This is a final report on the research activities carried out under the above grant at Dartmouth. The grant itself descends from one obtained at the University of Iowa in the 1970s, transported with the P.I. to The College of William and Mary in 1977, and then to Dartmouth in 1984. Under instructions from Dr. John Sauter of the Office of Fusion Energy, only the part from the mid-1980s onward is summarized here.

During the period considered, the grant was identified as being for "nonlinear magnetohydrodynamics," considered as the most tractable theoretical framework in which the plasma problems associated with magnetic confinement of fusion plasmas could be studied. During the first part of the grant's lifetime, the P.I. was associated with Los Alamos National Laboratory as a consultant and the work was motivated by the reversed-field pinch. Later, when that program was killed at Los Alamos, the problems became ones that could be motivated by their relation to tokamaks. Throughout the work, the interest was always on questions that were as fundamental as possible, compatible with those motivations. The intent was always to contribute to plasma physics as a science, as well as to the understanding of mission-oriented confined fusion plasmas. Twelve Ph.D. theses were supervised during this period (six and William and Mary and six at Dartmouth), and a comparable number of postdoctoral research associates were temporarily supported. Many of these have gone on to distinguished careers, though few have done so in the context of the controlled fusion program.

Our work was a combination of theory and numerical computation, in gradually less and less idealized settings, moving from rectangular periodic boundary conditions in two dimensions, through periodic straight cylinders and eventually, before the grant was withdrawn, to toroids, with a gradually more prominent role for electrical and mechanical boundary conditions. We never had access to a situation where we could initiate experiments and relate directly to the laboratory data we wanted. Computers were our "laboratory." Most
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of the work was reported in refereed publications in the open literature, copies of which were transmitted one by one to D.O.E. at the time they appeared. The Appendix to this report is a bibliography of published work which was carried out under the partial support of this grant from the mid-1980s onward. It is this collection of papers which is considered to have been the principal "product" of this research support. Only the broadest-brush summary of the details of this work will be given here. The statements made are documented in detail in the publications that appear in the Appendix.

It had seemed to the P.I. for thirty years or more that the official conceptual picture prevailing in controlled fusion research was largely wishful thinking, often oversimplified to the point of caricature. It has been the goal throughout the work to add ingredients of realism to that picture one or two at a time, and to bring the picture closer to what the P.I. saw as reality. Among the original conceptual ingredients of the official theoretical paradigm which could be identified were: (1) a belief in static, ideal, magnetohydrodynamic steady states as an adequate model for a confined plasma; (2) linearly calculable "instabilities" as the principal dynamical ingredient that might lead to time-dependent complications of those static steady states; (3) the possibility of discussing the nonlinear stages of the temporal evolution of these alleged instabilities (assuming somehow that the unstable steady state could be produced and then allowed to display that it was "unstable") using only those degrees of freedom involved in the linearized instability calculations ("mode saturation"); (4) a lack of a perceived need for sharply-formulated boundary conditions as crucial to dynamical prediction; and (5) the likelihood that energetic pursuit of the questions raised in (1)-(4) would lead to experimental modifications that would improve confinement and heating of fusion plasmas. All five of these assumptions, as perceived by the P.I., seemed dead wrong from the beginning and still do. Even so, experimental confinement and heating techniques over that period have slowly improved, with theory largely in a decorative "post-diction" role. Some of the notions in items (1)-(5) above have quietly and without noticeable public retraction been withdrawn from the approved formulations of the program, but others remain still firmly in place.

During the period in which the primary motivation was the reversed-field pinch, fully-developed magnetohydrodynamic (MHD)
turbulence was the dominant interest. The study of MHD turbulence was largely introduced into fusion research by the P.I., beginning about 1975. It remains a very active area of investigation. Originally, all the formulations were in rectangular periodic boundary conditions, mimicking the emphasis in Navier-Stokes homogeneous turbulence theory, with a concentration on cascades, inverse cascades, coherent structures, and relaxation processes in which some ideal invariants were conserved better than others. One particular problem of interest in the early to late 1980s was relaxation towards a "force-free" state. This led us out of rectangular periodic boundary conditions. Such relaxation was found, but to a state that differed significantly from the celebrated "Taylor state," in that there were persistent flows (velocity fields) plus boundary layers in which the current density and magnetic field were not parallel. In an attempt to reformulate a theoretical basis for the observed 3D computational results, a principle of "minimum rate of energy dissipation" was exhumed from the 19th century, and proved to be a more satisfactory indicator of the relaxed state than the "minimum energy" principle did, particularly for the case in which the plasma was subjected to applied toroidal voltages.

Gradually, the theories evolved into statements in more realistic geometries, three-dimensional rather than two-dimensional, first periodic cylinders with square cross-sections, then with circular ones, and finally, at the time the work was terminated, in true toroidal geometry. When the stated basis of support moved from the reversed-field-pinch to tokamaks, the emphasis changed from fully-developed MHD turbulence to the "transition" region on the slightly unstable side of stability boundaries. These stability boundaries were shown to depend sensitively on the Hartmann number, which involves the product of magnetic diffusivity and viscosity and makes clear that it is illegitimate to set either of them equal to zero in discussing either steady states or their stability. This realization has been slow in coming. But in this respect, MHD has come to look much more like fluid mechanics than it did. This has further raised the issue of the numerical value and algebraic form of the proper viscous MHD stress tensor to be used in fusion research (there are few measurements, and the only available theory is the Braginskii-Balescu collision-dominated one of doubtful applicability to the regime of present confinement devices), and the issue remains open and crucial at the present time. It is at least as crucial as interminable discussions of which device to build one of next, and is a puzzle that remains for the next century to sort out.
Using a classical Newtonian viscosity term, it proved possible to develop a weakly nonlinear theory that fit quantitatively the computed, helically-deformed, steady states with flow on the slightly unstable side of the stability boundary in the Hartmann-number pinch-ratio plane. Increasing the axial ("toroidal") current further was seen to drive one into a weakly turbulent state that could sustain a quasi-periodic oscillation reminiscent of sawteeth, but with a necessarily quite different explanation than the standard temporally-variable-resistivity explanation that is usually offered for sawteeth. Both the quasi-periodic oscillations and the helical deformation were shown to be suppressible by inducing poloidal rotation in the magnetofluid. To do this, one has to step outside the MHD framework slightly to permit effects induced by slight electrical non-neutrality, if the poloidal rotation is to be produced by the mechanism used experimentally by R.J. Taylor and collaborators at UCLA. This was done in a quite ingenious way by X. Shan.

By this point (1994), we felt we were ready to replace the periodic straight cylinder by the true torus as a setting for our theory and computations. The first surprise was in discovering that it was by no means straightforward to find resistive steady states as zeroth-order models for the plasma conditions we wanted to compute or calculate. Perhaps the most extraordinary result to emerge from our whole program occurred in the last two years of it: namely, that all resistive steady states in an axisymmetric torus required zeroth order flow if Ohm's and Faraday's laws were to be taken into account as well as mechanical force balance. This is before one ever starts worrying about complications due to instabilities or turbulence. These flows are qualitatively different from those conjectured in the 1960s by Pfirsch and Schlueter, and are there for a different reason. A characteristic "dipolar" or double vortex flow pattern was identified, which acquired a higher-order toroidal flow component when the expansion in viscous Lundquist number that made the calculation possible was carried to higher order.

It was at this point that we felt ready to begin spectral-method, three-dimensional, toroidal MHD computations, which clearly were going to differ more than had been expected from the straight cylinder approximation. It was also at this point that we were notified that our support was being withdrawn. All this occurred on the fringes of simultaneously much larger upheavals within D.O.E. concerning future directions of the program, whether to build ITER...
and de-commission TFTR, indeed whether it was feasible to contemplate controlled magnetically-confined fusion at all. We are under no illusions that our predicament was of interest or indeed even accessible to the decision-makers within the Office of Fusion Energy at that time. But to us it seemed very real. The P.I. offers the suggestion that if more fundamental physics considerations, of the kind that we have consistently pursued, had been thought worth the attention of the administration of the CTR program over the years within D.O.E., those larger convulsions would not need to have taken place. But it is impossible to replay history, and one will never know the answer to that speculation for certain.

In conclusion, let it be said that the P.I. has enjoyed making such contributions as were possible under this grant over a period of twenty years, and firmly believes they will prove important if anything in MHD ever does. They could have been much greater if the funding and responsibility had been greater. Permanent gratitude will remain for the many years of support that were made available. However, it has also been permanently disappointing that it was, after the early 1980s, never possible even to engage anyone at D.O.E. in conversation about the technical questions we were addressing ourselves to, or to influence any of the larger directions of the program. D.O.E. officials had set the entire tone and direction for the program administratively in the 1970s, largely disdaining the opinions of those who were engaged in daily work on the subject, and then suddenly retreated into a posture in which their responsibility for the state of the program was disavowed in favor of "peer review," then used as the excuse for everything that happened afterward. D.O.E. forgot, or perhaps never knew, that the quality of scientific advice obtained by "peer review" is never better than the peers, and that D.O.E. itself had selected all those "peers" over a rather considerable period of time, usually with an eye to how well they fitted in to the prescribed program. How to restore a truly independent and competent scientific framework to guide the program at this point is neither a quick nor easy problem, and the P.I. is on one level at least thankful that it is not his problem.
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