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by

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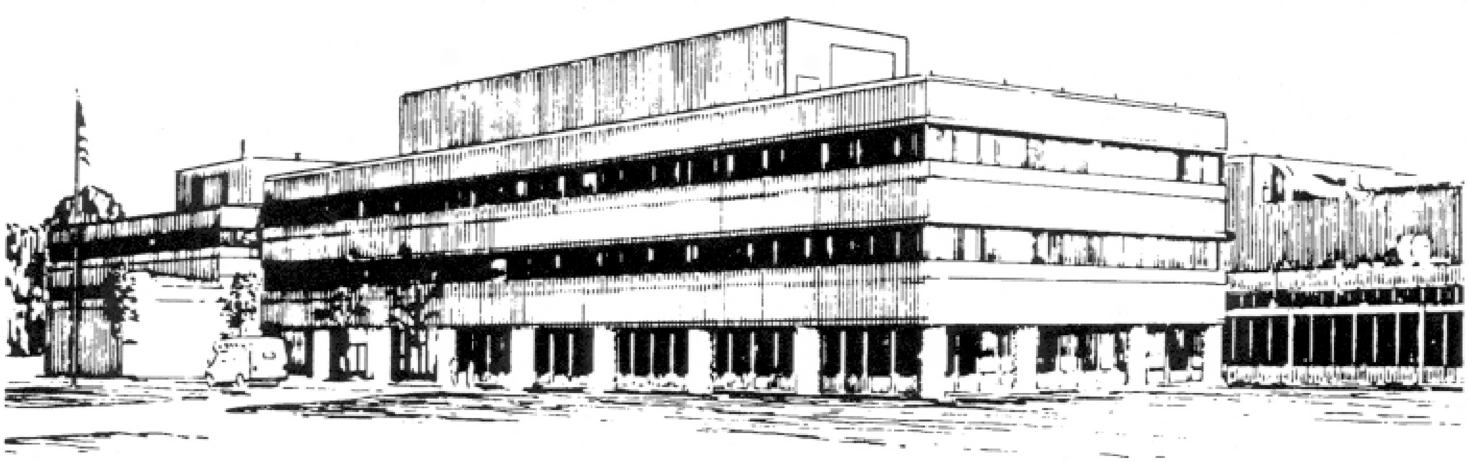

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13th International Stellarator Workshop
The Evolution of Stellarator Theory at Princeton

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Late in March, 1951, an article appeared on the front page of the New York Times saying that the Argentinian dictator, Peron, had announced that Ronald Richter achieved controlled thermonuclear energy production. Lyman Spitzer, chairman of the Princeton University Department of Astrophysics, who was leaving the next day on his annual skiing vacation, was alerted by his father-in-law. Spitzer spent his time in the chair lift thinking about how this could be accomplished. Legend has it that he defined the problem and worked out most of the details for the stellarator program by the time he returned to Princeton. He showed that one would have to contain a plasma with $T \sim 10^8$ °K, $n \sim 10^{15}$ cm^{-3}, and $\tau \sim 1$ second, with the average particle having a mean free path of 3 km because of the necessity of having more fusion power retained in the plasma than that lost to radiation. This led to a magnetic field confinement concept. The desire for steady-state operation made him discard the pinch and tokamak approaches. Spitzer recognized that charge separation due to the ions and electrons drifting in opposite directions because of the $B \times \nabla B$ forces in the U-bends of a torus would produce an $E \times B$ plasma flow across the field that would destroy confinement. He showed that twisting the torus into a figure-8 shaped pretzel would allow current to flow along the field lines to cancel the electric field. He even saw that particles with little velocity along the field lines would stay in the U-bends long enough to get lost and predicted that their loss would create a self-healing radial electric field which would rotate the plasma. In early May a proposal for funding was submitted to the U.S. Atomic Energy Commission. Further analysis over the next few years included studies of plasma breakdown, Ohmic heating, classical diffusion in a fully ionized plasma, some studies of Bohm diffusion and ion acoustic waves, particle orbits in low-density and high-density stellarators, effects of Pfirsch-Schlüter currents along the field lines that cancel the electric field, and the properties of high-temperature, fully-ionized plasmas. All of these calculations were carried out with simple, clearly understandable models, the most restrictive assumption being that the plasma would be quiescent so that classical diffusion could be achieved.

Spitzer proposed a research program consisting of four devices: Model A, a low field device to test the efficacy of having a rotational transform on plasma breakdown; Model B, also with a two inch diameter tube, to test electron and ion confinement, Ohmic heating, magnetic pumping, and divertor action; Model C, with an eight inch diameter vacuum vessel, to serve as a quarter-scale prototype for Model D which could be given to industry.

The two theorists, Spitzer and Schwartzschild, sat on the floor of the Astronomy building in 1952 to wind the coils on the Model A stellarator. It was quickly declared a success and Model B was constructed. During this time the small theory group continued investigations on Ohmic heating and various forms of magnetic pumping, transit-time, collisional, and acoustic heating. Kruskal and Schwartzchild determined the Kruskal-Shafranov limit for $m = 1$ kink instabilities driven by Ohmic heating current.

Spitzer borrowed four scientists and engineers from Westinghouse and General Electric to help him carry through the fusion program's first reactor study. The August, 1954, "Model D Report" was based on the assumptions that there would be no anamolous diffusion, a 50-50 D-T mixture, tritium breeding in a blanket, steady-state operation with a divertor, scallops in the field to reduce the current along B in the straight sections, and $\beta \sim 75\%$. The high value of β, the ratio of material to magnetic pressure, was required to supply energy for the magnets. (Later Bob Mills showed that using cyrogenic coils would decrease the energy consumed so that $\beta \sim 1\%$ would be acceptable.) The study concluded that a stellarator with a power output approximately four times that of Hoover Dam would be feasible, and that the low cost and inexhaustible supply of fuel justified continued work.

Model B suffered badly from impurities and was converted to Model B-1 with stainless steel walls, augmented by B-2 with a large magnetic pumping port, a well engineered B-3, and B-64 (a device which looked square from the top and was to be named B-8^2 until the AEC objected because the number 8 was secret). Although good confinement was not achieved on any of these devices, design studies were started on Model C.

At a February, 1955, meeting of fusion scientists, Ed Teller described an interchange instability that would destroy stellarator confinement. Spitzer canceled work on the Model C and introduced the idea of using rotating magnets to provide stabilization. The theory group put an emphasis on developing an energy principle to attack this problem. I had been hired by Westinghouse the previous summer and was sent to Princeton for a year to work on it. On my arrival I was given seventy pages of algebra in which Ed Frieman introduced a displacement vector ξ and expanded the energy of the system to second order in ξ to obtain an energy principle. By the time the energy principle paper was published the authors had derived it with the more elegant method of multiplying the equation of motion for perturbations from an equilibrium by ξ and integrating over the volume of the system. Their biggest problem was in naming the integral; Spitzer had to referee whether it should be W, δW, or $\delta\delta W$.

Over the next several years we worked out the "stellarator ordering" between the pressure, the amplitude of the perturbation field, and the length of the system, $\beta \sim \delta^2 \sim a/R$, with δ a measure of the distortion of the plasma surface from cylindrical, a the plasma radius, and R identified with the periodicity length of the system. We showed that Spitzer's magnets could be replaced by helical windings which sheared the magnetic field lines. I remember telling Lyman that a helical winding with six coils, adjacent ones carrying current in opposite directions, could provide stability if $\beta \leq \delta^2$ in a straight system. He noted that a separatrix would limit δ to about 1/3 and assumed that finite gyration effects would double this, so that $\beta \sim 20\%$.

The effectiveness of shear stabilization revitalized the effort at PPPL. Since scaling considerations predicted that increasing the size of the machine would provide an a^2 improvement in containment to allow more time for plasma heating, work on Model C was restarted. Laboratory spirits were high as we prepared for the International Conference in Geneva in 1958 where world-wide fusion research was declassified.

Many types of problems were attacked by the theory group in the following years. Greene and I puzzled over the effect of curvature on stability and extended the stellarator expansion to where we could examine the equilibrium and stability of a toroidal stellarator. Kulsrud investigated the effect of finite gyration radius on interchange instabilities. Kruskal and Oberman developed other energy principles containing more physics, Dawson and Oberman investigated plasma oscillations, Bernstein and Lenard looked at run-away electrons, Bernstein, Greene, and Kruskal studied the stability of nonlinear waves, *etc.* Gradually the magnetohydrodynamic efforts became centered on the geometric aspects of stellarator confinement while the more physical models were developed in a slab geometry.

Under Mel Gottlieb's leadership, the theoretical effort in the next decades was marked by the presence of many graduate students, postdoctoral fellows, and visiting scientists. The availablility of computers made it possible to augment the analytic studies with computational work. Dawson initiated a program of plasma simulation which evolved into a major area of physics. Kruskal and Zabusky started a study of the Fermi, Pasta, Ulam problem which ultimately led them to a solution of the Korteweg DeVries equation and the discovery of solitons. Oberman and Dawson initiated a major effort on ion-wave instabilities, Frieman pioneered in the development of kinetic models of a plasma, a program that has evolved into a major part of plasma theory.

John Greene, Katherine Weimer, and I tried to design an unstable high-β stellarator which could be used to study the behavior of instabilities. We showed that there was a critical β above which the Shafranov shift of the magnetic axis with respect to the vacuum vessel would be inward, rather than outward, and that such an equilibrium would be unstable to an $m = 1$ axisymmetric shift of the plasma. This mode was responsible for the demise of the Scyllac high-β stellarator which was built at Los Alamos a few years later.

By the mid-60's we were designing stellarators with favorable average magnetic field line curvature, $V'' < 0$, so that interchange modes would be stable. Kulsrud and Coppi were showing that ballooning modes could be a problem even when this is the case, and Furth, Killeen, and Rosenbluth showed that resistive instabilities could grow on a time scale intermediate between that of ideal MHD and transport. We were able to use the stellarator expansion to obtain the FKR starting equations and put considerable effort into understanding the problem. Transport studies showed that particles trapped in the helical wells in a toroidal stellarator would have "super-banana" orbits, drastically limiting confinement. Because of the dismal theoretical prospects, the poor experimental results, the belief of our engineers that a stellarator could not be built, and the achievement of high temperatures in the Russian T-3 Tokamak, the Princeton Plasma Physics Laboratory converted the Model C stellarator into the ST tokamak and abandoned stellarator research at the end of the decade.

General theoretical studies flourished after we became a tokamak laboratory. Krommes investigated the basic principles of kinetic theory. Tang began a major program to study micro-instabilities and their contribution to anamolous transport. Kulsrud and Perkins led efforts on atmospheric and stellar physics. Much work on the nonlinear development of the different types of instabilities was done, both analytically and computationally. Park and his collaborators developed a MHD three-dimensional code for studying the nonlinear evolution of instabilities which was recently integrated with a gyrokinetic treatment developed by Lee into a hybrid code. Rutherford followed the growth of nonlinear MHD instabilities analytically. Much work was done on parametric instabilities in conjunction with plasma heating. Dawson initiated a program on inertia confinement. Jardin developed an initial value code for the evolution of plasma in a tokamak which eventually became the TSC code, much used in tokamak design. Grimm, Greene, and I started work on the PEST code for design and interpretation of tokamak experiments. Lee's gyrokinetic simulation model eliminated the major problem of fast time scales in realistic simulations. Chance and Cheng showed that GAP modes are important. Chen and White explained fishbone instabilities by incorporating kinetic effects near a rational surface into the energy principle model.

In 1980, the Garching group was able to reduce the current to zero in their Wendelstein 7-A stellarator and show that this led to better operation. This led to revitalization of the world stellarator program. The Oak Ridge National Laboratory built a stellarator and established a strong program in three-dimensional theory. Furth noted that a helical wire wrapped around a central conductor could produce a set of detached magnetic surfaces while trying to improve the PDX tokamak by making the plasma cross section “bean shaped”. This led to the HELIAC configuration and made stellarator theory acceptable in the laboratory. We built a stability code around the “stellarator expansion” and used it for studies of the ATF stellarator at Oak Ridge, the CHS and LHD stellarators at Nagoya and Toki, and the TJ-2 heliac at Madrid, and for validation of many of the world’s MHD stability codes. Mynick developed a code using a Balescue-Lenard Collision Operator for stellarator transport studies and White modified a Monte Carlo guiding center code to three dimensions. Boozer provided a straight magnetic field line coordinate system which simplifies stellarator analysis. Nührenberg and his collaborators worked out a new technique for designing stellarators, working from a description of the plasma surface to determine both the plasma properties and the necessary coils. They found ways to almost eliminate currents along the magnetic field and developed the concept of “quasi-symmetric” stellarators, which was further developed by Garabedian. Reiman initiated work on the PIES code, an iterative procedure to investigate the nature of the magnetic surfaces in finite-β equilibria. Rewoldt modified the micro-instability code and Lin did a similar extension of the gyrokinetic simulation code so that they could be applied to stellarators. Tang has been able to provide service to other groups around the world, which has enabled him to restore part of our old Postdoctoral program and to reinvigorate the Theory Department.

More recently the laboratory together with Oak Ridge has proposed a new device, the National Compact Stellarator Experiment. Neilson encouraged development of design tools as part of the project and, with the active participation of Peter Merkel, Jürgen and Carolyn Nührenberg at Greifswald, Tony Cooper at Lausanne, Steve Hirshman at Oak Ridge, and other scientists and engineers in several laboratories, the group has made dramatic progress. Monticello and Reiman have succeeded in speeding up the PIES code by about two orders of magnitude and Hudson has extended a magnetic island correction technique of Cary and Hanson to finite-β equilibria and incorporated it into PIES. The optimization tools have been taken to a level beyond that used in the design of W7-X, with the capability of incorporating finite-β equilibrium, free-boundary kink stability, ballooning mode optimization, and transport considerations into the engineering design tools so that they can determine optimum coil configurations and currents without much human guidance.

It is a pleasure to see the viability of the stellarator effort at Princeton. Princeton Plasma Physics Laboratory has developed extensive resources for design of devices and interpretation of experiments and I expect that Rob Goldston and Bill Tang will see to it that our present expertise will be shared with workers on other stellarators around the world.

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