January 1976 ESL-P-647

TRENDS IN MODERN SYSTEM THEORY*

by

Michael Athans
Director, M.I.T. Electronic Systems Laboratory
Professor of Electrical Engineering
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Text of a keynote address delivered at the Engineering Foundation Conference on Chemical Process Control, Asilomar Conference Grounds, Pacific Grove, California, on January 18, 1976.

* The preparation of this note was supported by AFOSR under grant 72-2273, NASA Ames Research Center under grant NGL-22-009-124, NSF under grant ENG 75-14103, and ONR under contract N000014-76-C-0346.

1. INTRODUCTION

We are approaching the twentieth anniversary of modern control theory, which probably started with the return to the time domain through the use of state variables, Bellman's Dynamic Programming algorithm, and Pontryagin's celebrated Maximum Principle.

A true revolution in analysis and design has occurred during this time period, and new research directions have blossomed. The words "Riccati equation" have the same underlying power for design as "root locus". Computer-aided design has freed the engineer from much dogwork, redirecting his natural and valuable talents to issues of modeling, performance evaluation, and reliability analysis. We are seeing now the third generation of students versed in the intricacies of modern system theory and its algorithms. The unique Joint Automatic Control Conference that served as the intellectual meeting ground in the early sixties, is now only one of several national and international conferences, symposia, and meetings devoted solely or partially to system theory and its applications; it seems that one could spend the major part of a year travelling from one control conference to another. Similarly the number of journals and published papers devoted to control have mushroomed.

Yes indeed, Modern System and Control Theory is a dynamic field. And yet, despite its intellectual excitement and powerful design methodology, its utilization by several industrial organizations, and I must regretfully include the Chemical Process Control industry in this class, has been minimal, especially in the United States. I sincerely hope that the presentations, discussions, and critiques presented in this conference will serve a useful purpose in closing the proverbial gap and that they will supplement the NSF sponsored workshop which was held in Baton Rouge in 1973.

I feel very strongly that constant interplay between applications, available hardware, design methodologies, and theory is absolutely necessary to sustain a dynamic growth in control. I will elaborate on this point later on, when I shall indicate by example some new research directions that our group at M.I.T. has undertaken, motivated by specific applications areas. Both abstract theory and applications are important. But let us not forget the words of Boltzman:

"There is nothing more practical than a good theory."

It is perhaps the relative "goodness" of the theory with respect to different application areas which may account for the unevenness of its applications. Let us not forget that control is a strongly interdisciplinary area. I am often asked by undergraduate students what are the boundaries of modern system and control theory and I reply 'Anything that wiggles and squirms is fair game as long as we can kick it', so as to try to stress the underlying dynamic and stochastic nature that presents a cornerstone for much of the theoretical development associated with modern system theory. A healthy interplay between practical problems and state of the art theory is absolutely essential to provide the spark for at least relevant theoretical directions. Whether or not the theory which will be generated is good is a risky proposition. At the very least one should not expect the theoretician to evaluate the goodness of the theory.

Only the practitioner can provide the feedback and only after he has applied the theory. If the practitioner wants to influence the modification of existing theory and its long term development, then the user of the theory must communicate the advantages and disadvantages of the state of the art to the theoretical community. We see very little feedback about this at the present time, especially in industrial applications.

The way we face the issue of the interplay between theory and practice at the MIT Electronic Systems Laboratory is to complement the faculty and students with research staff whose responsibility is to try out advanced concepts in control to realistic applications. In this manner, we accelerate the feedback process on the advantages and shortcomings of the theory. Needless to say, such a research operation is expensive, and we often bid in a competitive way to work on applications. We have been successful in the aerospace, transportation systems, and power systems areas. Unfortunately, I would not know where to start for industrial process control; certainly not at NSF. Hence, at this time it is fair to say that our research is not directed by the needs of the industrial community. The situation could change if a long range applications research program could be initiated. Until then, only secondary fallout could be expected.

Let me now indicate some of the exciting areas of research that I feel are very important, and in which a significant amount of both theoretical and applied future research is essential.

By necessity, I shall only touch upon certain research areas in which I have a certain degree of competence. These are

(1) Linear Control System Design

(2) Adaptive Control

(3) Failure Detection, Control under Failure, and System Reliability

(4) Large Scale Systems and Decentralized Control.

I believe that all of these topics are relevant for industrial and process control systems. I apologize if your favorite research area is not covered in the list of trends. The omission does not imply that it is not important. My selection of topics is dictated more by some familiarity with the subject and the people that are doing research.

I will conclude the talk by some brief remarks on research funding.

2. LINEAR CONTROL SYSTEM DESIGN

For a whole variety of applications the design of a linear feedback control system which regulates a process about a desirable "set point" or "steady state condition" in the presence of disturbances is a truly bread-and-butter problem. Both in classical and modern approaches the linearized dynamics of the process are used for design purposes.

The advantage of the design methodology associated with modern control theory, and especially with the Linear-Quadratic-Gaussian approach [1] is that multi-input multi-output systems can easily be handled without the need for decoupling. It is fair to say that much of the design methodology is well developed for both the classical and the modern approaches, together with the existence of computer aided design packages.

What is lacking in both approaches is the sensitivity of the design to large parameter variations. I must admit that I cannot fully appreciate Horowitz's [2] contributions from a classical point of view in this area. However, with the notable exceptions noted below, modern control theory has not produced anything spectacular either.

In classical servomechanism theory the notions of gain and phase margins presented an excellent vehicle for evaluating the robustness of a compensator. It is perhaps less known that optimal linear-quadratic designs enjoy excellent gain and phase margin properties. For the single-input single-output case Anderson and Moore (in their book [3], pp. 70-77) have shown that optimal linear-quadratic regulators have infinite gain margin and a phase margin of at least 60°. Very recently as yet unpublished work by my students Wong [4], [5], and Safonov [6] have extended the single-input single-output results of Anderson and Moore to the multi-input case. Our results imply that if one designs a linear-quadratic optimal system with an arbitrary number of inputs then any control channel can have an arbitrary gain increase and a phase reduction of up to 60°, and the resultant system would still be stable. In addition, properties of the multivariable optimal linear regulator with respect to gain reduction (which may be due to multiple saturation) have been obtained.

In Figure 1 we show the typical linear-quadratic design, in other words, the solution of the optimal control problem of a linear time-invariant system with respect to a quadratic performance criterion. The state feedback matrix G is determined from the solution of the well known algebraic Riccati equation. It was demonstrated by several empirical studies that the resultant linear quadratic design was characterized by a certain degree of robustness. The results that have been obtained by Mr. Poh Wong [4] can be described as follows:

Gain Margin Theorem

In reference to the block diagram of Figure 1, suppose that the

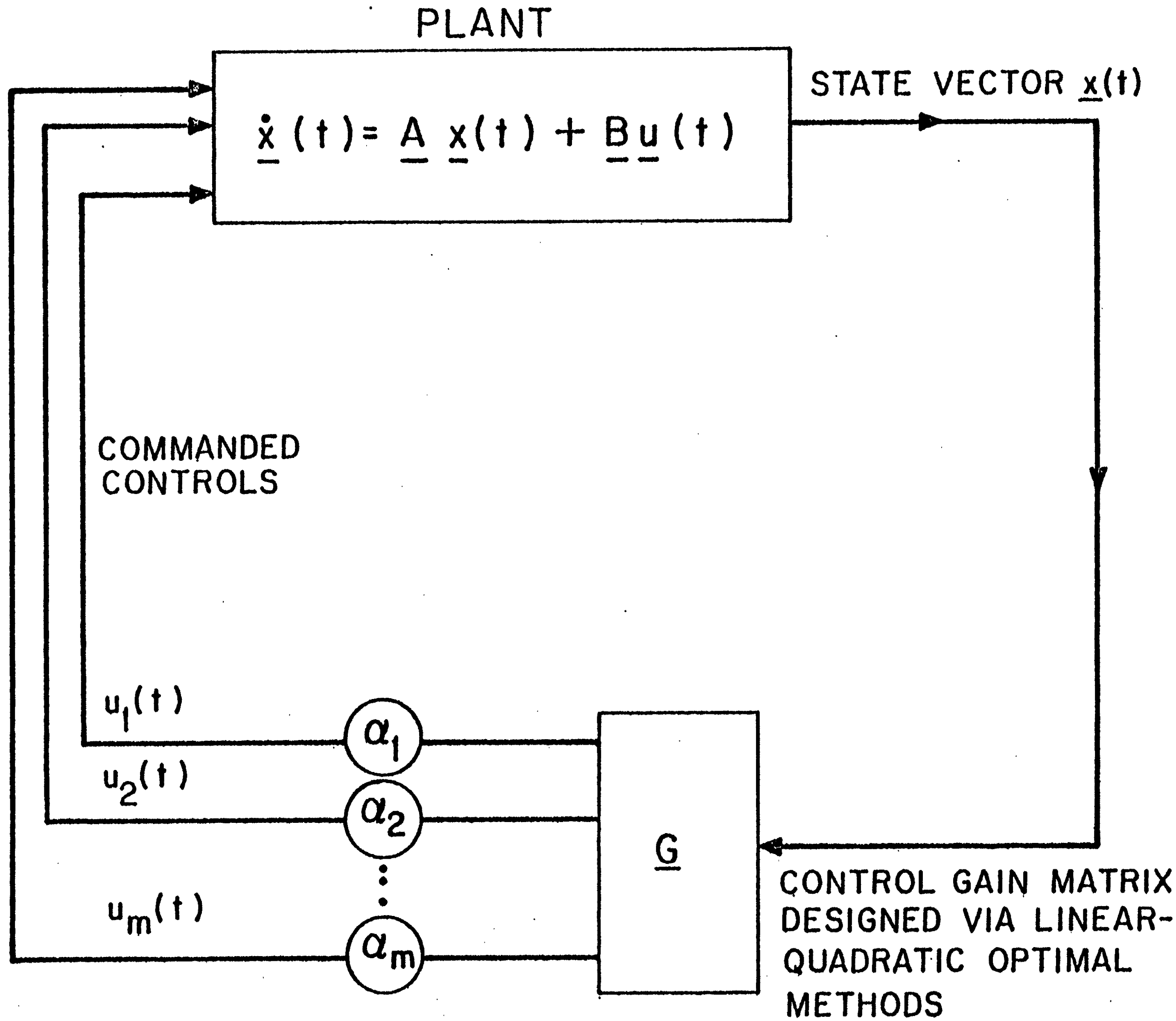
PLANT
ẋ(t) = A x(t) + B u(t)
STATE VECTOR x(t)
COMMANDED CONTROLS
u1(t)
u2(t)
um(t)
α1
α2
αm
G
CONTROL GAIN MATRIX DESIGNED VIA LINEAR-QUADRATIC OPTIMAL METHODS

Fig. 1

constants

$$\alpha_1, \alpha_2, \ldots, \alpha_m$$

represent gains. Then the <u>multivariable gain margin theorem</u> states that the resultant closed loop system will remain stable for all or any variations in the gains α_i greater than unity

$$\alpha_i \geq 1$$

<u>Gain Reduction Theorems</u>

Again in reference to Figure 1, another important problem is when

(1) saturation or other nonlinearities, or

(2) changes in the elements of the nominal values of the open loop $\underline{A}$ and $\underline{B}$ matrices

can be reflected as changes or reductions in some or all of the gains α_i, i.e., some or all α_i have the property

$$\alpha_i < 1$$

Under these conditions, Mr. Wong has derived a set of sufficiency conditions which guarantee the stability of the closed-loop system. Interestingly enough, these conditions appear to be constructive in the sense that they provide guidance on how one should select the weighting matrices in the quadratic performance index so as to increase the robustness of the design in the presence of reduced loop gains.

<u>The Multivariable Phase Margin Theorem [6]</u>

Michael Safonov in his initial doctoral research has obtained a set of general results in the case that the α_i's are more general operators. A special case is when the α_i's in the block diagram are unity gain pure phase shifting elements. He showed that any or all of the α_i's can introduce up to 60° phase lag and one is still guaranteed stability of the basic linear-quadratic optimal design.

Needless to say, we are furiously working on exploiting these results (from both a theoretical point of view as well as from an applications points of view).

In my opinion, such results can provide exciting new possibilities in multivariable system design.* Overall qualitative measures of robustness and large parameter variation sensitivity studies are important future research areas.

A final note on the above results. Even the most hardnosed engineer will have to admit that gain and phase margin properties are "good stuff". Nonetheless, in order to derive these multivariable gain and phase margin properties we had to use some of the most abstract theory (geometric system theory concepts a la Wonham [7] and functional analysis, e.g. extended L_2 spaces).

3. ADAPTIVE CONTROL

By adaptive control I mean real time control of physical processes which involves to some degree parameter and/or structure identification. There are several recent survey articles that deal with this topic; see references [8] to [10].

---

* For example, we plan to evaluate the impact of our results with respect to the design of aircraft stability augmentation systems in the presence of large uncertainties in the aircraft aerodynamic parameters, simply because NASA and the Air Force are willing to foot the bill. The implication of the results with respect to chemical process control design, in which modeling uncertainties, and time delays that introduce phase shift are more important will have to wait.

At the present time there exist several design methodologies toward adaptive control ranging from simple to sophisticated. The stumbling block in many adaptive design methodologies is associated with the amount of real time computation which is necessary. The most reliable identification methods, [11], [12] ,[13], e.g. maximum likelihood techniques, are characterized by the greatest real time computational requirements; furthermore, the iterative nature of the maximum likelihood estimation algorithm makes it unattractive for some applications. On the other hand, simpler real time parameter estimation techniques in a stochastic environment, which are recursive in nature, such as variations of the extended Kalman filter algorithm may be characterized by poor convergence properties (and even divergence if the nonlinear filtering algorithm is not tuned properly).

With the exception of the work of Astrom's group in Sweden, which has used several adaptive techniques and in particular the self-tuning regulator concept [14], there have been very few applications of advanced adaptive algorithms to industrial problems. In fact, the overall theory and algorithms associated with the self-tuning regulator will have to be extended to deal with the multi-input case.

What is needed is a systematic set of case studies of alternate adaptive methodologies to real systems. The only real case-study that I know of is funded by the NASA Langley Research Center in which several groups of researchers are using different designs for stability augmentation systems for the F-8C Digital-Fly-By-Wire aircraft [15] to [19]. Such studies are critical if we are going to obtain a more basic understanding of the performance, and computational requirements as well as the advantages and disadvantages of different advanced stochastic adaptive algorithms.

From a theoretical point of view, I feel that the next decade will be devoted to a consolidation and modification of existing concepts in adaptive control. We still do not have a clear understanding of the dual-nature of adaptive control, i.e. the simultaneous interplay of inputs that are "good" for identification while, at the same time, are also "good" for control. Further clarification of the problems associated with system identification under stochastic closed-loop conditions is absolutely essential.

4. FAILURE DETECTION, CONTROL UNDER FAILURE, AND SYSTEM RELIABILITY

Another exciting area for research in the next decade deals with the overall problem of reliable system operation. The motivation for studying these types of problems is self evident, since reliable operation is crucial in a variety of applications.

At the present time, we do not have a systematic methodology or theory for handling such problems. Reliability theory, as a discipline of its own, does not appear to be well suited for dealing with complex dynamic situations.

Although we do not have a theory, there are several theoretical investigations and results which are emerging in the literature that appear to represent promising entries to this very important problem. Several of these concepts were presented at a workshop held at MIT, and funded by the NASA Ames Research Center, on Systems Reliability Issues for Future Aircraft, in August 1975. The proceedings of this workshop will be published as a NASA Special Publication in the summer of 1976. It was evident from the presentations in that workshop that the present state-of-the-art in constructing reliable designs is to use triple or quadruple redundancy in crucial actuators, sensors, and other key components.

With respect to future high performance aircraft, often called control configured vehicles or active control aircraft, the trend is to utilize a greater amount of control devices and sensors, which will be under complete automatic control. If each new sensor and actuator is constructed to be quadruply redundant, this will result in a prohibitively expensive design. The idea is then to try to arrive at systematic means for designing the aircraft control system such that the redundancy requirements are reduced, while in the case of sensor/actuator failures (when recognized), one can reorganize the control system so that the operative sensors and controllers can still maintain safe flight.

Failure detection and isolation is then of paramount importance and some extremely important work has been done in this area during the past four years. The field is well surveyed in a recent paper by Willsky [20]. Essentially, the idea of failure detection and isolation relies very heavily upon the blending of dynamic estimation concepts (e.g., Kalman filters) with hypothesis testing ideas. Under normal operating conditions the residuals (innovations) of Kalman filters are monitored. A failure exhibits itself as a change in the statistical properties of the Kalman filter residuals. Once a failure has been detected one can formulate a set of alternate failure modes, and through the use of generalized likelihood ratios one can isolate the failed component.

Once more NASA and DOD are funding several efforts to apply these ideas to different types of aircraft. Within the next three years we are going to see two or three case studies which will give us a great insight into the entire issue of failure detection and isolation, and obtain a much better understanding of the inevitable tradeoffs associated with the

(a) rapidity of failure recognition

(b) rapidity of failure isolation and classification

(c) false alarm probabilities

(d) computational complexity.

The application of failure detection to other non-aerospace areas is also emerging. For a concrete example, the use of these dynamic failure detection techniques to automated EKG processing is under investigation by a group headed by Gustafson at Draper [21]. We have recently submitted a proposal to the U.S. Department of Transportation, University Grants Office, to study the feasibility of incident detection in freeways using stochastic dynamic models of freeway traffic flow. Non-dynamic hypothesis testing ideas for freeway incident detection have been recently reported by Payne [22] in a definitive study that points out the limitations of quasi-static algorithms. Payne has shown that in order to have a 0.108% false alarm probability one may have 68% of undetected incidents.

It goes without saying that such concepts are of paramount importance in industrial and chemical process control systems. However, I am not aware of any studies, much less results, in this area.

Failure detection and isolation is only the tip of the iceberg in the broad area of designing reliable systems. The whole issue of alternate ways of reconfiguring and reorganizing the control system, <u>in real time</u>, following the onset of a failure is a wide open research area. Much research at both the theoretical and the applied level needs to be carried out during the next decade. From a theoretical point of view, the work of Varaiya and his students [23] on the <u>optimal control of jump processes</u>

may represent one definitive point of view in dealing with such complex issues. Many other approaches are desperately needed.

5. LARGE SCALE SYSTEMS AND DECENTRALIZED CONTROL

I would like to next focus my remarks upon a class of problems that will provide the motivation for the development of new theoretical investigations during the next decade. These problems are loosely referred to as large scale systems, and the control methodology as decentralized control (see references [24] to [26] for partial surveys). Typical application areas that fall into this broad category are indeed numerous. For example:

Power systems. In power systems we have dynamic interactions of hundreds or thousands of variables. Challenging problems include [27]

(1) system security

(2) economic power generation and dispatch

(3) transient stability emergency control.

Transportation systems [28]. In the transportation systems area there are opportunities for the coordinated control of ordinary freeway and city traffic as well as in the area of dynamic scheduling and control of automated vehicles, often referred to as personal rapid transit systems, which have headways of less than a second.

Aerospace systems. Future high performance aircraft (often referred to as control configured vehicles) will be characterized by

(a) reduced weight

(b) changed geometry, which will reduce the aircraft static stability.

Future aircraft will require many additional sensors and control devices, under automatic control, to compensate for the decreased natural aircraft stability and the increased dynamic interaction between the rigid, flexure, and flutter modes.

Communication systems. Few control theorists realize that ordinary communication systems, both in the civilian and military sectors, as well as data communication networks, such as the ARPANET, present formidable stochastic and dynamic control problems.

Figure 2 shows the performance of the Bell Telephone system during peak periods of demand (Mother's Day, Christmas Day). As the demand increases, the telephone system performs well. However, after a certain point, instead of reaching saturation (like an overcrowded freeway), the number of completed calls rapidly decreases. This is due to the fact than an increasing percentage of the telephone network is used up by the switching centers communicating status information to each other that they have reached capacity!!

Similar ,and stranger, instability phenomena arise in the ARPANET [29] which employs a message-switched strategy. Under a message-switched strategy ,a message is split into submessages called "packetts". Different packetts are transmitted over different links to a destination node, where they are reassembled (if they all arrive at about the same time!!). You can easily visualize the real time control and communications problems.

One could go on and on describing additional large scale systems that certainly require the development of improved dynamic control strategies. However, let us pause and reflect upon their common attributes. They are

(1) topologically configured as a network

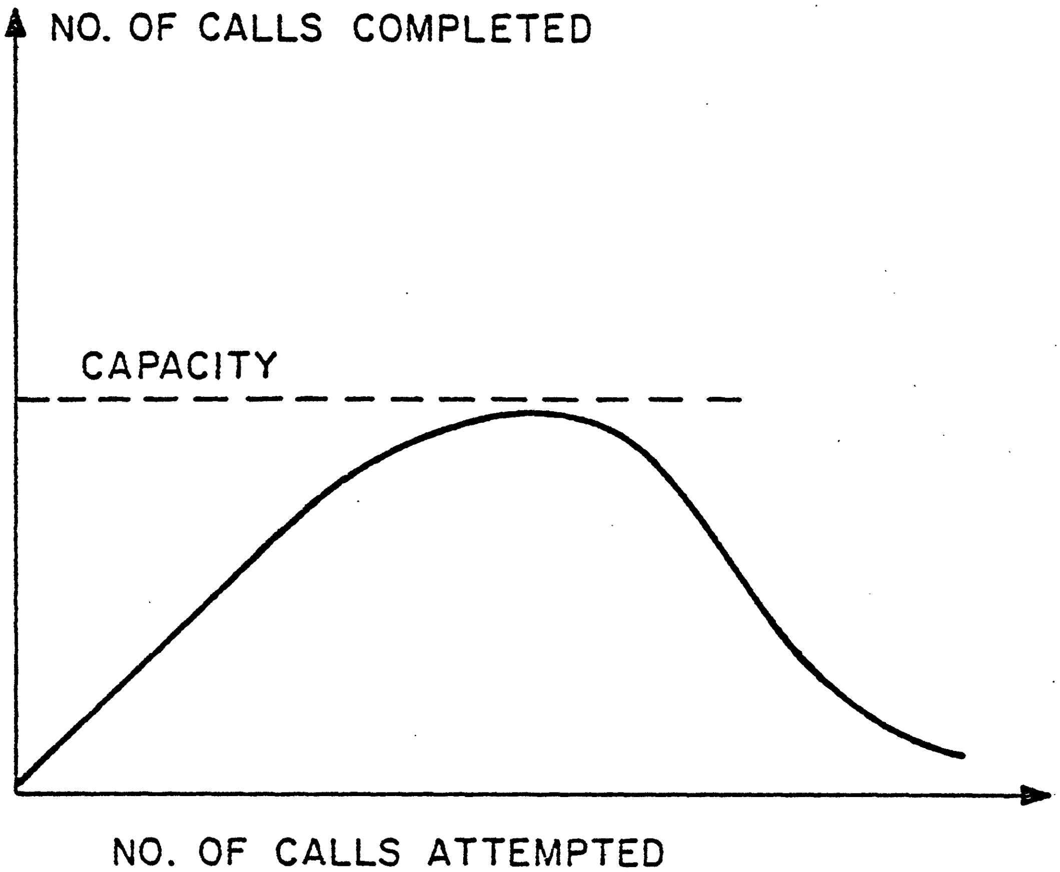

Fig. 2 Typical Performance of the Bell Telephone System under very Heavy Demands

(2) they are characterized by ill understood dynamic interrelations

(3) they are geographically distributed

(4) the controllers (or decision points) are many and also geographically distributed.

State of the Art

This class of large scale system problems certainly cannot be handled by classical servomechanism techniques. Current designs are almost completely ad hoc in nature, backed by extensive simulations, and almost universally studied in static, or at best quasi-static, modes. This is why they get in hot water when severe demands or failures occur.

We do not have a large scale system theory. We desperately need to develop good theories. The theories that we must develop must, however, capture the relevant physical and technological issues. These include not only the traditional performance improvement measures but in addition the key issues of

(a) communication system requirements and costs and

(b) a new word - "distributed computation".

Digression

Before I discuss the few theoretical developments in large scale system theory, please permit me to digress for a few minutes and present a historical perspective which, I feel, is relevant to the point that I would like to make.

The emergency in the late fifties and early sixties of what is now called modern control theory was strongly influenced by two factors.

(1) The missile and aerospace age. This class of problems presented the control engineer with the need to control highly nonlinear

systems with several inputs. This required the development of new design methodologies, because classical servomechanism methods were not suitable.

(2) The digital computer. It is fair to say that without the digital computer modern control theory would be only of academic interest. The existence of the digital computer was essential for the development of computer aided control system design.

For example, the translation of the Wiener-Hopf theory into the time domain (in other words, the Kalman Bucy filter) was crucial. Digital computers "love" to solve differential equations, such as the Riccati equation, rather than integral equations, such as the Wiener-Hopf equation.

In addition, the off-line solution of the nonlinear two-point boundary value problems arising in the necessary conditions provided by Pontryagin's maximum principle, would have been impossible without the digital computer.

Turning to large scale systems, it is my strong belief that we are facing a similar situation today, a critical technological turning point. The magic word is microprocessors. We are in the beginning of a microprocessor revolution. These cheap and reliable devices offer us the capability of low cost distribution computation. It is obvious that relevant advances in the theory and design methodologies must take into account the current and projected characteristics of microprocessors, distributed computation, and decentralized control.

New Concepts, New Thinking

The development of a theory for decentralized control, with special attention to the issues of distributed computation via microprocessors, has to have the elements of a relatively drastic departure in our way of thinking. Figure 3 illustrates the notion of centralized control inherent in both classical servomechanism theory and traditional optimal control and estimation theory. The philosophical commonality is that a single controller has access to all sensor measurements and generates all control commands. In modern stochastic control theory this problem is treated by optimizing the expected value of a scalar index of performance.

Centralized control represents a special case of what is called <u>nested information</u> structure or a <u>classical information pattern</u>. What this means is that in the mathematical formulation of the problem one implicitly assumes that the central controller has access to all past measurements and controls and furthermore has instant recall.

The implications of this classical information pattern are many. Conceptually there is no difficulty in understanding precisely what is the meaning of the <u>principle of optimality</u>. This also leads to a well formulated stochastic dynamic programming algorithm. A very fine, <u>but crucial</u>, technical point is that the nested information structure allows us to evaluate certain double conditional expectations in the stochastic dynamic programming algorithm. The situation changes drastically when we attempt to deal with decentralized control. Figure 4 shows the type of structure that we must learn to deal with. Once more we have a complex dynamic system which is being controlled by several distinct controllers. These controllers may consist of a single or many microprocessors, so that they provide means for distributed computation.

As shown in Figure 4, we now have several controllers or decision

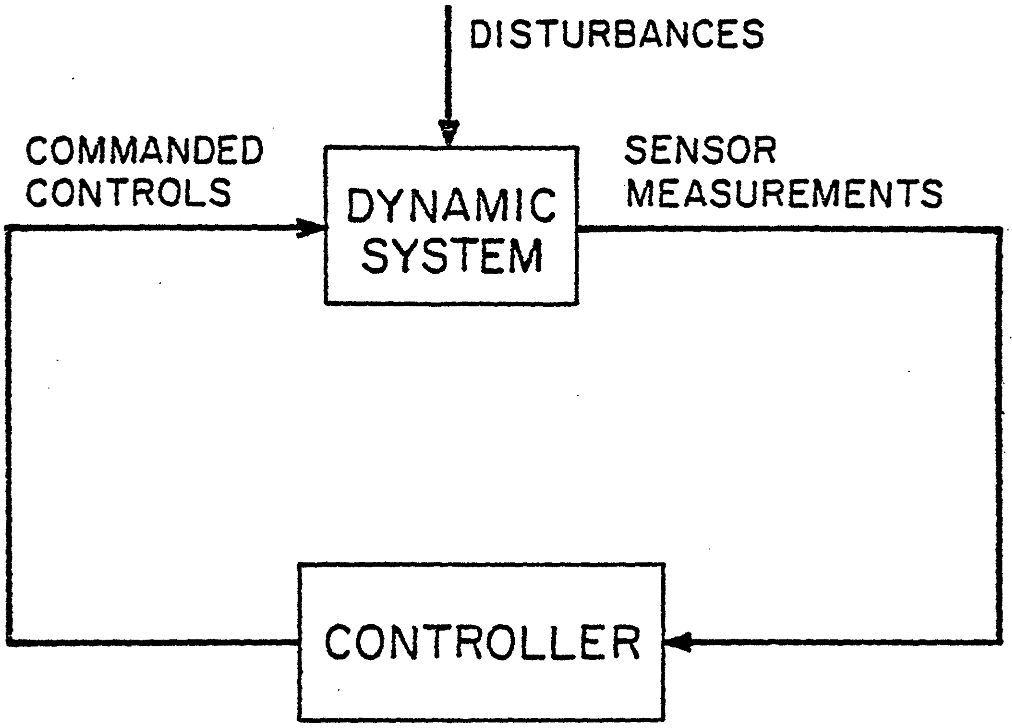

Fig. 3 Structure of Centralized Stochastic Control System

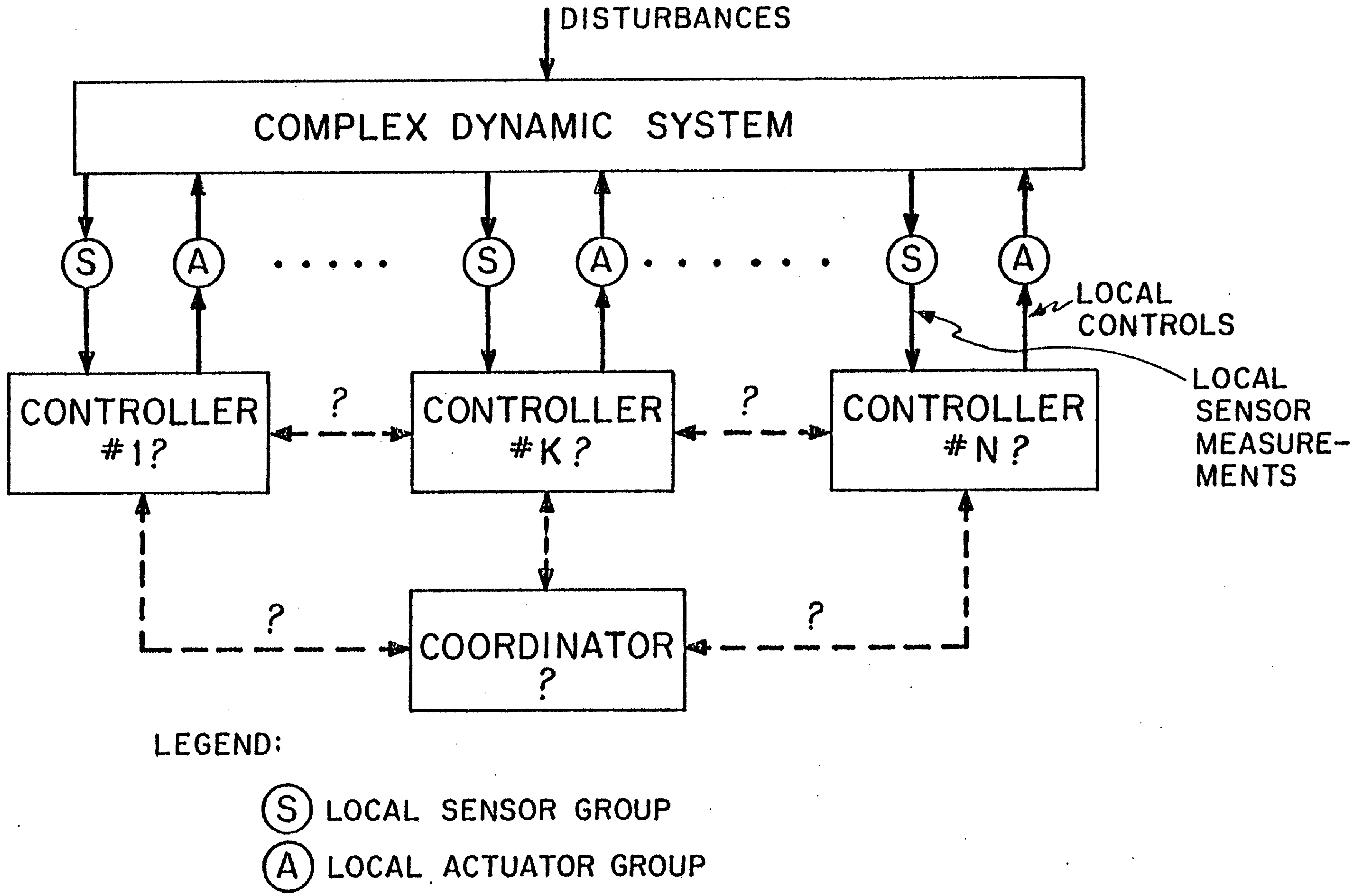

Fig. 4 Structure of Decentralized System

makers. Each controller only receives a subset of the total sensor measurements. Each controller only generates a subset of the decisions or commanded controls.

The key assumption is that each controller does not have instantaneous access to the other measurements and decisions. To visualize the underlying issues involved, imagine that the "complex dynamic system" of Figure 4 is an urban traffic grid of one-way streets. Each local controller is the signal light at the intersection. The timing and duration of the green, red, and yellow for each traffic signal is controlled by the queue lengths in the two local one-way links as measured by magnetic loop detectors. In this traffic situation some sort of signal coordination may be necessary. In the general representation of decentralized control, shown in Figure 4, the dotted lines represent the communication/computer interfaces. All boxes and lines with question marks represent design variables. To systematically design the underlying decentralized system with all the communication and microprocessor interfaces, is the goal of a future large scale system theory.

The conceptual, theoretical, and algorithmic barriers that we must overcome are enormous. There are many reasonable starting points that lead to pitfalls and nonsense. Some of these were described in two recent survey papers [25] and [26]. Such decentralized control problems are characterized by so-called non-classical information patterns or non-nested information structure. This means that each local controller does not have instantaneous access to other measurements and decisions.

Such situations can lead to complicated results. The classic paper of Witsenhausen [30] that demonstrated, via a counterexample, that a very simple linear-quadratic-gaussian problem has a nonlinear optimal solution was an early indication of the difficulties inherent in decentralized control. Since that time some advances have been made in such fields as

(1) dynamic team theory

(2) dynamic stochastic games, and

(3) finite memory stochastic control

which, nonetheless, have only scratched the surface. We have not seen as yet spectacular theoretical breakthroughs in decentralized control. We are at a normative stage where old ideas such as feedback are reexamined (believe it or not!!) and new conceptual approaches are being investigated.

My feeling is that concurrently with the theory we have to obtain a much better understanding of the key issues associated with different physical large scale systems. Then, and only then, will we be able to obtain a deep understanding of the true generic issues associated with large scale systems, as distinct from the physical, technological and even sociopolitical peculiarities of each system.

We must answer the question of "how important is a bit of information for good control". We may have to translate or modify certain results in information theory (such as rate distortion theory) to accomplish our goals. Perhaps the deep study of data communication networks will provide a natural setting for basic understanding, since the commodity to be controlled is information and the transmission of information for control routing strategies, or protocol as it is often called, share the same resources, have the same dynamics, and are subject to the same disturbances.

6. <u>FUTURE RESEARCH AND FUNDING</u>

I have outlined above what, at least in my mind, appear to be some of the exciting areas of research in modern system and control theory. I have tried to stress the importance of relevant theory and the crucial nature of rapid interplay between theory and a variety of applications.

How successful we are going to be in the future depends on many factors. Control is a truly interdisciplinary area, and it must remain interdisciplinary to retain its vitality and vigor. Nonetheless, this creates problems because it cannot be easily placed in a specific box in an organizational chart of a funding agency. Also, program managers in funding agencies or group leaders in industry, who do not have any appreciation of the importance of system and control theory, tend to be extremely skeptical about general methodologies.

I have heard a story at an agency that will remain anonymous, which will make my point. A new division director who did not know anything about modern system theory asked one of the program managers to explain some interesting applications of control. The program manager cited a study that dealt with optimal ways of growing lobsters [31]. The division manager indicated that perhaps the Fish and Wildlife Division of the Department of Interior should fund all theoretical developments in control, since they are the direct beneficiaries.

This true story reflects two things. At the present state of development it would be disastrous to have basic research funded by purely mission oriented agencies. Nonetheless, I feel that the users of the methodology <u>must</u> somehow foot in a direct way the basic research costs in addition to the applications. However, we are facing a crisis in research funding and this will have a stronger impact on systems science and control engineering than many other fields. In the industrial sector I see that traditional centers of control research excellence, such as Bell Labs, IBM Research, G.E., Westinghouse, to mention just a few, are not doing much basic research anymore. The pressure then for generating relevant advanced research falls upon the universities, most of which are not necessarily well equipped to do a good job.

Richard Bellman predicted at the 1973 Ohio State JACC that control scientists will replace the physicists as the people that will run the technology of our country by the year 2000. I hope so and I agree, by the way, with Bellman's forecast. If this is the case, there should not be much of a funding problem in the year 2000. The crucial problem is where the funding is going to come in the next decade. The development of relevant theory for the complex problems that I have mentioned will call for tighter partnerships between universities, industries, and government laboratories. I feel that government has been doing its share. Industry, and in particular, process control industry, has not.

It is important for industry to realize the difficulties that research-oriented universities, such as MIT, are facing in tackling problems of direct industrial relevance. If a student walked into my office today , and many have done that, and wanted to do a thesis on computer control of a particular manufacturing process then I would have to decline because no such research funds are available, rather than because of lack of research interest. If he needs financial support, to support a graduate student at MIT for 12 months via a research assistantship costs (counting all the overheads) $16,360.00 per year. This does not include any type of faculty supervision time, equipment costs, computer costs, secretarial charges, etc. If one counted all these costs, \$20,000.00 per year per paid student is what appears to be a reasonable figure. Thus, the cost of research is very high and it is not unreasonable to conclude that because of the skyrocketing student support situation the universities have to look for research associations that are not only long range in nature, but well financed as well.

In conclusion, I feel that modern system and control theory is an extremely active and dynamic area. The next decade should provide valuable interplay between well established design methodologies and diverse applications. In addition, I predict that we are going to witness new theories for exciting problems.

With respect to applications to chemical process control, let me say the following. In 1962 Professor Ho of Harvard stated, with respect to applications in general,

> "Ask not what modern control can do for you, but
> what you can do for modern control".

In 1976 I say:

> It is good! You should!

REFERENCES

[1] M. Athans (ed.) ,"Special Issue on Linear-Quadratic-Gaussian Problem", IEEE Trans. on Automatic Control ,Vol. AC-16, No. 6, December 1971.

[2] I.M. Horowitz ,"A Synthesis Theory for Linear Time-Varying Feedback Systems with Plant Uncertainty", IEEE Trans. on Automatic Control, Vol. AC-20, No. 4, August 1975, pp. 454-464.

[3] B.D.O. Anderson and J.B. Moore, Linear Optimal Control, Englewood Cliffs, N.J., Prentice Hall, 1971.

[4] P.K. Wong, "On the Interaction Structure of Linear Multi-Input Feedback Control Systems", MIT Electronic Systems Laboratory, Report ESL-R-625, Cambridge, Massachusetts, September 1975.

[5] P.K. Wong and M. Athans, "Closed-Loop Structural Stability for Linear-Quadratic Optimal Systems", MIT Electronic Systems Laboratory, Report ESL-P-641, Cambridge, Mass., December 1975 (submitted to 1976 IEEE Conference on Decision and Control).

[6] M. Safonov, Ph.D. Dissertation, M.I.T. (in progress).

[7] W.M. Wonham, Linear Multivariable Control: A Geometric Approach, Springer-Verlag, Berlin, 1974.

[8] M. Athans and P.P. Varaiya, "A Survey of Adaptive Stochastic Control Methods", Proc. Engineering Foundation Conference on Systems Structuring, Henniker ,N.H., August 1975, available as ERDA Report CONF-750867.

[9] I.D. Landau, "A Survey of Model Reference Adaptive Techniques", Automatica, Vol. 10, 1974, pp. 353-380.

[10] B. Wittenmark, "Stochastic Adaptive Control Methods: A Survey", Int. J. of Control, Vol. 21, No. 5, 1975, pp. 705-730.

[11] T. Kailath (ed.), "Special Issue on System Identification and Time Series Analysis", IEEE Trans. on Automatic Control, Vol. AC-19, No. 6, December 1974.

[12] K.J. Astrom and P. Eykhoff, "System Identification: A Survey", Automatica,Vol. 7, 1971 ,pp. 123-162.

[13] I. Gustavson, "Survey of Application of Identification in Chemical and Physical Processes", Automatica, Vol. 11, January 1975 ,pp. 3-24.

[14] K.J. Astrom and B. Wittenmark ,"On Self Tuning Regulators", Automatica, Vol. 9, 1973, pp. 185-199.

[15] M. Athans et al., "The Stochastic Control of the F8-C Aircraft Using the Multiple Model Adaptive Control (MMAC) Method", Proc. 1975 IEEE Conference on Decision and Control , Houston, Texas, December 1975 ,pp. 217-228.

[16] J.M. Martin-Sanchez, "Implementation of an Adaptive Autopilot Scheme for the F-8 Aircraft" ,submitted to IEEE Trans. on Automatic Control.

[17] H. Kaufman and G. Alag, "An Implementable Digital Adaptive Flight Controller Designed Using Stabilized Single Stage Algorithms," Proc. 1975 IEEE Conference on Decision and Control, Houston, Texas, December 1975, pp. 231-236.

[18] G. Stein et al., "Adaptive Control Laws for the F-8 Flight Test", Proc. 1975 IEEE Conference on Decision and Control, Houston, Texas, 1975, p. 230.

[19] R.C. Montgomery and H.J. Dunn, "Adaptive Control Using a Moving Window Newton-Raphson Parameter Identification Scheme", Proc. 1975 IEEE Conference on Decision and Control, Houston, Texas, December 1975, p. 229.

[20] A.S. Willsky, "A Survey of Design Methods for Failure Detection in Dynamic Systems", M.I.T. Electronic Systems Laboratory, Report ESL-P-633, Cambridge, Mass., November 1975 (submitted to Automatica).

[21] D.E. Gustafson et al., "Final Report: Cardiac Arrhythmia Detection and Classification through Signal Analysis", The Charles Stark Draper Laboratory, Cambridge, Mass., Report No. R-920, July 1975.

[22] H.J. Payne, "Freeway Incident Detection Based Upon Pattern Classification", Proc. 1975 IEEE Conference on Decision and Control, Houston, Texas, December 1975, pp. 688-692.

[23] R. Boel and P.P. Varaiya, "Optimal Control of Jump Processes", University of California, Electronics Research Lab Report M-533, Berkeley , California, November 1974.

[24] P.P. Varaiya, "Trends in the Theory of Decision Making in Large Systems" , Annals of Economic and Social Measurement, Vol. 1, No. 4, October 1972, pp. 493-500.

[25] M. Athans, "Survey of Decentralized Control Methods", Annals of Economic and Social Measurement, Vol. 4, No. 2, 1975, pp. 345-355.

[26] N.R. Sandell ,P.P. Varaiya, and M. Athans", A Survey of Decentralized Control Methods for Large Scale Systems", Proc. Engineering Foundation Conference on Systems Structuring, Henniker, N.H., August 1975, in ERDA report CONF-750867.

[27] L.H. Fink and K. Carlsen (eds.), "Systems Engineering for Power: Status and Prospects", ERDA Report CONF-750867, 1975.

[28] J.J. Fearnsides, "Trends in Transportation (editorial)", IEEE Trans. on Automatic Control, Vol. AC-20, No. 6, December 1975, pp. 725-726.

[29] H. Frank, R.E. Kahn, and L. Kleinrock, "Computer Communication Network Design-Experience with Theory and Practice", Networks, Vol. 2, No. 2, 1972.

[30] H.S. Witsenhausen,"A Counterexample in Stochastic Optimal Control", SIAM J. on Control, Vol. 9, 1971.

[31] L.W. Botsford, et al., "Optimal Temperature Control of a Lobster Plant", IEEE Trans. on Automatic Control, Vol. AC-19, No. 5, October 1974, pp. 541-543.