ON THE HISTORY, THE PRESENT, AND THE FUTURE OF SYSTEM DYNAMICS

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Summary

This is a personal recollection of the history of system dynamics and observations about its present condition and future prospects. It treats the history in two parts: first my personal background and how all the threads came together to initiate the field of system dynamics, and second, the historical development of the early cornerstone projects that shaped the field. These early works included industrial dynamics, urban dynamics, world dynamics, and the National Economic Model. The article continues with a brief assessment of the present condition of the field and finally ends with my expectations for the future. My long-term expectations and projections deal primarily with educational issues.

1. The History

Here is a personal recollection of the historical development of the field of system dynamics. It consists of two parts: first, to relate how my personal background and experiences gradually converged to form the field of system dynamics, and second, to describe the historical development of the early cornerstone works in system dynamics.

1.1. Origins

I grew up on a cattle ranch in Nebraska in the middle of the United States. A ranch is a crossroads of economic forces. Supply and demand, changing prices and costs, and the economic pressures of agriculture become a very personal, powerful, and dominating part of life. Furthermore, in an agricultural setting, life must be very practical. It is not theoretical; it is not conceptual without purpose. It is full-time immersion in the real world. In high school, I built a wind-driven electric plant that provided our first electricity. That was a very practical activity. When I finished high school, I had received a scholarship to go to the Agricultural College when one of those important turning points intervened. Three weeks before enrolling at the Agricultural College, I decided it was not for me. Herding cattle in Nebraska winter blizzards never had appealed to me. So instead, I enrolled in the Engineering College at the University of Nebraska. Electrical engineering, as it turns out, was about the only academic field with a solid, central core of theoretical dynamics. And so, the road to the present began.

After the University, my first year as a research assistant at the Massachusetts Institute of Technology brought another turning point. I was commandeered by Gordon S. Brown, who was the pioneer in "feedback control systems" at MIT. During the Second World War my work with Gordon Brown developed servomechanisms for the control of radar antennas and gun mounts. Again, it was research toward an extremely practical end that ran from mathematical theory to the operating field, and I do mean the operating field. At one stage, we had built an experimental control for a radar to go on an aircraft carrier to direct fighter planes against enemy torpedo bombers. The captain of the carrier *Lexington* came to MIT and saw this experimental unit, which was planned for redesign to go into production a year or so later. He said, "I want that, I mean that very one, we can't wait for the production ones." He got it. About nine months later we heard that the experimental control units had stopped working. I volunteered to go to Pearl Harbor to see why the controls were not functioning. I had discovered the problem, but not had time to fix it, when the executive officer of the ship came to me and said they were about to leave port. He asked if I would like to come with them and finish my job. So I said "Yes," having no idea quite what that meant. We were offshore during the invasion of Tarawa and then took a turn through the middle of the Sunrise and Sunset chains of the Marshall Islands. The islands were occupied on both sides by Japanese fighter-plane bases and they didn't like having a US Navy Task Force wrecking their airports. So they kept trying to sink our ships. After dark they dropped flares along one side of the task force and came in with torpedo planes from the other side. Finally at 2300 hours they succeeded in hitting the *Lexington*, disabling one of the four propellers and setting the rudder in a hard turn. Again, it gave a very practical view of how research and theory are related to field application.

At the end of the Second World War came another turning point. I had about decided either to get a job or start a company in feedback control systems. Gordon Brown again intervened; he was my mentor for many years at MIT. He had a list of projects that he thought might interest me. I picked from the list the building of an aircraft flight simulator. It was to be rather like an aircraft pilot trainer, except that it was to be so precise that instead of acting like a known airplane, it could take wind tunnel data of a model plane and predict the behavior of the airplane before it was built. This project was promoted by Admiral Louis deFlorez of the US Navy. The aircraft simulator was planned as an analog computer. It took us only about a year to decide that an analog machine of that complexity would do no more than solve its own internal idiosyncrasies. An analog computer could not deal with the problem at hand. Through a long sequence of changes we came to design the Whirlwind digital computer for experimental development of military combat information systems. The Whirlwind computer eventually evolved into computers for the SAGE (Semi-Automatic Ground Environment) air defense system that was installed across North America.

The SAGE air defense system was another one of those practical jobs where theory and new ideas were only as good as the working results. The SAGE system had about 35 control centers, each 160 feet (ca. 49 meters) square, four stories high, and containing about 80,000 vacuum tubes. These computer centers were installed in the late 1950s, and the last was decommissioned in 1983. They were in service for about 25 years. The statistics show they were operational 99.8% of the time. That would be less than 20 hours a year that a center was out of operation. Such reliability was a remarkable result, considering that these centers contained so many vacuum tubes. Even today, such reliability is a hard record to match.

It was time for yet another turning point. In 1955, James Killian, who was then president of MIT, brought a group of visiting dignitaries to see us at Lincoln Laboratory. While I was walking down the hall with Killian, he told me of the new management school that MIT was starting, and suggested that I might be interested. The Sloan School of Management had been founded in 1952 with a grant of $10 million dollars from Alfred Sloan, the man who built the modern General Motors Corporation. The money was given in the expectation that a management school in a technical environment like MIT would probably develop differently from one in a liberal arts environment like Harvard, Columbia, or Chicago. Maybe better, but in any case different, and it was worth $10 million dollars to run the experiment.

In the four years before I joined the School in 1956, standard courses had been started, but nothing had been done about what a management school within an engineering environment might mean. By that time, I had 17 years in the science and engineering side of MIT and it seemed like an interesting challenge to look at what an engineering background could mean to management. It was assumed that an application of technology to management meant either to push forward the field of operations research, or to explore the use of computers for the handling of management information systems. I had a year free of other duties except to decide why I was at the Sloan School.

Chance intervened when I found myself at times in conversation with people from General Electric. They were puzzled by why their household appliance plants in Kentucky were sometimes working three and four shifts and then a few years later, half the people would be laid off. It was easy enough to say that business cycles caused fluctuating demand, but that explanation was not convincing as the entire reason. After talking with them about how they made hiring and inventory decisions, I started to do some simulation. This was simulation using pencil and paper on one notebook page. It started at the top with columns for inventories, backlogs, employees, orders, and production rate. Given these conditions and the policies they were following, one could decide how many people would be hired in each following week. This gave a new condition of employment, inventories, and production. It became evident that here was potential for an oscillatory or unstable system that was entirely internally determined. Even with constant incoming orders, one could get employment instability as a consequence of commonly used decision-making policies. That first inventory-control system with pencil and paper simulation was the beginning of the system dynamics field.

The early supply-line simulation is perpetuated today as the "beer game," available from the System Dynamics Society.

Out of that first dynamic analysis came the early beginning of what is now the DYNAMO compiler for system dynamics modeling. An expert computer programmer, Richard Bennett, worked for me when I was writing the 1958 article, "Industrial dynamics: a major breakthrough for decision makers," for the *Harvard Business Review*. That article is chapter two of *Industrial Dynamics*. For that article I needed computer simulations and asked Bennett just to code up the equations so we could run them on our computer. However, Dick Bennett was a very independent type. He said he would not code the program for that set of equations but would make a compiler that would automatically create the computer code. He called the compiler "SIMPLE," meaning "Simulation of Industrial Management Problems with Lots of Equations." Bennett's insistence on creating a compiler is another of the important turning points; it accelerated later modeling that rapidly expanded system dynamics. Jack Pugh later extended the early system dynamics compilers into the very influential DYNAMO series.

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Biographical Sketch

Professor **Jay W. Forrester** received a B.Sc. in electrical engineering from the University of Nebraska in 1939 and an M.Sc. from MIT in 1945 and has been awarded honorary doctorates from nine universities. Before creating the field of system dynamics in the 1950s, Professor Forrester was a pioneer in the early development of digital computers and invented the random-access, coincident-current magnetic storage memory. He directed the System Dynamics Program at the MIT Sloan School of Management until 1989 and has written numerous articles and books on the theory and practice of system dynamics. Jay Forrester has been recently applying system dynamics to understanding economic behavior. He is also developing materials for system dynamics as a foundation for a new kind of education, for kindergarten through grade 12.

His honors include: Valdemar Poulsen Gold Medal from the Danish Academy of Technical Sciences (1969); Medal of Honor, Institute of Electrical and Electronics Engineers (1972); National Inventors Hall of Fame (1979); James R. Killian Faculty Achievement Award, MIT (1987), and National Medal of Technology (1989).