LETTER TO THE EDITOR

Leverage points to march "upward from the aimless plateau"

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Yaman Barlas received his Ph.D. in Industrial and Systems Engineering from Georgia Institute of Technology in 1985, joined Miami University of Ohio, department of Systems Analysis, and became tenured in 1990. He returned to Boğaziçi University in Istanbul in 1993, where he is currently working as a professor of Industrial Engineering and directing the SESDYN research laboratory (http://www.ie.boun.edu.tr/labs/sesdyn/). His interest areas are validation of simulation models, system dynamics methodology, modeling/analysis of socio-economic problems and simulation as a learning/training platform. He is a founding member and a former President of the System Dynamics Society, editor of *System Dynamics Review* for short articles and an invited Honorary Editor of the Encyclopedia of Life Support Systems.

Professor Forrester's paper, "System dynamics—the next fifty years" (Forrester, 2007) offers many provocative observations and tough challenges. This paper (and the speech upon which it is based) strikes a few dramatic themes throughout: (1) Many people doing system dynamics work have "only a superficial and unworkable preview of the potential of the field" mainly because they "enter the field without the training that would allow them to reach the full potential." (2) Yielding to academic pressures, qualified system dynamicists write to satisfy narrow academic interests only, "retreating from major real-world issues"; there are no recent books/activity on important policy issues, addressed to the public. (3) Largely due to (1) and (2) above, "System dynamics is still far from reaching the quality of work to which we should be aspiring . . . we should consider the possibility that work in the field is declining in average quality" and ". . . the need to begin debating how to raise quality and scope in applications, published papers, and especially in academic programs."

The above claims are quite radical, provocative and are particularly courageous, considering the fact that they are expressed by the founder of the field. After all, Prof. Forrester argues that most of us are unqualified, poor modelers, turning out low-quality models of problems that have no relevance to the real world! This is quite dramatic, given the fact that modeling is essentially what (we think) we do. I personally agree with the general sense of the above three observations. But when it comes to causes, consequences and potential cures for these problems, I have a few complementary items to add and also some disagreements. In this brief note, I will suggest a few potential leverage points and also comment on a couple of points of disagreement.

The importance of small, high-quality models

As Professor Forrester points out, there are too many system dynamics models—published or applied—that do not meet our minimum standards of quality. In my view, there are many causes of this problem, some of which are mentioned in Forrester's paper: lack of formal SD education, modeling the "wrong" problems, the inherent difficulty of SD, no formal/clear accountability for poor modeling, and the tendency to build (unnecessarily) large models.

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Large models are not only difficult to build: they are also nearly impossible to understand, test (by the modeler or a third party), and evaluate critically. One of the traps that novice modelers fall into is building big models to address big issues. The novice assumption is that the more comprehensive and detailed the model, the more "valid" it tends to be. This is wrong and dangerous. (Randers (1980) is a basic reference on model detail, validity and guidelines; Repenning (2003) discusses model size and usefulness.) I have seen many modelers at conferences mentioning the size of their models (number of equations, variables) as an indication of model realism and quality. To make the problem worse, in applied work, most clients pressure the model builder to construct models far more detailed than necessary (Roberts, 1978). Thus there is a tendency to model "big problems with big models". As Prof. Forrester invites us to model "big policy issues", I am concerned that we may end up with more huge models that are impossible to test and evaluate. I am certainly not suggesting that Prof. Forrester is unaware of this problem. Indeed, he has similar warnings regarding unnecessarily large/detailed models and on the importance of small generic models in several of his writings (*Industrial Dynamics*, Ch. 13 and Appendix O; Forrester, 1961). I am suggesting that model parsimony and simplification should be explicitly stated and used as a criterion of model quality (see Saysel and Barlas, 2006, for a recent discussion of simplification and validation). But can all big issues be captured by small models? This is related to the next item.

Focusing on the defining characteristics of SD in choosing problems

A big issue can be modeled nicely by a small SD model, if the problem involves a setting with dynamic feedback complexity for which SD is useful. Conversely, even a small problem cannot be meaningfully addressed with a huge SD model if the problem is of a non-systemic nature. It is all in SD problem definition: the system dynamics method addresses problematic behavior patterns caused primarily by the feedback structure of the setting. So SD is not about static (scale or relation) complexity, or about point forecasting or about detailed I-O simulation. There are many well-known methods (OR, regression, event simulation) much better suited for such problems. Dynamical *patterns* and the role of internal *structure* are the two defining keywords in SD problems. On the other hand, my observation is that many so-called SD modeling projects are about problems that simply do not have SD characteristics—lost battles to start with. Poor-quality models that Prof. Forrester is concerned about are in many cases due to the application of SD to problems for which it is not suited. Recently, with the proliferation of user-friendly modeling software, the misapplication of SD modeling has become an even easier temptation. One must keep in mind that an application of some SD software does *not* automatically

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yield an SD project. An excellent basic reference on choosing and defining good system dynamics problems is Roberts *et al.* (1983, several chapters) and more recently Sterman, 2000 (see also Barlas, 1998). We should exercise special care in applying "SD modeling to SD problems", or else use other methods.

Communicating with the non-SD world

Prof. Forrester talks about one aspect of our communication problem: talking to the public; I will emphasize another aspect: communicating with the scientific and technical community. Being part of several such communities (Industrial engineering/OR, simulation, some other "systems" communities and SD), I argue that we have a serious communication problem (see Barlas, 1998). Our communication failure is due to two extreme attitudes: (1) we sometimes try to sell our *method*, acting like an SD salesperson or evangelist; (2) disappointed, we cut off all communication and completely isolate ourselves. Effective and meaningful communication should emphasize the problems addressed and our findings—not explicitly our method. A fascination with methodology can be fine within the SD community and may be needed to advance our modeling and analysis tools, but it is a terrible communication barrier with the outside world. At yet another harmful extreme, there is the habit of hiding that what we are doing is SD work. If we do high-quality modeling work and present the problem, the essential structure of the model (the model *structure*, not Stella or Vensim diagrams!) and our results, there is no point in either overselling or hiding SD (see Repenning, 2003, for a comprehensive discussion of this communication issue).

To communicate with the larger scientific community, we must consider their broader scientific standards, habits and jargon, without compromising SD standards. In particular, Prof. Forrester states that historical data fit is often irrelevant and even misleading in SD work. This may be true, if by historical fit we mean point-by-point fit. In SD modeling, proper measures of historical fit would stress fitting the past dynamical *patterns*, such as periods, amplitudes and trends. We can and must quantitatively measure and test such pattern fit and present the results as quantitative/empirical evidence of model quality (Barlas, 1996). The same principle applies to the prediction of future patterns: we should measure and test a model's predictive power of such patterns, but not individual points or events. There are now tools and software that focus on patterns: behavior-testing software (BTS) and indirect structure testing software (SiS) (Barlas *et al.*, 1997; Barlas and Bog, 2005). We must improve these tools and develop new ones. Finally, it is important to remember that in SD structural validity must precede output validity. So the comparison of model behavior against time-series and other data is useful and desirable, provided that the structure of the model has been sufficiently tested first (Barlas, 1996). Otherwise, as Prof. Forrester points out, historical data fitting may mask severe

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structural flaws in the model (see Homer, 1997, for a discussion of data and structure, and Sterman, 2000, for many examples of proper use of historic data).

Formal university-level education as the critical bottleneck (and leverage)

Prof. Forrester makes two important points in several places: (1) we need to write books for the public, on big policy issues; (2) we face a severe shortage of high-quality SD experts. I basically agree, but when these two points are put together, it seems like there is an infeasible situation and the primary bottleneck is in the education of SD experts. If we over-publicize without sufficient modeling capacity, we would experience the well-known boom-then-bust (stagnation) dynamics. One may even argue that we did experience such dynamics in the 1970s when *World Dynamics* and *Limits to Growth* created unprecedented popularity and debate that a very limited SD workforce capacity was unable to sustain and pursue. To build up our capacity, we need increased SD education, and for this we need sufficient SD educators. Since educators (university or K-12) are primarily trained at universities, the conclusion is that the urgent bottleneck is "university-level SD education" (undergraduate or graduate). University-level SD education is critical, since this is the only type that can produce educators. Establishing universities as an institutional base is important in tackling the other problems listed above: it would contribute to our communication with other fields and gain recognition. Formal education and publishing with academic standards would lead to an increase in the quality of published and applied models. The field needs more examples of books and articles that can be used as examples of excellence. Such work can only be accomplished at universities, by rigorous education and research.

How we start new SD programs, hire faculty and attract students it is beyond the scope of this note. As Prof. Forrester also states, creating SD programs at universities is a formidable task. I would add that it is not realistic to expect university programs exclusively devoted to SD in the short or medium term. A much more realistic and productive goal may be to create "systems" programs, by joining forces with other systemic disciplines that have much in common with SD. Such programs would have foundation courses common to all systemic fields and then special courses for various tracks like SD. Such systems science schools, colleges or institutes could be like engineering schools, but consisting of different options or tracks, not necessarily departments. Currently we are facing a proliferation of systemic disciplines (systems management, soft systems, systems science, systems ecology, systems engineering, systems biology, systemic medicine, etc.), but unfortunately all in isolation. There are remarkable similarities and much potential for synergy between the courses, projects and publications of these fields. It seems like they all belong naturally to an "implicit" field: "systems science" (Sterman, 2002). It may be

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time to turn this new scientific field into an explicit and formal university program. Prof. Forrester ends his talk with a challenge: to develop a plan for the next 50 years. In facing this challenge, may I suggest that the "development of university-level systems education" be the focal point of such a plan.

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