Challenges to Control: A Collective View

Report of the Workshop Held at the University of Santa Clara on September 18–19, 1986

During the last seven years, while structural changes have been taking place in the industrial sector of the U.S. and questions of technological leadership and technology transfer have been debated widely, new areas for research have been developing. This led the leadership of the IEEE Control Systems Society to recommend that a workshop be organized to assess the state of the art of the field and outline directions of research. An Organizing Committee, consisting of D. D. Siljak (Chairman), G. F. Franklin, A. H. Levis, and W. R. Perkins, submitted a proposal to the Systems Theory and Operations Research Program of the National Science Foundation to hold such a workshop at the University of Santa Clara, in Santa Clara, CA, on September 18–19, 1986. As part of the proposal effort, a Steering Committee was constituted to assist the organizers in selecting workshop participants and to carry out the preparatory work for the meeting. The twelve-member Steering Committee consisted of the four organizers and eight other individuals: R. W. Brockett, E. J. Davison, Y.-C. Ho, P. Kokotovic, A. J. Laub, S. I. Marcus, W. F. Powers, and S. S. Sastry.

In early 1986, the organizers issued an open call for participation in the workshop that was published in the April issue of the IEEE CONTROL SYSTEMS MAGAZINE. In addition, more than 150 letters were sent to leaders in the field, inviting them to apply for participation in the workshop. The Steering Committee was also asked to identify individuals who could provide unique perspectives. The cut-off date for applications, that included a statement of proposed contribution, was May 31.

In June, during the 1986 American Control Conference, the Steering Committee met for many hours to select the participants from the many applicants and to decide on the final structure of the program. One of the decisions was to invite all the Presidential Young Investigator awardees who had applied, another was to limit the total number of participants to fifty. In addition to the Steering Committee, the following persons attended and contributed to the deliberations: K. J. Astrom, M. Athans, D. Auslander, J. S. Baras, T. Basar, G. Blankenship, S. P. Boyd, A. E. Bryson, Jr., J. Burns, J. Cassidy, J. B. Cruz, D. F. Delchamps, C. A. Desoer, R. F. Drenick, T. Edgar, J. S. Freudenberg, D. Gangsaas, J. Grizzle, A. H. Haddad, W. E. Hopkins, Jr., M. Ilić-Spong, P. Ioannou, T. L. Johnson, T. Kailath, A. J. Krener, R. E. Larson, W. S. Levine, J. L. Melsa, J. M. Mendel, G. Meyer, J. B. Pearson, H. E. Rauch, G. N. Saridis, J. L. Speyer, J. N. Tsitsiklis, P. Varaiya, G. C. Verghese, M. Vidyasagar, A. S. Willsky, and M. Wonham.

The National Science Foundation was represented by Dr. M. P. Polis, Program Director of the Systems Theory and Operations Research Program, and by Dr. G. Hazelrig, Acting Program Director of the Instrumentation, Sensing, and Measurement Systems Program.

The workshop was organized into plenary sessions and break-out sessions, during which Working Groups discussed and wrote the material that forms the basis of this report. Seven keynote talks addressed the main themes of the workshop: accomplishments in the past twenty five years and definition of important current and future research problems drawn from the needs of the industrial and service sectors of the economy. The Working Groups were structured so that, while discussions could take place with few constraints, written materials could be produced by the end of each session. To that effect, each group had one or two persons designated as recorders, with the primary responsibility of keeping notes during the discussion and assisting the group leaders in preparing the Working Group draft reports. In order to provide some focus in the deliberations of the Working Groups, eight persons were asked to prepare position statements based on their own perspectives. Seven of these position papers are being published concurrently in the April 1987 issue of the IEEE CONTROL SYSTEMS MAGAZINE. The eighth one is appearing in this issue of the TRANSACTIONS as a perspectives paper.

The findings of the workshop were documented in a final report; the paper that follows is the main body of that report. It was prepared by a group of Editors (A. H. Levis, S. I. Marcus, W. R. Perkins, P. Kokotovic, M. Athans, R. W. Brockett, and A. S. Willsky) on the basis of the contributions, both oral and written, of all the participants, the advocacy papers, and the keynote talks. The draft was reviewed and edited further first by the members of the Steering Committee and then by all the participants. It is indeed a collective effort—A. H. L.

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INTRODUCTION

A Historical Perspective

THE field of control has a rich heritage of intellectual depth and practical achievement. From the waterclock of Ktesibios in ancient Alexandria, where feedback control was used to regulate the flow of water, to the space probes and the automated manufacturing plants of today, control systems have played a key role in technological and scientific development. James Watt's flyball governor (1769) was essential for the operation of the steam engine, which was, in turn, a technology fueling the Industrial Revolution. The fundamental study of feedback begins with James Clerk Maxwell's analysis of system stability of steam engines with governors (1868). Giant strides in our understanding of feedback and its use in design occurred with the work of Black, Nyquist, and Bode at Bell Telephone Laboratories in the 1920's. Minorsky's work on ship steering, beginning also in the 1920's, was of exceptional practical and theoretical importance. Tremendous advances occurred during World War II, in response to the pressing problems of that period, as summarized in the classic M.I.T. Radiation Laboratory volume by James, Nichols, and Phillips. The technology developed during the war years led, over the next twenty years, to practical applications in many fields.

Since the 1960's, there have been many challenges and spectacular achievements in *space*. The guidance of the Apollo spacecraft on an optimized trajectory from the Earth to the Moon and the soft landing on the Moon depended heavily on control engineering. Today, the Shuttle relies on automatic control in all phases of its flight. In *aeronautics*, the aircraft autopilot, the control of high-performance jet engines, and ascent/descent trajectory optimization to conserve fuel are typical examples of control applications. Currently, feedback control makes it possible to design aircraft that are aerodynamically unstable (such as the X-29) so as to achieve high performance. The National Aerospace Plane will rely on advanced control algorithms to fly its demanding missions.

Control systems are providing dramatic new opportunities in the *automotive* industry. Feedback controls for engines permit federal emission levels to be met, while antiskid braking control systems provide enhanced levels of passenger safety. In *consumer products*, control systems are often a critical factor in performance, and thus economic success. From simple thermostats that regulate temperature in buildings to the control of the optics for compact disk systems, from garage door openers to the head servos for computer hard disk drives, and from artificial hearts to remote manipulators, control applications have permeated every aspect of life in industrialized societies.

In *process control*, where systems may contain hundreds of control loops, adaptive controllers have been available commercially since 1983. Typically, even a small improvement in yield can be quite significant economically. Multivariable control algorithms are now being implemented by several large companies. Moreover, improved control algorithms also permit inventories to be reduced, a particularly important consideration in processing dangerous material. In *nuclear reactor control*, improved control algorithms can have significant safety and economic consequences. In *power systems*, coordinated computer control of a large number of variables is becoming common. Over 30 000 computer control systems have been installed in the U.S. alone. Again, the economic impact of control is vast.

Accomplishments in the *defense* area are legion. The accomplishments range from the antiaircraft gunsights and the bombsights of WWII to the missile autopilots of today and to the identification and estimation techniques used to track and designate multiple targets.

Key Concepts of Control and Systems Theory

While the devices, vehicles, and processes described above are physical objects that are highly visible, the *control* that has contributed so much to their success is invisible. One can hold the parts of an advanced automobile brake system in the hand; but one cannot hold the antilocking control algorithm that has made the brakes effective. It is the same with the other key concepts of this field, so that it is particularly important that these concepts be identified and described to establish the central role they play in so many important advances in all branches of technology and the contribution they make to the quality of life.

Among the key concepts that have influenced substantially the accomplishments in control during the last twenty-five years are Pontryagin's Maximum Principle, Bellman's Dynamic Programming, and Wiener and Kalman Filtering. Combined with the concepts of *feedback*, *sensitivity*, and *dynamic stability*, a multifaceted, but coherent body of theory has emerged. Its intellectual content and fundamental notions contribute not only to further technological advances, but also to other scientific disciplines. The notion of feedback, for example, plays a central role now in biology, psychology, and economics, with audience surveys and marketing being some obvious applications. Causal phenomena are now approached not only as static cause–effect chains, but also as dynamic cause–effect–cause "feedback loops."

In engineering systems and decision-making, the paradigm of feedback control addresses the problem of using the information about the output (effect) to design or modify the input (cause) for a given task. Tasks range from controlling a robotic manipulator to grasp an object, to regulating temperatures, pressures, and concentrations in a chemical process, to dynamic routing of information packets in a communication network or of parts in a manufacturing system, to stabilizing a power system or a large space structure. The complexity of even a small-scale system, such as a compact disk drive, may be due to the very stringent accuracy and speed requirements. In large-scale systems, the task of meeting rigorous performance requirements is much more challenging because of the uncertainty of the system model and its environment.

Another set of breakthroughs of the 1960's and 1970's was the theory of *controllability*, *observability*, and *feedback stabilization* of linear state-space models and the associated theory of state-space realizations of input–output models. This structural theory answers the question of whether a linear model with inputs (actuators) and outputs (sensors), if unstable, can be stabilized and whether it can perform a given tracking or regulation task. This theory has been implemented in many software packages for computer aided design of control systems. The applications of this theory, although impressive, have been limited in scope due to the often unrealistic assumptions that the mathematical model of the system be completely known, and that the model have the form of linear differential (or difference) equations.

Present Needs and Future Directions for Control and Systems Theory

Models of realistic systems are seldom completely known and, if known, they are seldom linear. Control theorists are now challenged to expand their horizons and to extend their concepts and methods to be applicable to incompletely modeled systems, to systems whose models are initially poorly defined, but can be improved on-line during the system operation, and to dynamic systems that are driven by discrete events. In other words, control system designers are to incorporate the process of modeling in the design process and treat the models not as given, but as evolving entities. At the same time, new classes of models need to be developed involving not only analytical and numerical, but also qualitative and symbolic data. An example of systems which require such models are networks of interacting discrete events, such as manufacturing systems or computer operating systems. They are causal and dynamic, but also highly asynchronous, nonanalytic, and even nonnumerical. In parallel with new classes of models, control theoretic methods need to be extended to a wider range of traditional models: nonlinear, stochastic, and models with distributed parameters.

A control theory for incompletely known systems and systems described by nontraditional models must create a wider repertoire of control laws, algorithms, strategies, rules, and protocols. Thanks to faster and cheaper computers, the new control laws can incorporate adaptation, leading to an on-line learning and to better control. On the other hand, computer-aided procedures for off-line design of control laws can involve a more comprehensive experience-guided search for robust and fault-tolerant control structures.

The most recent results and current trends of control theory show that this ambitious agenda of theoretical tasks is a realistic goal. First steps towards a conceptualization of discrete event systems are extremely encouraging for a control theoretic approach to this uncharted area of research. New tools for algorithm design, such as the merger of advanced stochastic control concepts with physically motivated models of simulated annealing, and the merger of queueing network theory with perturbation analysis, promise to revolutionize the optimization of communication and manufacturing networks. There are already successful extensions of linear geometric concepts to nonlinear systems, which, with the help of symbolic computation, lead to their wider applicability. The intuitively appealing idea of adaptive control has reached the stage at which modeling uncertainty can be related to the convergence properties of adaptive estimation and control algorithms.

At this point in the evolution of the control field, there exist several paramount challenges. These are driven by an interplay of computers and electronic technology, the demands of current and future applications (such as the fields listed above), and by progress in the development of underlying concepts. To articulate these challenges, fifty-two contributors to the field of control gathered at the University of Santa Clara and, in a number of working sessions, produced the material on which this report is based. The integrated and edited version of the challenges that were identified is presented in the next section. For each challenge, the important research issues are identified. In some cases, major extensions of current paradigms are needed to meet stringent performance requirements. In other cases, new paradigms are needed to capture the properties of new, man-made systems. It is crucial that research in all these challenging aspects of control theory be encouraged and supported, in order to ensure that the necessary theory and design tools will be there to stimulate the technological breakthroughs of the future.

Finally, control scientists and engineers must face head-on the challenge of increased complexity. This is apparent in the research agenda that follows. Future research must provide the methodologies, theories, and algorithms that will allow the control engineer to plan, manage, and control systems of unprecedented complexity. The challenge becomes compounded by the realization that future systems will include a mix of symbolic and numeric subsystems, exhibit pronounced dynamic behavior, and will be expected to satisfy even more stringent performance requirements.

Current and Future Challenges: A Collective View

In this section, a set of research problems is presented. These problems range from the development of control system design techniques for multivariable systems to the modeling of discrete event dynamical systems, and from geometric and asymptotic methodologies for nonlinear dynamical systems to the need for experimental data for distributed architectures. The research topics have not been ranked; that was deemed to be counterproductive at this stage. However, they were selected because of the challenges they pose and the promise they hold for current and future technological developments. Each of these topics was discussed by one or more of the working groups in the workshop and documented by several participants. Their presentation is organized in terms of thematic groups that are ordered, in a general way, from hard current problems to new problems that are just beginning to be investigated.

Many subjects and applications, such as the control of dynamic economic systems, are not included in this report. This was not done because they are not important. Some were omitted because of the realization that not all areas can be pursued when resources are limited, while some are best left to other professions or disciplines.

The common feature of all the research topics in this report is the potential they have for substantial impact on technological developments. Two major themes underlie all the discussions that follow. The first is the impact that higher performance standards have had on design methodologies; the second is the impact that digital computers and microprocessors have had, not only as computational tools, but as system components.

Multivariable Robust, Adaptive, and Fault-Tolerant Control

Industrial processes, machines, vehicles and similar dynamic engineering systems operate in changing environments of different raw materials, fuels, load levels, weather conditions, etc. It would be impractical or even impossible to describe the dynamics of such systems by exact mathematical models. In the presence of such modeling uncertainties and in the presence of unmeasurable disturbances, it has been found effective to use *feedback control* to meet performance specifications; open-loop control simply cannot meet stringent command-following and/or disturbance-rejection requirements in these situations. Thus, the most fundamental reason for using *feedback* is to enforce good performance in the presence of *uncertainty*; also, feedback is used to enable processes to work in the neighborhood of open-loop unstable operating conditions, i.e., to *stabilize* unstable plants. Care must be exercised that the feedback system is *reliable*, i.e., that it should continue to operate in the presence of hardware and software failures.

Research in feedback control proceeds on two levels. The goal of the first level is to invent, analyze, and describe control schemes that allow the construction of new physical processes which perform better. The goal of the second level is to improve the productivity of the control system designer who specifies how this new process will be built; this productivity goal is enhanced with the emergence of new *design methodologies* and with *computer-aided control engineering software* that supports the design process.

The recent advances in *multivariable* feedback control design methodologies illustrate the engineering issues. The emergence of inherent multivariable design methods (such as LQG-based methods, geometric and polynomial methods, $H\infty$ optimization, etc.) influences the physical design by allowing for the real-time dynamic coordination of multiple actuators and effectors based upon the real-time measurements of several sensors. Performance is improved because we can coordinate simultaneously many degrees of freedom to control or regulate several physical variables in real-time; however, as we increase the number of sensors and actuators we must pay increased attention to the consequences of failures. In the past ten years, the merger of time-domain and frequency-domain methods has increased the relevance of optimization-based tools and has contributed to the improved utility of these results to the practicing engineer. Simultaneously, advances in VLSI technology allow for the reliable and economical implementation of sophisticated feedback control algorithms in digital controllers. For these reasons, we are witnessing an explosive growth in several applications (missiles, aircraft, spacecraft, jet engines, automotive controllers, process controllers, to name just a few which actively utilize these multivariable methods). From a practicing engineer's viewpoint, the availability of first generation computer-aided control engineering software has vastly accelerated the applications process (such as MATLAB, MATRIX-X, CTRL-C, Program CC, L-A-S, LQGALPHA to mention a few).

Open research questions in the area of *multivariable* linear control systems include improved theoretical and pragmatic understanding of the "directional" issues inherent in any multivariable control system design. The interplay between performance and uncertainty, and the impact of multiple nonminimum-phase zeros and significant time-delays in stability/performance trade-offs are examples. In the case of *digital* multivariable controllers we need to develop explicit design methodologies that exploit multiple time-scales and other directional properties for the development of multirate controllers. The understanding of the relation between geometric (vector space) and algebraic (polynomial and matrix-fraction) descriptions is an area of intense current activity, and an important area for future research. Furthermore, the development of all these approaches in a large-scale setting (decentralized control, structural model decomposition and reduction, and the like) is vital to many engineering applications (e.g., flexible structure control, power systems).

Control engineers must live with uncertain models and understand that the impact of the level of modeling uncertainty on the design of controllers is crucial. Uncertainty and achievable performance tend to oppose each other, even though the very reason for using feedback is to obtain better performance over open-loop control. The controllers are required to be *robust* with respect to modeling uncertainty and to *adapt* to slow changes in the system dynamics. Since multivariable control systems utilize an increased number of sensors and actuators, it is important that the system is designed to be *fault-tolerant* to sensor, actuator, and perhaps some other component failures; this may require real-time reconfiguration of the feedback control structure. Finally, since we are relying upon digital electronics to implement the control strategies, the feedback designs must be fail-safe to software and processor failures. All these requirements present novel challenges for research to control engineers and system theorists. We have barely begun to develop the proper analysis methodologies and design theories.

Robust control strives to characterize the uncertainty in the model of the plant to be controlled and to evaluate the degrees of freedom left to achieve the control task within specified bounds.

Considerable effort has been made to satisfy the sine-qua-non *stability robustness* requirement, that is to guarantee closed-loop stability under a wide range of plant variations and disturbances. Bounds under which the stability can be preserved have been derived and a rigorous mathematical theory has been developed to minimize the sensitivity with respect to disturbances and norm-bounded plant uncertainty. More research is needed to reduce the conservatism of available stability robustness results; indeed, one must exploit "directional information" to decrease the conservatism of existing results. The payoff will be improved performance, because stability robustness tends to limit the bandwidths of the closed-loop system, and thereby deteriorate command-following and/or disturbance-rejection performance. Research is under way, and should continue, to explore algorithmic and numerical implementation of such theoretical results and to create software support for practical design of these types of robust controllers, by means of higher level computer languages which convert intricate analytical results of input-output and state-space theories into user-friendly notions convenient for interactive computer-aided design.

A confluence of such diverse experiences is preparing the ground for an attack on a most challenging issue, that of *performance-robustness* for both single-input single-output and multivariable feedback control systems. Performance-robustness is the characterization of and the feedback design under the so-called "structured" uncertainty to meet predefined performance specifications together with a guarantee that these performance specifications will be met by a fixed gain controller or compensator for any value of the plant "structured" uncertainty in a prior known parameter set. To avoid overly conservative "worst case" designs, the modeling of uncertainty is to be "structured" so as to exploit all relevant *a priori* information about the plant to be controlled, including not only numerical, but also qualitative and linguistic descriptions.

Adaptive control is a promising approach to achieve performance robustness. Its present setting is limited: it makes use of the most structured type of uncertainty in which the plant model has a known form, but unknown parameters. The adaptive feedback loop attempts to actively reduce the uncertainty by on-line parameter estimation and then to use the parameter estimates to continuously retune the controller. Adaptive control algorithms lead to increased productivity of existing processes and the development of new and more efficient industrial processes and products. A typical industrial process incorporates hundreds of conventional controllers which require frequent retuning and expert maintenance. This task can be performed by adaptive algorithms. Adaptive algorithms can also be used for tuning of controllers in mass produced items such as automobiles, furnaces, bio-medical instruments, etc. In addition, they permit adaptive filter design and signal and image processing.

Following the application of Lyapunov stability theory to adaptive control, a significant theoretical breakthrough in the late 1970's was the formulation of sufficient conditions under which adaptive control laws converge, that is, possess a *self-tuning property*. However, it became clear in the early 1980's that some of these conditions are too restrictive for practical applications, while others imply that adaptive algorithms themselves may become nonrobust, that is the adaptive control system could become unstable in the presence of the inevitable unmodeled high-frequency (parasitic) dynamics and unmeasurable output disturbances.

This realization has had a most stimulating effect on researchers. Under more realistic assumptions coming from industrial applications, much of the present research effort is directed toward the invention of new more *robust adaptive* algorithms for identification and control. Analytical tools that have been employed to explain the behavior of earlier adaptive schemes are being expanded by methodologies which explicitly address the issue of algorithm design. Also, research is directed in the development of "safety nets" which supervise the operation of an adaptive system and turn it off when it is detected that the system is heading toward instability. They may include heuristic rules or extensive real-time spectral calculations so as to monitor the frequency content of the signals which may excite the unmodeled dynamics, and turn off the adaptation process when closed-loop stability appears to be in danger.

There are many open research questions in the field of *robust adaptive control*. A basic understanding of the quantitative performance benefits to be expected from adaptive control is lacking; the research must take into account the fact that exogenous excitation signals may introduce unwanted disturbances in the adaptive loop. Another challenging task in this area is to allow arbitrary parameterizations with a smaller number of adjustable parameters; this would remove a disadvantage of most existing parameterizations, which, in order to preserve linearity, often involve an unnecessarily large number of parameters and cause convergence difficulties. An ultimate goal in this direction would be the development of *nonparametric adaptive control*. New parameterizations and algorithms appear to be needed to generalize results in the single-input single-output case to the multivariable case. New identification algorithms are needed which not only generate a nominal plant model within the "structured" uncertainty set but, also, generate frequency domain bounds that can be used to represent the "unstructured" uncertainty. Such novel identification algorithms can be used to periodically redesign a multivariable compensator in real-time (e.g., adaptive LQR or LQG/LTR designs).

A more general class of control systems which adapt to significant changes in their environment is the class of *fault-tolerant* control systems. In this class of problems we admit that one or more key components of the physical feedback system will fail and that this failure can have a significant impact on stability or performance. At the simplest level we can think of sensor and/or actuator failures, while at a more complex level we can think of other system failures, e.g., partial structural damage to an aircraft due to a mid-air collision or weapon damage. In the same vein, we can also worry about computer hardware and software failures. The idea is to design the control system so as to retain stability and lose performance in a gracefully degraded manner. It may be necessary to reconfigure the control system following the detection of such failures. Such reconfiguration may be as simple as reading a new set of control gains from a precomputed table or as complex as complete redesign of the control system in real-time. A challenging problem for control theory is to take into account advances in computer technology and to stimulate the development of real-time and concurrent systems which allow the implementation of such control strategies in hardware form.

What we lack at present is a set of prescriptive methodologies that can be used to design fault-tolerant feedback control systems. Such a theory will surely have to take into account the immense combinatorial aspects of multiple hypothesis-testing and option-generation algorithms; it most likely will involve a blend of numerical and symbolic manipulations, thus combining concepts of control theory and artificial intelligence (AI). For example, a real-time decision will have to be made on whether we should continue to worry about the same performance variables, accepting some performance degradation, or, drastically reducing performance requirements, concentrate on maintaining stability and, perhaps afterwards—after the transients have died out—attempt to reconfigure again to achieve optimal performance. Such hypothesis generation, option enumeration, and response selection is characteristic of high-level cognitive processes that will have to be captured in a symbolic (nonnumeric) setting in the computer system. We believe that existing concepts in AI are not powerful enough to be applicable to fault-tolerant control; the reason is that control problems are much more dynamic and stochastic than the problems for which these concepts have been developed. Thus, we have to blend not only symbolic with numerical calculations, but also we must extend the symbolic computations into a more dynamic and stochastic setting. Need-less to say, any theoretical advances must be accompanied by suitable software not only for off-line analysis and design of fault-tolerant systems, but also for real-time decision making. Software considerations will undoubtedly spark novel interdisciplinary research into languages, symbolic computations, and knowledge representation theories relevant to the domain of fault tolerant control.

Such fault-tolerant control theory and software would find immediate application in many areas; in particular, the flight control systems of both commercial and military aircraft. Following crashes involving failures of major portions of the aircraft, analyses have pinpointed circumstances of existence of controllers under which the aircraft would have remained flyable.

Stochastic Control

Realistic models of most engineering systems involve random environmental disturbances. Stochastic control theory is the study of controlled dynamical systems in which environmental disturbances are explicitly modeled as random processes. Past accomplishments of research in this area include: 1) effective models and analytical methods for certain industrial and aeronautical control problems, such as minimum variance tracking control laws in industrial batch processing and the Apollo guidance and control system; 2) a well-developed theory for control of systems with a centralized controller and complete observations of the state of the system; 3) models and analytical methods for the optimal control of queueing (and flow control) networks, e.g., management of hydro power reservoirs; and 4) models and analytical methods for certain classes of econometric problems, such as the derivation of strategies for risk-averse investing and portfolio management. Stochastic control theory is relevant in design problems where explicit provision must be made for disturbances and where quantitative models are available for the disturbances. For example, the "control of large deviations," that is, selecting design variables and controls to minimize the probability that a key system variable passes outside a "safe" operational range, is clearly important in many areas, including structural dynamics (with active damping), network flow control, and power systems security.

Despite these accomplishments, much important work remains to be done in a number of challenging areas.

First, future progress in the development of controllers for highly nonlinear plants depends on the development of effective nonlinear *signal processing* and nonlinear *estimation* algorithms for the reconstruction of key unmeasured state variables and parameters which are required for use in feedback control policies. These are very difficult problems, but steady progress has been made over the past decade, and many analytical aspects of the partial differential equations arising in recursive nonlinear filtering are now understood. A major research challenge here is the study of numerical aspects of nonlinear filters and signal processors, particularly those which exploit current and future technology through the use of parallel processing and other new computer architectures; for example, can nonlinear filters be implemented on an inexpensive microprocessor chip?

Second, many practical stochastic control problems involve sensors and controllers which are geographically *distributed*. There has been thus far a lack of models and conceptual methods, let alone analytical methods, for the treatment of such multiagent (or decentralized) stochastic control problems; this area presents major research challenges. For example, there is a need to understand the relationship between distributed decision-making and the flow of information in the system.

Further progress in stochastic control has also been limited by an inability to develop an effective analytical framework for stochastic optimal control, that is, for the problem of *integrated* control law optimization and state estimation and the problem of "dual" control. For the many situations in which a solution of a stochastic control problem cannot be found, approximate methods are extremely important. There has been considerable progress in the study of approximations by controlled Markov chains and by asymptotic expansions, but these methods need to be refined and applied to a broader class of problems.

Each of these problem areas offers important opportunities for basic research addressing needs in such applications as industrial process control, advanced avionics control systems, and distributed decision-making in uncertain environments.

Nonlinear Control

Most engineering systems encountered in practice exhibit significant nonlinear behavior. For systems exhibiting nonlinearities, the normal design procedure in the past has employed a linearized approximation of the process model followed by the application of linear control methodology. However, this procedure can yield unsatisfactory performance, especially when the system is highly nonlinear and undergoes large motions, and thus operates over wider nonlinear dynamical regimes, as is often the case in the problems of attitude control, advanced aircraft control, and the control of robotic manipulators, brushless dc motors, and chemical processes. During the past fifteen years, there has been considerable progress in the understanding of *nonlinear systems*, primarily due to the application of mathematical concepts derived from the field of differential geometry. There has been substantial work on qualitative concepts, such as controllability and observability, for nonlinear systems, but this work has had little impact from a control point of view. Techniques for the control of systems described by nonlinear mathematical models are difficult to find, but a major breakthrough occurred during the past decade with the development of techniques which solve such control problems as *disturbance decoupling*, *input–output decoupling*, and *feedback linearization*. Feedback linearization utilizes state and feedback transformations to transform a nonlinear system into an equivalent linear system; then, already developed linear control tools and criteria are available for design. This technique has been successfully applied to the very difficult problem of controlling aircraft, such as helicopters, with multiaxis nonlinear dynamics; in fact, a controller designed using this technique has been successfully flight tested. The mathematically dual technique, linearization by output injection, is being employed to attack the difficult problem of nonlinear state estimation.

The theory and applications are, however, in their early stages of development, and there are many limitations, open problems, and questions. For example, further research is needed to develop performance, sensitivity, and robustness properties and to evolve a practical design methodology, with associated computer-aided engineering tools. Furthermore, all of these techniques have assumed that static state variable feedback is to be employed. Future important directions are to develop methods for constructing nonlinear observers (in order to implement the above results) and a theory of dynamic nonlinear compensation. It is clear that the linear theory has profited from the interplay between state-variable and input–output techniques. A *nonlinear input–output theory* addressing such issues as left and right invertibility and decoupling with output feedback is just now emerging and must be further pursued. The implications for tracking and servo-design are clear, and should enable further progress in the applications mentioned above.

By far, the most important unsolved problem is that of *nonlinear control system stabilization*. Techniques must be developed for determining whether a nonlinear system can be stabilized within a given desired region of stability. This will involve delineating specific classes of systems and associated methods for synthesizing a stabilizing controller. A current limitation of the geometric approach is that highly accurate models must be available in order to verify that certain exact conditions of the theory are satisfied. Techniques for controller synthesis which tolerate modeling errors need to be developed. A merging of geometric and asymptotic methodologies may yield conditions under which "almost" properties such as approximate decoupling become exact as the errors vanish.

The solution of all of the important problems discussed above would be greatly facilitated by the development and understanding of the appropriate nonlinear generalizations of the methods from the frequency domain approach to linear systems; these methods could provide (possibly graphical and computer aided) tools for the design of nonlinear control systems.

Control of Distributed Parameter Systems

Distributed parameter systems are those in which the variables of importance depend on both spatial and time variations. Such systems are usually modeled by partial or integro-differential equations. Large flexible space structures, flexible robots, adaptive optics, and deformable mirrors, flutter suppression, and active control of fluid flow are examples of systems involving unsolved distributed parameter control problems. Although there has been considerable progress in understanding the stabilization of distributed parameter systems, and in the development of fast algorithms and computational schemes specifically for control of systems governed by partial and integro-differential equations, there remain a number of very hard theoretical and practical problems that must be solved before distributed parameter control can become a practical tool for the design of these highly complex control systems.

The development of models specifically suited for control design and analysis must be pursued. For example, the sensors and actuators which may be used on large flexible structures can strongly affect the dynamic performance of the structure, and this must be taken into account both in the design of the structure and in the design of the control system. A model suitable for (open-loop) simulation may not be adequate for (closed-loop) control design.

Due to the infinite-dimensional nature of distributed parameter models, and since inherent damping is small and poorly understood in many of the applications mentioned above, it is essential that one develop new identification schemes, robustness criteria, and effective control algorithms. In addition, the active damping of vibrational modes is an important problem which should be investigated.

It is now known that stability definitions commonly used for lumped parameter systems are inadequate for describing the closed-loop stability of even simple systems modeled by partial differential equations. The stability properties of such systems are extremely sensitive to small time delays or changes in sensor/actuator positioning; for example, the problem of noncollocated sensors and actuators is quite difficult and is of great interest. *Sensitivity analysis* for these problems is an area of increasing importance.

It is also known that certain widely employed "finite element" codes give erroneous results for simple infinite-dimensional models. The utility of the results produced by these codes for control design for complex structures or fluid flow problems is thus open to question. Therefore, the general area of *approximation techniques* for control must be addressed.

Control of Systems with Discrete Variables and Discrete Events

The classes of systems traditionally studied by control engineers involve continuous variables and have dynamics which can be described by either differential or difference equations. There are many situations, however, in which such models are not appropriate, as the following examples illustrate. First, consider a system being regulated by an "intelligent" *rule-based controller*. Such a controller would, at each time stage, first execute a series of diagnostic or decision tests, and only then execute a more traditional type of (numerical) control action described by a differential or difference equation. In this case the "state" of the system consists of a set of real numbers together with a set of logical (or Boolean) variables. Similar *hybrid* situations, in which the state of the system involves both continuous and discrete variables, arise when there is discretization due to switches or limiters, or in systems with multiple modes, such as reconfigurable aircraft, robotic manipulators, computer controlled subsystems of automobiles, and systems with many failure modes (such as large-scale power systems). The implementation of systems with multiple modes has been made practical by the introduction of the *programmable microprocessor* into control systems, and such systems are now being implemented in automobiles and aircraft without the benefit of any powerful modeling or control techniques specifically designed for such systems. The second class of examples consists of complex man-made dynamical systems, the evolution of which is governed not by differential equations, but by the intricate interactions of discrete events; such systems are thus known as *discrete event dynamical systems*. Examples of such systems are flexible manufacturing systems, computer/communication networks, and traffic systems, while examples of the discrete events are the completion of a task or arrival of a message. The state of the system, which changes only at these asynchronous discrete instants of time instead of continuously, consists of an assortment of numbers and discrete variables, such as the number of parts waiting at each station, the job list, the readiness status of each station, or the processing time. For each of these important classes of systems, there is a dearth of elegant and succinct models and there are no control techniques rivaling the economy and power of those for systems described by differential equations. Some of these classes of systems have been studied to some extent by researchers in other fields, but researchers in control and systems engineering have a unique and valuable perspective and a considerable array of tools which can be brought to bear on these problems.

The control of *hybrid dynamical systems* will require a theory for dealing with a mixture of both continuous and discrete signals and for interfacing the numerical controllers with higher level planners and human operators using symbolic reasoning. An initial step in this direction is exemplified by the technique of *multiple model hypothesis testing*. In this situation, one has 1) a discrete set of alternative interpretations of a sequence of observed data; 2) a model for each of these alternatives; and 3) optimal processors (Kalman filters) for each that produce statistics which form the basis for efficient and rational assessment of which alternative is most likely to be correct. This higher level discrete decision is then incorporated into a feedback control policy. Generalizations of this approach should provide a framework in which many types of knowledge and information can be incorporated into a control system design. These generalizations will involve a synthesis of concepts from control and system theory with those from symbolic reasoning and computer science. From a technological point of view, these advances will be possible due to the availability of hardware, such as low-cost microprocessors, and software, such as symbolic manipulation languages and tools.

There are many basic problems which should be investigated in order to develop a methodology for the control of such hybrid systems. First, high-level *intelligent supervisory control* devices must be modeled and studied in an overall control system design context. Such a high-level controller should be able to deal with situations which may involve deciding what to control and the development of strategies to achieve control in the face of altered configurations; such altered configurations occur in both systems with multiple modes and in those in which unanticipated failures result in alternative modes of operation. In addition, it should act as a transducer (or "interpreter") which is able to convert high-level linguistic instructions into nominal trajectories and feedback control policies, and it would also combine data from multiple sensors necessary to return a linguistic summary of sensor data. A very basic question which must be addressed here is how the plant with the numerical controller together with the higher level supervisor or interpreter should be modeled and, indeed, what type of language or symbols should be used. Appropriate *symbolic* (nonnumerical) *representation* methodologies for such systems must be investigated; this investigation will probably be inspired by more traditional control and system theoretic concepts (such as state, linearization, equivalence, stability, approximation, and robustness), which will then be extended or modified. For example, an appropriate analog of formal language theory dealing with trajectory segments may be useful in the context of modeling high-level supervisors or interpreters which convert linguistic instructions into trajectories. In addition, a methodology for supervisor control synthesis must be developed. Intelligent management strategies which can improve performance and productivity should be investigated. The use of optimization frameworks combined with artificial intelligence methods in scheduling is an example of the integration of types of theoretical areas which will need to be brought to bear on these problems.

As is the case with hybrid dynamical systems, *discrete event dynamical systems* (DEDS) are pervasive in our increasingly technological society, but there is a lack of good dynamically oriented models, or analysis and control techniques which can be applied to them. Although finite-state Markov chains, Petri nets, queueing networks, and discrete event simulation have all been used with some success to model special classes of discrete event dynamical systems, these models are of limited use since they are neither mathematically as succinct nor computationally as feasible as are differential equations for continuous variable dynamical systems. There is thus no agreement as to which is the "best" model, particularly for the purpose of control. Future research should include a concentrated investigation of the modeling problem; for example, recently posed models involving *generalized semi-Markov processes* could prove very useful. More specifically, the following modeling questions should be answered: How does one formally specify and reason about discrete event dynamical systems? In particular, for a given model, is it possible to find an algorithm which decides whether or not it models satisfactorily a given set of real-time data? What is the complexity of such a decision procedure? A typical example would be the validation of a complex simulation program, whether it represents a communication network or the air traffic control problem at a major regional center.

If good dynamic models of discrete event dynamical systems are available, many of the problems of design, operations management, scheduling, and control can be addressed. Some of the challenging and important research opportunities presented by such systems are the modification and application to DEDS of the analysis, control, and optimization techniques that have been so useful and successful in the past in dynamical systems governed by differential equations. For example, given a discrete event dynamical system and its performance specifications, the formulation and solution of the problem of designing a controller to meet the required performance is a crucial research problem. In the process, it must be determined whether such a controller exists, and whether there is an effective algorithm for obtaining it; in addition, there is the question of whether there is a controller which is optimal in some sense. The dynamic adaptive computer scheduling of flexible manufacturing systems and the decentralized flow control and routing of information packets in a reconfigurable communication network are important examples of such control problems. Another interesting question is that of what the concept of *modularity* means in the DEDS context, and what it implies in terms of functional decomposition and communication requirements among different local controllers and subsystems. For example, in a system described by differential equations, action in one part of the system can instantaneously affect every other part of the system; since this is not the case in a discrete event dynamical system, it may be possible to more effectively utilize the modularity of the system. In summary, research on the control of hybrid systems and discrete event control theory will integrate control and optimization ideas and paradigms with new techniques drawn from formal logic, languages, queueing theory, and probability, and will be closely linked with counterpart developments in computer science and communication technology.

Signal Processing

The existence of mutually enriching interactions with neighboring fields is a hallmark of research areas of vitality and growth. In this spirit, the traditions of control and systems in estimation, identification, and the exploitation of structure to develop efficient algorithms provides numerous opportunities for significant research in signal processing. Several of these opportunities are outlined in the following paragraphs.

Model-Based Signal Processing: We have a paradigm that has extremely broad relevance. Based on uncertain models of phenomena and processes we develop procedures for inferring or estimating attributes of these models and for quantifying how well this can be done. This focuses attention on the objectives of signal processing problems. The model-based and explicit objective-based approach provides a rational basis for assessing assumptions and for defining experiments to validate assumptions. Also, since the objective is part of the formulation, assumptions can be assessed within the context of their validity for the stated objectives.

The perspective described in the preceding paragraph has been of great value for many years in areas such as navigation and guidance and the monitoring of chemical processes, in which estimation and information extraction based on stochastic models have had a long history. Moreover, in recent years, there has been a growing recognition that these ideas hold much promise in a number of other research areas of present and future importance. One such area is *computational vision*. This topic includes object recognition, motion estimation, scene analysis, visual sensing, and remote sensing. Important applications are found in robotics (e.g., obstacle avoidance, vision for feedback) and in automation (e.g., visual inspection systems).

A second such area involves so-called *inverse problems*. These signal processing problems arise in applications such as seismic signal processing and geophysical exploration, nondestructive testing and medical imaging (CAT scans, ultrasonic imaging, NMR). These problems have many of the features one finds in the field of *system identification* in that one is attempting to reconstruct parameters of a physical phenomenon from knowledge of externally applied and observed signals (which in this case may depend on space rather than, or in addition to, time). Consequently, the tenets of identification theory provide a sound basis for formulating such problems, and for evaluating quantitatively the success of proposed approaches. A critical issue in these problems is controlling the number of degrees of freedom to match the available information. A related issue is the complexity of the inversion method. The perspective of exploiting structure is critical here in developing fast efficient signal processing and information extraction algorithms.

Estimation of Discrete Events and Probabilistic Symbolic Reasoning: Multiple model hypothesis testing is a very important process in symbolic reasoning. In such problems we have a discrete set of alternative interpretations of data, we have models for each, and we have optimal processors for each that allow us to produce statistics that form the basis for efficient and rational assessment of which alternative is most likely to be correct. This concept, which has its origin in system identification and adaptive control, has already been expanded to much more complex problems, such as multisensor correlation and multitarget tracking and identification, where one may be interested in making discrete decisions (e.g., concerning vehicle identity). Another example is electrocardiogram analysis in which continuous waveforms provide the input to algorithms charged with detecting discrete events and providing diagnoses. This hybrid model approach, incorporating higher, discrete levels as well as continuous data is relevant to vast numbers of applications. Multimodal control of automobiles, requiring identification of the road characteristics, is one specific application. Distributed detection and identification of anomalies in power systems before they lead to blackouts is another. The hybrid model approach provides a framework in which it is possible to think about fusing all types of knowledge and information (e.g., operator inputs can be introduced in multiobject tracking as direct measurements of the discrete symbolic level). It also very naturally reduces data and knowledge to statistics as the basis for higher-level reasoning and is well set up for parallel processing.

Efficient Algorithms and VLSI Processors: Systems theorists bring to algorithmic design a perspective of, and grounding in, the identification and exploration of structure. This provides us with a very important opportunity for suggesting and developing new algorithms and novel *computer architectures* for signal processing and control. For example, efficient solutions of inverse problems, based on estimation theoretic ideas, have had great impact already. There is also a significant opportunity for exploiting a system-theoretic perspective to develop a unique approach for the design of special purpose computing arrays (such as systolic or massively parallel arrays). For example, the system-theoretic interpretation of the problem of implementing a particular algorithm on a systolic array is that of a realization problem. This leads naturally to a formalization of what a systolic algorithm is and to the establishment of a rational, systematic approach to determining if such an implementation exists for a particular problem and, if it does, for constructing the algorithm. One would also expect such an approach to lead naturally to new classes of architectures that avoid some of the restrictions of systolic architectures. There have been already some initial successes in this direction, demonstrating the potential of this area of research.

A related area of research is the development of efficient algorithms for two-dimensional (2-D) signal and *image processing* problems. One promising direction that has grown naturally out of control and systems paradigms is that of "acausal" system theory for signals in several dimensions. This theory points to new notions or recursion for spatial data (essentially propagating inward from and outward toward the boundaries of the data set) that suggest new parallel decompositions for 2-D signal processing and opens up very different filter stability questions than one finds in standard 2-D signal processing algorithms. Examples such as this indicate the unique nature of the contributions that can result from an increased involvement of systems theorists in problems of the integrated design of processing algorithms and the computer architectures on which they are implemented.

Large-Scale Systems: Distributed Information Processing and Decision-Making Architectures

A revolution in sensor technology, coupled with increasing information transmission capabilities, confronts the designer of complex engineering systems with overwhelming amounts of data about the environment and the process to be controlled. For instance, in flexible robotic manufacturing cells, a variety of sensors deliver information about position and spatial relations between robots and objects in the workspace. Data are generated not only by force sensors, proximity sensors, tactile sensors, or visual sensors (cameras), but also by CAD models of various objects as well. Designing the architecture of the information processing system that will *fuse* the information from all these sources to obtain a consistent view of the workspace is an open problem. Similar problems exist as well in other complex systems, such as in command and control systems, computer networks, complex aircraft control systems, and power systems.

These *large-scale systems* consist of many subsystems which have access to different information and are making their own local decisions, but must work together for the achievement of a common, system-wide goal. Each subsystem often possesses limited prior knowledge of the details of the overall organization; its model often describes only the subsystem itself and portions of other subsystems strongly coupled with the former. Consequently, each subsystem may be operating with limited knowledge on the structure of the remainder of the system, which may be reconfiguring itself dynamically due to changing requirements, changes in the environment, or failure of components. The representation of such prior knowledge in a modular way to provide a consistent interface between different subsystems is a challenging, unsolved problem.

Each subsystem has its own sensors collecting data and local processors generating information, which must be integrated with the information obtained by other subsystems to be used for decision making. Some key research issues that arise under these circumstances are the following:

a) determination of how much sensor data should be transmitted directly and how much processing and aggregation should be done at the local, subsystem level;

b) representation of aggregate information so that its dissemination conforms to the interfaces and the knowledge of each subsystem and so that it does not overload other subsystems;

c) maintaining consistency of the distributed data bases which store the available information;

d) establishing who should communicate what, to whom, and when, while taking into account the fact that information is needed not for its own sake, but in order to support decision-making; and

e) setting up communication links and establishing protocols to anticipate information transmission requirements.

Promising research directions include the determination of novel aggregation methods which lead from fine-grained variables to more qualitative coarse-grained variables which capture the most useful information needed for a specific decision; the development of relational databases containing multiple views of the same object, and systematic rule-based techniques for coherent data update; and the development of algorithms for the systematic retrieval and processing of data to obtain a consistent view of the objects to be identified or the situation to be assessed.

These challenges range from problems of *distributed detection* and signal processing (at one end), to optimal methods for *information aggregation* (in the middle), and to consistency of dynamically updated *distributed data bases* (on the other end).

Some elements that appear in this research on *distributed information processing and decision-making architectures*, which have not been fully accounted for in the past are as follows:

a) The need to limit the complexity of the information processing as reflected in the computational requirements. Tools from computer science (such as complexity theory) should be blended with concepts from decision theory to determine, for example, realistic models of the computational capabilities and intelligence of the decision-making units. Integration of parallel processing algorithms with the operation of a distributed decision-making architecture is another typical problem;

b) Viewing reliability and survivability as key performance specifications, rather than issues of secondary concern; and

c) Ability to cope with sudden, qualitative changes of the state of the system.

Large-scale, high performance systems often cannot be controlled (whether in direct or supervisory mode) by a single individual; the man–machine paradigm of a single human operator in the loop is not adequate for this case. Furthermore, classical *organization theory* also does not meet the need for designing the organizational structure or architecture of a team supported by complex *decision support systems*. The design of air traffic control centers, energy control centers, the tactical command center of a military organization, or the higher level control of intelligent machines are applications where the design of the control systems cannot be decoupled from the design of the decision-making architecture.

The behavior of multiple decision-makers operating in an uncertain environment with partial information can be very different from their optimal behavior, if they were operating in isolation. *Team theory* has provided several important paradigms which furnish insight into complex and realistic systems. While more such paradigms would be useful, team theory has its limitations due to the inherent complexity of the problems being addressed. New theories need to be developed which, instead of insisting on optimality under well-modeled conditions, should rather provide flexible, reliable, and robust mechanisms for making decisions. For example, a new class of problems that has to be addressed concerns the *real-time scheduling* of stochastically generated tasks, in a situation where no decision-making unit has a complete view of the full set of pending tasks.

The need to include models of *human decision-makers* in designing distributed architectures introduces a host of different problems. Our limited knowledge of human cognitive processes has resulted in the absence of qualitative—let alone quantitative—models that are useful for analysis and, especially, design. Furthermore, the kind of uncertainties that arise from inclusion of the human decision-makers in the analysis of systems, such as in command and control, are not well understood or modeled. Specific research needs include the development of models of humans that incorporate behavioral considerations; the modeling of the interactions between decision-makers, whether they take the form of information sharing or the issuing of commands; the understanding of the effects of differences in the conceptual models, used by different decision-makers, on the performance of the system; and understanding the notion of robustness when decision-making is done in part by humans. The latter consideration is particularly important in design, where account should be taken of the fact that different individuals will be operating the same system at different times, e.g., different shifts in an air traffic control center.

Currently, huge investments are being made in decision support systems of various degrees of sophistication to allow decision-makers to control large complex systems in the presence of uncertainty. However, this is done with very little modeling and analysis of components, such as decision aids or the decision support systems themselves, and without even a rudimentary understanding of the dynamics of the organizations they support. This is a specific research area where the development of models and analysis and design tools could have substantial impact.

The design of the information processing architecture and the decision-making architecture cannot be done separately when distributed architectures are considered; performance requirements, such as reliability and survivability, make it necessary that an integrated approach be taken.

One ubiquitous class of architectures that has received much attention from several disciplines is that of *networks*. The functioning of our society is increasingly dependent on communication, transportation, and power system networks. Computers used to control such systems are themselves connected to form computer networks. Networks form the backbone of distributed systems. The discussion that follows is closely related to the discussion of discrete event systems presented in the previous section. In fact, queueing networks are but one class of discrete event dynamical systems.

Network analysis and optimization is one nexus between control theory and operations research. Queueing theory has been used to address the steady-state operation of a typical network, while mathematical programming methods have been used to solve resource allocation problems. This is meaningful only if the dynamic transients can be disregarded. In most present-day networks this is not the case and the need for dynamic analysis and control algorithms is urgent.

Dynamic analysis of networks is needed to determine *flows* of packets and their *sojourn times*, as well as the *throughput* of network sections or whole networks. To stress the complexity of this task, one need only consider the throughput of a manufacturing assembly line consisting of a series of three machines each with a finite buffer: the complete solution to this problem is as yet unknown. Promising analytical tools here are Markovian models and discrete event systems.

Control of dynamic networks generally includes *flow control* and *routing*. The flow control regulates the admission of packets into the system according to such criteria as overload prevention or fairness, while routing determines their travel through the network. At present, these tasks are accomplished by heuristic protocols. In addition, each class of networks has its own emergency control procedures. For example, in power systems, they involve load shedding (i.e., disconnecting some of the customers), network restructuring, and use of reserve generation. It is not always possible to predict the outcome of such ad hoc actions and much research remains to be done to develop a systematic approach to network control design.

Recent developments in stochastic control, Markov decision processes, jump Markov processes, and global optimization methods such as simulated annealing offer a promise that such a methodology can be developed. Due to the size and geographic configuration of most networks, aspects of decentralized and distributed controls must be stressed.

System Integration, Experimentation, and Implementation

Increasingly, control engineers must design controls for complex, large-scale, spatially distributed systems, often consisting of multiple subsystems operating asynchronously on several time scales. The resulting systems must be configured for reliable, fault tolerant, and diagnosable operation and must perform complex functions while meeting multiple criteria. Consequently, the design process must include the integration of the controller design with the other elements of design as an interactive, iterative process. Consider, for example, the interaction between material selection, truss structure, placement of actuators and sensors, and implementation of control algorithms in large, flexible space structures or in a flexible robotic arm.

Further examples of such complex systems are flexible manufacturing systems, distributed computer systems, power systems, command and control systems, and aerospace control systems. Traditional approaches to control, which are focused on the determination of control schemes for given localized plants modeled by a collection of different and/or difference equations and with relatively simple sensors, are not adequate for such systems. For example, additional actuators and sensors may be required for graceful degradation of performance due to system faults. Current approaches for integration of multiple sensors, computers, actuators, and a variety of physical plants are ad hoc and have not been applied in a consistent manner.

Current commercial aircraft, automobiles, and some rail systems contain numerous microprocessor-based, computer-controlled subsystems (associated with engines, suspension systems, control surfaces, braking, etc.) Most of these subsystems operate independently, and have been designed with subsystem performance as the major goal. As these subsystem controllers evolve into the next generation of microprocessors with large memories, integration of subsystems to provide better performance and higher total system reliability will become dominant design objectives.

Control system designs more and more often include implementation of the control laws as software designed to interact with hardware, and to do so in real time. The exceptionally high reliability requirements imposed by this arrangement are increasingly being met by using an "object-oriented" approach in the design. Such a design approach treats a software system as a network of modules, each modeled as a finite-state machine. However, no rationale exists at this time for a compatible modeling of the hardware with which the software system must interact, or for the hardware which is to execute the software. A comprehensive design would include the topology of the software module, the hardware device interconnections, as well as the nature and timing of the communications among them.

A key issue in developing integrated design approaches as a solution to the *system integration* problem is the derivation of system specifications from a set of mission-derived requirements. A methodology for doing so that does not stifle creativity and ingenuity, the essence of engineering, is missing.

The problem is multilayered: requirements reflect mission objectives, and the extent to which they are met is a measure of the effectiveness of the system. Specifications reflect measures of performance of the system—measurable attributes against which the design can be tested. The tacit assumption is that if specifications are met, then requirements will also be met and the resulting system will be effective. However, this presumes that the implicit tradeoffs often imbedded in the requirements can be made explicit in the specifications. This has been the case in the design of component subsystems, but is not the case for the large-scale systems that must carry out a variety of missions in a wide range of uncertain environments. Consequently, basic research, both conceptual and analytical, in understanding, modeling, and analyzing the relationship between requirements and specifications is needed as a first step in developing quantitative approaches for system integration. System theoretic aspects include the classic problem of predicting system behavior from component behavior and the evaluation of the sensitivity of specifications to requirements.

The complexity and variety of the systems considered renders standard mathematical analyses of the designs often impractical. In that case, the only recourse for testing and verification is *simulation*, whether exclusively on a computer, or in a test bed. The latter combines computer simulation of some subsystems and functions with actual components and software. These large-scale simulation systems with their many degrees of freedom have raised serious technical problems about the design of experiments and the sequences in which they should be carried out in order to obtain the maximum relevant information. Examples of systems requiring such simulation are computer communication networks, power system failure recovery systems, flexible manufacturing systems, and command, control, and communication (C^3) systems.

In addition to simulation, direct *experimentation* is becoming once more an important component of analysis and design of systems. One cannot over-emphasize the importance of experimentation (including simulated experimentation) particularly in the study of some dynamical systems for which there does not exist a hundred-year tradition of modeling effort upon which we can rely. Successful modeling efforts in manufacturing systems, communications networks, and command and control systems must have a solid empirical base that is currently lacking. Pure, abstract mathematical contemplation is an unlikely lead to useful models. The discipline of physics provides an appropriate parallel: advances in theoretical physics are more often than not inspired by experimental findings. In mathematics, experiments are becoming acceptable as motivation and guideposts in the search of proofs for difficult problems. We must foster an environment for high-quality experimental control system research, if we are to succeed in achieving new breakthroughs in the future, especially for the classes of systems that include discrete logical states and discrete events.

New analytical and statistical methods to evaluate results from such experiments and simulations are needed. In addition, innovative computing methods are necessary. Examples of hard problems in this area include effective evaluation methods for simulation results on flexible manufacturing systems, and the quantitative analysis of the islanding phenomenon of large power systems under failure conditions.

Implementation is the most rapidly changing component in systems engineering. Computer, sensor, and actuator technology is evolving rapidly, enabling the development of new, bold, and higher performance designs. The systematic development of computers with features particularly suited for control (including VLSI chip architectures) is only one aspect of implementation. The design of reliable and fault-tolerant computer controllers which, together with sensors and actuators, will result in maintainable systems poses a considerable challenge. Therefore, the development of new synthesis and design theories that can incorporate the hardware implementation constraints of the future constitutes a significant new research focus.

Concluding Remarks: A Longer Range View

Control technology is moving to take advantage of the new opportunities made possible by advances in electronics. Now, the question arises as to what form will the scientific underpinnings of the field take. This is important to researchers, educators, and textbook writers, as well as to entrepreneurs and designers of engineering systems, since a suitable and generally understood language is essential for the rapid communication of ideas. It is arguable that in addition to the dependence on mathematics and physics, which has characterized engineering in general and control theory in particular, dependence on ideas originating in other scientific traditions will grow. To continue to serve the needs of its traditional constituency, the automotive, aerospace, and process control industries, control engineers need to conceptualize, in a common framework, topics ranging from computer science to life science. In this brief epilogue, some possibilities are discussed as to how control engineering might shift in response to these forces already at work.

Cybernetics—derived from the Greek word for steersman—is a term coined by Norbert Wiener in the 1940's. For most, Wiener's neologism brings to mind an as yet unfulfilled dream. It was born of the expectation that there would evolve a synergism between man and machine that would extend broadly and deeply over the intellectual landscape, encompassing a range of activities from mathematics and engineering to physiology and psychology: machine in the service of man and machines imitating man. As far as control theory is concerned, it is ironic that in the 1960's the exceptional effectiveness of highly analytical techniques in solving the shorter range problems posed by aerospace engineering made cybernetics seem premature at best and naive at worst. Today, the situation is completely different; developments in computer technology have made possible a whole range of new applications including the important fields of robotics and computer vision. These developments are encouraging researchers to take a new look at some old problems; all of a sudden the nature of the constraints has changed. The result is a search for paradigms which will be effective in the new setting. Can we draw inspiration from the structures discovered in biology to design better machines? Is it realistic to use animal behavior as a benchmark against which to judge the performance of our creations? Will it turn out that the cybernetic point of view is an important source of new ideas?

Solving almost any significant engineering problem requires finding a framework for identifying subsystems which interact with each other in easily described ways. In designing automatic control systems, engineers have used transfer functions to describe the actuators, sensors, and controllers and to use feedback theory to describe the interactions. On such a scale, control engineering ideas have been useful in solving problems ranging from the hydrogenation of vegetable oil to renal failure. However, we now face the need to describe systems in terms of larger pieces. A team of engineers may find itself with a system to design which begins with symbolic input and ends with the specification of the motions which carry out the fabrication and delivery of a product. Although it is doubtful that any one presently recognized discipline can provide the complete solution for problems of this scope, there are strong practical and intellectual reasons for exploring frameworks which have the potential to treat problems in terms of larger pieces.

At the same time, machines have become more symbol oriented. In this respect, engineering is imitating man. The use of the word intelligence as an adjective for machines may seem to some to be a contradiction of terms, but to say less understates the real situation. We have already gone beyond the point where the engineering of significant systems consists exclusively of building better feedback loops. Improving the performance of many control systems depends on achieving a greater degree of system integration.

What does this suggest for the future of control? Which disciplines have the most to offer in terms of tools and modes of thought having the capacity to shed light on the problems to be faced by control engineers? Is it not reasonable to expect that there is something to be learned from the studies of psychologists, who have struggled for years with the problem of how the human mind coordinates the several hundred degrees of freedom of the body? What can one learn from software engineers by studying the techniques, such as object oriented languages, which they use to control complexity? Is it feasible to develop a theory of real-time control that addresses the problems of computer architecture and dynamical system performance in a substantial way? Is it time to replace the classical open-loop/closed-loop dichotomy of control strategies with a classification that recognizes that, at a higher level, many systems operate by combining open-loop and closed-loop modes? These questions provide strong evidence that the field of control has a significant role to play in engineering and science—one requiring contributions from a broad spectrum of well trained and intellectually vigorous researchers who have a long-term commitment to the field.