At the Gates of the Millennium: Are We in Control?

Panel discussion organized by
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Panelists

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1 Introduction

Panos Antsaklis:

The 20th century has been full of marvelous advancements in science and technology that have changed dramatically the way we live and work, with the most recent example being the role the internet has assumed in our everyday lives. Our area of Systems and Control is based on firm mathematical foundations and significant theoretical contributions to the area have been made in the past half century. However it sometimes appears that we have not been taking full advantage of the incredible advances in sensor, actuator and microprocessor technologies that are being taking place. If that is the case, what do we plan to do in the future to meet the challenges of the 21st century?

There are challenges in designing highly complex engineering systems to meet very ambitious goals in manufacturing and process industries, in transportation and communications to mention but a few. In addition, all these systems are expected to perform well with minimum human supervision, that is with higher autonomy. This presents considerable challenges but also wonderful opportunities, as advances in sensors, actuators, microprocessors and computer networks offer unique opportunities to implement ambitious control and decision strategies. To meet the challenge we will need to develop new methodologies, new ways of addressing control problems and will also need to adjust the way we teach control to students. Changes in control education together with adjustments in research directions and improvement of the public's awareness of our role and contributions may provide the necessary foundations and tools to meet this challenge in the 21st century.

It was decided to organize this panel discussion to hear the opinions of a number of experts who agreed to comment on these issues. The panelists come from universities and industry, and their contributions to the field of systems and control collectively span many decades. They bring to the discussion their considerable expertises and experiences, they review past notable research results and make recommendations on how to meet the future challenges.

Organization: This is a conference wide event lasting approximately 90 minutes. For the panel discussion, the panelists were asked to prepare brief presentations in response to the first 2 questions below (past, and desirable future research milestones) and in addition to be prepared to discuss in the ensuing discussion the last 3 issues in the list below (education, technology, computational tools). Here is the list:

1. Identify the 5 most notable research results in systems and control theory in the past 100 years.

2. Identify 5 future research milestones (for the next 5-10 years) which will have a most significant impact on the field. Comment on what we should be doing now and in the near future to make such accomplishments possible.

Be prepared to discuss:

3. Education issues. How should we be training our students to meet the future challenges?

4. Technology issues. What do we see coming up that will change the control landscape?

5. Computational tools. Are we addressing the need for computational tools to apply our new theories and methodologies?

Schedule: The panel discussion is structured as follows:

a) Introductory remarks by the chair Christos Cassandras and Kishan Baheti of the US National Science Foundation.

b) Introduction by the moderator Panos Antsaklis and introduction of the panelists.

c) Presentations by the panelists.

d) Discussion. Questions collected in advance are posed by the moderator to the panelists.

The panelists were asked to address the above issues in brief summaries appropriate for the conference proceedings. In the following the (4) summaries contributed by the panelists are included in alphabetical order.

2 Summaries Contributed by Panelists

John S. Baras:

The five most notable research results in systems and control are from my perspective:

> The maximum principle
>
> Dynamic programming
>
> System realization theory (both linear and nonlinear)
>
> Nonlinear filtering theory and the general separation theorem in partially observed stochastic control
>
> Robust control synthesis (in the sense of linear and nonlinear H_∞ theory).

An important limitation of current theories is that they do not take into account explicitly hardware implementation limitations. An important such limitation is limited bandwidth in feedback loops, limited complexity and computational capabilities of the controller. Developing a methodology for the systematic design of single and networked controllers under severe bandwidth resources in the feedback is an important challenge for the next 5-10 years. As implementations with MEMS and microsystems become more attractive this challenge will translate in many benefits and applications.

We still do not have a satisfactory and quantitative way in which to characterize the "intelligence" of a controller or of a system. The late George Zames had initiated an effort for defining such an index, as roughly a measure of the "tasks" and "satisfactory" performances an "intelligent controller" could achieve, vs. the "tasks" and "satisfactory" performances of a classical controller. George focused on adaptive controllers in notes and discussions I had with him. The challenge involves characterization of performance in unknown environments, learning, controller and task complexity, and associated trade-offs. At the conservative end we have "robust control". What lies on the other end? Can one develop a theory to start developing meaningful and useful such indices for interesting classes of systems?

MEMS, nanoelectronics, nanosensors and nanoactuators, bring sensors and actuators in much closer coupling than before. At these scales the physics are quite different and our traditional models need to be rethought out. More specifically, one should think of the combined design of sensors and actuators without early decisions on system architecture. How can we develop systematic theories for such designs? To what extent these new systems at these extreme scales touch upon quantum systems and quantum computations? The recent excitement in quantum computations and related physical implementations involves some fundamental questions on measurement/sensing and actuation. Systems and control theorists can make significant contributions here.

Networks of systems, each equipped with sensors and actuators, is a fundamental paradigm of recent technological and other systems. In such networked systems, subsystems interact through local interactions. An important challenge is to develop modeling and control theories that explain coordination, and emerging global behavior, from these local interactions. This is an important challenge but a promising one. From sensor webs, to microrobots, to biological systems, this is a central problem.

On the educational side, we should be promoting systems and control education, as part of the fundamental education any Engineering College undergraduate should receive. We must accomplish this goal within the next decade. On both the undergraduate and graduate level we should be emphasizing a more balanced view between system modeling and control; not just control. In addition it would be important to arrange for both undergraduate and graduate students, with specialization in systems and control, to spend some times in industry internships targeted at industrial strength design projects.

The ability to miniaturize sensors and actuators, and to produce essentially materials, sensors and actuators "made to order", will change the metrics we currently use to evaluate controls and systems implementations. Handling efficiently the enormous amounts of information needed to describe such systems, controls, and performance criteria, via new appropriate abstractions will require fundamentally new developments.

John C. Doyle:

I'll interpret "systems" very liberally and broadly, perhaps too broadly, and include both notable research results and the subsequent larger programs that followed, with an emphasis on "classical results" from the mid-century. I'll particularly highlight the fundamental tradeoffs in feedback systems that were first articulated in Bode's Integral Formula and later in various interpolation results by Zames and others, which I would, mostly for sake of an interesting argument, rank at the top of my "systems top 5," which are:

1) Feedback, dynamics, and causality (Bode, Zames, ...)

2) Undecidability and computational complexity (Turing, Godel,...)

3) Chaos and dynamical systems (Poincare,

Lorenz,...)

4) Information (Shannon, Kolmogorov,...)

5) Optimal control (Pontryagin, Bellman,...)

The twentieth century may be viewed as bringing near closure to the first scientific "revolution," which aimed for a simple, certain, reproducible view of nature, in part by a radical denial of the complex and uncertain. Quantum mechanics, relativity, the nature of the chemical bond, and the role of DNA in genetics were among the highlights of this "reductionist" program, which could presumably be placed in some similar "top 5." Mainstream science has focused overwhelmingly on characterizing the "fundamental" material and device properties of natural systems, and in contrast, has provided few rigorous and predictive tools for dealing with the complexity and uncertainty of the "real world" outside the laboratory. Unfortunately, current mainstream advocates of a "new science of complexity" have further abandoned rigor and predictability, in favor of vague notions of emergence and self-organization.

Hopefully this collection of "systems" results will form the basis for truly new science of complex systems, which despite the recent hype does not yet exist. The existing theory is far too disconnected and fragmented, and creating a more unified picture of computation, dynamics, feedback and information is the great challenge of the next decade and next century. Of course, this has been the aim of many researchers at least since Wiener, and the accomplishments so far have not been at all encouraging.

It is natural that Bode's integral formula should have a central place in any theory of complex systems, as it was the first result to focus completely on robustness tradeoffs, in this case imposed by causality. The "part count" in complex systems, from biology to engineering, is dominated by the need to provide robustness to uncertain environments and components. Indeed, most systems could be built in the laboratory under idealized circumstances with orders of magnitude less complexity than is required in their natural environment. Thus robustness tradeoffs must be at the heart of any theory of complexity, with limitations due to computation, dynamics, nonlinearity, and information playing important supporting roles.

Yu-Chi Ho:

The "test of time" and rules of history rules out mentioned anything developed in the past 25 years or involving living persons. Furthermore, scientific discovery often is a matter of standing on the shoulder of others. To single out specific results do not seem to be fair to others who laid the foundation. Instead, I propose to list couple of ideas that seems to me have influenced the development of our field in a major way.

1. The fundamental role and the myriad ways of probability and stochastic process in system work.

2. The concept of what constitutes a solution to a problem, e.g., that which can be reduced to a routinely solved problem such as numerical integration and how technology influences it.
3. The notion of dynamics and feedback in all their ramifications.

The first item represents how knowledge from outside the field influenced our research while the third states what specific concepts our field contributed to other fields. The second item deals with how practices in science and mathematics are changed by technology. These notions are generic and have parallel in other topics and fields.

Scientific crystal balling has a notorious record in the past. The dust heap of past predictions is filled with gross miscalculations and estimations by noted scientists with the best of intentions. Let me try to approach the question "what's next in control systems in the 21st century?" in a somewhat different way. During my travel and lectures, I am often asked by young scientist/engineers starting out in their careers on what are profitable avenues of research to pursue. One is often tempted to point to ones own current research topic, which by definition must be the most interesting things to do. However, this is selfish and dangerous advice. My considered reply, which I myself have followed, is this:

Go find a real world problem that a group of people is eager to solve, that happens to interest you for whatever reason, and that you don't know much about. Make a commitment to solve it but not a commitment to use tools with which you happen to be familiar.

Such an approach has several immediate advantages. First, if you are successful then you have some free built-in PR. Unsolicited testimonial by others is the best kind of publicity for your work. Second, most probably you have discovered something new or have found a new application of existing knowledge. In either case, you can try to generalize such discovery later into a fruitful research area for which you will be credited with its founding. Third, in a new problem area there is generally less legacy literature you will have to learn and reference. Fourth, a new problem area is like a newly discovered mine. For the same effort you can pick up more nuggets lying near the surface than digging deep in a well worked out mineshaft. By the same reasoning, the probability of serendipity at work is also by definition higher in a new area. Lastly, even if you are unsuccessful in solving the original problem, you will have at the very minimum learned something new and broadening which will increase the chance of your success in future tries.

My own personal experiences whether it was differential games, manufacturing automation, perturbation analysis in discrete event simulation, or ordinal optimization reinforces the above belief. Above all, faith in the ability of the future generation of scientists and engineers makes me an optimist in saying, "the best is yet to be, you ain't seen nothing yet". It is fine to make predictions and to look forward, but there is no need to get too obsessed with divining the future.

As far as technology and computational issues are concerned, I believe I have already given my answer in the recent op-ed piece published in the June '99 issue of the IEEE Control System Magazine "The No Free Lunch Theorem and the Human-Machine Interface". I shall simply add by repeating what I said at my Bellman Award acceptance "the subject of control which is based on mathematics, enabled by computers, is about to have a new birth of freedom under computational intelligence".

Timothy L. Johnson:

(1) Five most notable research results in Systems and Control Theory in the last Century:

  (a) Bode (1920's) - Stability of operational amplifiers and supporting approach of the "Bode plot". Applications too numerous to mention.

  (b) Caratheodory (1935) - Variational calculus leading to the Maximum Principle. Applications to orbital dynamics and spacecraft.

  (c) R. E. Bellman (1957) - Dynamic Programming. Applications to social sciences and business.

  (d) R. E. Kalman (1960) - Kalman Filter. Numerous applications to tracking, estimation, and signal processing.

  (e) G. Zames (et al, 1970's) - Internal model principle and conic sector theory for Nonlinear Control. Basis for stability analysis for numerous nonlinear and adaptive control schemes.

  (f) Robust Linear Multivariable Control - (1980's) - Contributors too numerous to mention. Applications just beginning.

(2) Five research milestones for the next decade:

  (a) A practical and general theory of discrete dynamical systems. Petri nets, queueing theory, and CSP notations are all of limited application. A tool with the generality of difference or differential equations is lacking. This has impeded progress in practical applications. These should be presented as special cases of a more general theory which is computationally tractable.

  (b) A method for the analysis of qualitative properties of hybrid systems. A full theory of these systems probably will not be developed within the decade. However, useful methods for the analysis

of qualitative properties, and stability in particular, would open the door to further progress.

  (c) Formal verification methods for control systems. Given a control system (software) implementation, and a model of the "plant" it should be possible to prove that the implementation is "correct" to the extent that it achieves the performance specifications. Applicability of these methods to complex control algorithms involving rule bases, AI, Neural Nets, and other more modern methods. (This is an analog of formal verification of computer programs).

  (d) Results which are central to the synthesis of control and communications systems. In particular, results which lead to a practical solution to problems of decentralized control in the presence of communications bandwidth limitations.

  (e) Quantum control systems. Application of feedback theory to quantum mechanical systems, e.g., to understand quantum phenomena in particle physics, biochemistry or in astronomy where feedback is present.

(3) Education issues: Students should have a broad engineering (or science) background, detailed practical expertise in at least one applied field, and (in the case of Ph.D. students) either leading edge theoretical knowledge in one area, or patentable inventions in a leading edge technology. A top priority continues to be closing the widening gap between theory and practice in control engineering.

(4) Technology Issues: Control is becoming a specialty of applied mathematics and embedded software engineering. The field must either recognize and pursue excellence in these fields or make major changes to reintroduce its linkage to physical systems and system design engineering.

(5) Computational tools: The historical trend for new control methods to prove themselves first through physical applications may be changing. An alternative path will be to introduce new theories in the form of design software accessible via the Web, and then to let market demand pick the winners. Currently popular control algorithms (e.g., linear time invariant compensation) are far too restricted (as classes of computations), and will likely give way to more general algorithms which synthesize many approaches and/or use on-line adaptation and design.