Special Section on Challenges to Control

Editor's Note: On September 18–19, 1986, 52 invited attendees participated in a workshop on "Future Direction in System Theory and Applications," which was sponsored by the National Science Foundation and held at the University of Santa Clara, Santa Clara, California. Professor D. D. Siljak was Chairman of the workshop and G. F. Franklin, A. H. Levis, and W. R. Perkins were on the Organizing Committee. The purpose of the workshop was to assess the state of the art in systems and control theory and engineering and to develop a research agenda for the future. The culmination of the workshop is a document called "Challenges to Control: A Collective View." The main body of this report is being published in the April 1987 issue of the *IEEE Transactions on Automatic Control*. Copies of the complete document with appendices can be obtained by writing to Prof. D. D. Siljak, EECS Dept., University of Santa Clara, Santa Clara, CA 95053.

The two-day workshop was organized so that there were plenary talks followed by discussion each morning and smaller breakout groups each afternoon to treat detailed topics. The six plenary talks were by A. E. Bryson, W. F. Powers, Y.-C. Ho. P. Varaiya, K. J. Astrom, R. W. Brockett, and D. M. Auslander. In advance of the workshop, eight specialists were asked to prepare position papers to advocate research areas for discussion by the working groups: Six of the advocate position papers are presented here. A seventh advocate paper is appearing in the April 1987 issue of the *IEEE Transactions on Automatic Control*. The first three papers presented here treat areas for future research by control systems workers: (1) signal processing, (2) computer and control, and (3) computer science and control. The last three papers provide perspectives from areas of control applications: (4) aerospace control systems, (5) process control, and (6) robotics. These six papers were intended to present new, perhaps controversial, ideas and stimulate the discussion in the working groups at the workshop. In addition, the plenary talk by D. M. Auslander, which reported on the findings of a series of workshops on the control of mechanical systems, is included.

We hope you will find these advocate papers interesting reading. Furthermore, when considered together with the material in the April 1987 *IEEE Transactions on Automatic Control*, these papers provide a unique statement of the state and direction of control theory.

Challenges to Control in Signal Processing and Communications

Alan S. Willsky

Introduction

In this paper, I will attempt to give my views on several research areas in which the field of systems and control can and should be making important contributions for some time to come. In each area, one can certainly point to such contributions that are already being made, but it is my opinion that there is an opportunity, and in fact a *need*, for an increased presence of the control community in these areas. Obviously, this paper represents a biased viewpoint, as it reflects my perspectives and focuses for the most part on areas about which I know something. I hope, however, that it will accomplish its stated purpose, which is to provoke responses and stimulate discussion that can then be used to shape a statement to which we all can subscribe.

In coming up with the topics discussed in this paper, I focused on addressing three questions:

(1) In what areas can the control community have an impact?

(2) What is it about the methods and perspectives of control and systems that provide us with these opportunities?

(3) What form might our contributions take?

Specific answers to these questions are provided in the following sections, but it is appropriate to make several general comments about questions 2 and 3 at the outset.

In my plenary address at the December 1981 IEEE Conference on Decision and Control, I presented my views on why I felt that the control community could have an impact in the field of signal processing [1]. The views presented in this paper represent an updated and expanded version of these previous remarks. In particular, one point I stressed in my address was the value of the model-based approach to formulating and solving complex problems, which is an essential part of the control and systems approach to research and problem-solving. The

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discipline of precise thought involved in this process is of significant value in itself, as this process forces one to organize, analyze, and question one's understanding of the phenomenon under investigation. A model-based approach provides a rational basis for pinpointing and critiquing assumptions and for finding tractable and meaningful problem formulations. Also, I think that our expertise in dealing with dynamics, optimization, and recursion can be of great value in developing algorithms in a far wider variety of applications than would be apparent if one took a narrow definition of the field of control.

Concerning what form our contributions might take, let me first state what form I *do not* think they will take for the most part. Specifically, I do not think that the fields of signal processing and communications are sitting around waiting for us to solve their problems. I also do not think that what will generally be involved are simple translations of problems so that control and systems techniques can be directly applied. Indeed, there are clear dangers to the credibility of our efforts if attempts are made to force problems into mathematical frameworks with which we feel comfortable but which are totally inappropriate. While the specific approaches we have developed in other contexts will no doubt be of value, the real key to our contributions will come from the perspective we bring, which, when blended with those of other disciplines, can provide the basis for truly innovative problem formulations and methodologies.

Recently, I had the opportunity to participate in a University Research Initiative proposal effort in the area of "intelligent control," and one of my colleagues made an interesting observation about our proposal. Specifically, he pointed out that there was very little in the proposal that dealt with what one might take as the historic but narrow definition of "control." On the other hand, there were numerous ideas in the proposal that had the clear stamp of individuals from the field of control. I think there is an obvious and important point to be inferred from these observations.

Signal Processing

In this section, I will briefly discuss several research areas concerned with the extraction of information from signals. The specific areas addressed in this section are:

(1) computational vision
(2) inverse problems
(3) complex and hybrid signal processing problems
(4) computational aspects and parallel processing

As may own background is in estimation theory, you might expect to see an estimation-oriented flavor in these discussions.

Computational Vision

There are a wide variety of problems involving the processing of spatially distributed data in which we in control can make significant contributions. Indeed, a variety of optimal estimation and variational formulations of image restoration, segmentation, and analysis problems have been and are being developed by individuals in the control and estimation field, and I see an opportunity for an expanded role in this area. For example, there is a major need for efficient algorithms to solve the complex optimization problems arising in such image analysis investigations. One optimization approach that arises naturally in this context, because of the use of Markov random field models, is simulated annealing. Not only is there a need to develop methods for analyzing this and related stochastic search algorithms (a topic addressed at the 24th Conference on Decision and Control), but there is also plenty of room for the application of more sophisticated optimization methods and the development of new methods adapted to the imaging context (for example, the use of multiple spatial scales and renormalization groups comes to mind).

A second area of great current interest for robotic and other applications is motion estimation from sequences of images. In particular, one such problem is the estimation of "optical flow," i.e., the estimation of the velocity vector field in an image sequence. By examining consecutive image frames and locating a particular boundary in each, one can extract a measurement of the component of velocity normal to the boundary. The problem then becomes one of estimating the tangential component. A variety of methods have been developed, primarily in the computer science field, for this and related problems, such as estimating optical flow throughout an image or extracting higher-level information about translational and rotational motion of objects in the field of view. There are, however, significant control-theoretic aspects of such problems. For example, there is the question of determining *if* and *how well* the optical flow *can* be reconstructed. As Roger Brockett has shown, the control-theoretic concept of observability is exactly the right tool to analyze such problems. Also, in our work, we have developed estimation-theoretic interpretations of several well-known optical flow reconstruction algorithms. Not only does this lead to significant computational savings, thanks to the use of recursive optimal smoothing algorithms, but it also suggests the potential value of model-based estimation methods in this context. Indeed, there are numerous problems, such as dynamic tracking of motion and the estimation of object depth given knowledge of image motion (resulting, for example, from the motion of a mobile robot), to which I believe we can contribute.

A third area in which I see considerable potential is computational geometry. Typical problems in this area are determining the convex hull of a set of points, estimating polygonal objects given knowledge of sets contained in and containing the object, and reconstruction of three-dimensional (3-D) objects given knowledge of their two-dimensional (2-D) silhouettes from different viewing angles. Applications include computer graphics, motion planning for robots, and object identification from a sequence of 2-D images. Most standard approaches to solving problems in computational geometry are combinatorial in nature and do not allow for the presence of measurement error. In my opinion, this is an area in which there is considerable opportunity for novel estimation problem formulations and new algorithms. For example, George Verghese has had success in developing efficient iterative algorithms for particular geometric problems. The employment of a system-theoretic perspective led, in this case, to novel algorithm structures and geometric constructs. For example, these iterative algorithms can be thought of as geometric counterparts of classical iterative algorithms for solving sets of nonlinear equations. However, in this case, analysis of algorithm convergence does not involve the examination of fixed points of mappings, but rather fixed figures of geometrical constructions. Also, in some of our initial work, we have found that estimation versions of particular geometric reconstruction problems lead directly to quadratic programming problems with considerable struc-

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ture to be exploited. These examples merely scratch the surface of what I feel is an area in which the infusion of a systems perspective can have a dramatic impact.

There are a variety of other problems in this area that one can describe, such as the use of images in generating feedback controls, but I hope that the few I have chosen to describe provide a picture of an area in which I see great promise.

Inverse Problems

In recent years, there has been considerable interest in developing signal processing solutions to various inverse problems of mathematical physics. Examples include acoustic, ultrasonic, and seismic inversion problems, x-ray tomography, and inverse electromagnetic problems. Applications range from medical imaging to exploration geophysics.

I can see at least two areas in which we have made and/or can make important contributions. The first is in developing efficient algorithms for solving inverse problems. As the work of researchers such as Bernard Levy and Thomas Kailath makes clear, the methods and perspectives of systems, estimation, and control have deep connections with inverse problems that have provided the basis for developing efficient algorithms. Furthermore, as more and more ambitious applications are considered, the need for efficiency becomes increasingly important. Successes to date suggest that there is great potential benefit to be gained by combining the methods and perspectives of mathematical physics and systems and control.

The second area is in the development and investigation of novel estimation and identification problems derived from inverse problems. In particular, the direct interpretation of classical inverse problems as signal processing problems raises a number of questions. The large number of degrees of freedom to be estimated in such approaches—typically one is seeking an entire 2-D or 3-D image of some physical quantity, such as wave velocity or electrical conductivity—make many inverse problems fundamentally ill posed. The framework of estimation and identification provides a natural way in which to regularize these problems by modeling the presence of uncertainty and noise and by incorporating a priori information. Jerry Mendel's work on seismic inverse problems indicates that contributions of this type can have an impact. Furthermore, I personally see considerable opportunities for other innovative approaches and contributions. In my plenary address, I argued that much a priori information in inverse problems is geometric in nature, leading to nonlinear estimation problems—even for linear inverse problems—but with far fewer unknowns. Also, and perhaps most importantly, inverse problems are essentially problems in system identification, and I believe that the marriage of system identification and inverse problems will very likely lead to extremely important contributions. During his stay at the Massachusetts Institute of Technology (MIT) during the past year, Lennart Ljung engaged in a dialog with Bernard Levy and myself concerning this area. Out of this have come some interesting problem formulations involving iterative inversion at several spatial scales to overcome problems both of runaway numbers of degrees of freedom and of algorithm complexity (the forward or prediction problem is usually very complex in mathematical physics, but its repeated solution is needed in likelihood function evaluation). We also now have a strengthened conviction that a control and systems perspective has a great deal to offer, in this area as well.

Complex and Hybrid Signal Processing Problems

There are numerous signal processing problems in which the ultimate objective is the extraction of sequences of discrete pieces of information. Speech recognition is an excellent example, as are many problems in biomedical signal processing, such as automatic diagnosis of electrocardiograms. Other examples can be found in automatic fault detection in complex interconnected systems. All of these problems are examples of *hybrid* signal processing problems, in which we wish to estimate continuous *and* discrete variables from the observed signals.

In many of these cases, there is a need for symbolic manipulation and reasoning in piecing together an explanation of the observed data (i.e., the sequence of discrete estimates), and for this reason, methods of artificial intelligence have often been proposed and used. There is, however, a major place for estimation-based approaches in these problems. In particular, such approaches provide rational and consistent procedures for comparing and deciding among alternative interpretations of the observed data. Also, the use of an estimation formalism opens up a variety of important research questions. In particular, in many problems there is a significant separation in the time scales at which various events occur (for example, this appears to be the case in speech). Can we develop estimation methods to exploit this?

In addition, there is the important problem of performance analysis, a question that can be examined in precise terms in the context of an estimation-theoretic formulation. There are very interesting opportunities here for defining and examining novel performance measures that are more appropriate for such applications than criteria such as mean-squared error. In particular, hybrid estimation problems can be viewed as complex decoding problems. Criteria that reflect error rates are natural in such contexts, as are measures that take into account time shifts (e.g., a relative time shift between estimated and actual discrete sequences may or may not be a significant error).

Finally, there are certainly opportunities for development of identification methods appropriate for such applications. In particular, there are often important modeling questions associated with the dynamics of the discrete variables (the hidden Markov models used in speech processing and recognition come to mind) and with the way in which discrete events influence the observed signals. A related and very important question that, in my opinion, has not received the attention it should is the identifiability and observability of models that have been proposed. Can we really identify and distinguish the large numbers of models proposed in applications such as speech processing?

The various questions raised in the preceding paragraphs arise naturally when we view these problems from an estimation perspective. If we abdicate our role in this area, we lose an opportunity to make important contributions that are unlikely to be made by others

Computational Aspects and Parallel Processing

My comments in this area will be brief, since related issues are addressed elsewhere in this workshop. The point I want to make is that there are significant opportunities for developing estimation and signal processing algorithms that can take advantage of and, in fact, can influence the development of special-purpose parallel computer architectures. Hybrid problems of the type described in the preceding section are ideal examples, as such problems involve the parallel exploration of alternate interpretations of the data. Simulated annealing is another example of a processing/optimization algorithm ideally suited to parallel processing. Furthermore, there is just as great a need for parallel processing algorithms for problems involving spatial data, as discussed in the subsections "Computational Vision" and "Inverse Problems." Indeed algorithm complexity for such problems typically depends upon the size of the data array to be processed, and thus there are definite benefits to be gained if methods can be developed to decompose solutions to spatial estimation problems into interacting algorithms operating either on small subsets of the overall data array or on the data viewed at several aggregated spatial scales (the latter, of course, suggests connections with multigrid methods for solving partial differential equations).

In my opinion, the systems-oriented perspective we bring to signal processing problems places us in a position to make unique contributions in this area. I think it is worth noting that one of the primary reasons for the success of the Kalman filter is that it suggested a different concept of a solution to a least-squares estimation problem: the solution was not a closed-form expression for the optimal filter but rather an *algorithm* for its specification. The recognition that the computer made such a redefinition meaningful was a very important contribution. I put forth the statement that similar breakthroughs at this stage may very well involve another concept of solution that takes advantage of the capabilities of multiprocessor computer architectures. The research on distributed algorithms being performed by individuals such as John Tsitsiklis and Dimitri Bertsekas seems to me to be an important step in this direction, but there is room for much more to be done.

Communications

As this is an area about which I know less, I will have less to present. In particular, my comments will focus on problems related to data communication networks, an area in which a number of my MIT colleagues work and in which members of the control community are already playing important roles.

I can see at least four areas related to data communications networks in which there is considerable overlap with the interests and expertise of researchers in control. The first of these is in network design. The development of optimal design algorithms accounting for variations in traffic, finite buffer sizes, possible link and node failures, etc., is a complex optimization problem to which many in our field can contribute or have contributed.

A second area is in the on-line dynamic control of distributed networks. Controlling connectivity in networks subject to failures and dynamic routing in multiaccess networks are two examples of current research areas. The fact that coordination and control information must use the very resource whose efficient usage is to be controlled makes this a challenging problem requiring the blending of ideas from control and communication. In addition, the distributed nature of these problems combined with the desire to minimize communications associated with coordination provides additional and compelling motivation for a third area of research, namely the development of theories and methodologies for designing distributed asynchronous algorithms mentioned at the end of the preceding section. In any communication network, different decision nodes must operate with different sets of information, and thus the issues that must be confronted are the same as those that form the focus of research in distributed estimation and control.

Finally, I believe that researchers in systems and control can make important contributions in developing methods for the dynamic analysis of complex, distributed communication networks. In particular, such networks are characterized by the occasional occurrence of sequences of events (unusually high demand at several nodes, transmission errors, link or node failures, ...) that can lead to major systemwide disruptions (deadlocks, losses of connectivity, turning away of customers, ...). In order to evaluate alternate network designs and control strategies, it is therefore of great interest to have tools that allow one to analyze the probability of occurrence of such events and to isolate critical sequences of events that point to weaknesses in network design or the accompanying control mechanisms. The importance of this problem has been recognized for some time, as has the fact that efficient approximate methods are needed to overcome the enormous complexity of real networks. A number of researchers motivated primarily by computer network problems have developed techniques for analyzing steady-state probabilities of occurrence of various systemwide problems. This is only partially satisfactory, however, since it is the transient or dynamic behavior that is also needed to determine principal causes and likely sequences of events leading to major problems. The need for a dynamic view of such problems makes this a natural one for research within the control field. Indeed, methods for analyzing interconnected systems and, in particular, those that involve examining aggregated system models at different time scales would seem to be natural points of departure for such research efforts.

Conclusions

I hope that my comments will provide a useful starting point for real dialog on directions in which the control community can and should contribute to research in signal processing and communications. The picture I have painted is without question biased by my own knowledge, perspective, and interests. However, I hope that I have been able to convey my strong belief that signal processing and communications offer numerous important and challenging opportunities for control.

Reference

[1] A. S. Willsky, "Some Solutions, Some Problems, and Some Questions," *IEEE Contr. Syst. Mag.*, vol. 2, no. 3, pp. 4–16, Sept. 1982.