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THE KEY TO PROMOTING INTERACTION BETWEEN SIMULATION MODELING AND ANALYSIS RESEARCH: PRACTICE

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ABSTRACT

The main thesis in this article draws on the old philosophical maxim from Plato's Republic that "necessity … is the mother of invention." One of the best ways to promote interaction between modeling and analysis research is to look at what is needed in practice. Practitioners focus on solving problems that require good modeling and analysis techniques. However, practitioners tend to be ill-equipped to apply simulation techniques partly because we, as academics, have focused on the distinction between modeling and analysis too much. I share some of my experiences with practice as well as some ideas on how to address these issues.

1 INTRODUCTION

As researchers, we tend to become very specialized and narrowly focused on a particular topic. The premise behind this workshop indicates that this is indeed the case in the simulation field. Furthermore, it is reinforced by our journals and conferences. For example, the *Winter Simulation Conference*, has had separate tracks in Analysis Methodology and Modeling Methodology for at least 20 years. While *WSC* provides the opportunity for interaction by hosting these tracks at the same conference, the fact remains that the two communities remain quite distinct.

Does this mean that specialization in research is bad? Not necessarily because researchers require specialized technical skills to develop theory and advance the field. However, the theory developed should be motivated by practice in the manner suggested by Cooper and McAlister (1999). By necessity, researchers who work on "applications-driven theory" are less likely to lose sight of other aspects of the field and more likely to engage in interactions with a broader set of researchers across a field (or across multiple fields).

2 SCHLUMBERGER: A PERSONAL CASE STUDY

As a case in point, let me share from my own experience on work found in Morrice et al. (1999), Butler et al (2001), Gavirneni et al. (2001), and Mullarkey et al (2007). From 1996-1999, I worked as a visiting research scientist and consultant with Schlumberger at its Austin Product Center in Austin, TX in a corporate research group known as End-to-End Simulation. In this group, I worked with other researchers and field personnel to improve operations primarily in seismic explorations which are vital to the discovery of oil and gas.

The largest project, chronicled in Mullarkey et al. (2007), involved the development and analysis of a simulation model for global land seismic operations. Since these operations differ significantly worldwide due to factors such as geophysical requirements, climate, terrain, economic factors, and legal restrictions, the modeling was extremely complex because the model had to be robust enough to be deployed anywhere that seismic exploration was used. For this project, we were privileged to meet key field personnel from around the world, visit several job sites, and gain access to a lot of good data. Interacting with practitioners and observing operations were both keys to the success of this project not only from a modeling perspective but from an analysis perspective as well. Note that Mullarkey et al. (2007) contains some fairly basic simulation output analysis by the standards of our community, but this was what was needed by the users of the simulation model at the time. Subsequent interactions with field personnel lead to the development of more sophisticated procedures found in Butler et al. (2001).

Over the same time period that I was working with Schlumberger, I attended several talks on Ranking and Selection at WSC. On multiple occasions, I heard the same question from practitioners. How does one do ranking and selection if the simulation has multiple performance measures? While I continued to have a mild interest in this problem, I did not really start researching it until I was faced with the same problem at Schlumberger.

During an actual land seismic survey, a project manager monitors multiple performance measures. These include project cost, project duration, and utilization for all types of crews. Project cost represents the bottom line and is considered the most important performance measure. Project duration, which is positively correlated with project cost since variable costs such as labor increase with the duration of the job, is included because certain things such as reputation for finishing the job in a timely manner may be difficult to price. The crew utilization is monitored to ensure that crews are not over-utilized. Crews work under very adverse conditions on many land seismic surveys. Over-utilization of crews can lead to worker dissatisfaction, poor quality work, attrition, and unsafe working conditions. Again, things such as worker dissatisfaction and work quality may be difficult to cost. Therefore, these measures are monitored in addition to cost.

When faced with this problem, a few things became apparent:

1) The literature contained very little research on how to address this problem.
2) Vector comparisons are hard.
3) I needed help from someone who had more experience at addressing problems under item 2), so I teamed up with a decision analyst.

At first, the problem seemed rather simple. We would apply multiple attribute utility theory (this seemed natural since we had access to business managers who were comfortable expressing their preferences to construct utility functions), get a scalar performance measure, and then apply a standard indifference zone approach ranking and selection procedure. The problem is the indifference zone had no direct physical meaning on a utility scale. Hence, we had to develop a utility exchange approach where we selected one criterion as the standard of measurement and then exchanged utility on the other performance measures for utility in the standard measure. Initially, we were able to do this for an additive multiple attribute utility model, but then we generalized it to a multi-linear utility function.

The bottom line is that by interacting with practitioners and being motivated by practical issues we were able to understand and develop advanced modeling and analysis techniques and work with others, in this case, from across different fields. We were also able to apply other OR methodologies when appropriate both inside the simulation model and separately (Morrice et al. 1999).

Incidentally, this is not the only project on which I have used this approach successfully. Work found in Loveland et al. (2007) and Monkman et al. (2008) was done with Dell, Inc. While most of it is not simulation but rather optimization., the same principle applies. Currently, I am working with a retailer to determine inventory and distribution management techniques in the face of natural disasters such as hurricanes.

3 POSSIBLE SOLUTIONS

3.1 Better Education (Especially in Business Schools)

What I have found from practice is that practitioners are much more willing to model (at least at first) than analyze. There are probably several reasons for this but I will only discuss two:

1) The disconnect between modeling and analysis often found in simulation software.
2) The lack of practitioner preparedness to do simulation, especially on the analysis side.

I speak from the experience of an American business school professor, so my reference set is probably at the lowest level you will find. This is not the students' fault. In fact, my students are some of the brightest you will find anywhere (the McCombs School of Business Undergraduate Program is in the top 10 in a number of US national rankings <http://colleges.usnews.rankingsandreviews.com/college/spec-business> and <http://bwnt.businessweek.com/interactive_reports/undergrad_bschool_2009/>). They just have very little context for simulation, especially discrete event simulation. When I teach simulation in the framework of process analysis, most students grasp the basic modeling concepts but many struggle with even basic statistical concepts. Simulation software has a nice graphical user interface to build "flowchart-like" models but the data, reports, and occasional graphs tend to confuse or overwhelm them (note: they do like the animation even though I warn them that it is mostly just for demonstration purposes). Maybe more user-friendly analysis tools are the solution, but I think the problem goes deeper. I think there is a fundamental disconnect between modeling and analysis that needs to be corrected. Like many others, I try to solve this problem by starting with very basic process analysis (simple examples, completely deterministic, using spreadsheets, etc.) which helps, but I do not think this is the complete solution.

Now one might be tempted to suggest that business schools are a lost cause and that simulation technology is more relevant in engineering anyway. I would argue against this because in business, processes abound. Furthermore, one who knows how to manage these processes better is more competitive than one who does not. If we as academics have learned anything from the current financial crisis, it is that we should not leave it all up to Finance in business schools and have business school graduates view everything else as secondary. Another factor is that the information technology that serves as process backbone in many companies is ripe for simulation to do prediction and forecasting. The flow-chart like model structures are already in place. It is time to work with practitioners to develop simulation methods that address their problems. As it turns out, most of these systems are highly dynamic and never achieve steady state because of changing factors. Hence, they cannot be analyzed using steady state analysis techniques. Rather transient analysis methods must be employed. While the problems are harder, there is a real need for more research on how to analyze transient systems efficiently.

3.2 Mimic Our Successes

I think we have some good examples to mimic in our field:

1) Semi-conductor manufacturing
2) Risk Analysis
3) Healthcare.

Due to the level of complexity in semi-conductor manufacturing, practitioners and researchers interacted out of necessity. This resulted in good modeling and analysis work that has solved significant problems for the industry (see, for example, Park et al, 2002, Dabbas et al., 2003, Godding et al., 2003, Schruben and Roeder, 2003).

Risk Analysis combines advanced stochastic modeling with finance. Finance as a discipline is particularly adept at developing theory that impacts practice. This has provided researchers in the simulation community who do work in risk analysis the opportunity to interact with practitioners and at the same time do fundamental research. What distinguishes the risk analysis research track at WSC from other tracks is the level of rigor in the work, the relevance of the work to practice, and the integration of modeling and analysis (see, for example, Chen and Glasserman, 2008, Glasserman and Kim, 2008, Baysal et al, 2008, Lesnevski et al. 2007).

There have been "waves" of OR research (including simulation) in healthcare over the years. While it may be too early to tell yet, I am most excited about the latest wave. The medical US medical community seems more open than ever to work with people who do work in OR. OR researchers are more willing than ever to partner with medical experts to come up with implementable solutions to growing problems in our healthcare system. (see, for example, Swisher and Jacobson, 2002, Roberts et al., 2007, Alexopoulos et al., 2008)

These are just some of the successful examples with which I am familiar. I am sure that other examples exist from which we can learn things that I have not covered.

3.3 Publish Our Successes

For an academic, publishing successful applications can be somewhat tricky. For several good reasons, our profession demands that we spend time developing specialized theory to advance knowledge. In many academic settings, the time and incentives to publish the successful applications that may have inspired the theory are lacking. Additionally, relatively few high quality journals exist that publish applications on a regular basis.

In spite of these obstacles, I think that academics in our field should engage with practitioners more regularly and from time-to-time publish the results of successful applications on which they have done work. With the appropriate balance of time, I believe this approach leads to better research in terms of questions, problems, and solution approaches. It would also help a much broader audience understand the relevance of our research. The only articles on which I have received comments from people outside the academy are those which appear in *Interfaces* and in the practitioner section of *Geophysics*. On the flip side of this issue, I believe that a large potion of our work remains largely hidden from business executives because as a field we have been somewhat reticent about publishing successful applications.

So what can we do? Encourage more publications of successful applications, especially amongst senior faculty in top quality outlets like *Interfaces* (in the interest of full discloser, I am the Simulation Editor-at-Large for this journal) and the practice area of *Operations Research.* In the simulation field, we need to come up with standards for practice articles so that they meet the scholarly standards that we expect in our journals. We have worked on this at *TOMACS* (where I am an Area Editor) and have had some success with articles such as Roberts et al. (2007), but I believe that more needs to be done.

The bottom line is that practitioners need to see well-written, substantive articles on successful applications. These articles provide the opportunity to demonstrate the power of sophisticated modeling and analysis techniques developed in our research.

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