WHY
MODELS
DON'T
FORECAST

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Abstract

The title of this paper, "Why Models Don't Forecast", has a deceptively simple answer: models don't forecast because *people* forecast. Yet this statement has significant implications for computational social modeling and simulation in national security decision making. Specifically, it points to the need for robust approaches to the problem of how people and organizations develop, deploy, and use computational modeling and simulation technologies.

In the next twenty or so pages, I argue that the challenge of evaluating computational social modeling and simulation technologies extends far beyond verification and validation, and should include the relationship between a simulation technology and the people and organizations using it. This challenge of evaluation is not just one of usability and usefulness for technologies, but extends to the assessment of how new modeling and simulation technologies shape human and organizational judgment. The robust and systematic evaluation of organizational decision making processes, and the role of computational modeling and simulation technologies therein, is a critical problem for the organizations who promote, fund, develop, and seek to use computational social science tools, methods, and techniques in high-consequence decision making.

WHY MODELS DON'T FORECAST

Introduction

The title of this paper, "Why Models Don't Forecast", has a deceptively simple answer: models don't forecast because *people* forecast. Yet this statement has significant implications for computational social modeling and simulation in national security decision making. Specifically, it points to the need for robust approaches to the problem of how people and organizations develop, deploy, and use computational modeling and simulation technologies.

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Computational Social Science in the Post 9/11 World

Computational social science is a diverse, interdisciplinary field of study whose practitioners include (but are not limited to) computer scientists, physicists, engineers, anthropologists, sociologists, physicists, and psychologists. Computational social modeling and simulation has lineages in computer science, mathematics, game theory, sociology, anthropology, artificial intelligence, and psychology dating back to the 1950s. However, the application of computational simulation to social phenomena exploded in the 1990s due to a number of intellectual, social and technological trends. These included the popularization of complexity studies (Gleick, 1987; Wolfram, 2002); the rapid spread of personal computing throughout multiple facets of work and social life; the rise of electronic communications technologies, including the Internet, email, and cellular telephony (Eagle, 2010; Eagle et al., 2009; Eagle and Pentland, 2004); the subsequent explosion of interest in social networks (Watts, 2003a, 2003b, 2004; Watts and Strogtaz,1998; Barabasi, 2003); and the development of object-oriented programming. Together, these generated new sources of data about social phenomena, democratized computational simulation for researchers, and opened the door for a creative explosion in modeling methodologies and techniques (Macal and North, 2006; White, 2003).

Researchers in a range of fields see tremendous promise for computational social modeling and simulation as technology for producing knowledge about human behavior and society. Modeling usefully supports development and refinement of hypothesized causal relationships across social systems in ways that are difficult to achieve in the real world (Gilbert and Terna, 1999). For example, agent models allow researchers to develop artificial societies in which "social scientists can observe emergent behaviors in terms of complex dynamic social interaction patterns among autonomous agents that represent real-world entities" (Yilmaz, 2006). Moreover, researchers can and do use simulated data instead of, or in addition to, real-world data (Defense Science Board, 2009). Researchers in a range of fields are using these new modeling techniques to explore phenomena that are difficult to study in the real world because of ethical, temporal, or geographical constraints; and to implement conceptual models or theoretical abstractions and simulate outcomes using the computer as a kind of "*in silico*" laboratory (Tesfatsion, 2002; Epstein, 2006).

Perhaps not surprisingly, a kind of revolutionary excitement and anticipation permeates much of the interdisciplinary literature on computational social science (Koehler, 2000; Ormerod, 1995). For example, David Levin, professor of public policy at Harvard's Kennedy School recently argued that "social science will/should undergo a transformation over the next generation, driven by the availability of new data sources, as well as the computational power to analyze those data".[1] Many computational social scientists believe that we are on the brink of a computationally-driven paradigm shift that will change social science permanently (Epstein, 2006; Koehler, 2000; Ormerod, 1995). For example, political economist Joshua Epstein has argued that agent-based modeling and complexity thinking are driving a broader conceptual shift to an explanatory or ***generative*** social science in which the ability to computationally generate social phenomena becomes a standard for evaluating truth claims (Epstein, 2006, 1999).

A number of practitioners in computational social science not only see a promising future for computationally-enabled social research, but also believe that policy and decision makers would benefit from using computational modeling and simulation technologies to understand the complicated social, political, and economic events, and perhaps support the formation of more effective policies. For example, in the wake of the recent financial crisis, physicist J. Doyne Farmer and economist Duncan Foley argued in ***Nature*** that econometric and general equilibrium models are inadequate for understanding our complicated economic system, that agent-based models can help decision makers formulate better financial policies, and that an ambitious goal would be to create an "agent-based economic model capable of making useful forecasts of the real economy" (Farmer and Foley, 2009; p.686). Similarly, Joshua Epstein opined that policy and decision makers would benefit from using agent-based modeling techniques to understand the dynamics of pandemic flu and make appropriate interventions (Epstein, 2009).

This brings us to the issue of computational social science in national security policy and decision making. It is worth noting that as the Cold war was coming to an end in the late 1980s and early 1990s, computational social science was experiencing explosive growth. This confluence perhaps explains why so many decision makers in federal departments and agencies are looking to computational social science to meet some of these new technological needs. In particular, the 9/11 attacks mark an important turning point in the relationship between the computational social science community and

[1] http://www.iq.harvard.edu/blog/netgov/2009/02/paper_in_science_tomorrow_on_c.html

national security decision makers. The reader may recall how several observers working with open-source information (i.e., newspapers and the Internet) developed retrospective (and I emphasize the word *retrospective* since so much of the national security discussion in this regard is focused on forecasting) social network analyses that very clearly "connected the dots" among the attackers (Ressler, 2006). One highly publicized example came from organizational consultant Vladis Krebs who spent weeks combing through newspapers to find information about the hijackers, piecing together a sociogram that mapped relationships among the participants. Krebs argued that the Qa'ida network was optimally structured to address competing demands of secrecy and operational efficiency, and pointed out that social network might be useful as a diagnostic tool to identify and interdict criminal activities. Soon after, Krebs was asked to brief intelligence experts on the analysis and detection of covert networks (Krebs, 2002; Keefe, 2006; Bohannon, 2009).

Of course, the idea that analysts should have been able to forecast the 9/11 events using signs that are retrospectively obvious is a case of hindsight bias (MITRE, 2009; Heuer, 1999). Moreover, the government's failure to interdict the 9/11 plot before the attacks involved multiple failures beyond simply connecting the proverbial dots with or without a sociogram (National Commission on Terrorist Attacks Against the United States, 2004). Nevertheless, analyses like Krebs' drew both popular and government attention to the idea that arcane research areas like graph theory, social network analysis, and agent-based modeling might be predictive at a time when terrorism research was undergoing "explosive growth" as measured by publications, conferences, research centers, electronic databases, and funding channels (Reid and Chen, 2005). Over the past decade, a number of computational social scientists have argued that modeling and simulation techniques are uniquely suited to understanding the dynamics of emerging threats at a time when national security decision makers are urgently looking for new frameworks, data sources and technologies for making sense of the post 9/11 world (Silverman et al., 2007; Carley, 2003; Carley et al., 2004; Kuznar et al., 2009). Indeed, within the computational social science literature, there is a significant sub-category of post 9/11 academic and policy writing that examines how computational social modeling and simulation, particularly agent-based simulations in combination with social network analysis techniques, might enhance understanding of a wide range of national security problems, including state stability, insurgency warfare, bioterrorism, flu pandemics, and terrorist network detection (Krebs, 2010; Keefe, 2010; Bohannon, 2009; Weinberger, 2010; Gilbert, 2009; Sageman, 2004; and many others).

From Research to Decision Making

With this confluence, it is not surprising that agencies like the Department of Defense have made substantial dollar investments in social science including computational modeling and simulation for understanding human social, behavioral, and cultural patterns (US House of Representatives, 2008, p.5). National security decision makers, including those in the Department of Defense, can highlight a number of ways in which they would like to use computational social science techniques, including training simulations, characterization of adversary networks, and situational awareness. Among these, the ability to forecast is an implicit goal of many projects (Hayden, 2009, p. 25).

The expectation is that social science-based modeling and simulation tools can be used to forecast future social, political, and cultural trends and events; and that these forecasts will improve decision making.

Computational modeling and simulation technologies have played an important role in a wide range of human knowledge activities from academic research to organizational decision making. The utility of these technologies has been demonstrated over several decades of development and deployment in multiple fields from weather forecasting to experimental physics to finance. However, it is important to remember that computational modeling and simulation tools are ultimately human artifacts, and like all human artifacts they come with very real limitations. How we recognize and deal with these limitations depends very heavily on the context in which we are using models and simulations. After all, models and simulations have different lifecycles in scientific research contexts than they do in decision making contexts. Generally speaking, researchers use computational modeling and simulation to support knowledge-producing activities such as to refine conceptual models, examine parameter spaces, and identify data needs and possible sources to address knowledge gaps. Moreover, models and simulations that are embedded in ongoing cycles of scientific knowledge production benefit from continuous comparisons between empirical data/observations and model outputs as well as peer review.

Unlike researchers, decision makers often look to modeling and simulation technologies to help refine courses of action that may have very high public consequences. They are frequently dealing with problems characterized by high levels of epistemic uncertainty – i.e., lack of knowledge and data – and are addressing problems for which scientific and expert consensus may be neither mature nor fixed (Jasanoff, 1987). For decision makers, modeling and simulation technologies may be seen as useful "what if" tools to help them evolve their understanding of a problem space (Bankes, 1993, 2002). However, decision makers are probably not focused on improving the model's correctness or assessing how well it corresponds to a real-world phenomenon of interest. Decision makers tend to be more focused on identifying courses of action and moving forward and, in doing so, they typically face legal, economic, and political motivations and constraints that researchers do not. In the context of the national security community, decision makers may be addressing problems that involve high resource commitments or even human lives.

The contextual difference between research environments and decision making environments is a critical one that carries significant implications for the design, implementation, and evaluation of computational models and simulations. The decision to employ computational modeling and simulation technologies in high-consequence decision making implies a responsibility for evaluation, not just of the models themselves but assessments of how these technologies fit into, shape, and affect outcomes in the real world. Higher consequence decision spaces require proportionally greater attention to assessing the quality of data, methods, and technologies being brought to bear on the analysis as well as the analytic and decision making processes that rely on these technologies.

In this regard, I briefly highlight three areas of evaluation that I believe require careful attention for computational social science. These include verification and validation (V&V), human-computer interaction, and forecasting as an organizational (not computational) challenge.

Verification and Validation

Verification and validation (V&V) are processes that assess modeling and simulation technologies for internal correctness (verification), and external correspondence to real-world phenomena of interest (validation). There is an enormous body of literature dating back to the 1970s that addresses methods, techniques, tools, and challenges for V&V (Aigner, 1972; Sargent, 1985). Most of this research has been done in fields like computational engineering, artificial intelligence, and operations research. However, in the computational social science community, there is an emerging body of literature addressing the challenges of verifying and validating computational social science models and simulations (Yilmaz, 2006; Hayden, 2007; Turnley, 2004, p.8; Axtell et al., 1996; Fagiolo et al., 2006; Yahja, 2006; Wilenski and Rand, 2007; Windrum et al., 2007; McNamara et al., 2008; Moss, 2008; Carley, 1996; see also Breiger et al., 2003 and National Research Council, 2007.

I am not going to review the voluminous V&V literature here except to make two points: firstly, computational social modeling and simulation raises specific V&V issues that are probably unique to the social sciences. Secondly, despite the marked epistemic differences between computational social science and computational physics, engineering, or even operations research, the broader V&V literature does have lessons for organizations investing in predictive computational social science.

Verification and validation in computational physics and engineering is both similar to and divergent from computational social science. For example, in computational science and engineering, determining whether a software tool is accurately solving a set of partial differential equations (verification) is a logically internal process: when large systems of partial differential equations are typically in play, "correct" in the context of verification means a "mathematically accurate solution". It says nothing about whether or not that solution adequately captures the behavior of a real world phenomenon. As such, verification requires no engagement with the world of observation. Similarly, in the context of agent-based modeling, assessing whether or not an agent-based model accurately executes a conceptual model requires the ability to rigorously assess the mathematics, algorithms, and software engineering of the system. That may require the development of agent-specific verification techniques but does not require engagement with the external world.

On the other hand, determining whether a partial differential equation is correctly representing a real world phenomenon of interest – that is, performing validation – *does* require engagement with the external world. Correctness in the context of validation must be centered on observations derived from valid sources, i.e., systematic observational data or controlled experiments. Along those lines, assessing whether an agent-based model is built on correct requirements, implementing an appropriate conceptual model, and producing outputs that correspond to the real world requires comparison with observation.

How we perform a meaningful and objective comparison among the conceptual model, the simulation, and the real world is a critical challenge in the computational social sciences. For one thing, it is difficult to escape the problem of explanatory/theoretical contingency and plurality in the social sciences, in which cross-disciplinary challenges to explanatory frameworks are common and demonstrable certainty is rare. Although some might see quantitative modeling as a way of introducing rigor into the social sciences, it is not clear that modeling helps researchers get around this problem. In the realm of the physical sciences, models derive from stable epistemology rather than vice versa. In the social sciences, there are basic debates about the role of theory as a descriptive, explanatory, or causal framework, and whether or not a nomothetic enterprise is even possible (i.e., the generation of broadly applicable, generalizable explanatory theories for human behavior). As anthropologist Jessica Turnley points out, evaluation techniques that rest on a logical positivist philosophy that a) assumes the existence of objective data, and that b) presumes stable relationships between data and theory, are a poor fit for the social sciences where multiple frameworks can be evoked with equal credibility, depending on one's discipline, to explain similar phenomena (Turnley, 2004). Indeed, evoking computational modeling and simulation to assert epistemological rigor is highly problematic in areas where theoretical consensus is lacking. In particular, confirmation bias is a well-recognized form of cognitive bias in which people subconsciously put greater emphasis on information that is consonant with their reasoning, while simultaneously discounting disconfirming evidence. Insofar as computational models and simulations reify and help us visualize our conceptual models, they can make those models seem more credible than they perhaps are as critics of computational social modeling projects have pointed out (see Andrew Vayda's discussion of Stephen Lansing's work (Vayda, 2009)).

Issues with theory and conceptual validity are intertwined with the problem of data validity, a second challenge for verification and validation in computational social science. In computational physics and engineering, validation depends on two things: identifying a validation referent, or a known point of estimated "truth" for comparison that enables one to evaluate the validation correctness or accuracy of the model vis-à-vis reality; and the ability to generate valid observational data around that referent. In the social sciences, this requirement for known points of truth to act as referents – and the associated need for high-quality empirical validation data – are serious challenges.

In this regard, data will probably be a major, ongoing problem for the verification and validation of computational social models and simulations, since it is impossible to assess the value of a model or simulation without systematic ways to tie the model to observed reality. For one thing, some forms of social knowledge simply resist quantification. At a deeper level, the issue of how evaluate the "objectivity" of data in the social sciences is a long-standing epistemological debate. This is because social scientists are embedded in the very social matrices they are studying: we cannot speed up or slow down society, or miniaturize it in relation to our senses, to observe the manifold and multilevel dynamics that interest us. As Lucy Resnyansky points out, "Data that are used for understanding the threat of political violence, extremism, instability and conflict are essentially different from what is considered to be data in natural sciences. The former kinds of data have a representational nature and are sociocultural constructs rather than results of objective observation and measuring" (Resnyansky, 2009, p.42). Lastly, empirical data that are used to develop a model cannot be used to rigorously validate it, which means that validation requires investment in the systematic collection of additional validation quality data. This can be challenging if the phenomenon of interest involves the dissemination of an idea through a large population or assessing the causes of intergroup violence in a particular region of the world, in which case data collection could easily span many countries and several decades.

This brings me to my second point: the computational physics and engineering literature that deals with verification and validation is relevant and important for computational social science models and simulations intended for application in real-world decision making contexts. This literature emphasizes that the main benefit of V&V is not (perhaps counter-intuitively) increased focus on the model but the contextual issue of how the model will be used and, therefore, how the organization and its members identify what decisions they are responsible for making and how they negotiate acceptable levels of risk. This is because verification and validation emphasize whether or not a software application is credible *for an intended area of use*. These discussions force clarification about the decisions, tradeoffs, and risks across stakeholder communities, and what is required for a model to be considered credible and appropriate in relation to a decision. In this regard, I have come to view verification and validation as a form of sensemaking through which stakeholders in a decision space negotiate the benefits and limitations of modeling and simulation technology.

Forecasting, Simulation, and Decision Making

A great deal of the literature on computational social science in national security decision making focuses on challenges of theory, methods, and data to support computational modeling and simulation for a range of problems ranging from training to forecasting. What this focus misses is that forecasting is not a technological problem, and that no model or simulation ever makes a prediction or develops a forecast. Models and simulations generate information. People make predictions and develop forecasts. Whether or not a simulation is actually "predictive" of something is always human judgment, not a technological one, and humans are always in the loop.

In this regard, we call the reader's attention to an extensive body of interdisciplinary scholarship – much of it rooted in economics, business, psychology and management – that focuses on the topics of forecasting and decision making in organizations (Armstrong, 2006, 1985, 2001a; DiLurgio, 1998; Cox and Loomis, 2001). This literature highlights a larger family of forecasting approaches that include quantitative (statistical), qualitative (judgmental), and integrated quantitative-qualitative approaches to developing forecasts. This literature treats modeling and simulation tools as technological inputs to forecasting techniques, methods, and principles; and emphasizes that tools are only as good as the processes through which they are created and used. In particular, Armstrong identifies eleven different families of forecasting techniques (Armstrong, 2006, 1985, 2001a) and suggests principles for a robust multi-stage forecasting process. Forecasts, he argues, include multiple stages of activity including formulating a problem, obtaining data, selecting and implementing forecasting methods, evaluating forecasting methods, using forecasts in planning and decision making, and auditing forecasting procedures to ensure that appropriate principles have been applied (Armstrong, 2001b; see also Cox and Loomis, 2001; Tashman and Hoover, 2001). Armstrong's principles point to a kind of "verification and validation" for forecasting beyond the correctness of a model and beg the question of whether or not a model is actually the best analytic methodology for a particular decision space. Moreover, Armstrong's work highlights forecasting as an organizational problem not a technological one, which is a difficult challenge because planning and decision making activities tend to be highly distributed within and across stakeholder groups.

No area of research makes this point more thoroughly than weather forecasting, which has been studied extensively by psychologists, decision theorists, and economists for six decades as part of an ongoing effort to assess and increase the political, social, and economic value of weather forecasts. Weather forecasting is unique for several reasons: first, the United States National Weather Service issues many tens of millions of forecasts a year (Pielke, 2000). Second, weather forecasts are highly public with federal, state, and local agencies and individual citizens incorporating weather and climate forecasts into a wide array of daily activities from purchasing road-clearing equipment to planning weddings. Third, weather forecasters get regular feedback not only on the correctness of their predictions but on the value of the forecast information they provide. As a result, weather forecasting has been a subject of intense interdisciplinary study for many decades because weather forecasting is one of the few areas where it is possible not only to evaluate the correctness of a forecast and to suggest improvements, but also to document how forecasts are incorporated into decision making processes. As Pielke suggests, weather forecasting "provides some lessons about how we think about prediction in general", not just weather forecasting specifically (Pielke, 2000, p. 67).

A great deal of this literature is relevant to computational social models and simulations being used for predictive purposes. The weather forecasting literature treats modeling and simulation technologies as only one element of a much larger "process in which forecasters assimilate information from a variety of sources and formulate judgments on the basis of this information" (Murphy and Winkler, 1971). Moreover, forecasting is not just a problem for meteorologists but involves a complex ensemble of people, organizations, tools, data sources, and activities through which forecasts are developed, disseminated, acted upon, reviewed, and evaluated – what Hooke and Pielke call the "symphony orchestra" of the weather forecasting system (Hooke and Pielke, 2000). The forecasting orchestra includes three principal activities: forecasting, communication, and incorporation, all of which are working in parallel at any particular point and each of which can be subjected to rigorous evaluation. Ensuring that this orchestra provides the best public service possible depends on rigorous evaluation of how well each of these activities is performed.

The weather forecasting community not only works to improve the performance of its modeling and simulation tools but also the skill of the forecasters who develop and disseminate forecasting products. How to evaluate and improve forecasting skill, communicate forecasts, and increase the value of forecasts to decision makers have been research challenges for meteorologists, psychologists, statisticians, economists, and decision theorists since at least the 1960s (Murphy and Winkler, 1971a and 1971b; Murphy and Epstein, 1967a and 1967b; McQuigg and Thompson, 1966). Forecasting is a process of continuous learning that demands prompt, clear, and unambiguous feedback in a system that rewards forecasters for accuracy (Fischhoff, 2001; p. 543) since they need feedback to identify errors and assess cause (Murphy and Epstein, 1967b). Lacking prompt feedback, intermediate-term feedback can help forecasters get a better sense of how well they are doing but only when the forecaster's predictions are clearly and precisely recorded, along with the inputs and assumptions or external considerations that went into the forecast. Systematic, regular, comparative evaluation provides more than accountability – it improves the forecaster's skills.

At the same time, forecasting skill depends not only on the forecaster's cognitive abilities but also on "the environment about which forecasts are made, the information system that brings data about the environment to the forecaster, and the cognitive system of the forecaster" (Stewart and Lusk, 1994, p.579; Stewart et al., 1989). Thomas Stewart has argued that the forecasting challenge is best understood as an example of the Brunswik lens model, which relates the observed event to the forecast through a lens of "cues" or information items that people use to make the forecast. The quality of a forecast depends not only on the ecological validity of the cues – that is, how the cues are related to the phenomenon being forecasted and what those cues indicate about the phenomenon – but also on the ability of the forecaster to use those cues properly in assessing the event of interest (i.e., whether or not the forecaster is using the right information and whether she is using that information correctly).

As complex as this system is, when all these elements come together properly, weather forecasters are tremendously accurate and reliable in their predictions. However, good forecasting also involves packaging meteorological expert judgment for non-meteorologist consumers. One issue of perennial concern of the forecasting community is the communication of uncertainty in weather forecasts. Forecasting is an inherently uncertain process because of the inexactness of weather science and the many sources of error that can throw off accuracy including model uncertainty, issues with data, inherent stochasticity, and forecaster judgment. Accordingly, the communication of uncertainty is a major element in whether or not people can use forecasts. In 1971, Murphy and Winkler found that even other scientists had trouble explaining what meteorologists meant by "a 40% chance of rain" (Murphy and Winkler, 1971a, 1971b). More recent research in human judgment and decision making indicates that even today, seemingly unambiguous probability statements are prone to misinterpretation: as a simple example, Gerd Gigerenzer and colleagues found that populations in different metropolises interpreted the seemingly unambiguous statement "a 30% chance of rain" in different ways depending on the assumptions about the reference class to which the event was oriented (Gigerenzer et al., 2005). Not surprisingly, the National Oceanic and Weather Administration (NOAA) continues to invest resources in the development of techniques for communicating uncertainty across its stakeholder communities.

Uncertainty is likely to be a major research challenge for forecasts of social phenomena. Research is likely to focus on methods for quantifying, bounding, aggregating, and propagating uncertainty through both models and the forecasts derived from models. Indeed, a National Research Council report on dynamic social network analysis identified uncertainty as one of the key under-researched areas in quantitative and computational social science (Breiger et al., 2003). This research is critical for developing a decision-oriented computational social science but it is probably not sufficient. If NOAA's experience in this regard is any indication, forecasts of social processes and phenomena will have to deal not only with multiple sources of uncertainty, but also the challenge of representing and communicating uncertainty to consumers with varying levels of skill in interpreting quantitative, graphical, and/or qualitative expressions of uncertainty.

Lastly, it is important to emphasize that forecasting and decision making are two different activities. That improvements in decision making do not necessarily depend on improvements in forecasting is illustrated in case studies examining how forecasting failures actually lead to *better* public policy and decision making (see for example, Nigg, 2000) All decisions involve both stochastic and epistemic uncertainty. Putting too much emphasis on forecasting as a means of improving planning can lead decision makers to focus on the correctness of the forecast at the expense of the planning process. Forecasts are helpful as long as they do not divert attention from potentially more robust ways of dealing with uncertainty such as flexible resource allocation practices or hedging strategies (Armstrong, 2001a, see Introduction pp.1-14.).

Users, Transparency, and Responsibility

Verification and validation techniques assess the goodness of a model/simulation from an internal (verification) and external (validation) perspective. In the context of high-consequence decision making, such as that performed in military and intelligence contexts, there is another dimension that requires assessment. This dimension is the relationship between the model/simulation technology and the person or people using the technology, i.e., the relationship between the human and the computer.

All software projects have various stakeholders including developers, funders, and end users. In the software engineering community, it is generally understood that getting end-users involved in the design and development of the tools they will use is critical if the software is to be usable, useful, and relevant to real-world problems. Even so, end users tend to be the silent stakeholder in a software project because so many software projects begin, progress, and end without much consideration of who will use the software or what they will do with it. I think of this as the "over-the-fence" model of software development. In my experience, over-the-fence software projects are quite common in the national security community, and a key challenge for the applied computational social science community is the transition of modeling and simulation technologies into usable, useful, and adoptable systems that support analytical reasoning.

The over-the-fence model of software development may be particularly poor for computational social modeling and simulation efforts. This is because computational science projects tend to be complicated interdisciplinary efforts that bring together an array of subject matter experts (Resnyansky, 2008). Very sophisticated models can require deep expertise in a number of areas, from computer hardware to uncertainty in social science data. The process of developing the model is a critical forum for knowledge exchange because model development activities afford developers the chance to learn from each other and to develop shared understandings about the technology under construction (Barreteau, 2003; Dare and Barreteau, 2003).

This raises the question of how much of this experiential or contextual knowledge is required to effectively use modeling and simulation technology. Because modeling and simulation technologies can embody so many layers of expertise, it can be difficult for end users who are not subject matter experts to understand what the model is doing or how it performs its functions. Sometimes this is not an issue because the modeling and simulation technology is not going to be used outside the domain in which it was developed. It might be a tool that a research or analysis team develops for itself; in this case, the developers are the end users for the technology and, because of that, they have a rich understanding of the model's uses, limitations, and biases. Alternatively, the tool may not be traveling very far outside the domain of its creation. For example, a sociologist might develop an agent-based social network modeling tool and might post it on her website so that other sociologists trained in these techniques can apply it to their data. In this case, the domain of use is epistemically adjacent to the domain of development so that new users can credibly bring their domain knowledge to bear on the software artifact they are using.

However, when modeling and simulation technologies are going to be transferred across epistemic domains, the question of how and if non-subject matter experts can engage the technology as a tool becomes more problematic. There is an ethical issue in this regard, insofar as users who do not understand the application space, benefits, and/or limitations of a modeling and simulation tool are unlikely to use it well. Fleischmann and Wallace have argued that ethically responsible modeling implies three elements: a commitment to develop models that a) are faithful to reality, b) reflect the values of stakeholders, and c) are maximally transparent so that users and decision makers can employ the model appropriately. This latter property, transparency, is "the capacity of a model to be clearly understood by all stakeholders, especially users of the model" (Fleischmann and Wallace, 2009; p.131). Developing processes to deal with epistemic gaps will be an important aspect of tool development and deployment in the national security community. This is an organizational problem, not a technological one, and addressing it requires careful planning and stakeholder negotiations.

Conclusion

As the computational social science community continues to evolve its techniques and approaches, its practitioners may play an important role in shaping our rapidly evolving national security community. In a reflexive way, to the extent that the computational social science community attracts and leverages national security investments, national security topics like terrorism and insurgency warfare are likely to provide major focus areas for the evolution of the field's techniques and specialty areas. In moving computational modeling and simulation technologies out of the realm of research and into the realm of policy and decision making, we should perhaps consider what is required to develop a realistic, robust understanding of what it means to use models and simulations as decision support tools. I want to re-emphasize a point that I made earlier: *there is no such thing as a computational prediction.* Computational models and simulations provide outputs but predictions are a form of human judgments. Computational models and simulations are created by human beings and like everything we create, our models and simulations reflect (even reify) our state of knowledge at a particular point in time. Focusing our attention on the limitations of models and simulations as tools for human users and investing resources in assessing what those limitations imply for real-world decision making, can help us build a stronger understanding of how, where, when, and why computational models and simulations can be useful to people working in fraught, high-consequence decision making contexts.

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