P.J. Antsaklis, "At the Gates of the Millennium: Are We in Control?," Panel Discussion Report and Position Papers, P roc o f t he 3 8th I EEE C o nference o n D ecision a nd C ontro l , pp. 1721-1724, Phoenix, AZ, Dec 7-10, 1999. Also in the I EEE C ontrol S ystems M agazine , Vol.20, No.1, pp. 50-55, February 2000.

A panel discussion titled "At the Gates of the Millennium: Are We in Control?" was held during the IEEE Conference on Decision and Control in Phoenix, Arizona, on December 8, 1999. It was organized and moderated by Panos Antsaklis. The panelists were John S. Baras, University of Maryland; John C. Doyle, California Institute of Technology; Yu-Chi Ho, Harvard University; and Timothy L. Johnson, GE Corporate R&D. This article is a report on the panel discussion.

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

©Michel Tcherevkoff/The Image Bank 1997

By Panos J. 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, at least since the late 19th century, 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 occurring. If that is the case, what do we plan to do in the future to meet the challenges of the 21st century? Should we keep doing what we have been doing for about the last 40 years, or by doing so, are we simply missing out on many truly exciting opportunities?

The author is with the Department of Electrical Engineering, University of Notre Dame, Notre Dame, IN 46556 (antsaklis.1@nd.edu). Web: http://www.nd.edu/~pantsakl

P.J. Antsaklis, "At the Gates of the Millennium: Are We in Control?," Panel Discussion Report and Position Papers, P roc o f t he 3 8th I EEE C o nference o n D ecision a nd C ontro l , pp. 1721-1724, Phoenix, AZ, Dec 7-10, 1999. Also in the I EEE C ontrol S ystems M agazine , Vol.20, No.1, pp. 50-55, February 2000.

There are challenges in designing highly complex engineering systems to meet very ambitious goals in manufacturing and process industries, in transportation, and in 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. We will need to develop new methodologies and new ways of addressing control problems, and we 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 these challenges in the 21st century.

We still do not have a satisfactory quantitative way to characterize the "intelligence" of a controller or of a system.

The 1999 CDC Program Chair, Christos Cassandras, decided to organize a conference-wide panel discussion on these issues to hear the opinions and comments of a number of experts. The panelists came from universities and industry, and their contributions to the field of systems and control collectively span many decades. The panelists were John S. Baras, University of Maryland; John C. Doyle, California Institute of Technology; Yu-Chi Ho, Harvard University; and Timothy L. Johnson, GE Corporate R&D, and they brought to the discussion their considerable expertise and experience.

Panel Organization

The panel discussion was a conference-wide event held on Wednesday morning, December 8, from 10:30 to 12:00 noon. The discussion was moderated by Panos Antsaklis. The panelists were asked to prepare brief presentations in response to the first two questions below (past and desirable future research milestones) and, in addition, to be prepared to discuss the last three issues (education, technology, computational tools). Here is the list:

1. *Highlights of past research.* Identify the five most notable research results in systems and control theory in the past 100 years.

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

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?

The panel discussion opened with brief introductory remarks by Christos Cassandras and by Kishan Baheti of the U.S. National Science Foundation. Panos Antsaklis then introduced the topic for discussion, briefly outlined the reasons for organizing the event, described the procedures to be followed, and introduced the panelists. The four panelists then made brief presentations outlining their thoughts. This concluded the first half of the event. During the second half of the discussion, the panelists were asked to respond to specific questions posed by the moderator. The questions were addressed to either the whole group or to specific panelists, with an opportunity for further comments by the other panelists. These questions had been collected and prepared in advance as follows: A description of the panel discussion together with early versions of the panelists' brief position papers were posted and highlighted on the 1999 CDC Web site in late October with an invitation to submit questions to the moderator via e-mail. The moderator, with the help of a small blue-ribbon panel, compiled and edited these questions.

In the following, the summaries contributed by the panelists are included, followed by the questions posed and summaries of the panelists' responses to those questions.

Brief Position Papers Contributed by the Panelists

The panelists were asked to address the above five issues in brief written summaries. In the following, the summaries contributed by the panelists are presented in alphabetical order. Earlier versions of these position papers appear in the 1999 CDC *Proceedings*.

John S. Baras, University of Maryland

Highlights of Past Research: The five most notable research results in systems and control from my perspective are:

- 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).

Future Research Milestones: An important limitation of current theories is that they do not explicitly take into account hardware implementation restrictions. These include limited bandwidth in feedback loops and limited complexity and computational capabilities of the controller. Developing a methodology for the systematic design of single and networked controllers under severe bandwidth limita-

P.J. Antsaklis, "At the Gates of the Millennium: Are We in Control?," Panel Discussion Report and Position Papers, P roc o f t he 3 8th I EEE C o nference o n D ecision a nd C ontro l , pp. 1721-1724, Phoenix, AZ, Dec 7-10, 1999. Also in the I EEE C ontrol S ystems M agazine , Vol.20, No.1, pp. 50-55, February 2000.

tions 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 into many benefits and applications.

We still do not have a satisfactory quantitative way to characterize the "intelligence" of a controller or of a system. The late George Zames initiated an effort for defining such an index as roughly a measure of the "tasks" and "satisfactory" performances an "intelligent controller" could achieve versus those that a classical controller could achieve. 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 tradeoffs. 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 into much closer coupling than before. At these scales the physics are quite different, and our traditional models need to be rethought. 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 do 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, are a fundamental new paradigm for 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 but promising challenge. From sensor webs to microrobots to biological systems, this is a central problem.

Educational Issues: 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. At both the undergraduate and graduate level, we should be emphasizing more balance between system modeling and control, not just control. In addition, it would be important to arrange for both undergraduate and graduate students specializing in systems and control to spend some time in industry internships targeted at industrial-strength design projects.

Technology Issues: The ability to miniaturize sensors and actuators, and to produce materials, sensors, and actuators essentially "made to order," will change the metrics we currently use to evaluate controls and systems implementations. Efficiently handling 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, California Institute of Technology

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 midcentury. 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 the sake of an interesting argument, rank at the top of my "systems top 5," which are:

- Feedback, dynamics, and causality (Bode, Zames, ...);
- Undecidability and computational complexity (Turing, Gödel, ...);
- Chaos and dynamical systems (Poincaré, Lorenz, ...);
- Information (Shannon, Kolmogorov, ...);
- Optimal control (Pontryagin, Bellman, ...).

The 20th 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.

The hope is that this collection of "systems" results will form the basis for a 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 complex than is required in their natural environment. Thus, robustness tradeoffs must be at the heart of any theory of complexity, with limitations due to

P.J. Antsaklis, "At the Gates of the Millennium: Are We in Control?," Panel Discussion Report and Position Papers, P roc o f t he 3 8th I EEE C o nference o n D ecision a nd C ontro l , pp. 1721-1724, Phoenix, AZ, Dec 7-10, 1999. Also in the I EEE C ontrol S ystems M agazine , Vol.20, No.1, pp. 50-55, February 2000.

computation, dynamics, nonlinearity, and information playing important supporting roles.

Yu-Chi Ho, Harvard University

Highlights of Past Research: The "test of time" rules out mentioning anything developed in the past 25 years or involving living persons. Furthermore, scientific discovery often is a matter of standing on the shoulders of others. To single out specific results does not seem fair to others who laid the foundation. Instead, I propose to list three ideas that seem to me to have influenced the development of our field in a major way:

- The fundamental role and the myriad applications of probability and stochastic processes in system science.
- 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 this concept.
- The notions of dynamics and feedback in all their ramifications.

It is natural that Bode's integral formula should have a central place in any theory of complex systems.

The first item represents how knowledge from outside the field influenced our research, whereas 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.

Future Research Milestones: Scientific "crystal balling" has had 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 travels and lectures, young scientists and engineers starting out in their careers often ask what are profitable avenues of research to pursue. One is tempted to point to one's own current research topic, which by definition must be the most interesting thing to do. However, this is selfish and dangerous advice. My considered reply, which I have followed myself, 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 testimonials by others are 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 that you will be credited with founding. Third, in a new problem area there is generally less legacy literature that you will have to learn and reference. Fourth, a new problem area is like a newly discovered gold mine. For the same effort you can pick up more nuggets lying near the surface than digging deep into a well-worked mineshaft. By the same reasoning, the probability of serendipitous discovery is also by definition higher in a new area. Finally, even if you are unsuccessful in solving the original problem, you will at the very minimum have learned something new and broadening that will increase your chances of success in future tries.

My own personal experience, whether in differential games, manufacturing automation, perturbation analysis in discrete event simulation, or ordinal optimization, reinforces the above belief. Above all, faith in the ability of future generations 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.

Education Issues: Another impact of technology on our field will be in the educational arena. Certainly, e-mail and audio-visual technology coupled with the Internet will overcome the space-time limitation of the traditional form of knowledge dissemination (fixed place and fixed time for classes). Although printed material such as books will not be replaced in the near future, reading is basically an open-loop and linear form of information transfer. Modern technology now permits multimedia interaction (using visual, audio, and motion channels) and can perform cost-effective illustrations and demonstrations not possible before. I simply mention one possibility. The software PowerPoint® is extremely impressive for anyone who has used it to display equations and animate graphics in presentations. Less well known is the fact that you can synchronize recorded voices with any object or objects in your PowerPoint® slide. Thus, one can use this feature to approach a near-perfect imitation of a live lecture without the presenter, especially if the lecture is well organized. A computer-generated presentation has many advantages over videotape. Two principal ones are the ease of editing and the clarity of equations and graphics. Six hours of an illustrated and animated lecture with recorded voice can be easily stored on one CD-ROM and reproduced at a cost of about two dollars each. I predict this form of "multimedia books" will be an important tool for technical education in the 21st century.

Technology Issues and Computational Tools: As far as these are concerned, I believe I have already given my answer in the recent op-ed piece, "The No Free Lunch Theorem and the Human-Machine Interface" (*CSM*, June 1999). I shall simply add to that 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."

P.J. Antsaklis, "At the Gates of the Millennium: Are We in Control?," Panel Discussion Report and Position Papers, P roc o f t he 3 8th I EEE C o nference o n D ecision a nd C ontro l , pp. 1721-1724, Phoenix, AZ, Dec 7-10, 1999. Also in the I EEE C ontrol S ystems M agazine , Vol.20, No.1, pp. 50-55, February 2000.

Timothy L. Johnson, GE Corporate R&D

As knowledge progresses from plateau to plateau, only a few key theoretical results really stand the test of time. However, each result that stands the test of time must be supported by thousands of individual research efforts that confirm its general applicability and usefulness in engineering applications. This is a requirement of scientific research, and we should always be eager to contribute to it. In fact, as we attempt to identify those results that rest on the edges of new plateaus, we should recognize that they rest on the many layers below and are almost never without antecedents.

Highlights of Past Research:

- Analytic criteria for stability and robustness of feedback systems. I single out the work of Bode due to the use of the Bode plot even beyond the control field, although the related works of Routh, Hurwitz, and Nyquist of course deserve equal emphasis.
- Calculus of variations and the maximum principle. I have nominated Caratheodory, due to the broader applicability of the calculus of variations, but could equally well nominate Pontryagin for the relevance of his results in control theory.
- Dynamic programming. Clearly Bellman has been the most influential among early practitioners of dynamic programming, who also include Blackwell, Arrow, Karlin, Masse, and others.
- Optimal estimation and filtering. The name Kalman has become almost synonymous with the solution of optimal estimation and filtering problems, although many others such as Wiener, Bucy, Wald, Stratonovich, Kailath, and Luenberger should also be noted.
- Qualitative properties of linear and nonlinear multivariable systems. I have nominated George Zames for his early work in stability and in qualitative properties, although many more recent works—for instance, those of Youla, Kalman, Brockett, Willems, Wonham, Wolovich, Francis, and others—should receive essentially equal mention.

Future Research Milestones:

- A practical and general theory of discrete dynamical systems. Petri nets, queuing theory, and CSP notations are all of limited application. A tool with the generality of difference or differential equations is lacking.
- 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.
- 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. (This is an analog of formal verification of computer programs.)
- Results that are central to the synthesis of control and communications systems. In particular, results that lead to practical solutions to problems of decentralized control in the presence of communications bandwidth limitations.
- Quantum control systems. Application of feedback theory to quantum mechanical systems, for example, to understand quantum phenomena in particle physics, biochemistry, or in astronomy where feedback is present.

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.

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.

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 and will likely give way to more general algorithms that synthesize many approaches and/or use online adaptation and design.

Questions and Answers

Several questions were submitted in advance via e-mail and posed to the panelists by the moderator. Summaries of answers to selected questions appear below.

Q1. *The systems we are attempting to close the loop on, and the controllers we're developing for this purpose, are both becoming increasingly more complex. In the large-scale, nonlinear, hybrid, nonconvex, etc., world, what do you think the prospects will be for closed-form solutions and tractable analytic techniques, and what role do you think "heuristics" will play?*

The panelists thought that for such complex systems we may have such (closed-form, analytic) tools for analysis, but it does not appear that we will have similar tools for synthesis. It was predicted that such complex systems will probably be massively overdesigned at some level of the hierarchy—see, for example, the current TCP/IP Internet protocols—with emphasis on robustness and not on performance. Even today very few closed-form solutions for practical problems are actually implemented. We typically propose control solutions (derived via some methodology) to problems that are later verified to have the desired properties. At certain (micro) scales we may have closed-form solutions, but typically such expressions are more useful in organizing information rather than implementing controllers.

P.J. Antsaklis, "At the Gates of the Millennium: Are We in Control?," Panel Discussion Report and Position Papers, P roc o f t he 3 8th I EEE C o nference o n D ecision a nd C ontro l , pp. 1721-1724, Phoenix, AZ, Dec 7-10, 1999. Also in the I EEE C ontrol S ystems M agazine , Vol.20, No.1, pp. 50-55, February 2000.

Q2. *Complex systems were mentioned by several panelists. How would you define complexity?*

Certainly this is a concept that is almost impossible to define precisely, as it should apply in different ways depending on what we are requested to do. In biological systems, most of the genes (over 90%) appear to perform sensor, actuator, or feedback tasks; that is, tasks that are important for uncertainty robustness and complexity. So even if we cannot define complexity, we can assert that it is intimately related to control.

Q3. *Is our field "science" or "engineering"? Some of the panelists appear to believe that it is the former, whereas others definitely feel it is the latter. This perhaps captures some of the schizophrenia that characterizes our field: We belong to "engineering" departments, we are funded largely by agencies devoted to solving engineering problems, and yet we often view ourselves as "scientists." So which is it? And if the answer a panelist ventures to give is "both," he'd better justify it ...*

Science explains how things work, whereas engineering is concerned with how to make them work better. We cannot separate what we do into science versus engineering, although most of us feel that we are engineers.

Q4. *In the '80s it was suggested that "design for control" should be encouraged for mechanical systems, for instance. Current work on smart structures suggests that this idea's time has come. Will we in the next century show how to integrate control methods into general system design? If not, I fear that control theory will be absorbed into mathematics at one extreme and distributed-logic technology at the other.*

One panelist thought this to be a very important question applicable to modeling of complex systems and to highly parametrized architectures. Another panelist thought that design for control has not been very successful, as we must always first define the system to be controlled, and that control design in the abstract is of little practical interest. We typically design the system first and then control it to keep it performing at some desired level.

Q5. *A young faculty member from Hong Kong wrote: "Many undergraduates tell me that they do not like studying control at all because it is too mathematical. It seems that the gap between control theory and control engineering is becoming wider and wider. The question, I think, is not 'are we in control?' but 'what control are we in?' Do we concentrate much more on getting theorems than on how to use highly developed sensor, actuator, and computer technology in control? I deeply wish that control always be an attractive and fresh subject for young people—like sunrise, but not sunset."*

A panelist said that dealing with physicists and biologists was for him a rather rude awakening. It seems that we have kept feedback as our secret and we have been writing only for ourselves. Furthermore, we tend to explain things in a highly mathematical way without attempting to find simple explanations. We need to teach and explain our subject to others much better. This was agreed on by all the panelists.

Q6. *In the fall of 1998, an NSF/CSS workshop on control education took place (see* CSM, *October 1999). There it was said that control is everywhere already, and it was predicted that its applications will increase dramatically and that the common conception of control is too limited. So we should significantly broaden introductory control courses and make experimental projects an integral part of control education. Here is the question: Sometime in the past, engineering was broken into departments (electrical, mechanical, chemical, civil, and so on). In view of the fact that control is interdisciplinary by nature, and in view of the findings of the report, is it a good idea to have control labs that serve all engineering departments and not specific ones? Do you think that this will avoid duplication, emphasize the interdisciplinary aspects, and make it easier to introduce decision and control concepts earlier in the curriculum?*

The panelists were very supportive of this common laboratory idea. Furthermore, it was stressed that we need to explain our subject better without giving up rigor. Another panelist thought that there are tremendous opportunities in exposing high school students to pre-engineering, and he applauded the workshop planned by NSF at the 2000 American Control Conference. It was also mentioned that other professions make very substantial efforts to explain what their field is about, whereas we tend to treat it as our secret code. We need to explain what we do better and at the appropriate level, as required.

Concluding Remarks

The panel discussion generated some very positive feedback from the attendees. The feeling was that it was a worthwhile and enjoyable event. I should say that I was pleasantly surprised by how much in agreement the panelists were about the need to expand our horizons, to broaden our appeal by explaining to others much better what we do, to be more application driven in our research, to modify the way we teach controls to undergraduates, and to start thinking in new (more systems-oriented) ways to address the control needs of the complex systems of the future.

Panos J. Antsaklis is a professor of electrical engineering and director of the Center for Applied Mathematics at the University of Notre Dame. He received his undergraduate degree from the National Technical University of Athens (NTUA), Greece, and his M.S. and Ph.D. degrees from Brown University. He has held faculty positions at Brown University, Rice University, and Imperial College of the University of London. During sabbatical leaves he has lectured and conducted research at MIT, Imperial College, NTUA, and the Technical University of Crete, Greece. His research interests are in the area of systems and control, with emphasis on hybrid and discrete event systems and on autonomous, intelligent and learning control systems. He has served as program chair and general chair of major systems and control conferences and he was the 1997 President of the IEEE Control Systems Society. He is an IEEE Fellow.