuss, albeit briefly, is design. Some of those involved with M&S think and operate top-down; others are bottom-up in inclination. Who is right? This issue is ultimately one of those paradigm-level disputes that will go on forever—not only because people have different predilections and skills, but because there are conflicting considerations.

The answer to "who is right? is, of course, "neither." Let me make the following observations, however (Davis and Bigelow, 1998; Davis, 2002):

- Strategic planners need high-level structures with relatively few variables. They need to be able to see the whole and to be able to reason coherently with relatively few variables. Because of the uncertainties mentioned above, they also need to be able to ask numerous what-if questions, and to explore the consequences of many different assumptions. All of this calls for low-resolution models and exploratory analysis (Davis, 2002).
- System engineers and commanders also have such needs. Thus, the virtues of relatively simple low-resolution models and exploratory analysis apply across such categories as strategic planning, acquisition, training and operations, and personnel

Figure 1. An Illustrative Factor Tree



The real world, however, is truly complex. Details matter. Indeed, many seemingly compelling top-level structures prove wrong. Further, evaluating the high-level variables can depend on very detailed work. Efforts to oversimplify will almost necessarily eliminate the ability to represent our full knowledge.

These observations suggest a top-down approach with drill-down capability (Davis and Dreyer, 2009). That may seem to relate more to viewing results than to designing models, but in fact it has major implications for design. If the questions to be answered involve high-level variables, and if exploring the consequences of uncertainty is important, then it is very difficult for modelers to respond well unless their models are designed accordingly. Unfortunately, what that means is by no means straightforward. A basic problem is that when one examines systems in any detail, one begins to see that "everything is connected to everything." That is somewhat of an exaggeration, but it doesn't feel that way when one is in the middle of the modeling. Further, it often seems that "everything matters."

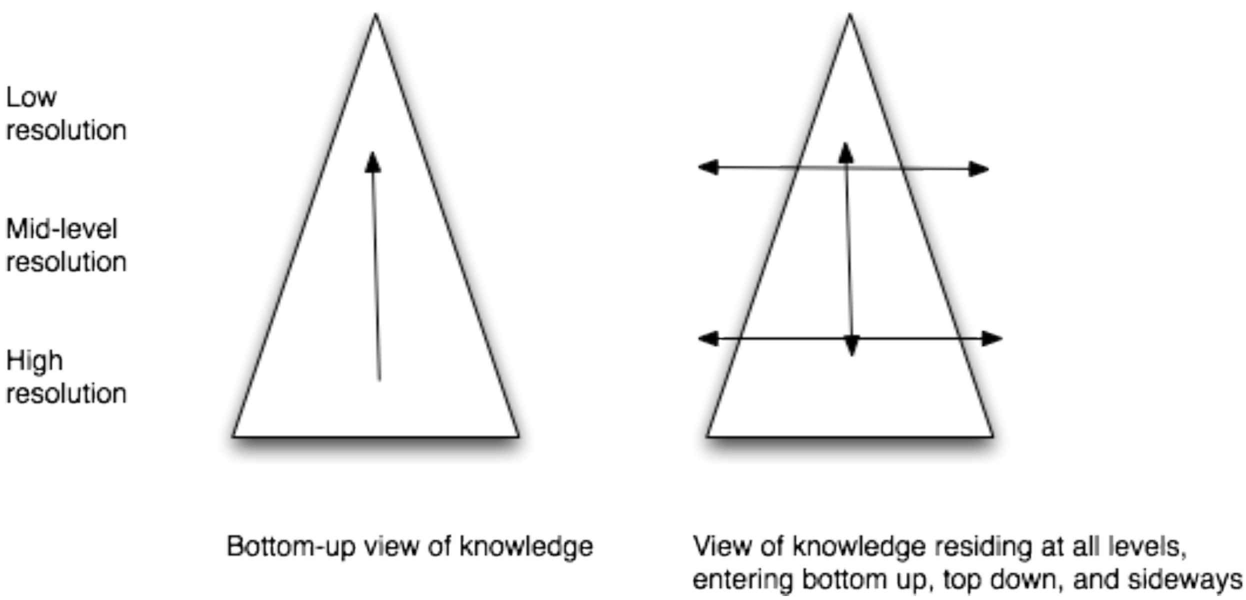
There is no perfect solution to these problems, but my own conclusion, reinforced over the years, is that we need to teach, think, and design routinely with multiple levels of resolution in mind. Further, we need to understand that knowledge comes not just bottom up (the conceit of many simulationists, nor top down (the goofy notion of those who sit at the top), but in all directions (Figure 2, adapted from an earlier study (National Research Council, 1997)). Our aspiration should be to have a set of models that, taken together, allow us to represent all of our knowledge in a self-consistent way, and to apply it in a diversity of ways that require different levels of detail and, often, different perspectives .

In many cases, it is possible to design an individual model to have multiresolution features, by which I mean the option to enter the problem (i.e., enter the inputs) at alternative levels of detail. Sometimes, this can even be done rigorously, but more often it will depend upon approximations.

When dealing with larger systems, designing an entire model with multiresolution capability is difficult or impractical. It then becomes desirable to think in terms of model families. A low-resolution member may be designed from a good but aggregate-level conception of the problem, or it may be what I call a "motivated metamodel," a model that has some core structure suggested by an aggregate-level concept, but with numerous potential correction factors. The motivated metamodel can, in some cases, be tuned to agree with results from computational experiments with a more detailed model, or with results from physical-world experimentation. This approach is quite different from normal statistical modeling, which may reject with prejudice any a priori notion of model structure. It is, however, a more fruitful form of inquiry in my opinion, especially for scientists (a point also made in .Box, Hunter, and Hunter, 2005). Once one gets into the spirit of having a family of models, it follows rather easily than one actually needs a family of tools that include man-machine simulations, human war gaming, and historical analysis among other things. This paradigm is to be recommended for analytic organizations, as discussed in a national academy report for the Navy (National Research Council, 2005).

As a final observation on the matter of design and issues of top-down versus bottom-up, I would like to emphasize that the very terminology is terribly confused and, in some ways updated. To illustrate this, I note that the Nobel physicist Murray Gel Mann correctly describes agent-based modeling in the study of artificial life as top-down, whereas most others think of it as bottom-up (Gell Mann 1994; http://edge.org/documents/ThirdCulture/zc-Ch.19.html).'

Figure 2.
Old and New Paradigms for Knowledge Flow



Similarly, many people think of the protocol that enabled growth of the Internet as a bottom-up phenomenon, but it might instead be seen as the result of a highly enlightened top-down requirement (the protocol). Similarly, in the realm of network-centric operations we find ourselves seeking organizational structures and behaviors that have a great deal of distribution and decentralization, but those structures and the incentives that cause the behaviors may be top-down directed! The point, then, that when designing systems or M&S in the modern world, we need to be seeing things with a mix of top-down, bottom-up, and sideways perspectives, and to use the terminology in ways very different from in earlier years.

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