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FUSION RESEARCH AND PLASMA PHYSICS: A STORY OF PARADIGMS

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I was pleased, and honored, and, I confess, somewhat unnerved by the prospect of presenting the keynote talk on this occasion. Pleased and honored because I have such warm feelings toward our honoree; unnerved by the task of doing justice to the topics I would like to discuss.

A paradigm--Webster defines the word as meaning "an outstandingly clear example." And so: "A Story of Paradigms"--because I believe fusion research and modern plasma physics together contain a remarkable spectrum of paradigms, one of them being our honoree himself.

So, as the title of my talk suggests, I intend to talk about more than one topic, and about more than one outstanding example, but all are interrelated by a common denominator.

The common denominator is, of course, the goal of fusion power, a goal no less desirable today than when it was first being seriously discussed--at least four decades ago.

One thing I share with Marshall is that both of us have been involved in fusion research for more than three of those four decades. Looking back, I am not sure that any one of us that was involved in the research at that time realized at the outset just how formidable a problem the achievement of fusion power represented--and still represents. You can derive a broad hint as to the collective naivete at the time from a quote taken from the conclusion of an article on fusion research published in 1956.[1] The quote: "It is the firm belief of many of the physicists actively engaged in controlled fusion research in this country that all of the technological problems of controlled fusion will be mastered, perhaps in the next few years." So much for prophecy!

In my defense (since I was the one that wrote that article), I did in a later paragraph in the same article introduce a caveat and a more plausible prediction. First the caveat: "That an early success in achieving a self-sustaining controlled fusion reaction would lead to economically competitive power in the near future is highly unlikely." And the prediction: "...in the fusion reaction are implicit new dimensions--those of power obtained, possibly by direct electrical conversion, from an inexpensive, safe, and virtually inexhaustible fuel. These possibilities will surely someday play a dominant role in shaping the world of the future." End quote.

Since those early days the motivations for pursuing fusion research to a practical result have grown even stronger than they were then. Perceived problems (then) of the progressive depletion of fossil fuels, of air pollution from their use, and of potential hazards from fission power plants, have become real problems today, and fusion is increasingly recognized as one of the best, perhaps the only viable, long-range response to these concerns.

My first paradigm, then, within which all later ones will be contained, is fusion research itself. By all measures it is an outstanding example of a long-range applied science effort aimed at achieving a specific and vitally important objective for the benefit of all of mankind. If I try to think of another comparably outstanding example I can think of only one: research toward the elimination of cancer.

This brings me to the second paradigm I will discuss--namely the emergence and the growth of the research field of high temperature plasma physics, a research field that flourished in response to the problems posed by the fusion goal. It is a true paradigm in that it represents a classic example of the development of a whole new field of scientific endeavor stimulated by a perceived important human need.

It has become increasingly apparent to us all, that as the field of fusion power research has broadened and deepened during the past three decades, the central issue has always been and still remains: to understand the plasma state of matter. All roads lead to Rome, and all roads to fusion power must have their surfaces laid on a roadbed (and it is no bed of roses!) of plasma physics. And as long as there remain stretches of the roadbed not firmly in place, passage to the goal is still not assured.

It is in this area of laying the plasma physics roadbed where we can from the onset clearly discern the dominant role played by our honoree, and I will later on refer specifically to some of his contributions to the field. But first I would like to clarify what I mean by "the modern field of plasma physics." I am referring to the study of the plasma state of matter under conditions where there exist strong mutual interactions between the plasma and the electromagnetic fields--of external and/or internal origin--in which it is immersed. In the fusion context it is also typically implied that inter-particle collisions and atomic phenomena enter only as weak perturbations, rather than as dominant processes. This circumstance is in marked contrast to the way these processes enter into most of the older disciplines of plasma physics.

In the context of the research toward fusion power our imperative is therefore that we must achieve a thorough understanding of the physics of plasmas in the so-called collisionless regime. A historian might therefore examine the growth of our knowledge of plasma physics over the past three or four decades and tag those periods when landmark advances were made. For my purposes I prefer to take another approach. Namely, since it is the <u>properties</u> of an unfamiliar form of matter that must be understood, I will begin by asking: What properties must we understand in order to utilize this form of matter, in particular, for fusion? Armed with such a list we could then begin to measure our ability to predict and to control the behavior of this state of matter. Having that ability is obviously an essential step on the way to the achievement of fusion power.

My hierarchy of properties that need to be understood goes as follows:

- At the first level, understanding the intricacies of the motions of charged particles in strong magnetic and electric fields.
- Next, having in hand the formalism by which to predict the consequences of collisional interactions between an ensemble of charged particles when they are immersed in strong magnetic and/or electric fields--the Fokker-Planck problem.
- Next, understanding the nature of the pressure equilibrium state between a plasma and a magnetic field, including the stability of this state--the MHD problem in its most basic form.
- Next, understanding the stimulation and the propagation of waves in plasmas, and the conditions required for the onset--and for the control--of unstable modes of these waves.
- Finally, getting it all together by developing theoretical techniques for predicting, and effective means for controlling, the transport of particles and of energy within plasmas under the spectrum of conditions expected to be encountered in fusion power systems.

It should come as no surprise to you that Marshall has made major contributions to the theory undergirding every single one of the items in the properties check list that I have given. I do not have the intention, nor would there be time in a talk ten times as long as this one, to discuss all of these contributions, but along the way I will single out a few of them for special mention. In fact, most of those examples will be taken from mirror research. I hope you will understand that I have chosen them because they are the ones that are most familiar to me. Another speaker might well have chosen a different set to illustrate these same points.

In fact, now is a good time to recall some early work by Marshall that has been as a two-edged sword in my own--I'll be generous and say "persistent" instead of "dogged"--pursuit of the mirror idea. This work was Marshall's contribution to converting the Fokker-Planck equation into forms that made it eminently useful for magnetic fusion research.[2] This contribution comes under the rubric of the second item in my hierarchy of plasma knowledge--namely the unraveling of the role of collisions in the velocity space of fusion plasmas. Out of this early work came the "Rosenbluth potentials" and their subsequent employment in the development of increasingly sophisticated computer codes. These codes have played, and continue to play, a crucially important role in both the theoretical analysis of mirror confinement and stability and in the design of and the interpretation of experimental data from mirror systems. As you all know, these same computer codes are being increasingly applied in the analysis of other fusion systems, including the tokamak.

A moment ago I said "a two-edged sword" in connection with the applications of the Fokker-Planck equation to mirror systems. I said that because the Fokker-Planck equation provides a mercilously rigorous standard by which to judge the fusion power balance potential of proposed mirror fusion systems. The existence of this standard, with its predictions of marginal Q values for simple mirror systems, led--or probably I should say "forced"--me to invent a direct conversion system for mirrors, and later led Dimov, and Fowler and Logan to the invention of the tandem mirror.

Reduction of the Fokker-Planck equation to tractability is only one of many instances in which Marshall's work has contributed in a fundamental way to magnetic mirror research. In fact, my third paradigm will be drawn from one of those contributions. But before I discuss that example I will mention two earlier works that have a special place, not only in mirror physics, but also in the wider field of fusion plasma research.

The first of these contributions comes under the third category of my hierarchy of plasma understanding, namely the basic problem of MHD equilibrium and stability in magnetically confined plasmas. Today it is difficult to remember that at the time this work was performed--1957--the ideas concerning MHD stability were in a very primitive state, and in the mirror approach experimental results were either incomplete or actually misleading on that issue. It was at this point that Rosenbluth and Longmire[3] published a landmark paper, setting forth in the clearest of terms an analysis--based on orbit theory rather than on fluid equations--of the MHD stability of mirror machines in the axially symmetric forms then in use. Though a dismay at the time to mirror researchers, that paper was a harbinger of the solution to the mirror MHD stability problem, and I am sure it also provided a stimulus to later theoretical work dealing with MHD stability in other systems.

Now for the second notable contribution: Those of us in the business (at that time) of exploring the stability of axially symmetric mirror systems were given a partial reprieve from the concern raised by the Rosenbluth-Longmire paper by another paper five years later. This 1962 paper was the major work by Rosenbluth, Krall, and Rostoker on "Finite-Orbit Stabilization."[4] In this paper Marshall and his co-workers employed the now familiar "method of characteristics" to solve an otherwise very difficult theoretical problem. The paper not only helped explain earlier experimental results on mirrors, but its concepts and methods of analysis fed into an ever-widening circle of later important analytical works where the use of the particle kinetic equation approach to stability theory would be essential.

Marshall's intimate involvement at the time in helping mirror researchers to sort out some very puzzling data can be deduced from the fact that he was a co-author with Ellis, Ford, and Post in a publication[5] based on results from our mirror experiment at Livermore named "Table Top." In this experiment (which we reported in 1960; that is two years before the Rosenbluth-Krall-Rostoker "Finite-Orbit Stabilization" paper), plasma injection and magnetic compression were used in an axially symmetric mirror field to produce a spindle-shaped column of dense and apparently stable plasma composed of 20 kilovolt temperature electrons and much colder ions. The puzzles posed by the Table Top data were twofold: First, in view of Rosenbluth-Longmire, why was the column grossly stable at all? And second, in the light of the then-prevalent appearance of Bohm diffusion in stellarators, why did this plasma have a transverse diffusion rate that was five orders of magnitude slower than the Bohm rate?

As to the first question, Marshall's contribution was to introduce a preview of the idea of finite orbit stabilization. Taken together with the so-called "line tying" stabilization effect idea we had at least a partial answer to the first puzzle.

As to the second puzzle, we had to wait for many years in order to begin to explain that one, in this case within a much broader set of plasma issues.

In fact, those same issues are the ones that are involved in what I will submit as my next paradigm. The issues involved here are the ones contained in the fourth item of the hierarchy of plasma physics issues that I alluded to earlier--namely microinstabilities. Early work by Harris[6] and others in this country, and by Sagdeev, Vedenov,[7] and others in the Soviet Union gave clear warning that departures from velocity-space isotropy, or distortions away from a Maxwellian speed distribution could give rise to unstable growing waves--microinstabilities--in collisionless plasmas. One of these early works, describing an instability of particular interest recently to mirror researchers, was presented by Marshall in one of his lectures at the 1960 Plasma School at Risc in Denmark.[8] It concerned what is now known as the "Alfven Ion Cyclotron Instability," an Alfven-wave mode driven unstable by velocity-space anisotropy of the ions.

Now for the solution of the microinstability problem in mirror systems. This one took over 10 years to unfold. I have highlighted it here because it not only is it an example with which I am very familiar, but also for the reason that it personifies a particular approach to the solution of a major problem in plasma physics. It is also the one which I remember with the greatest pleasure, since in its early history it involved a close and fruitful collaborative effort with Marshall in developing a theoretical attack on the problem.

By 1964, having been shown the way by the now-classic "Ioffe Experiment" (which was a paradigm in its own right), at Livermore we had succeeded in suppressing all MHD-like plasma activity in our experiments by the use of mirror fields of the magnetic well type. It was therefore becoming increasingly apparent from the experimental data that the new circumstance limiting particle confinement was the presence of high frequency microinstabilities. These one could blame, in a qualitative way, on the known "loss-cone" nature of the ion and/or electron distribution functions of mirror-confined plasmas, with their velocity-space anisotropy and their non-Maxwellian speed distributions. It seemed that the time was ripe for making a directed theoretical attack on the problem, addressing the problem in terms that were specific to the mirror machine, rather than in general terms. The hope was that one would thereby learn how to suppress the microinstabilities, once having deduced the conditions required for their stimulation. As it turned out that was not merely a pious hope. It was therefore in 1964 that I appeared on Marshall's doorstep at General Atomic in San Diego. I came equipped with some experimental results and some fuzzy ideas as to how the problem might be formulated theoretically. In what seemed to be (and may have actually been) overnight, Marshall came back with an elegant analysis that yielded criteria for the stabilization of what has come to be called the "High Frequency Convective Loss-Cone" (HFCLC) mode.[9]

Out of the further evolving of the collaboration, including some time together at Culham, came the work on the "Drift Cyclotron Loss Cone" (DCLC) mode,[10] in which Marshall contributed the analytical work and I contributed the generation of needed computer codes. This collaboration was so stimulating to me that I was able to come up with an analysis of the idea of "warm plasma stabilization"[11] of the two modes for a meeting at Gatlinburg about a year later.

Though we were by no means the only ones that were working on the theory of microinstabilities at the time, I feel that Marshall's clear formulation and analysis of the problem, in the specific context of mirror physics, played a decisive role in what would eventually become a major success story in mirror research.

Following those two early papers there elapsed 10 years before the happy ending. During that interim period yet more realistic theoretical models were analyzed and increasingly sophisticated experiments were performed in an attempt to verify the theoretical predictions. Though there were successes, for example in making contact with the HFCLC mode and its suppression through experimental checks and through theoretical work by Baldwin and Callen,[12] there remained a major mystery. The mystery was that under plasma conditions where theory predicted that the DCLC mode should be strongly driven, activity at the cyclotron frequency was weak and/or end losses were not noticeably enhanced. Work on Ioffe's mirror machine in Moscow[13] seemed to demonstrate the reality of the mode, and indicated the positive effects of warm plasma stabilization, but left unexplained the behavior of the 2XII experiment at Livermore. In 2XII a dense plasma at about 1 kilovolt ion temperature was created that seemed to decay at about classical collisional rates (that is, in this case within about a half millisecond) without evidencing the presence of the DCLC mode.

But any complacency was shattered, and the "mystery" was exploded when 2XII, rebuilt as 2XIIB, was brought into operation with its powerful array of neutral beams. Now, as the plasma density and temperature built up there appeared a virulent instability that destroyed the confinement and that had all the hallmarks of the DCLC mode. The happy ending was provided by Fred Coensgen and his co-workers, who overnight tried out warm plasma stabilization on 2XIIB and showed that it really worked.[14] On the theory side it was the paper of Baldwin, Berk, and Pearlstein in 1976[15] who employed quasi-linear theory to explain the puzzle by showing how the DCLC mode drove itself to marginal stability through generating a warm plasma stream by its own activity. This theory thereby explained both the weakness of the mode at lower temperatures and its increasing effects at high temperatures. When 2XIIB was followed by the use of sloshing ions in TMX-Upgrade to further weaken the DCLC mode and to suppress the Alfven Ion Cyclotron mode, the story was essentially complete. A decade and a half of cooperative effort between theory and experiment solved what had once been the problem that was most threatening to the future of mirror systems.

Among the several reasons that I chose the mirror microinstability story as the third paradigm is that I intend to use it as a model, at the conclusion of my talk, to argue for another hoped-for, future, paradigm. But before going into that I would like to address, briefly, two other topics.

The first of these topics, one I cannot hope to do justice to, concerns the wide scope of Marshall Rosenbluth's contributions to the entire field of fusion plasma physics. He was, for example, the author of a landmark paper in 1972 on the role of parametric instabilities in laser-irradiated plasmas.[16] Recently, he has been author or co-author on numerous papers on the theory of high power free-electron lasers. I am sure that stories of Rosenbluthian contributions similar to the one I have given you from the mirror research field could be given to you by colleagues in many other plasma research fields.

From those other fields I recall some especially notable works including the paper on average-minimum-B systems he co-authored in 1964 with Harold Furth,[17] the paper on neoclassical transport in 1972 with Hazeltine and Hinton,[18] and the paper on the tilt instability of the spheromak with Bussac in 1979.

Over the years that I surveyed through a literature search, namely 1960 through 1985, I found that Marshall was author or co-author of some 195 published papers on plasma physics topics (corresponding to an average rate of 7.5 papers per year and a peak rate which reached 14 per year) This does not even count the numberless unpublished reports that he authored or co-authored. He also has served on countless blue ribbon panels or committees, given a myriad talks, and helped who-knows-how-many other researchers with physical insights or helpful hints on analysis. More specifically, we in the mirror business have continued to benefit from his advice and counsel, and from his contributions to tandem mirror confinement and stability theory.

And there is something more that I want to say in all this. From my own experience, and that of others, there emerges the picture of a gentleman in the best sense of the word; also of a physicist who is willing to listen (that is a rare breed in our kind!). These attributes are coupled with an uncanny ability to analyze difficult problems by methods exactly tuned to the problem at hand--being neither more or less complicated than is required to do the job. I guess you could call it "laziness with style"--and what remarkable style!

An additional observation about Marshall that I would make is to emphasize that his dedication to the fusion goal runs much deeper than being an accomplished professional in the scientific side of fusion. He also has consistently been a staunch advocate of fusion research in all of his contacts both inside and outside the scientific community. For example, in his remarks made on the occasion of his receiving the Fermi Award last February, Marshall called on the nations leaders to take the long view with respect to their support for fusion research. He asks all of us the disturbing question: "... Can we be a proud and successful nation twenty years from now if we abandon the struggle?" And then he answers his own question with the upbeat response: "I am not pessimistic. I have a great faith in the wisdom of America's people, and in the workings of the American system of government. In often mysterious and sometimes tortuous ways the right decisions are made, the path to greatness is followed."

A phrase in that last sentence resonates with what I would like to submit to you as my candidate for a future chapter of this "story of paradigms": "Mysterious and sometimes tortuous ways" is not a bad description both of how fusion plasmas behave, and of the steps in the growth of our understanding of that state of matter. Though we have made enormous progress in ascending the steps of the hierarchical ladder than I earlier enumerated, we have not yet reached the top of the ladder. The last step that I listed has not yet been climbed. If you recall, it was: "... developing theoretical techniques for predicting, and effective means for controlling, the transport of particles and of energy within plasmas under the spectrum of conditions expected to be encountered in fusion power systems." What I am suggesting is that the same philosophy should now be adopted in tackling that major remaining area of ignorance in plasma physics that was perforce adopted by the mirror community in solving the microinstability problem for mirrors. There the paradigmatic sequence was: theory to expose the problem, experiment to lend it reality, theory to propose the solution, followed by experiment to confirm the predictions. Could not the same sequence work for the last remaining puzzle? That is, to elucidate those processes that cause anomalous transport (drift waves, for example), to use theory to define geometries, and/or to suggest techniques to suppress or minimize that transport (for example, feedback methods), then to test the theory in the laboratory. The end result device might or might not look like a tokamak, a reversed field pinch, a mirror, or whatever--but it would be a winner, and it would be predictable. What is not predictable is whether such a program would indeed be successful. On the other hand, what is predictable is that the problem it addresses will not solve itself.

I am sure that each of you, and especially our honoree, recognizes that the issue that I am now discussing is by no means a new one, since it underlies almost everything that we do in magnetic fusion research. But just possibly we may have, in the pursuit of specific devices, lost sight of the larger aspects of this last step. I believe there is a real opportunity here to generate a new paradigm in the fusion story.

So at the end of my talk--what is the most striking paradigm in my story of paradigms, the most remarkable example? Isn't it obvious? And I am honored to have participated in this occasion that has been laid on for him.

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