The Evolution of Systems Analysis and Control: A Personal Perspective

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The foundations of systems analysis and control as we know them today were laid for the most part at MIT's Radiation Laboratory during World War II and a period thereafter. Most of the founders—both in the United States and abroad—are no longer with us. As one who had the privilege of knowing Wiener, Bode, Nyquist, Guillemin, Gordon Brown, Sam Mason, John Coales, Aizerman, Pontryagin, Letov, Bellman, and many others, I have some personal perceptions and reminiscences that your Editor has asked me to put on paper. It is my pleasure to do so. However, what should be stressed is that—given the vastness of the subject—what I will have to say will touch upon only a small subset of the issues and events that were at the center of attention.

Some Personal History

I came to the United States in 1944 to pursue graduate studies in electrical engineering at MIT. At that time, with the end of the war not far away, MIT did not have many graduate students. Just the same it was an exciting place, towering as a center of instruction and research among all other institutions of higher learning in science and technology. Lectures and writings by Wiener, Guillemin, McCulloch, Pitts, and others opened a window to the world of communications, control, computers, and cybernetics. The cold winds of the Cold War were beginning to blow, but the future of science and technology looked bright and full of promise.

As a student at MIT, I was deeply influenced by Guillemin, as were many of my classmates. Guillemin was the foremost exponent of network analysis as a discipline with a precise framework, elegant synthesis procedures, and promise of important practical applications. I was enthralled by Guillemin's lectures, but I could also discern that network analysis as taught by Guillemin was oblivious of noise, nonlinearities, and imprecision. In my discussions with Guillemin, I expressed the view that at some time in the future networks would have to be designed through the use of computers to bridge the gap between the ideal and the real world. To Guillemin that was rank heresy.

I was also influenced by Professor Fano, who taught a mathematically-oriented course on antenna theory. As a topic for my master's thesis I decided to explore the concept of a helical antenna. Fano did not think much of the idea and declined to supervise my work. But I had confidence in my judgment and decided to pursue the topic under the supervision of Professor Parry Moon. My master's thesis was never published, but helical antennas became important anyway.

After I received my S.M. degree early in 1946, Guillemin urged me to stay on as his research assistant, but I felt an obligation to move to New York, where my parents settled after coming to the United States. I found a job as an instructor in electrical engineering at Columbia University and, progressing through the ranks, stayed there till 1959—when I moved to Berkeley.

The beginning of my teaching career in 1946 at Columbia University coincided with the beginning of the Cold War. There were many sources of support for defense-oriented research, and I became a member of a group headed by Professor John Ragazzini that did consulting work for the U.S. Air Force through the M.W. Kellogg Company and Norden Corp. Much of the research centered on methods of prediction in the presence of noise. Out of this work came my 1949 report (and 1950 paper) "An Extension of Wiener's Theory of Prediction," which was co-authored with Ragazzini. This paper introduced some concepts and techniques that had a discernible impact on prediction and filtering in later years.

I received my Ph.D. degree in 1949. My thesis was concerned with the frequency analysis of time-varying networks. Although my thesis ostensibly dealt with networks, in reality it was a method for dealing with time-varying linear systems. This was my first venture into systems analysis and marked a turning point in the orientation of my work. After the publication of my thesis, I wrote a number of papers on linear time-varying systems and initiated what I thought was a novel direction in the analysis of nonlinear systems. In 1952, in a joint paper with Ragazzini, what has come to be known as the method of z-transformation was described. Z-transformation is in wide use today in filtering and signal processing. I was also fascinated by Shannon's information theory and intrigued by the possibility of designing machines that could

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mimic human reasoning. In 1950, I wrote a paper entitled "Thinking Machines—A New Field in Electrical Engineering," which was published in Columbia Engineering Quarterly. Like many others. I had greatly underestimated the difficulty of designing machines that can approximate to the remarkable human ability to reason and make decisions in an environment of uncertainty and imprecision.

Formative Years of System Theory

The early '50s at Columbia University were the years of flourishing research on systems analysis and control. Among those who were active participants in this research were Eli Jury, Gene Franklin, Jack Bertram, and Bernie Friedland. It was during this period that the idea of what is now known as system theory began to crystallize in my mind. There was some earlier work by Ludwig von Bertalanfy on what he called "Theory of General Systems," but his approach had a different agenda and was philosophical and biological in its orientation. I described my conception of system theory in a 1954 paper entitled "System Theory," which—like my thinking machines paper—was published in Columbia Engineering Quarterly.

A work that made a pronounced impact on my thinking was that of E.F. Moore on finite-state automata. I was highly impressed by its elegance, simplicity, and relevance to real-world problems. Influenced by Moore's work, I began to teach a course on sequential machines and continued to do so for more than a decade. Another direction that I began to pursue involved the use of multivalued logic in coding and systems design. The 1953 thesis by my student Oscar Lowenschuss on the application of multivalued logic to logical circuit design was one of the first in its field. Another important thesis by my student Werner Ulrich dealt with applications to multivalued coding.

At about that time Rudy Kalman entered Columbia as a graduate student. I remember distinctly that Ragazzini asked me to look at his application for admission. I could discern in his application that Kalman was a highly original thinker and a man of great promise. His later accomplishments confirmed my expectations. Kalman's work on filtering and systems analysis opened new directions and had, and continues to have, a major impact.

Although I am an electrical engineer by training, I have always been close to mathematics and mathematicians. As a strong believer in the power of mathematics, I viewed my mission as a teacher and researcher to be that of "precisifying and mathematizing" the foundations of systems analysis and control. This is the influence that I exerted on my students and colleagues. The 1956 paper in which I coined and defined the term "system identification" exemplifies my motivation at that time.

The mathematization of systems analysis and control got a strong boost from the launching of the Sputnik and the beginning of the Space Age in 1956-57. The entry into the field of eminent mathematicians like Pontryagin, Bellman, Lefschetz, and many others has marked a turning point in the direction of research in systems analysis and control, placing a high priority on the development of optimal control methods for space guidance and navigation. I will have more to say about this at a later point.

In 1956-57, I became a visiting member of the Institute of Advance Study in Princeton, NJ. In Princeton, I learned a great deal about logic from Stephen Kleene, one of the foremost logicians of our time. I also developed a close relationship with Professor John B. Thomas in electrical engineering, with whom I collaborated on research in communications and system analysis. My stay at the Institute had a significant impact on my intellectual development and laid the groundwork for my later work on fuzzy logic.

In 1956, thanks to my knowledge of Russian, I became one of the first to become familiar with the work of Pontryagin on what has come to be known as Pontryagin's maximum principle. I was deeply impressed by Pontryagin's work and through lectures and talks contributed to the propagation and adoption of his ideas in the United States. However, with the growth in my familiarity and understanding of Pontryagin's work, I began to feel that—beautiful through it was—its effectiveness as a tool for the solution of realistic problems was rather limited. This was the beginning of my doubts about the ability of mathematical tools of high sophistication to address the complex and not very well defined problems that pervade systems analysis and control.

In the early '60s my doubts were not shared by many. The ascendancy of mathematical methods was unchallenged, and Lyapounov's stability theory and differential-equations-based theory of nonlinear systems moved to the center of the stage, as did the problems relating to optimal control and systems optimization. The highly original work of Rudy Kalman on controllability, observability, and filtering was at the center of attention. Another contribution of major importance, whose impact transcended disciplinary lines, was Bellman's development of dynamic programming. I became acquainted with Bellman's work in 1954 and perceived dynamic programming as a powerful tool of wide applicability. I suggested to Bellman to submit to the IRE Proceedings a paper describing his work. He did so, but to my embarrassment his paper was rejected by the referees, who felt that Bellman did not provide convincing examples of practical applicability. It is ironic that about 30 years later, Bellman was awarded IEEE's Medal of Honor for his development of dynamic programming.

Fuzzy Logic

Earlier in 1962, I wrote a note entitled "A Critical View of our Research in Automatic Control," which appeared in the *IEEE Transactions on Automatic Control*. In this note, I articulated my feelings that the solution of real-world problems in systems analysis and control was being subordinated to the development of mathematical theories that dealt with over-idealized problems bearing little relation to reality. In expressing this view, I was not questioning the power of mathematics *per se*. In essence, what I was questioning was the effectiveness of traditional mathematical methods—methods that are intolerant of imprecision and partial truth. A decade later, I articulated similar views in a note entitled "A Rationale for Fuzzy Control," which was published in the *ASME Journal of Dynamic Measurement and Control*.

After moving to Berkeley in 1959, I continued to teach courses on linear systems and finite-state automata. In 1963, I co-authored with Charles Desoer the book "Linear System Theory: The State Space Approach," in which a foundation for the state space approach was laid. It was during the writing of this book that my earlier doubts concerning the effectiveness of classical mathematics became reinforced. I began to realize that a mathematization of system theory can be carried only up to a point, and that beyond that point attempts to formulate precise definitions of imprecise concepts like adaptation, robustness, and decentralization were doomed to failure. I began to recognize more clearly than I did before—as articulated in my 1961 paper "From Circuit Theory to System Theory"—that the problem lay in the fuzziness of concepts which I tried to define within the framework of classical mathematics. It is this realization that led me to the concept of a fuzzy set, described in the 1965 paper, which marks the beginning of my work on fuzzy sets and what is now known as fuzzy logic.

My 1965 paper on fuzzy sets drew a mixed reaction. A few, notably Bellman and the logician Grigori Moisil, were supportive and enthusiastic. For the most part, however, what I experienced was skepticism and hostility. Even though I have a thick skin, there were occasions when I had to control my emotions.

One such occasion was a 1972 meeting in France at which I described for the first time the concept of a linguistic variable, that is, a variable whose values are words rather than numbers. After I concluded my presentation, Rudy Kalman delivered a scathing attack. What he said was sanitized in the written version of his comments. Here are a few excerpts, which I cite because of their historical interest.

> "I would like to comment briefly on Professor Zadeh's presentation. His proposals could be severely, ferociously, even brutally criticized from a technical point of view. This would be out of place here. But a blunt question remains: Is Professor Zadeh presenting important ideas or is he indulging in wishful thinking?
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> "The most serious objection to 'fuzzification' of system analysis is that lack of methods of system analysis is *not* the principal scientific problem in the 'systems' field. That problem is one of developing basic concepts and deep insight into the nature of 'systems,' perhaps trying to find something akin to the 'laws' of Newton. In my opinion, Professor Zadeh's suggestions have no chance to contribute to the solution of this basic problem.
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> "Let me say quite categorically that here is no such thing as a fuzzy concept. We do talk about fuzzy things, but they are not scientific concepts. Some people in the past have discovered certain interesting things, formulated their findings in a *non-fuzzy* way, and therefore we have progressed in science.
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> "No doubt Professor Zadeh's enthusiasm for fuzziness has been reinforced by the prevailing political climate in the U.S.—one of unprecedented permissiveness. 'Fuzzification' is a kind of scientific permissiveness; it tends to result in socially appealing slogans unaccompanied by the discipline of hard scientific work and patient observation. I must confess that I cannot conceive of 'fuzzification' as a viable alternative for the scientific method; I even believe that it is healthier to adhere to Hilbert's naive optimism, 'Wir wollen wissen: wir werden wissen.'
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> "It is very unfair for Professor Zadeh to present trivial examples (where fuzziness is tolerable or even comfortable and in any case irrelevant) and then imply (though not formally claimed) that his vaguely outlined methodology can have an impact on deep scientific problems. In any case, if the 'fuzzification' approach is going to solve any difficult problems, this is yet to be seen.
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> "The question, then, is whether Professor Zadeh can do better by throwing away precise reasoning and relying on fuzzy concepts and algorithms. There is no evidence that he can solve any nontrivial problems."

Today, the concept of linguistic variable underlies most of the many successful applications of fuzzy logic. Why did this concept and many other concepts in fuzzy logic engender so much opposition?

I believe that the main reason is that fuzzy logic clashes with the deep-seated tradition of according more respect to numbers than to words—and to modes of reasoning that are precise rather than approximate in nature. But what should be recognized is that precision carries a cost, and that many problems become intractable when precise solutions are sought. My favorite example that relates to this point is the problem of parking a car. We solve this problem every day without making any measurements. We can do this because the final position of the car is not specified with precision. If it were, the cost of solution would be prohibitive. In effect, it is the human ability to exploit the tolerance for imprecision that makes it possible to achieve tractability, robustness, and low solution cost. This is what the conventional methods of system analysis and control fail to do.

Present and Future

Where does control theory stand today? An examination of a typical issue of the IEEE Transactions on Automatic Control reveals a wide gap between the theory and real-world problems. Increasingly, control is becoming task-oriented, especially in the realm of robotics. By contrast, classical control—as reflected in the Transactions—is set-point-oriented. It is a sobering thought that much of control theory as it is taught today is of little if any relevance to task-oriented control. A case in point is the problem of parking a car, which was alluded to earlier. What does classical control theory have to contribute to the solution of this problem in a realistic setting?

In contrast to the classical, differential-equations-based control, fuzzy logic control is fuzzy-rule-based. The use of fuzzy rules provides a language that can be employed by the designer to specify a desired input-output relationship in words rather than numbers. This is the way in which fuzzy logic control is employed in automobile transmissions, air-conditioning systems, and many consumer products. In fact, the main contribution of fuzzy logic is a methodology for computing with words.

The concept of intelligent control was introduced close to two decades ago by Saridis and Fu. Despite its intrinsic importance, it aroused little enthusiasm in the control systems establishment. Today, intelligent control is gaining in recognition and visibility. In my view, fuzzy logic provides a methodology that can serve as a part of the foundation for intelligent control.

In addition to fuzzy logic, the methodologies of neurocomputing and genetic algorithms form a part of the foundation for intelligent control. In a broader perspective, intelligent control rests on what might be called *soft computing*. In essence, soft computing is a consortium of methodologies that provide a foundation for the conception, design, and deployment of intelligent systems. The principal partners in the consortium are fuzzy logic, neurocomputing, genetic algorithms, and probabilistic reasoning. These methodologies are for the most part complementary rather than competitive. Increasingly, the methodologies in question are used in combination, giving rise to what are re-

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ferred to as hybrid systems. At this juncture, the most visible systems of this type are neuro-fuzzy systems, which for the most part are fuzzy rule-based systems in which neural network techniques are employed for tuning and optimization. Hybrid intelligent systems and, in particular, intelligent control systems are likely to become ubiquitous in the years ahead. The time is approaching when it will be necessary to develop measures of what I call MIQ—Machine Intelligence Quotient. Eventually, MIQ may play the role of an important index of machine performance and user-friendliness.

In conclusion, we are entering the era of intelligent systems. The systems that we deal with are becoming more complex, more interdependent, and less amenable to analysis by conventional methods. To adapt to this trend, control and systems analysis are in need of reorientation. More specifically, I believe that systems analysis and control should embrace soft computing and assign a higher priority to the development of methods that can cope with imprecision, uncertainty, and partial truth.

Classical systems analysis and control can point with pride to many brilliant successes. But these successes should not obscure the fact that the world is changing, that high machine intelligence is becoming a reality, and that methods that have proved to be so successful in the past may not provide the right tools for addressing the problems of the future.

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Officer's Communique

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had arrived. He said there were special sessions on Federal Programs in Control Systems Engineering and the History of Control. He said there were 1,388 papers submitted to the 1995 CDC. He noted that Kumpati Narendra was the recipient of the 1995 Bode Prize. He noted there were problems with the review procedures for conference papers. The results of the paper review process should be available much earlier to the program committee. He noted that Djaferis had submitted a report on conference proceedings publications. He stated that 45 students had received travel grants to support attendance at the 1995 CDC using a $16K grant from the NSF.

He summarized other conferences, including the 1994 CDC, which he said had not yet closed its books but was showing a profit of $66K. Yurkovich made some brief comments about the 1996 CCA and reminded the board that the CSS is running only two conference in 1996—the CCA and the CDC. A report was received from Taylor on the 1997 CCA. Antsaklis also covered technical co-sponsored conferences and noted two conferences would be held immediately before and after 1996 CDC in Japan. A report was received from Chow on the 1995 CCA.

Polis asked about the 1996 IFAC World Congress and asked how the attempt to raise money from industrial sponsors was going. Johnson noted that this was in progress and the committee was trying to raise $50K. He stated the expenditures for the conference were close to target. Kimura briefly presented an update on the 1996 CDC. He showed some pictures of the conference site and discussed the arrangements with the hotel and conference center.

Krogh questioned the viability of ISIC. Antsaklis replied that he is worried about this as well. Masten noted the IEEE Book Brokers Program will change and may affect conference income significantly, but that details are unknown at this time. A report was received from Passino on the 1996 ISIC.

Member Activities. Looze stated membership totals were unknown at this time. The Women in Control Committee now has 150 members. There will be a 1996 Chapters Conference held with CSS providing some financial assistance. Looze stated that the History Committee would videotape an interview with Karl Astrom during the CDC. In response to a question on brochures, he noted that 11,300 had been distributed, of which 8,000 were distributed to chapters around the world.

Secretary/Administrator's Report. Birdwell announced the next meeting of the BoG will be held on Tuesday, July 2, 1996, at the San Francisco Marriot Hotel in San Francisco, CA, beginning at 1 p.m., with lunch at noon for BoG members.

Other Business

Atherton announced Doug Looze and Doug Birdwell were retiring from the Executive Committee. He presented them with certificates.

Adjournment

The meeting was adjourned at 6:10 p.m.