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EPISTEMOLOGY OF MODELING AND SIMULATION

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ABSTRACT

While ontology deals with the question of being or existence, epistemology deals with the question of gaining knowledge. This panel addresses the challenge of how we gain knowledge from modeling and simulation. What is the underlying philosophy of science of M&S? What are our canons of research for M&S? Is it sufficient to apply the foundational methods of the application domains, or do we need to address these questions from the standpoint of M&S as a discipline? The invited experts illuminate various facets from philosophical, mathematical, computational, and application viewpoints.

1 INTRODUCTION

In his essays on life itself, Robert Rosen (1998) states: "I have been, and remain, entirely committed to the idea that modeling is the essence of science and the habitat of all epistemology." Rosen's view on modeling is best captured in the left part of Figure 1, that shows a natural system we are interested in which is modeled using a formal system that we use to understand the natural system better. Observed correlation are assumed to result from a causality embedded in the natural system. By encoding our understanding of the natural system using symbols and rules of a chosen formalism, we can make inferences in the formal system that by decoding help us to understand the natural system better. We actually do not get knowledge of the natural system itself, but of the representing formal system that we used to describe the system.

The modeling relation is directly derived from the scientific method: empirical data are collected and analyzed to discover correlations that may be described by causalities. A hypothesis is formulated that allows the description of the observation (formal system) and explains the observed correlations (inference) to explain the observed system. Experiments can be conducted to further test the hypothesis until she is accepted by the broader scientific community and becomes a theory.

It can be observed that Rosen follows the argument of Karl Popper (1935) who actually introduced the three worlds: the *physical world of objects*, the *mental world of conceptualizations*, and the *formal world of objective knowledge*. He differentiates between the natural system and the cognitive layer of conceptualization: how we perceive the natural system. The formal representation reflects our perception of the natural system, which is heavily influenced by sensors as well as by the education of the observer. More accurate observation tools as well as new insights from related scientific domains often lead to new hypotheses and ultimately new theories. A very similar idea, introduced by Ogden and Richards (1923) as the semiotic triangle, shown in the right part of Figure 1, was applied to modeling in simulation (M&S) by Turnitsa and Tolk (2008).

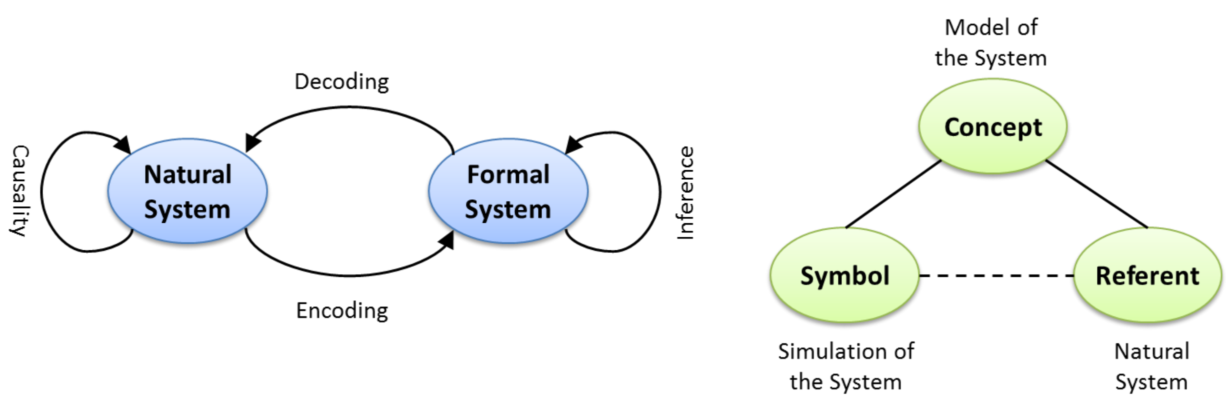

Figure 1: Rosen's Modeling Relation (left) and Ogden's Semiotic Triangle and M&S Relation (right).

When we talk about a real world referent of a natural system, we are actually talking about the concept that we have, utilizing symbols we agreed upon. This explains why people often 'talk pass each other' even if they look at exactly the same natural system: their perceptions and concepts are different. In M&S, our model is the conceptualization of the naturalization, and our simulation becomes the symbol standing for it. Overall, the idea that we are using models to represent our knowledge, and that within M&S we build simulation derived from these models seems to be well established. Nonetheless, there are several challenges that have to be addressed by the community. This paper collects several position papers by experts in the domains of philosophy of science, modeling, and simulation to contribute towards a philosophical foundation of M&S as a discipline.

2 ARE WE READY FOR SIMULATION TO BE THE EPISTEMOLOGICAL ENGINE OF OUR TIME? (HEATH)

As computers become more pervasive in our lives, it has been said that simulation will be the epistemological engine of our time (Ihde 2006). Simulation has achieved this honor because of its ability to extend our conceptual models with the speed and efficiency that is beyond our cognitive abilities. This implies that simulation will continue to be a key source of knowledge discovery in the future and in essence become the third leg of the "science stool" that helps us augment, fully understand, and interpret experimentation and theory (Latane 1996). Certainly it is a good time to be a simulationist as the future looks very bright for the field, however are we ready for the full implications of what it means for simulation to be the epistemological engine of our time? Do we need to change how we practice simulation as the weight of human knowledge discovery becomes more dependent upon simulation? Do we have a philosophical belief system to be successful or are there cracks in the foundation?

The reason why simulation is becoming the epistemological engine of our time is exactly the same reason why we need to be considering whether we are ready for it to be the epistemological engine of our time: *simulations are complex*. Their ability to perform complex calculations and represent abstractions of our conceptual reality allow them to be powerful epistemological engines. In many ways they are going beyond just modeling aspects of a real system and are becoming theories by themselves. In most of today's literature on simulation philosophy, the majority of researchers treat simulations in this way (theories by themselves) and then utilize principles from the philosophy of science to validate that the simulation is an accurate representation of reality and therefore knowledge generated from the simulation is valid and pragmatically useful until proven otherwise (Heath and Hill 2009). This is all well and good if the community has decided to embrace the philosophy of science belief system; however accepting this means that the community must follow through with the scientific method or else be branded as hypocrites and a decidedly unscientific discipline. It is here that we face some of our largest challenges as a discipline touting the epistemological engine of our time.

While the complexity of simulations allow for them to extend our cognitive capabilities it also makes it extremely challenging and time consuming to *independently* verify and validate the simulation and it's results. Accordingly this independent testing is rarely practiced, but it is absolutely vital in the scientific method because without it the whole philosophy pragmatically falls apart and no significant progress can be made beyond individual self-interests. To provide some perspective as to the size of this problem in our field, first consider how many simulations of yours have been independently replicated and validated? Second, consider one extensive study of the field of Agent-Based Modeling (ABM) that showed over a 10 year period less than 16% of published articles gave a reference for others to independently replicate the results and only 35% of the articles even provided evidence that they had completely validated their own simulations (Heath et al. 2009). These figures are at best abysmal when compared to other published scientific research articles that are not using simulation. So, how do we turn it around and ensure that simulation continues to grow and last as an epistemological engine?

Below are two conceptual ideas that as a discipline that I believe need serious attention. First, we need to emphasis simulation philosophy as a key focus of the simulation field so we can begin to determine if the philosophy of science is sufficient or if we need to develop a new pragmatic philosophy of simulation. We need a solid philosophical foundation before we develop new technical advances, which is too often the singular focus in simulation curriculums. Second, our discipline needs to continue to develop methodologies and standards for the use of simulation in scientific research. This is especially true for published research. Journals need higher standards for what is truly required when simulation is used to generate knowledge and understanding. Now is a great time to be a simulationist but we absolutely need to address these issues so we can truly elevate simulation to extending our knowledge of the universe.

3 SIMULATION MODELING ROOTED IN THE PHILOSOPHY OF SCIENCE: A NON-STATEMENT VIEW APPROACH (IHRIG & TROITZSCH)

As early as in 1997, Axelrod (1997a) called simulation "a third way of doing science", and even earlier, Ostrom (1988) called "computer simulation: a third symbol system". Both had different views on which the other two ways or symbol systems were. For Axelrod, simulation was an alternative "way" to both deduction and induction; for Ostrom simulation as a symbol system was an alternative both to natural language and mathematics. We argue that simulation is a way of deduction which is gone by means of an alternative symbol system. Simulation research is not a distinct way of doing science, in the sense that it has a starting point of existing knowledge and ends up in new knowledge. Nevertheless, simulation results can be surprising, particularly when they show emerging macro phenomena. In a strict sense however, simulation results are not 'new', since they are only hidden in the micro-specifications from which they were generated (Epstein and Axtell 1996). It is anyhow questionable whether this holds for deduction without induction or for induction without deduction, to use the two words used by Axelrod. Thus it seems necessary to take a view on simulation which takes the whole research design into account. We agree with Harrison, Lin, Carroll and Carley (2007) that simulation is an appropriate means to deal with complex behaviors and system, but we doubt that simulation directly produces novel theory.

For both practitioners and academics to fully harness the power of simulation as a method, we need frameworks that give us an integrated view of the research process that involves simulation modeling. One such approach, rooted in the philosophy of science, is described here (Ihrig and Troitzsch 2013). We distinguish three modes of research: theoretical, empirical, and desk research. Since theoretical research can be conducted both without simulations and with simulations, we arrive at four columns of research, three of which that drive the development of new research insights: theoretical research without simulations, simulation research, and empirical research. The simulation research 'adventure' usually starts with a real-world issue (an empirical observation) backed by prior theory. We assume that some new, unexplained feature of the real world (Gilbert's and Doran 1994) awakens a researcher's interest. The researcher then becomes aware of the current approaches to explaining this or similar features in the existing literature (doing desk research).

Partly from casual observations, partly from background knowledge and desk research, a mental model is formed in the researcher's mind. He or she arrives at this most economic explanation by means of *abduction* (Peirce 1992). It is the starting point for the ensuing theory development process. In line with the "non-statement view" (Troitzsch 1992, 1994, 2012), using a set-theoretic description of theories (Sneed, 1979), we distinguish three classes of emerging models (Balzer et al. 1987), which goes back to the observation that (a) some terms used in a theory are measurable or observable no matter whether the theory has ever been formulated or tested — the list of these terms defines a set of partial potential models; (b) other terms used in a theory become meaningful only after the theory was formulated — the list of these terms extends the elements of partial potential models to elements of the set of potential model; and (c) the relations between both kinds of terms need to be defined as axioms — full models which conform to such axioms form the third set.

Most simulation exercises, in academia and especially in practice, are solely based on mental models, used directly to build executable simulation software that is supposed to model a particular real world phenomenon. However, we see this as a shortcut that should be avoided. Formalizing a mental model should lead to the definition of a *potential model* first, listing all the terms, both theoretical and non-theoretical with respect to a particular theory. By adding axioms, the researcher then arrives at a *full model* that instantiates a specific environment. Only then can an executable simulation model be properly built and meaningful virtual experiments conducted. By varying the parameter space, different simulation runs will result in simulated data that will yield testable propositions, which can be compared to both *partial potential models* (theoretically derived propositions, deduced from *full models*) and to empirical data. This is the first step in generating new research insights that will help improve existing theories and eventually create new ones. A research process like the one postulated here defines the role of simulation in a different and more precise way than previous social simulation work has done (Axelrod 1997a, 2007). Our approach extends Balzer's (2009) definition of the role of simulation in scientific research from the 'non-statement' point of view where (for the sake of brevity) he does not make a difference between the runs of an executable simulation program and the data generated by these runs.

Taking agent-based models as an example, the researcher specifies well-defined micro-behaviors of agents (based on the theoretical assumptions). The macro-level results that emerge from those individual-level activities cannot be predicted (especially when exploring different boundary conditions), and the researcher may gain valuable new insights from analyzing the simulation outcome and comparing it to empirical data. Another example are agent-based models that do not rest on prior theories. In these cases the agent-based simulation program is the first attempt ever at formalizing a mental model using Ostrom's (1988) third symbol system. For those, unambiguous conclusions cannot be derived and simulation results may reveal unexpected outcomes. More often than not, simulated data cannot be compared to purely theoretical assessments as a classical mathematical formulation of the axioms of the theory has no analytical solution (and a numerical solution of, for instance, a system of non-linear differential equations is also the result of a computer simulation for a specific combination of parameters), such that simulation sometimes is the only possibility to generate deductions from theoretical assumptions.

A research architecture that employs simulation tools is more comprehensive than conventional approaches (and the classic deductive and inductive reasoning that go with them) and so better suited for studying complex phenomena and obtaining new theoretical insights (Carley 2002). The conventional research entails predictions and analyses that are made based on existing theories in Ostrom's (1988) first and second symbol systems of natural language and mathematics, and the empirical data gathered on real world issues is compared to these theoretical accounts or propositions. What is lacking is the power of computer tools that enable us to study more complex processes by modeling micro behaviors that individually might be straightforward, but may result in unpredictable outcomes when considered together.

Finally, Martin Ihrig would like to thank Klaus Troitzsch for his contributions to this position paper and the work that did lead to it. Klaus Troitzsch has been professor and director of the Institute for Information Systems Research at University of Koblenz-Landau and is a pioneer of introducing simulation methods to the social science community.

4 MEASURING THE DEGREE OF VALIDITY: AN OPEN CHALLENGE (PADILLA)

If all models are wrong, how wrong are they? Furthermore, if some of them are useful, how useful? These two questions provide an idea of how models are evaluated today: truthfulness and usefulness. Currently, we answer these questions by either evaluating validity through techniques such as calibration or by their predictive capability. However, the first captures how well a combination of parameters follows a trend. The resulting challenge is to establish how truthful that parameter combination is. The predictive capability of a model/simulation provides part of a picture; how well simulated data matches real data. It does not tell how truthful the simulation that generated the data is. Both, calibration and predictive capability (predicting the past in most cases) could provide part of a more complete picture.

This extended abstract reports on ongoing research on the feasibility and the mechanics on how to measure validity. As such, it poses to the M&S and philosophy of science community a challenge of providing a metric for validity.

<u>Operating Definitions:</u> There are many definitions of validity and validation. For the purpose of this work, I will rephrase the most used one: validity is the correspondence between a model/simulation and reality. However, this definition is usually translated as: validity is the matching of results (an experiment for instance) to reality. Yet, this definition is usually reduced to: validity is how well a model predicts reality. While this reduction is "valid", for lack of a better word, it provides an evaluation of the *usefulness* of a model not of its *truthfulness*.

Usefulness and Truthfulness: Perhaps the best example of usefulness vs. truthfulness comes in the history of predicting planetary motion, especially Ptolemaic and Copernican models. Ptolomy's geo-centric model of planetary motion stood over a millennium, unchallenged and accepted as true. It was a good model as it provided great predictive capabilities, yet it was not true. It was useful and still is, but the model does not correspond to the phenomenon in reality; its output does. Copernicus heliocentric model challenged Ptolomy's not on its predictive capabilities, but on correspondence; it is a good abstraction of the phenomenon in question. While later Kepler provided a model based on ellipses and not on circles, Kepler's can be seen as an improvement on Copernicus'.

Validity Spectrum: Axelrod's (1997b) posits simulation as the third way of doing science: "*Like deduction, it starts with a set of explicit assumptions. But unlike deduction, it does not prove theorems. Instead, an agent based model generates simulated data that can be analyzed inductively. Unlike typical induction, however, the simulated data come from a rigorously specified set of rules rather than direct measurement of the real world.*"

As such, a simulation's validity should consider the evaluation of the 1) deductive portion; how well a system of premises reflects a static view of a phenomenon/system and how well that system reflects the dynamics of such phenomenon/system in a time scale; 2) the inductive portion; how well the resulting data provide insight into the phenomenon through statistical analysis; 3) the combination of the two. While 1 can be based on formal practices such as those found in mathematics and 2 in those found in statistics, 3 suggests measures unique to M&S; calibration for instance.

The evaluation along this spectrum should provide a metric, which is the challenge posed in this brief. Further, the evaluation should address usefulness and truthfulness; for instance, a statistical predictive capability provides usefulness and if paired with an evaluation of axiomatic validation may provide truthfulness. The mechanisms of this evaluation are part of current research.

Challenge: If validity has a degree, how do we measure it? It is the opinion of the author that an answer to this question would facilitate the discussion on the degree to which simulations generate and evaluate knowledge. Simulations have provided its engineering usefulness, yet their truthfulness as a scientific methodology are still under discussion in a paradigm where lack of data or complex phenomena may mean no capability for empirical validation. Yet, we need to question those that have been empirically validated (when considering prediction as the evaluation form) if a form of axiomatic validation is not provided. It is proposed that a validity metric would permit to evaluate simulations along a spectrum. This metric should also provide an idea of the usefulness and truthfulness of simulations.

5 ON SCIENCE AND MILITARY SIMULATION (PAGE)

In the summer of 2002, the Army Model and Simulation Office (AMSO) and the Defense Modeling and Simulation Office (DMSO) jointly sponsored a workshop on the *Scientific Exploration of Simulation Phenomena*. The workshop involved 17 participants from industry, academia and government who were asked to address the following questions: Could a fundamental knowledge of the nature of simulation exist? Would a scientific approach to the study of simulation improve that knowledge? What specific steps are needed to develop the science of simulation?

The proceedings of the workshop survive online (Harmon 2002) and many of the thoughtful position papers developed by the panelists remain relevant to Tolk's current efforts to study the relationships between science and Modeling and Simulation (M&S). Space limitations preclude revisiting most of these themes here, but the interested reader is commended to review the position papers and detailed session transcripts from the workshop.

This author's contributions to the workshop focused on the relationship between science and M&S in the defense sector. In a broad sense, the defense simulation sector was enjoying two decades of plenty – simulation, particularly interoperable simulation, had become a focal point for defense training systems at all scales; simulation-based acquisition was hailed as the solution to inefficiencies in DoD procurement practices; development budgets for simulation systems like JSIMS reached $1B, giving these programs the same status as the military's largest programs. Times were good. But under the surface, there were clearly some troubling issues (and these issues were largely what inspired AMSO and DMSO to convene the workshop). The defense simulation workforce was largely on-the-job trained. There were few University programs in Modeling and Simulation (and almost none in military aspects of Modeling and Simulation), and there were limited opportunities for professional development and certification. There were few incentives for the workforce to publish, and a dearth of quality outlets for their articles. The big simulation programs were slipping their schedules. The community seemed almost distracted by interoperability and standards to the exclusion of other pressing needs.

Clearly, a more rigorous approach to the practices associated with defense M&S was warranted. Whether this increased rigor meant doing better science, or doing better engineering, or doing something else, was unresolved in this author's 2002 position paper.

In the intervening years, the DoD M&S days of plenty have gone. The priorities of active combat are doubtless a primary factor. There has been progress. University programs for M&S are proliferating, and there are more opportunities for professional development and certification. The Journal of Defense Modeling and Simulation (JDMS) has been established. DMSO's successor, the Modeling and Simulation Coordination Office (M&SCO), actively pursues the codification of Best Practices and an M&S Body of Knowledge. But there have also been failures. JSIMS was cancelled after $1B spent failed to deliver a functioning system. Simulation-based acquisition never got off the ground.

Are the failures of defense simulation due to a lack of science, or something else? Edsger Dijkstra is purported to have said, "The question of whether a computer can think is no more interesting that the question of whether a submarine can swim." One wonders what he would think of the question "Is simulation science?"

6 THE MULTIDIMENSIONAL FUTURE OF M&S (SUAREZ)

The Field of Modeling and Simulation (M&S) has made undeniable and significant advances in the last few decades. Going against the current of modeling based on a reductionist view of the world, the rise of the nonlinear research agenda is now irreversible. The relative success of computational models has created a wave of research and practice that makes use of the increased capabilities to model complex systems, particularly those appearing in nature and society (Gilbert and Troitzsch 2005). Despite its broad appeal and expanded use, the field is searching for general methodologies for validation and verification (Tolk 2012) as well as for common languages and methodologies in which different models can interact with each other (Seck and Honig 2012).

The need for composability among models is an intrinsic characteristic of a multidimensional reality, given that diverse phenomena, aspects of reality, disciplines, bodies of knowledge, paradigms and their corresponding models must reflect an irreducible, nonlinear world. Emergent phenomena are ubiquitous in systems that are separated into distinct ontological levels, such as it is in human societies (Goldspink 2000). A framework for describing of behavior must therefore not only allow for the description of any physical aspect of the world, but also a way to describe the relationships, structures and processes that influence the contextualized decisions made by agents in these complex adaptive systems (Koestler 1967). In order to appropriately model complex human behavior we must define a canvas in which multiple dimensions of our existence can be defined (Suarez and Castañon-Puga 2103). Microeconomics, for example, has usually relied on simplistic definitions of what represents an indivisible actor. Generally speaking, firms and consumers are modeled as having a straightforward behavioral directive: to maximize their profits or utility, subject to the constraints imposed by the exogenous environment. This approach is incompatible with a view where decisions are contextualized and treated endogenously (Edmonds et al. 2011).

Much of the current work to develop the M&S paradigm has foundations in Multi-Agent Systems (MAS), and is thus similarly based on independent agents that strategically interact with each other. The independence granted to the agents in MAS is a computational one, in the sense that each agent processes information internally, without the need to resort to the outside world to draw conclusions about the inputs it receives (Epstein and Axtell 1996). In contrast, the proposition is that the M&S field should develop broader capabilities that allow for model interoperability, as nonlinearity should not be taken to mean that the multiple ontological dimensions are completely orthogonal; they must be ultimately reassembled in a holistic description. Models of each dimension must take into consideration this fact, for only then will disciplines of human behavior be able to communicate in an epistemologically compatible manner. The proposed general framework would also serve as a relative taxonomy in which bodies of knowledge can establish correspondences with each other and lay the epistemological web in which new models can be developed and integrated.

7 ONTOLOGIES: BEYOND DATA EXCHANGE FOR SEMANTIC INTEROPERABILITY (SZABO)

An ontology is defined as "a set of representational primitives with which to model a domain of knowledge. The representational primitives are typically classes, attributes, and relationships." (Gruber 1993). Ontologies are part of the W3C standards stack for the Semantic Web, where they are used to specify standard conceptual vocabularies in which to exchange data among systems, provide services for answering queries, publish reusable knowledge bases, and offer services to facilitate interoperability across multiple, heterogeneous systems. In contrast to a taxonomy that contains hierarchical definitions usually modeled as subclass/superclass relationships, an ontology enhances knowledge about the world by allowing the specification of more complex relationships, such as defining disjoint classes, intersections and various predicate logics (Gruber 2008). In modeling and simulation, ontologies can be broadly classified into two main categories. Methodological ontologies define methods and simulation techniques, and referential ontologies represent the real world entities to be simulated (Hoffman et al. 2011). Methodological ontologies are necessarily prescriptive as they need formally defined inference rules, whereas referential ontologies are descriptive and would need to reflect ambiguity and contradictory conceptualizations. This implies a balance between normative and epistemic perspectives, but is usually implemented as an exclusive choice.

Despite this two-edged nature of ontologies for M&S, there is general consensus that ontologies are the catalysts to achieve simulation model interoperability and composability at the highest levels (Tolk and Curt 2005). Since the early 2000s, ontologies have been abundantly used in modeling and simulation. The most widely spread use is to ensure semantic data exchange and thus achieve a level of interoperability that is close to the semantic interoperability level of the Levels of Conceptual Interoperability Model (LCIM) (Tolk and Muguira 2003). This includes either ensuring that exchanged data adheres to a specific format (Lacy and Gerber 2004; Mahmood et al. 2009; Teo and Szabo 2008), to discovering components that meet particular descriptions (Szabo and Teo 2011), to generating ontologies that will be used by model users to ensure meaningful exchange of information (Zeigler et al. 2008). In conceptual modeling, ontologies can help to better define the entities in the conceptual model (Benjamin et al. 2006, Mimosa 2012), and to perform concept-level matchmaking to facilitate the dynamic composability of disparate models (Yilmaz and Paspuleti 2005). An ontology-driven modeling and simulation approach maps from domain-specific ontologies to a simulation ontology and then further to the generation of model code (Silver et al. 2009). Further, ontologies can also be used to evaluate various discrete-event simulation languages (Guizzardi and Wagner 2010), to facilitate the modeling and simulation of agent-based systems (Christley, Xiang, and Madey 2004), and more specific approaches see various domain ontologies used together with ontologies defining simulation blocks to achieve a more coherent and semantically correct model (Novak and Sindelar 2011).

Several avenues remain unexplored. Firstly, improvements on the reuse of models could be achieved by defining an ontology in which various models, accessible through a shared model repository, are defined. Capturing the state-of-the art discussed above, this modeling and simulation ontology could define the entities in the conceptual model, the simulation model with its attributes and behavior, and, details about the simulation paradigm employed in the simulation code. This can be achieved either as a single ontology capturing all of the above, or as a series of ontologies that describe concepts in a higher ontology. Secondly, current use of modeling ontologies is limited to yes/no queries that only look at exact matches. This does not harvest the reasoning capabilities of ontologies to address partial matches but also, using the open world assumption, discover new knowledge. Thirdly, a more ambitious use of ontologies might lie in model validation, and in particular in the identification and validation of emergent behavior in models with a large number of communicating and interacting entities. Complex models often exhibit properties that are not easily predictable by analyzing the behavior of their individual, interacting model components: these properties, called *emergent properties,* are increasing becoming important, and methods to identify and validate emergent properties have the potential to increase model credibility but also to provide increased insight into the behavior of the system the model abstracts. A taxonomy of emergent behavior has been previously proposed by Gore and Reynolds (2007) and proposes to analyze emergent behavior based on reproducibility, predictability, and temporality. Reproducibility refers to the repeatability of a simulation for a given set of inputs. Predictable behaviors enable selective sampling towards testing user hypotheses. Temporality distinguishes between the simulation reaching a final state and residing in a particular state. The challenge remains in enhancing such a taxonomy with inference rules that, together with the simulation model ontology outlined before, are able to determine if the observed behavior of a simulation model has not been seen before, i.e., is emergent, and further from this, to determine if the behavior is beneficial for the system.

8 IDEALIZATIONS IN MODELS OF CHOICE (WEIRICH)

Normative decision theory advances standards of rationality for decisions. Descriptive decision theory presents regularities in the decisions of people. Both branches of decision theory use models that incorporate idealizations. A standard of rationality may apply to ideal agents who know all mathematical truths and so have no excuses for miscalculations. A regularity in subjects' choices among items a survey question offers may assume that the subjects understand the survey question. The idealizations of normative and descriptive models have similar functions, as Colyvan (2013) observes. Do they have the same explanatory role?

Normative decision theory uses at least two types of idealization, as Weirich (2004) explains. An *aspirational* idealization states a condition that rational agents aspire to meet. Knowing all mathematical truths is an example. A *controlling* idealization states a condition that grounds a principle. The assumption that an agent has precise probability and utility assignments, an idealization of the principle of expected-utility maximization, falls into this type. Descriptive decision theory features controlling idealizations. A controlling idealization for a psychological study's conclusion may state a condition that only a portion of the target population meets. Among the subjects responding to a survey question, a subpopulation understands the question, so the assumption that all understand it is a controlling idealization. In some cases a controlling idealization is so restrictive that no members of the target population meet it. A model using the idealization may describe a fictitious situation. A fictitious model of choice may assume that agents attach precise probabilities and utilities to their options' possible outcomes, whereas in fact their probabilities and utilities are imprecise.

The aspirational idealizations of normative decision theory guide improvement and construction of artificial agents. The design of an artificial agent often tries to meet aspirational idealizations as closely as possible. A designer may try to make a robot a perfect calculator. Although descriptive decision theory may build a model in which agents are perfect calculators, its goal is a more realistic model that dispenses with that assumption and incorporates people's tendencies to err. Its goal is a model that generates simulations of human behavior, including errors.

The epistemology of models and simulations asks whether a model's incorporation of aspirational idealizations affects the model's power to explain the target phenomena. A descriptive model may help explain choices by showing how in the model aversion to risk affects choices. The model may use idealizations to control for factors, besides aversion to risk, that affect choices, as described by Weirich (2012). Does a normative model help explain the rationality of choices by showing how in the model some factor affects their rationality, using aspirational idealizations to control for other factors that affect their rationality?

One negative answer maintains that rationality for ideal agents tells us nothing about rationality for human agents because humans are far from ideal. The critic contends that an aspirational idealization does not control for any factor that explains the rationality of human choices. The idealization may regulate factors in the explanation of the rationality of an ideal agent's choices. It may help us understand rationality for the agents we aspire to be but does not help us understand rationality for the agents we are. Aspirational idealizations may help to formulate a general theory of rationality that treats all agents, ideal and nonideal alike, but do not help to formulate a restricted theory that treats rationality for humans. Knowledge of all mathematical truths may be part of the explanation of the rationality of an ideal agent's choice but does not control for a factor in the explanation of a human agent's choice. The rationality of a human agent's choice may not depend on calculation at all but just on the operation of a heuristic such as one recommending flight from threats. The explanatory factors may belong to what psychologists call System 1 in distinction from System 2, in which calculation occurs. The possibility that a heuristic rather than a calculation explains a choice's rationality is especially plausible when a choice's rationality depends on its meeting procedural standards rather than standards for its content.

This argument against the real-world relevance of models for ideal agents fails because having knowledge of all mathematical truths in fact controls for a factor in the explanation of a human choice's rationality. The presence or absence of mathematical knowledge affects a choice's rationality. Ignorance of mathematical truths excuses reliance on heuristics for choice. Knowledge removes the excuse. A knowledgeable agent need not rely on heuristics but may instead calculate to discover a maximizing choice. An aspirational idealization may stipulate mathematical knowledge to control for its presence or absence. The argument against the idealization's relevance to the rationality of human decisions presumes that only the presence and not the absence of knowledge may explain a decision's rationality. However, the absence of mathematical knowledge explains the rationality of decisions that follow heuristics. Sometimes absences explain.

9 CLOSING THE CREDIBILITY GAP OF MODELING & SIMULATION (YILMAZ)

Reproducibility is a fundamental principle of the scientific method (Morin et al. 2012; Fomel and Hennenfent 2009). It refers to the ability to reproduce, and, if needed, independently recreate computational artifacts associated with published work. Emergence of reproducibility as a critical issue is based on growing credibility gap due to wide spread presence of relax attitudes in communication of the context, experiments, and models used in computational science (Mesirov 2010; Stodden 2010). Furthermore, as indicated in (Fomel and Claerbout 2009), a published computational science article is not the scholarship itself; it is merely advertising of the scholarship. The actual scholarship is the complete software development environment and the complete set of instructions, which generate the data and findings (Fomel and Claerbout 2009).

Replicability, which is the challenge examined in this position statement, involves implementation of a conceptual model in a simulation study that is already implemented by a scientist or a group of scientists. Unlike reproducibility of results by (re)using the original author's implementation via executable papers (Nowakowski et al. 2011), workflow systems and repositories (Anand et al. 2009; Freire et al. 2011), or provenance-based infrastructures (Koop et al. 2011), replication involves creating a new implementation that differs in some way (e.g., platform, modeling formalism, language) from the original model. Yet the original and replicate are sufficiently similar so that experiments conducted on both generate results that achieve prespecified similarity criteria: they cross-validate. The premise of independent replication is based on the following observation. Although eventual exposure to the original model and its source code is important, if done too early, it may result in "groupthink" whereby the replicater, possibly unintentionally, adopts some of the original developer's practices: features of the original model are essentially "copied". In so doing the replicater has failed to maintain scientific independence. In other situations, replicaters may have different implementation tools and infrastructure, or may be unfamiliar with the original model's plat-

form. Therefore, providing the ability to implement a conceptual model under specific experimental conditions and analysis constraints across multiple platforms and formalisms is critical to lowering the barrier to – and enabling broader adoption of – the practice of reproducibility. Furthermore, by replicating a model and ignoring the biases of the original model, differences between the conceptual and implemented models may be easier to observe. To facilitate replicability, it is critical to provide the larger community with an extensible and platform neutral interchange language for specification, distribution, and transformation of model, simulator, and experimental frame elements. Support for – and a lowered technical barrier to – independent replication will enable computational experimentation to become more scientific. Cross-validation will demonstrate (or not) that the original findings and observed results are not exceptional. Successful replications will strengthen the theories represented by the models.
These observations, coupled with disputes such as Climate Gate (Economist 2010), the microarray-based drug sensitivity clinical trials under investigation, and article retractions due to unverified code and data (Alberts 2010) suggest a pressing need for greater transparency (Peng 2009) in computational science. Besides, unless computational artifacts are designed and disseminated to be discovered, extended, or combined with other models, scientific progress can be hindered. Furthermore, the inability of others to independently replicate and cross-validate published results will slow adoption and use of knowledge embedded within software and models.

Increasing number of computational science communities are emphasizing the role and significance of reproducibility. For instance, the MultiScale Modeling Consortium of the Interagency Modeling and Analysis Group (National Institute of Biomedical Imaging and Bioengineering 2011) promoted credibility in multiscale modeling in biomedical, biological, and behavioral systems as a critical challenge. Among the proposed strategies include executable papers and scientific workflow environments. The Elsevier 2011 Executable Paper Grand Challenge provided a venue for exploring such practical and promising solutions. However, while reusing existing workflow and code scripts help verify published results, they carry the biases of the original implementation. Ongoing reproducibility work can be complemented with new strategies that exclusively aim to support independent replication of a study.

10 CONCLUSION

The position papers summarized in this conference contribution highlight various facets of M&S as a discipline and as a scientific effort. Earlier approaches, as described by Harmon (2002), need to be reevaluated in the light of current discussions, such as reinstated by Padilla et al. (2011) as well as a series of expert panel discussions during recent conference (including this one).

Overall it seems that the importance to better understand the philosophical foundations of simulation science are gaining more importance, hopefully soon resulting in a better understanding of M&S canons of research and other core contributions to the body of knowledge. The ideas presented here are neither a complete nor an exclusive enumeration of topics but meant to contribute to broadening the research agenda hopefully filled with active research over the next couple of years.

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