

## System Dynamics Beginnings – Some recollections by Khalid Saeed

### A maverick Group

In 1975, a largest ever group of system dynamics doctoral students joined the system dynamics program in course 15 at MIT. They included Peter Senge, John Morecroft, Barry Richmond, and Khalid Saeed. Three of us were introduced to system dynamics in our previous lives in varying degrees mainly through reading World Dynamics, Industrial Dynamics and Urban Dynamics. Peter was an exception in this group in that he had completed a Masters in System Dynamics at MIT and was already working on Jay's National modeling project, which was the big thing in system dynamics at that time.

Doctoral students already there included Alan Graham, Ali Mashaiykhi, David Andersen, George Richardson, Bill Shaffer, Gill Low, Dale Runge, Matts Lindquist (did I miss any one?). Dick Day was a visiting scholar and Frank Davidson was the chair of the SD Steering committee. Additionally Humayoon Dabiri was a special student and John Lamb a MBA student with intense SD interest. Faculty included Jay, Ed Roberts, Jim Lyneis, Nat Mass, and Ken Britting. Humayoon joined the doctoral program in 1976 and John Serman and Jack Homer came on board in 1977 while Matts and Humayoon dropped out. By 1977-78, Bill Shaffer, Gill Low, Peter Senge, Alan Graham and Dale Runge graduated and joined the faculty and research staff in the group. David Andersen graduated to join the faculty at Sunny Albany. John Lamb also graduated to join big time consulting. I'd say 1977 saw the largest and most vibrant SD group ever at MIT.

We were considered a maverick group by our other doctoral colleagues and in our conversations with them, when we indicated that we were working on system dynamics, it was not uncommon to hear "Why". Indeed, job prospects for a System Dynamics PhD were minuscule compared with other MIT PhD areas and our decision to pursue this area did not make any economic sense. We were all, however, passionate about working on system dynamics. Happily all four of my batch of doctoral students ended up building strong bridges with traditional disciples. Peter became a management guru (remember the Newsweek cover story?), I attempted to colonize economic development and public policy areas, John made name in Strategic Management and Barry invented Stella – the first simulation software with a graphical interface that helped spread the discipline to a very large cross section including K-12 education.

### A computer based discipline in World with limited computing infrastructure

I developed my first system dynamics model at the Asian Institute of Technology using punched cards and a first generation IBM mainframe. Using this technology was a time consuming process and I was thrilled to find printing remote terminals linked with an advanced mainframe in the System Dynamics Group at MIT. Computing resources were, however, still scarce and we were allocated these in terms of dollars representing the price of the computing time. You could not make an endless number of simulation runs within this limited allocation, hence we had to design each simulation experiment carefully. Although I resented this limitation, in retrospect, I think it did create a discipline that forced us to design each experiment carefully and spend some time digesting its results.

### How I discovered beer game?

In spring 1977, I worked as Ken Britting's TA at MIT for the introductory course in system Dynamics 15.871 POS-I. While looking into the SD group archives for teaching materials, I discovered several brown paper roles with stocks and flows drawn on them. I asked Ken what this was. "Oh, this is beer game, we used to run it in class some time ago. Do you want to run it in the course?" said Ken. I dug out all the D-memos I could find on the game and Ken and I ran it in the class after it had been dormant for some time. I worked as a TA for POS-I many times thereafter and always used the game as a part of the syllabus. Apparently this practice has caught on in a big way worldwide! I think, however, the old brown roles were better than the game boards we use today as they had actual stocks and flows drawn on them, but this is a matter of opinion. I have since run the game scores of times and have facilitated building its model and experimenting with it in the classroom, but I have never played the game myself.

### Safety Net

Having come from a third world country where the mention of system dynamics invoked blank stares, I was forced to create a safety net for myself by enrolling in 1977 also in a doctoral program in economic development in course 11 at MIT, without giving up my passionate pursuit of system dynamics. This was fortunate since it provided me an opportunity to bring system dynamics to economic development. I completed doctoral candidacy requirements separately in both the departments with some overlap and Professors Ed Roberts, Lloyd Rodwin and Karen Polenske agreed to serve on my dissertation committee. After expressing many reservations about methodology, Lloyd later stepped down with the admission that he did not understand my model. Alan Strout replaced Lloyd. I developed a model explaining income distribution under various land tenure and renting regimes, which continued to generate publications over some 20 years after I graduated. This link with economics would not have been possible without my attempt to create a safety net.

Lloyd and I later remained friends until he passed away and he invited me several times to his gracious home and to lecture in his SPURS program seminar when I worked briefly at Dartmouth College and later when I came to visit. He even came to visit me at the Asian Institute when I worked there. His personal reservations about the methodology were instrumental in driving my effort to integrate with the traditional literature and make my model robust.

### Publishing in traditional forums

It is always difficult for the students of a new emerging discipline to publish in recognized peer reviewed fora. Given that there was a substantial publication requirement in our doctoral program, it was important to find journal that would be receptive to our articles and would have the expertise to judge their content. It is not surprising that our earliest publications went to IEEE transactions and Simulation, both dominated by engineers but not limited to engineering agendas. My first Paper, coauthored with Tony Picardi was published in Simulation. Other journals my students and I targeted after I graduated included Technological Forecasting and Social Change, Socio-Economic

Planning Sciences, and World Development. My first article submitted to World Development sat in the Editorial office for several years. I finally gave up on it and informed the editor that I am withdrawing it for submission to Socio-Economic Planning Sciences, where it was published after a few revisions. A couple of years after its publication, I was surprised to find a letter in the mail from Paul Streeten, the Founding editor of World Development, saying that the article remained buried somewhere and he just found and read it and found it fascinating. He asked that if it was not yet published, he would like to publish it in World Development. The letter expressed profuse regrets for the delay and ended with “In sack cloth an ashes, Paul Streeten”. I did publish subsequently in World Development, twice, after this episode.

I also tried to submit to Economic Journal and received extensive reviews that contradicted one another. The article I submitted was about the multiple perspectives on income distribution that are implicit in conflicting economic theories and how they in fact represent multiple manifestations of the same economic structure. One of the reviewers said that this article was about neoclassical economics and that we already know all that was said about it. The other reviewer said that the article was in fact all about Marxist economics and that the audience knew all about it. I sensed it would be difficult to make the point I was trying to make in a journal length article, hence I started working on my book, Development Planning and Policy Design, A system Dynamics Approach, which was published by Ashgate in 1994 and is still in print 12 years after its first imprint. I was happily surprised to find a rather positive review of this book published in Economic Journal a couple of years after the publication of the book. Among other things, the review said that the book presented an innovative approach to development policy. It should be added that Economic Journal published the rather caustic critique of World Development titled Measurement Without Data by William Nordhaus in 1973, but declined publication of its refutation, which appeared in Policy Sciences in 1974.

More recently, our colleague Mike Radzicki has created collegial links with Journal of Economic Issues and the institutional economics audiences, which are important potential outlets of our work.

### Stella

I came to the Resource Policy Center at Dartmouth College on a visiting assignment in 1983 as a replacement instructor for Dennis Meadows who was taking a sabbatical to go to IASA in Vienna (what enormous shoes I needed to fill!!). I had the pleasure of working with my old pal Barry and Dana. Also there were Tom Adler, who worked on quantitative methods, Michael Masuch, a visiting scholar from the Netherlands and a wonderful group of extremely bright graduate students. In 1984, Apple came out with Macintosh and we all ordered our machines through Dartmouth's computer shop. When the machines arrived, Barry was already talking about a modeling software that would work like Mac did, went far beyond DYNAMO and reduced model building to playing with icons. I advised Barry not to get into software development as this effort may not contribute significantly to his tenure track at Dartmouth. I am so glad Barry did not listen to me and went ahead to develop Stella. Of course, I was right in predicting that this effort will not contribute significantly to his tenure track, but I could not predict the effect

it would have on the growth of system dynamics. Thank you Barry, for ignoring my advice.

While at Dartmouth, I once read an article in the AER by Professor Lewis, which suggested that developing country economies are different in structure compared with the advanced industrial economies. It advocated use of different models to design interventions in the two cases. I seemed to disagree with the latter inference, as I believed that the two cases were different manifestations of the same economic structure and that we'll lose sight of the policy space for intervention if we developed situational models to fit each manifestation. In our collegial discussions, Barry suggested writing to Professor Lewis about my viewpoint. Our friend Mike Masuch laughed and said that if a no-body wrote to a Nobel laureate, he/she will get a reply, but it would be a one-liner. I did write an elaborate letter to Professor Lewis outlining my perspective without expecting to get any reply. Several weeks later, there was a letter in the mail with Arthur A. Lewis as sender. I waived it to Mike saying that I did get a reply from a Nobel Laureate. My colleagues gathered around me as I opened it. It was a one-liner. It said: Dear Professor Saeed, I have retired and I do not review papers or respond to arguments any more. Yours sincerely, Arthur Lewis.