Role of Feedback Control in Fusion Energy Research

Amiya K. Sen
Columbia University

A White Paper for 1999 Fusion Summer Study, Snowmass, Colorado

I. INTRODUCTION

This white paper is intended to pose some serious questions about the role of feedback control in fusion energy research. The primary motivation for this is the inadequate level of limited scope of research in this area. This is unfortunate, because feedback control clearly has great untapped potential for significant impact on the performance, viability, size and cost of eventual fusion machines.

At the outset it is worth pointing out that modern control science is an extremely sophisticated discipline which has played crucial roles in many frontiers of technology. To mention only a few: very complex flight and guidance control of rockets, space probes, and satellites, highly precise spacecraft attitude and pointing control system (for example skylab, large space telescope, etc.), large industrial process control, control of very large interconnected electric power systems, optimal tracking of non-synchronous satellites and missiles, optimal stochastic estimation and prediction of trajectories and parameters of MIRV, adaptive optimal transmission and reception in modern communication systems. As a matter of fact, it will be difficult to think of complex technological system which operate without any control system whatsoever. Therefore, one is easily led to the puzzle: for an intrinsically unstable system such as confined plasma, why is there so little emphasis on the control aspect.

II. PAST AND PRESENT RESEARCH

It may be recalled that in the period 1969-71 there was a flood of transient fashion in feedback and dynamic control of plasma that resulted in a conference devoted to the subject. The reason it quickly petered out, can be state only by the following somewhat blunt but honest estimate: The control tools and techniques used in all these work were very rudimentary and consequently produced very limited and elementary results. The process simply stopped very early in the game, because plasma physicist had little or no formal training or interest in the advanced control science which was vital for further progress. One of the fundamental reasons for the very limited progress of feedback control of plasma instabilities is the fact that only one mode at a time could be stabilized (1). It should be noted that this basic deficiency has been substantially (but not completely) overcome in some recent research work. Therefore, I believe it was a poor judgement of the plasma physics community to largely abandon feedback control work after a short superficial abortive attempt.

Recently, there has been some resurgence of interest in feedback control in tokamaks. There are some very good physics ideas, but only a very few but excellent experiments that are focussed exclusively on magnetic feedback of external kinks/wall modes in the takamaks. However, the sophisticated system aspects that are essential for optimal results are not addressed. Paradoxically, control expertise of the highest order exists outside the plasma physics community, but it is of little value to the fusion program, because of the absence of plasma physics background in these experts. Furthermore, experimental research in non-magnetic suppressor like modulated NBI, ICRF and ECH, which may be much more suitable for internal kinks/ballooning modes are lacking.

This white paper will not be complete without mentioning the following basic point. Control science is almost exclusively developed for lumped parameter systems and therefore its formal tools and techniques are not directly applicable to a distributed parameter system like plasma. However, some concepts and tools of control science have been generalized (albeit with great difficult and complexity) to distributed parameter systems (2). There are extensive and intense international efforts in this area directed to a variety of distributed parameter problems: thermal and chemical process control, control of large elastic structure, control problems in biological systems, even a little bit on nuclear reactor (control of neutron flux distribution and xenon oscillation in large core reactors). Why should fusion programs remain largely an exception?

III. SUGGESTED DIRECTION OF FUTURE RESEARCH

The purpose of this white paper is to urge the funding agencies and the research community to examine critically the wisdom of enhanced investment in this area of research. Specifically, some of the following lines of research may be considered.

A. SYSTEM THEORETIC INVESTIGATIONS

To what extent modern control science can be extended and adapted to a distributed system like plasma. More precisely what are the prospects and benefits of extension of the following control concepts to plasmas.

(i) Optimal control
(ii) Adaptive control
(ii) Stochastic state estimation in conjunction with (i) and (ii) above

B. BASIC PROBLEMS IN PLASMA CONTROL

(i) Observability and controllability of plasma instabilities.
(ii) Complete multimode feedback stabilization in the presence of noise.

C. APPLIED RESEARCH IN FUSION ENERGY

(i) <u>Magnetic feedback for external kinks and wall modes.</u>
A very good start has been made in this area. However, significant enhancement of funding of the present effort is highly desirable.

(ii) <u>Other non-magnetic suppressors.</u>

Other non-magnetic suppressors should be conceived, developed and tried. These include modulated NBI, ICRF, ECH, etc, which may be much more suitable for internal kinks/ballooning modes.

(iii) Suppression of Turbulence and Reduction of Transport

It is considered almost axiomatic that one cannot feedback control broad band microinstabilities and turbulence, which are responsible for anomalous transport in fusion machines. However, this may be a half truth and a half prejudice. It is possible that the turbulence may be describable by a low order dynamic model whose feedback control may translate into a significant reduction in transport. As the high level of anomalous transport is a major roadblock to fusion, we need to make a serious effort in this direction in a 'no-stones-unturned' spirit.

IV. CONCLUSIONS

In conclusion, unlike all other high technology enterprises, the fusion energy research program has been remiss in not relying on the extensive use of modern control service. The prospects of the achievement of fusion may be critically enhanced by thorough, thoughtful and novel exploitation of the ubiquitous control service and technology. However, this requires in depth learning of control science by the plasma physicists and a strong collaboration with first rate control physicists and a strong collaboration with first rate control experts who are willing to research with the plasma physics problems. Only a true marriage between the two disparate sciences can significantly accelerate the progress towards fusion via enhanced MHD stability and lower anomalous transport.

References

(1) T.K. Chu and H.W. Hendel ed., Feedback and Dynamic Control of Plasma. AIP, (1970).

(2) By the eminent mathematicians and scientists like Zubov, Lyons, Butkovsky, Lerne, Russell, Bellman, Wang, and others.