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## *Magnetic Confinement Fusion Energy Research*

**Harold Grad**

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## ABOUT THE AUTHOR

Dr. Harold Grad is currently the director of the Magneto-Fluid Dynamics Division of the Courant Institute of Mathematical Science at New York University. He is also a Fellow of the National Academy of Science. Together with his collaborators, Dr. Grad has just completed a definitive study of tokamak theory and its projections for the economic development of fusion.

# *Magnetic Confinement Fusion Energy Research**

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*Abstract* Controlled thermonuclear fusion offers probably the only relatively clean energy solution with completely inexhaustible fuel and unlimited power capacity. The scientific and technological problem consists in magnetically confining a hot, dense plasma (pressure several to hundreds of atmospheres, temperature $10^8$ degrees or more) for an appreciable fraction of a second. The scientific and mathematical problem is to describe the behavior, such as confinement, stability, flow, compression, heating, energy transfer, and diffusion of this medium in the presence of electromagnetic fields just as we now can describe the behavior of air or steam. Some of the extant theory consists of applications, routine or ingenious, of known mathematical structures in the theory of differential equations and in traditional analysis. Other applications of known mathematical structures offer surprises and new insights: the coordination between sub-supersonic and elliptic-hyperbolic is fractured; supersonic propagation goes upstream; etc. Other completely nonstandard mathematical structures with significant theory are being rapidly uncovered (and somewhat less rapidly understood) such as non-elliptic variational equations and new types of weak solutions. It is these new mathematical structures that one should expect to supply the foundation for the next generation's pure mathematics, if history is a guide. Despite the substantial effort over a period of some 20 years, there are still basic and important scientific and mathematical discoveries to be made, lying just beneath the surface.

## INTRODUCTION

This paper could be considered a short course on the mathematical problems arising in fusion energy. In July 1974 the Courant Institute held a very abbreviated summer course of some 30 lectures on the same topic. One is reminded of the definition given in freshman physics of a gas, that is, that it expands to fill any size container. The converse, compression, may also lead to a liquid or solid and (to compound the metaphor) may lead to indigestion. Nevertheless, it is possible in a single lecture to give the flavor of the subject and an idea of its present scientific and technological status, plus a descrip-

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tion of its (past and future) interaction with mathematics.

The outline of this short review is as follows:

Controlled Thermonuclear Research (CTR)
and Magnetic Fusion Energy (MFE)

1. the field (CTR) and the working substance (plasma)
2. ultimate potential as an energy source
3. present status as a field of science and as a branch of technology
4. present status with regard to the power-producing goal
5. the role of mathematics (and mathematicians) in CTR
6. the role of CTR in mathematics
7. a few "typical" applications involving interesting new mathematical structures

Very briefly, with regard to items 3 and 4, we are beginning to approach a state of maturity in attitude, if not quite yet in content. With regard to item 5, our thesis will be that mathematicians can play a surprisingly large role, considering that the physical field is still "wild and woolly," so much so that the laws of physics are still being discovered; and with regard to item 6, we venture a prediction that mathematical structures arising in plasma physics will enter the mainstream of the next century's mathematical analysis, just as fluid dynamics and electromagnetics did in the last century.

## STATEMENT OF THE PROBLEM

Fusion involves the collision of certain light nuclei (e.g., isotopes of hydrogen, typically deuterium or deuterium plus tritium) that may fuse to produce other nuclei (such as neutrons and helium) and energy. In contrast, fission involves the splitting of certain heavy nuclei (typically uranium or plutonium), also with a release of energy.[1] There are many exothermic fusion reactions. About these we note only that the one that seems to involve the most accessible operating conditions uses a 50/50 mixture of deuterium and tritium. Pure deuterium as the fuel (a later development) would avoid the necessity of manufacturing and handling radioactive tritium (which is not found naturally but is obtained by using the neutrons generated in the reaction). Further down the road are technologically more difficult "esoteric" reactions that involve no neutrons at all and would eliminate all problems of radioactivity and neutron-activated structural damage.

Nature abounds in **operating fusion power plants**—for example, the Sun and most stars. It would not suffice to merely reproduce a portion of the Sun in the laboratory. The reaction must be accelerated from a burning time scale of billions of years to one involving seconds, at most. At the opposite extreme, the time scale of an uncontrolled, very rapid reaction, as in an H-bomb, is too short to be a useful source of energy unless it is slowed down. Elementary estimates turn out to require ignition temperature of several times $10^8$ degrees. The usual energy unit is the electron volt (1 eV ~ $10^4$ °C) or kiloelectron volt (1 keV ~ $10^7$ °C). We are interested in temperatures of 10-100 keV. The situation is entirely different from that in accelerators where the **directed** (not thermal) energy may be much higher; in a plasma we are concerned with a true kinetic temperature in which the velocity is more or less random and isotropic and is frequently close to Maxwellian. We say "kinetic" rather than "thermodynamic" temperature because the plasmas of interest will **not** be in equilibrium with regard to radiation. Radiation losses must be replaced by heat input or by the thermonuclear reaction itself; it is the latter that indicates an **ignition temperature** somewhere in the range given above. For an optimal temperature, ignition is conventionally given by the Lawson criterion, $n\tau > 10^{14}$, where n is the plasma density (ions per cubic centimeter) and $\tau$ is the lifetime of an ion (in seconds). For reasons that will become apparent, any unmodified figure of merit of this type is more appropriate for use in Sunday supplements than as a scientific or technological measure of past progress or of the remaining distance from the goal, or as a comparison of the relative status of two experiments.

The time required for a significant burn depends on the plasma state but, for practical reasons, must range from an appreciable fraction of a second to several seconds. At 10 keV, the speed of an electron is about 1/7 of the speed of light and that of a deuterium ion, about 1/400. In an appreciable fraction of a second even the ions will traverse (or circulate around) the apparatus many thousands to millions of times. Although an accelerator (even more so, a storage ring) confines particles for many more transits, this is done with very specialized techniques appropriate to particles in an extremely small part of phase space. The thermonuclear plasma, a true gas, requires confinement in a "bottle" that encompasses approximately all of phase space.

Turning to the density of a thermonuclear plasma, it is easily estimated from the **energy** density, or pressure. This must be comparable to that of steam in a conventional power plant, and efficiency evidently favors as high an energy density as is practicable. The pressure will be from one to several hundred atmospheres; the upper limit is determined by the strength of the structure. Given the temperature and pressure, we deduce that the plasma density is $10^{-2}$ to $10^{-6}$ of atmospheric—low, but by no means a good vacuum. Densities of actual plasmas of thermonuclear interest range from $10^{13}$ to $10^{17}$ particles/cm$^3$ (atmospheric density is $3 \times 10^{19}$).

To confine a hot, dense plasma and keep it from physical walls requires a field of force. Gravitation evidently works! But the minimum size would not be very much smaller than the Sun. The only other relevant force field is the magnetic field. A convenient conversion factor is that one atmosphere equals 5000 gauss = 5 tesla (and $p \approx B^2$). Fields over $10^5$ gauss (6000 psi)are not likely to be used in any large permanent structure (megagauss fields are usually obtained only explosively).

An evident complication is that magnetic fields (cf. the Maxwell stress tensor) exert highly anisotropic forces. This will be an important factor in the design of magnetic "bottles," and it also suggests that plasmas in contact with a magnetic field may frequently be anisotropic.

There is another confinement possibility, which has been given the unusual term "inertial" confinement.[2] Instead of confining the plasma (force per volume = $\nabla p$) by a magnetic field (Lorentz force = $\mathbf{J} \times \mathbf{B}$), it is confined by the "inertial force" $-\varrho(du/dt)$. In other words, it is not confined at all and is allowed to expand freely. A very small, very-high-density explosion, ignited by concentrating a high-intensity laser beam or electron beam on a solid pellet, will be sufficiently attenuated by the time it expands to the size of the physical container that it does no significant damage (a conventional thermonuclear bomb could be so used in a large piston, say, a mile in bore).

To be more precise, even with successful magnetic confinement, all the plasma and most of its energy eventually reaches a physical wall. The purpose of magnetic confinement is not to prevent but to slow down and control the loss processes, to deposit the plasma and its energy where it will do most good and least harm. More specifically, it is to ensure that most particles that reach the walls (both charged and neutral atoms) are relatively cool. Part of the charged particle energy may be recovered directly as electrical energy (at high efficiency) by expanding the plasma against the confining magnetic field; another part, escaping at "open ends" (see discussion of magnetic mirrors below) may have some of its streaming energy also converted directly into electricity; but most of the charged particle energy eventually reaches a physical wall where the energy is transferred to circulating coolant. Most neutrons deposit their energy in a neutron blanket surrounding the plasma, also transferring the energy to coolant. Some of what was originally charged-particle energy reaches the walls in the form of high-energy neutrals (through an intermediate charge-exchange collision). Radiation is distributed rather widely in all the surrounding structure. Harmful effects (other than too-rapid loss of plasma energy) are primarily dislodgement of impurities by energetic particles bombarding the walls, and structural damage of walls and coils by neutrons. Magnetic fields may serve almost as much to keep impurities out as to keep plasma in. In other words, the magnetic field is not so much a bottle as a porous plasma retardant. Nevertheless, many of the most useful theoretical models take the more ideal formulation.

The standard, popular (but correct) remark is that the fuel is inexhaustible and essentially free. There is enough deuterium in a gallon of water (for example, seawater) to provide the energy equivalent of 300 gallons of gasoline, and the cost of extraction of the deuterium is already negligible. What this means, of course, is that the cost to the consumer will be determined by the capital cost of a power plant over its lifetime, distribution costs, and whatever ecological costs there may be in comparison with the competitive cost, at some future time, of alternative energy sources.

The description of the fuel as inexhaustible is sometimes contested because tritium, which is an artificial element, is generated by neutron

**FIGURE 1**
ADVANTAGES OF FUSION POWER

1. Fuel supply plentiful, cost low
2. No combustion products, very little radioactive waste
3. Low radioactivity, reduced associated dangers
4. No chance of runaway
5. No diversion of weapons-grade materials
6. High efficiencies, possibility of direct conversion

bombardment in a lithium blanket surrounding the proposed D-T reactor, and it is estimated that there is only a hundred year visible supply of lithium (this, incidentally, is comparable to the resource lifetime claimed for fission reactors). However, since substantial use of lithium for this purpose would commence 25 to 50 years in the future, it seems safe to assume that 125 years of technological progress (cf. space flight, radar, solid state in 1850) will be adequate to develop tritiumless (D-D) and even neutronless power plants (using more esoteric nuclear reactions) in less than 100 years following the first operating D-T power plant.

The comparison between fission and fusion power with regard to economics, safety, time scale, ecological factors, etc. has probably already generated enough heat to fuel a medium-sized generator. This question would take us too far afield; let us just take as an exhibit, Figure 1, courtesy of Stephen O. Dean, Assistant Director for Confinement Systems, Division of Magnetic Fusion Energy, U.S. Energy Research and Development Administration.[3]

The basic conclusion is that a hypothetical fusion reactor does have inexhaustible free fuel, is almost certainly much cleaner and safer from the point of view of radioactivity and other ecological hazards and suffers from only one clearcut disadvantage compared to coal, oil, or fission—it does not yet exist.

## PRESENT STATUS AND FUTURE PROJECTIONS

As an introduction to the assessment of fusion as a field of science or technology it is necessary to evaluate the magnitude of the problem, in particular its complexity. A proper appreciation of this point dominates all scientific and technological evaluations. Every once in a while there is a newspaper article that briefly assesses the status of magnetic fusion by stating that the problem has been worked on for n years "without success." This is extremely misleading. In fact, the state of the art has advanced by many orders of magnitude, with innumerable "breakthroughs," each sufficient to solve an ordinary difficult problem. But the path ahead will also require quite a few additional scientific and technological "breakthroughs." There is a very difficult public relations problem in simultaneously "pointing with pride" to the past and realistically evaluating the distant future.

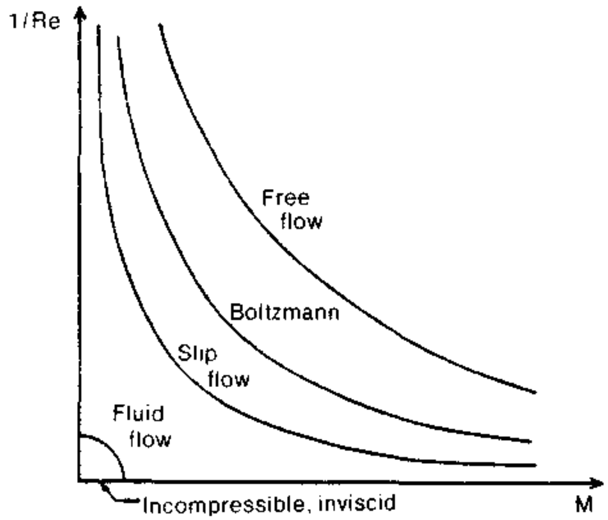
A very brief statement serves to illustrate the magnitude of the technological requirements. An eventual (first generation) power producing reactor, as it is visualized today, would have a plasma core a few meters in minor diameter at a temperature in excess of $10^8$ °C at a pressure of, say, 10 atmospheres, carrying a current of $10^7$ amperes. Something like one meter distant from the plasma are coils at room temperature (or, more likely, superconducting, at approximately absolute zero). The entire structure, under large, complex stresses, is bathed in X-rays and other types of radiation and also in neutrons. There is provision to replenish the plasma (continuously or intermittently, as in an internal combustion engine), to remove reaction products and the heat of reaction, with remote handling for possible repair and periodic replacement of radioactive structures. Even without detailed analysis, the engineering problem is, to say the least, formidable.

The theoretical problem is at least as formidable. A simple (and very crude) measure of the richness of physical phenomena to be expected within a field is given by counting dimensionless parameters. For example, fluid dynamics is held to be spanned by two parameters, Mach number and Reynolds number (this is misleading, but not relevant for the present argument). A similar count in the simplest plasma (fully ionized, one species of ion) yields seven or eight instead of two. To be more specific, in order to eliminate the geometry, consider an infinite domain without boundaries and the classical steady, plane shock transition. In a gas, there is one parameter, the shock strength (this determines the thickness and all profiles, such as density, temperature, and the entire molecular distribution function). The equivalent problem for a fully ionized plasma in a magnetic field (uniform at infinity) has **six** dimensionless parameters. The large variety of qualitatively different physical mechanisms that govern the shock thickness fill a six-dimensional phase space (the shock thickness is almost never simply related to a mean free path).

In Figure 2 the various regimes of classical fluid dynamics and kinetic theory are located on a two-dimensional figure with axes M (Mach number) and 1/Re (Reynolds number). Qualitatively, the value of M measures the importance of compressibility and Re that of viscosity (including turbulence). Rarefaction effects (finite mean free path) are not included in fluid dynamics and are physically and intuitively distinct from compressive and viscous effects, but they appear on the same diagram, since the Knudsen number, K (mean free path divided by representative length) is given by $K = M/Re$. In other words, classical continuum fluid dynamics is more properly described as a one-dimensional parameter space, the two semiaxes where $K \to 0$ (one parametrized by M, the other by Re).

Classical incompressible inviscid fluid flow is represented by the neighborhood of the origin in Figure 2. One might perhaps describe the content of this theory in a single volume. Compressible theory would require a shelf of books (lining the M axis) for comparable coverage, similarly for viscous flow,

**FIGURE 2**
REGIONS OF GAS FLOW




a row of books lining the 1/Re axis. A bookcase-full (not presently available) would be needed to provide classical kinetic theory of gases with similar coverage. An equivalent degree of coverage of all plasma physics would (perish forbid!) require the entire New York Public Library devoted exclusively to this subject (and the world's GNP for many years to amass the knowledge in these books).

Restricting the field to plasmas of thermonuclear interest does cut down the magnitude of parameter space and of physical phenomena, but not enough to hope to blanket the area, either theoretically or experimentally, by any expenditure of effort and funds (e.g., magnetic fusion plasmas allow a density range of $10^4$). Inevitably one must be extremely selective in choosing areas of study, and one must be constantly aware that the route followed is almost certainly not the best one. Nevertheless, whatever routes are followed **must** be pursued redundantly, with overlapping experimental parameters. Time does not permit elaboration of this essential point.

There is a further peculiarity that characterizes thermonuclear plasma physics in contrast to other areas of plasma physics and of physics in general. A plasma is a new substance, a "fourth" state of matter. What we must do is to learn its properties such as equations of state, transport coefficients, wave propagation characteristics, and so forth; we must also learn to manipulate it—heat, cool, compress, pump, confine, purify—just as we do air or steam (or a new superfluid). Although nature is always exceedingly complex, physics gains its power (as does mathematics) by rejecting the complexities that occur in nature in order to study isolated "basic" phenomena (or structures). The goal of experimental **plasma physics** (as distinct from CTR) is to construct experiments each of which isolates an individual phenomenon. However, before we can study a hot plasma we have to catch one. This stipulation conflicts with the best scientific procedures. To create a hot plasma and keep it away from the walls, hot and undefiled for long enough to study it, requires complex combinations of experimental procedures and complicated geometries that conflict with the desire to simplify and isolate individual phenomena. In other words, one must be fairly successful in reaching the practical goal of plasma confinement before one can study the basic plasma properties which should be known in order to learn how to confine it. For a contained hot plasma the scientific and technological problems are presented all at once rather than in sequence. Analysis of complex interacting systems is commonplace in engineering, but not when the individual phenomena have not themselves been scientifically explored. A reasonable analogy for the fusion energy problem would be a mission-oriented program to land a man on the moon where, as part of the project, one must at the same time discover (and exploit) Newton's laws of motion, Maxwell's equations, electronics, and solid state physics. In addition, this is expected to follow a program plan which synchronizes and coordinates delivery of condenser banks with the planned date of the discovery of the magnetic monopole.

To assess the status of this complicated combination of mission-oriented basic physics and brand new technology we look at two questions:

1. To what extent are the relevant laws of physics known (in a practical sense such as, will turning a knob in one direction improve or destroy confinement)?

2. Is the state of the art (experimental or theoretical or numerical) such that any level of effort at the present time is sufficient to answer a specific question, or must we wait a decade or a generation for the state of the art to reach an appropriate level?

With regard to the second question, it is clear that the space program had to wait for solid state technology to provide an appropriate level of miniaturization, also for computers to be able to calculate course corrections during flight. This state-of-the-art requirement applies equally well to theory. Quantum mechanics flourished in its early days because the mathematical foundations of spectral theory already existed; relativity progressed rapidly because differential geometry was available beforehand.

Returning to CTR we point out that two tokamak experiments, the TTT and the Alcator (for the present, these can be taken as undefined terms) reached some sort of terminal state during 1976. The first was a disaster; the second succeeded beyond all expectations. The discrepancy cannot be attributed to any significant difference in scientific ability, administrative planning, or funding hardship in the two organizations. The reason was simply that the relevant laws of physics were not sufficiently known beforehand (or even yet): there was (and is) no sufficiently refined theoretical plus computational model able to discriminate between the two disparate behaviors. In order to discover the answers, both experiments had to be performed.

Another fact (which requires too much space to document here) is that virtually every major confinement experiment to date has turned out to be **dominated** by a physical phenomenon which was not considered during its planning and design (this fact may be contested by some experts: it is nevertheless incontestable).

It is clear that the requisite practical understanding of the physical (or even empirical scaling) laws is not yet known. What about the more serious practical question of the state of the art? This can only be answered by looking backwards. As an experimental example, the decade of the sixties is sometimes considered to have been fairly stagnant with moderate increases in $n\tau$ and temperature, and numerous disappointments. But it was a time of revolutionary advances in plasma diagnostics and measurements. In 1960 if an experimenter was asked, what was the density or temperature of his plasma, one got a number in return (and, if pressed, a number surrounded by caveats. I recall having made the statement, in the late fifties, that if an operating thermonuclear plasma were presented to us by an extraterrestrial visitor from a superior civilization, we would have no means of confirming whether it indeed was what it was represented to be.) Ten years later, in 1970, one could expect, in reply, to receive a flood of data giving profiles, density or temperature as a function of plasma radius and of time; in a few cases one would be overwhelmed with a complete distribution function in physical and velocity space. One cannot claim to have a science of plasma physics until plasma properties can be measured. One cannot begin to compare theory with experiment until detailed (profile) measurements can be made; the stability of a plasma, for example, will depend sensitively on the detailed profiles—not on just a few global properties. With a few exceptions, there was no way to compare theory with experiment until diagnostics reached a certain level of maturity (though, of course, very many such comparisons were made). There are still important gaps, for example, in the measurement of current density profiles (essential to compare with most stability theories), and it would be most useful for comparison with theory to have localized measurements of the fluctuation spectrum.

A similar case can be made with regard to the state of the art in MHD theory of equilibrium, stability, and diffusion. In each of these, the degree of theoretical sophistication and professionalism that (with a very few exceptions) is absolutely necessary for purposes of experimental verification has only begun to be achieved since about 1970: briefly, for stability, global eigenfunctions rather than local criteria (nonlinear results are still in infancy); for equilibrium, nonlinear and topological evolution; for diffusion, time-dependent nonlinear evolution; in each case, with a very careful balance between analytical and numerical inputs.

To summarize the state of the art in CTR after 20 years of research and development, consider as an analogue the sequence:

Kitty Hawk
Lindbergh
Commercial jet
Vanguard Grapefruit
Appollo
Self-sufficient space station—population 1000

I would estimate that CTR has reached the Commercial Jet stage on the way to an eventual self-sufficient space station. Unfortunately, except for a few technical applications, there seem to be no significant commercially useful intermediate stages on the way to a fusion power plant.

One fault in evaluation which has caused untold misunderstanding of progress in the field is the "one-clever-breakthrough syndrome" which is propagated equally by scientists (usually outside the field) and by newspaper reporters. Progress to date has been measured in orders of magnitude (and in scores of breakthroughs); but the path ahead is also measured in orders of magnitude. To counterbalance this is the fact that the payoff is also valued beyond measure, provided that the end result is not exorbitant in cost.

Another fruitless evaluation is the comparison with the stages in the evolution of the fission reactor. The difficulties are noncomparable and occur in a different sequence. The relevant laws of physics in the fission problem were contained in the nuclear reactions and their cross sections. This is a triviality for the fusion problem, but there is no fission analogue to plasma turbulence, instability, or any cooperative effects in the neutron "fluid." The chemical problems of separating $U_{235}$ and manufacturing $UF_6$ bricks have no analogue in plasma physics. The actual construction and performance of the Stagg Field fission experiment (given the bricks) could be done by appropriately coached high school students, whereas to just carry out a contemporary (far from reactor stage) plasma confinement experiment, given all the equipment (including, say, Thompson scattering or laser holography), requires a highly trained professional team of experts linked to a complex, real-time computing system.

In any discussion of fusion someone is sure to ask, when will scientific feasibility, or a demonstration reactor, or commercial fusion power be reached? In 1962 the following reply was given:

1. The theoretical problems are extremely difficult.
2. The experimental and technological problems are even more difficult.

3. Guessing when the problem (in any phase—scientific feasibility, prototype reactor, commercial power) will be solved is most difficult of all.

There is no reason to change this reply after a lapse of fifteen years. Several orders of magnitude still separate the present from where we wish to be. In covering this territory it is certain that new, unexpected physical phenomena will be encountered; new problems not yet formulated will have to be solved; new laws of physics will be formulated. One can, with some risk, predict technological advances—but no scientist has (or should have) the temerity to foretell when new physical laws will be discovered, or how long it will take to solve a problem before the problem has surfaced. Such estimates, when they are necessary, are the proper province of scientific administrators, and where social issues are involved, elected representatives. But when they are made by scientists, they should not be confused with scientific judgments.

In a situation where predictions are so difficult (demonstrably so, historically), why are they so plentiful? There appears to be an equivalent of the axiom of business, that where there is a demand, it will be supplied. The same can be said of theoretical "explanations" of extremely complex experimental situations and predictions of the unpredictable. There seems also to be an application of a form of Gresham's Law, that poor science will drive out good. The pressure on theory to agree with experiments and on experiments to agree with schedules is unbearable. If an administrator asks for a prediction, or an experiment demands an explanation a definite answer is always more acceptable than an equivocation. There has been recent pressure (in other fields) for "one handed" scientists, but it would seem that a scientist who has lost his "on the other" hand has ceased to be a scientist (even if, on the other hand, he turns out to be more useful to society by choosing not to be a scientist).

There is a final and rather subtle point concerning predictions using complicated mathematical models in complex physical situations. In the development of fusion, we are presented with some extremely complex economic situations.[4] Social scientists have an advantage in almost never having succeeded in formulating a mathematically precise model of any part of their world; they usually retain a certain amount of cynicism with regard to mathematical models. "Hard" scientists are accustomed to accepting extremely accurate mathematical models as a norm; the fact is that, practically speaking, physics is **defined** to be that part of the universe that is accurately describable by mathematical models (for example, turbulence has almost been banished from the subject). The fact that one does not predict a real airplane's performance from a single formula, but, rather, builds an onion of successive layers of partial theories and specialized experiments, is beginning to bear analogues in plasma confinement theory.

All this talk of complexity may sound like pessimism or a cause for indecision. The opposite is in fact true. There are many examples (of which we cite only a few) where the **fact** of complexity and lack of accurate knowledge gives the correct, positive course of action. For example, should we concentrate on what seems to be the single best approach? The answer is clearly, no, it is much too soon. The laws of physics and the empirical scaling laws change with each experiment. Should we build one large experiment at \$N to test one hypothesis or four smaller ones at \$N/4 each? The better answer for **most rapid progress** toward a reactor is obvious (build four), even though this runs counter to actual administrative practice. Since not all avenues can be tried, the optimum procedure would seem to lie somewhere between a one-shot gamble and a cross-the-board, safe approach. Of course, a relevant social or political input may prevent the question from ever taking the above form. There is also an interesting psychological point in the use of the phrase "real world"—are the laws of physics or of society and the budgetary world more "real," and which laws are more dangerous to flout?

The physical complexity is actually the single most important reason for a belief in the ultimate success of the program. Whenever one direction proves disappointing, there are ten alternatives to try. All the leading contenders today defy Murphy's Law (if anything can go wrong, it will)—each one behaves significantly better than one had any reasonable right to expect when it was planned. And the fact that so far almost all experiments end up heading in directions that were not planned, is evidence of the intelligent use of the flexibility inherent in the variety of options.[5-9]

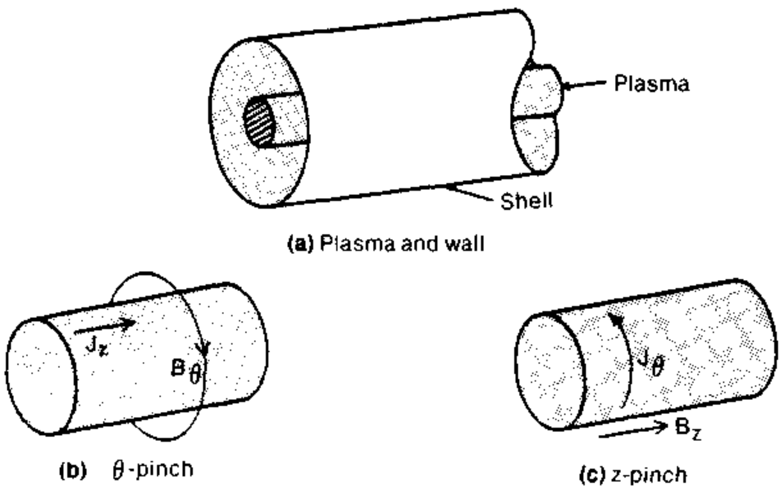
## BASIC CONFIGURATIONS

We now describe some of the basic magnetic confinement configurations, together with a glossary of common terms used to describe them. Some of the names are functional or descriptive; others have lost their original meaning with the passage of time.

A prototypical confinement geometry (Figure 3(a)) is an infinite plasma cylinder surrounded by a vacuum magnetic field, terminated at a conducting wall (which carries currents that can be considered to be the source of the field). In Figure 3(b) the field is axial, $B_Z$, and the current is azimuthal, $J_\theta$; this is called a $\Theta$ pinch. The dual configuration, in Figure 3(c), with azimuthal field $B_\theta$ and axial current $J_Z$ is called a z pinch. The name "pinch" is historical, referring to a transient phase; an axial electrical discharge between two electrodes gives rise to a distributed z current, the constituent elements of which attract one another, "pinching" until the electromagnetic attraction is balanced by the compressively increasing plasma pressure. The conceptually simplest pinch configurations contain a constant pressure plasma but no field within the cylinder and a vacuum (harmonic) magnetic field outside ($B_Z$ = const, $B_\theta$ = const/r).

For reasons of stability, the z pinch is always accompanied by an axial $B_Z$, taken to be uniform in the simplest case, with equal values inside and outside the plasma cylinder. The combination $B_Z$ inside, a different value of $B_Z$ plus

**FIGURE 3**
CYLINDRICAL EQUILIBRIA




$B_\theta$ outside, both $J_\theta$ and $J_Z$ on the interface, is called a "screw pinch." The pressure balance condition at the interface is that $p + \frac{1}{2}B^2$ be continuous, or that

$$p = \tfrac{1}{2}B^2_{out} - \tfrac{1}{2}B^2_{in} \quad (B^2 = B_\theta{}^2 + B_Z{}^2).$$

A more general cylindrical equilibrium, also termed screw pinch, has a distributed pressure profile p(r) together with fields $B_\theta(r)$, $B_Z(r)$ and currents $J_Z(r)$, $J_\theta(r)$. Ampere's law, $\mathbf{J} = \text{curl}\,\mathbf{B}$, or

$$J_\theta = B_Z{}', \qquad J_Z = -(rB_r)'/r,$$

coupled with the pressure balance, $\nabla p = \mathbf{J} \times \mathbf{B}$, or

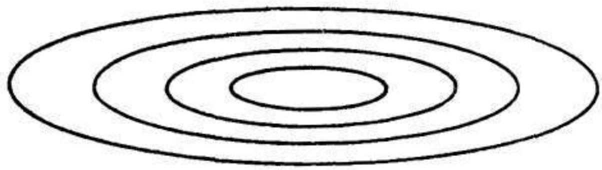
$$d(p + \tfrac{1}{2}B_\theta{}^2 + \tfrac{1}{2}B_Z{}^2)/dr + B_\theta{}^2/r = 0,$$

allow two of the profiles p, $B_\theta$, $B_Z$, $J_\theta$, $J_Z$ to be given arbitrarily. The plasma is confined (two dimensionally) if p drops to zero, $p(a) = 0$, and the surrounding field, $r > a$, is harmonic. With this boundary condition for p, $|B|$ is continuous at the interface; there can be a surface current, but it is usually set equal to zero ($B_\theta$ and $B_Z$ continuous).

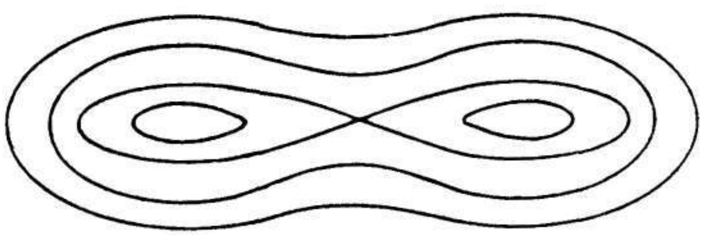
Other common plasma cross sections are the elongated **Belt-Pinch** (Figure 4(a)), the **Doublet** (Figure 4(b)) with topologically nonsimple flux surfaces, and the **Divertor** (Figure 4(c); the two indicated coils are outside the plasma, leaving a simple magnetic topology inside the D-shaped plasma).

To confine (or approximately confine) the plasma three dimensionally, the Θ, z, or screw pinch can be bent into a torus, eliminating the open ends (Figure 5(a)); or the ends can be squeezed to reduce the end losses (Figure 5(b)); or the cylinder can be made very long and the end-flow simply ignored;

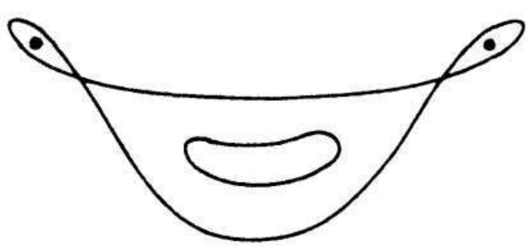
**FIGURE 4**
NONCIRCULAR CROSS SECTIONS



**(a)** Belt pinch

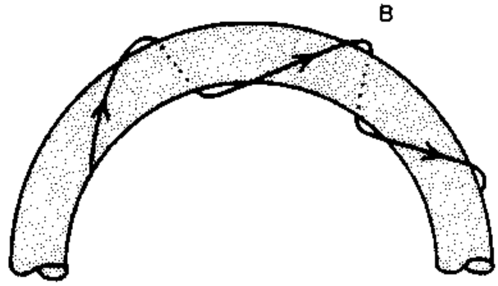


**(b)** Doublet

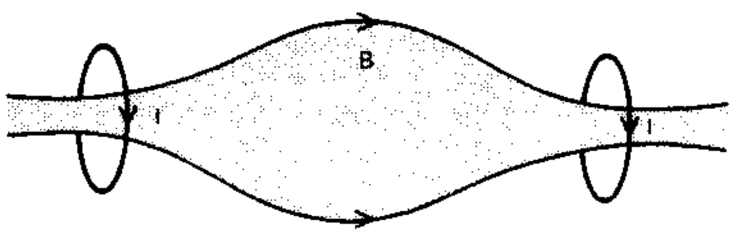


**(c)** Divertor

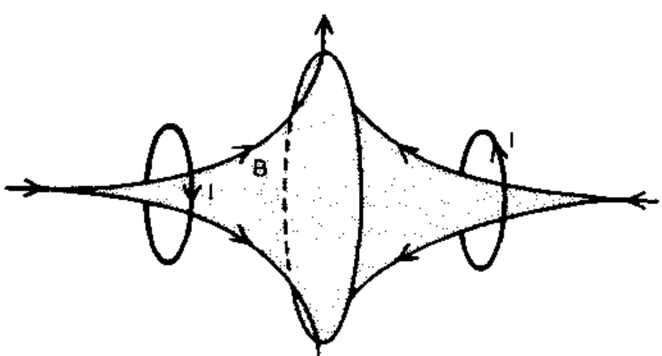
**FIGURE 5**
THREE-DIMENSIONAL CONFINEMENT




**(a)** Closed system




**(b)** Open system (mirrors)




**(c)** Cusped geometry

with no attempt at stoppering, reactor conditions would require a length on the order of a few kilometers.

According to original usage, a toroidal, ohmically heated discharge (generic screw pinch) of approximately circular cross section, enclosed by a circular cross-section conducting shell [as in Figures 3(a), 5(a)] with poloidal field small compared to toroidal (large toroidal current), low $\beta \equiv p/B^2$ (plasma energy small compared to magnetic energy), and the "safety factor" $q = RB_\theta/aB_Z$ (reciprocal of the magnetic rotation number, R and a are major and minor plasma radii) on the order of two or three, is called a **tokamak** (Russian acronym). Since the plasma has finite resistivity, toroidal current implies $\oint \mathbf{E}\cdot dx \neq 0$. The magnetic flux threading the torus (usually in a transformer core) must vary in time; thus this is an inherently transient system. The term tokamak has expanded in common usage to include all plasma shapes and values of $\beta$, heating by injected beams, compression, radio-frequency waves, shock waves and so forth; with this newly acquired flexibility the tokamak has become the most viable and popular reactor concept (to the tune of a $250 million proposed experiment).

The question of end losses, in an open system, is not simple. There are at least three qualitatively different mechanisms, depending on parameters. For small mean-free path (not usually relevant with reactor parameters), the flow is sonic at an end, or throat (but this is complicated by the fact that, in MHD as distinguished from ordinary fluid dynamics, there are three characteristic speeds and three possible sonic transitions). For large mean free path and **adiabatic** orbits (small gyro radius), the loss mechanism is **magnetic mirror** reflection[10] and velocity space diffusion into a **loss cone**; for nonadiabatic orbits (generally a consequence of locally weak fields or very high $\beta$) the mechanism is **cusp losses**. There are corresponding configurations, called **mirror machines** (Figure 5(b)) and **cusped geometries** (Figure 5(c)), but the loss mechanisms do not necessarily apply to the correspondingly named configurations.

An important consideration in the morphology of toroidal confinement geometries is that the conceptually simplest toroidal field configuration does not hold plasma, that is, any field in which all magnetic lines are circles with a common axis. A simple macroscopic argument shows that any solution of $\nabla p = \mathbf{J} \times \mathbf{B}$ with the stated symmetry must have $p = p(r)$ [in cylindrical coordinates $(r, \Theta, z)$], which states that p is constant on an unconfined infinite cylinder; alternatively, any (transiently) confined plasma will have a net, unbalanced (outward) force. A somewhat similar microscopic orbit argument shows that nonconfinement follows from oppositely directed ion and electron orbit drifts.

There are several common complications of this unworkable simple symmetry which are introduced in order to provide confinement. One goal, starting from the microscopic orbit model, is to create a family of confined flux surfaces which (to a certain approximation in orbit theory) guide particle orbits. In the tokamak, this is done by inducing a toroidal plasma current. In the **stellarator**, nested flux surfaces are produced in the vacuum field by bending the configuration into a Figure-Eight, or by applying suitable external windings, usually helical, to give a nonzero magnetic rotation number. A different solution (with closed magnetic lines) is to provide a series of bumps (as in a string of mirror machines) along the toroidal direction, maintaining closed but noncircular field lines; this is the EBT (Elmo Bumpy Torus).

From the point of view of the momentum balance, $\nabla p = \mathbf{J} \times \mathbf{B}$, one is led to consider external coil systems which counterbalance the net outward force on the plasma in a purely toroidal field; this is the **scyllac**. One practical set of scyllac coils is helical, as in a stellarator, but the optimal solutions (as well as simple physical intuition) for the traditionally low $\beta$ stellarator, based on vacuum flux surfaces, are quite different than for the high $\beta$ scyllac, originating as a $\Theta$ pinch, and based on force balance considerations.

It is interesting to point out that the original stellarator concept was the consequnce of a nonexistence theorem (under too much symmetry). Later, deeper analysis showed that all stellarator "solutions" remain subject to a more sophisticated lack of existence of static equilibrium, leading in practical cases to an irreducible fluctuation level and essential complications in the topology.[11-12]

The advantages and disadvantages of the various magnetic configurations coupled with the many ways of creating and maintaining plasma density and temperature are not at all evident, and, despite complex and sophisticated theoretical calculations and well-documented experiments are to a considerable extent black (or at least gray) magic. For example, a simple mirror machine created by radiofrequency heating is observed to be stable; but if it is created by plasma injection it requires special stabilizing windings [Ioffe Bars, or cusped windings, Figure 5(c)]. Another example is the tokamak which **requires** for its operation an initial "anomaly" factor of some 200 in resistivity, dropping almost immediately to close to unity. Without the large initial anomaly it would be an uninteresting cold plasma. Without the precipitous drop, the plasma losses would be intolerable.

State of the art confinement experiments at the present time are in the \$10 \$20 million range; in the next five or ten years they will reach the \$50 \$250 million range. The upper figure would, perhaps, produce an experiment with significant burning if filled with tritium as well as deuterium. The present magnetic confinement budget is approximately \$220 million for fiscal year 1977.[13]

## MATHEMATICS AND MAGNETIC CONFINEMENT RESEARCH

It is traditionally felt that mathematicians contribute significantly only to a mature scientific discipline after the dust has settled and the physical laws are well established. We wish to demonstrate exactly the opposite. Because of (not despite) the great complexity and the difficulty of performing experiments in magnetic confinement plasma physics, mathematics (and mathematicians) not only can but must play a vital role in formulating the basic framework for future development and exploitation of the field.

One could write down what many scientists would believe is a moderately accurate system of governing equations for a thermonuclear plasma. This would contain several coupled Boltzmann equations for ions, electrons, neutrals, $\alpha$ particles, and a large number of ionized species (such as multiply ionized oxygen, iron, etc.), together with Maxwell's equations and the coupling to various types of radiation, plus some model for the influx of impurities from the walls. Nonlinear effects, turbulence, and changes in magnetic topology are just a few factors which we know will play a very important role. To be quantitatively accurate, this model would have to include three space dimensions and time. Unfortunately, no such model could yield any useful information without drastic modification. Very crudely, one might assign a figure of merit to a theoretical model as the product of three factors, one being the faithfulness of the model to physical reality, the second to geometrical reality, the third being a measure of its mathematical tractability. To be precise, a model which is "solved approximately" should be described as a more elementary model which is **solved**. It is clear that any useful model will have to set aside 90 percent of the physical complexity or (and?) 90 percent of the geometrical complexity. Ultimately, understanding of such a problem will come, not from one ingenious model, but from synthesis of very many interlocking ingenious models.

There are three popular misconceptions worth brief mention. The first is that the validity of a physical theory is determined primarily by experimental confirmation; the second is that, in order to be useful, a model must yield quantitatively accurate results; the third is that physical evidence and intuition provide the equations, after which mathematical analysis assists in obtaining the solutions.

Relevant to the first two points is the fluid dynamics model of incompressible, irrotational potential flow. This is a very poor quantitative model of a real fluid; a half-century ago it was obsolete (d'Alembert's paradox, and so forth). Today, every fluid dynamicist spends a large amount of time developing a strong quantitative intuition about this nonexistent fluid. The reason is twofold. First, it makes accessible an enormous mathematical literature (in real geometries). Second, it provides the basis for describing a real fluid in terms of its deviation from ideal behavior. The ideal model can become quantitatively accurate after it is modified in certain well defined ways at boundary layers, wakes, and shocks. Even when not strictly quantitative, the combination of potential flow plus modifications is much more valuable for its physical insight than is the more accurate, full Navier-Stokes equations with their much more limited analytical theory and greater dependence on case by case numerical solutions. The value of potential theory is much greater today than it was a half-century ago, because today its theory is much more powerful and its limitations are quite precisely known.

A similar concept in plasma theory is the concept of frozen flux, that magnetic field is carried with the fluid (cf. vorticity in fluid dynamics).

$$\partial \mathbf{B}/\partial t + \mathrm{curl}(\mathbf{B} \times \mathbf{u}) = 0$$

(**B** is the magnetic field and **u** the fluid velocity). This is very rarely an accurate, quantitative description of a plasma. But it is almost impossible to hold a rational discourse about a real plasma other than in terms of deviations from this ideal model.

How does one determine which 90 percent of the "real" problem to throw away? One consideration is mathematical solvability: (a) is the problem well posed? (b) are powerful tools available? No physical evidence or intuition can ever have any relevance to the question of well-posedness. The answer to this question is known to depend on **arbitrarily small** changes in formulation. No arbitrarily small change can have physical (as distinguished from mathematical) meaning. The laws of physics are not the same as the laws of mathematics; equations obey the latter. What physical intuition **can** contribute is a strong psychological belief that **something similar** to a given formulation should be useful; this will encourage exploration of other neighboring systems if a particular one is mathematically unacceptable. What muddies this point in practice is that an approximate mathematical "solution" of an ill-posed problem may also be an approximate **solution** to an, in some sense, neighboring well-posed problem (or this may be only a pious hope). For example, an asymptotic formula can be asymptotic to two different exact results; and an asymptotic procedure applied to an improper equation may give a formula which is correctly asymptotic to another (not even formulated) equation.

To return to the necessary amputation of most of the physics and geometry, intuition and experience are evidently crucial. But **mathematical** intuition and experience are at least as important as **physical** intuition and experience. How is it that in many fields scientists obtain accurate results by what are, mathematically speaking, very sloppy means? The answer is complex; but most crucial is a feedback teaching mechanism which slaps your hand when you stray too far from the proper path. In a field with frequent and accurate recourse to experiment, this provides a very effective interactive mechanism. In a field such as the one we are now describing, the experimental feedback is very weak and ambiguous (the consequences are seen in the number of discarded models and calculations which litter the literature). However, since there is always a more accurate and more complicated **equation** which one would like to be able to solve, one can admit purely mathematical hand slaps as feedback, based on qualitative and semi-quantitative inputs from the more exact model. A careful evaluation will show that, up to now, mathematical care and mathematical intuition have played a large role in obtaining lasting results in confinement plasma physics.

"Mathematical care" must be carefully distinguished from mathematical rigor. A century of experience in applying mathematics to physical systems is available to offer reliable guidance as to what kinds of expansions, iterations, scalings, simplifications, and approximations are likely to work in differing mathematical contexts and which ones are likely to lead to a well posed formulation. In any complex problem on which a number of mathematically incompatible models have been brought to bear (for example, microscopic and macroscopic, ideal and dissipative), interpretation and synthesis of the disparate results in relation to the desired (unsolved) meta-problem is much more valuable than the raw, unevaluated data. The physical significance of singular and nonuniform results is generally meaningless if interpreted literally and is invaluable if put into proper context. Some of this is susceptible to proof; much is not. In large part it consists of being sensitive to mathematical warning signals that a formal procedure is about to run into a snag (this is analogous to the smell of burning insulation in an experiment). The misapplication of physical intuition (as derived from natural phenomena) to properties of equations is just as dangerous as is the misapplication of infinitely precise mathematical distinctions to nature (which latter error is, incidentally, the source of most "paradoxes" in physical science).

This type of **mathematical intuition**, used to guess at what is likely to produce a correct result, **is** mathematics even in the absence of proof, since it concerns mathematical structures, not directly nature. In a sense, mathematicians have given up a part of their birthright in downgrading qualitative and intuitive analysis of mathematical structures (leaving this important task to non-specialists). Restricting the subject matter of mathematics to what has been proved is both illogical and unwise.

As an illustration, let me relate a (true) anecdote concerning two scientists, one an applied mathematician, the other an experimental physicist, both listening to a theoretical lecture which involved a lengthy formal expansion leading to a result which was presented without further critical comment. Quoth the physicist, "He may have done the mathematics correctly, but he left out the physics." Replied the mathematician, "He may have done the physics correctly" (since formal calculations without error estimates are usually done by physicists), "but he left out the mathematics." In any event, from the terminology employed, both observers agreed that the most important component had been omitted.

A brief word about numerical computation is in order since this will inevitably be an essential tool in any complex field. We have observed an empirical rule that a modest amount of appropriate analytic preparation will frequently be rewarded a thousand fold in accuracy or calculation time.

In summary, since the traditional corrective action of experiment is weaker than usual in this field, more than the usual reliance on logical consistency and mathematical caution is demanded. In particular, if the model to be used has already been formulated, it is usually too late to take in a mathematical consultant. We remark that all the preceding generalities can be related to concrete examples.[14-18]

## MATHEMATICAL EXAMPLES

### Introduction

We briefly describe a class of related problems that serve to introduce some interesting new mathematical structures. The physical problems have great current interest and the mathematical formulations seem likely to lead to significant mathematical developments. They concern new types of well posed problems which have required development of a body of theory and discovery of practical techniques of solution; on the other hand, they are not so radically unfamiliar (all too common in plasma physics) as to lose contact with the body of established mathematics. They are related to ordinary and partial differential equations but are different from both; they can be termed nonlinear "functional" equations, but this serves no purpose. They exhibit the classical thread that a physical situation motivates the mathematical problem, but it does not give a precise formulation nor does it suggest practical methods of solution.

The family of problems concerns **adiabatic compression** and **resistive diffusion** of a plasma. Implicit in the word adiabatic and in the concept of (slow) diffusion as distinguished from (fast) wave motion and flow, is the concept of a quasi-static one-parameter family of solutions of the equilibrium pressure balance,

$$\nabla p = \mathbf{J} \times \mathbf{B}. \tag{1}$$

That we are concerned with a **family** of solutions of (1) comes from the (adiabatic) variation of external constraints such as coils, or through changes in internal pressure or current profiles induced by diffusion.

For simplicity (and almost by necessity)[19] we shall restrict ourselves to the two-dimensional form [(x,y), z ignorable]:

$$\begin{aligned} &\mathbf{B} = n \times \nabla \psi + nB_Z, \qquad n = \nabla z, \\ &p = p(\psi), \qquad B_Z = f(\psi), \\ &\Delta\psi = -\dot{p}(\psi) - \dot{f}(\psi), \qquad \dot{} = d/d\psi, \end{aligned} \tag{2}$$

or the axially symmetric form [(r,Θ,z), Θ ignorable]:

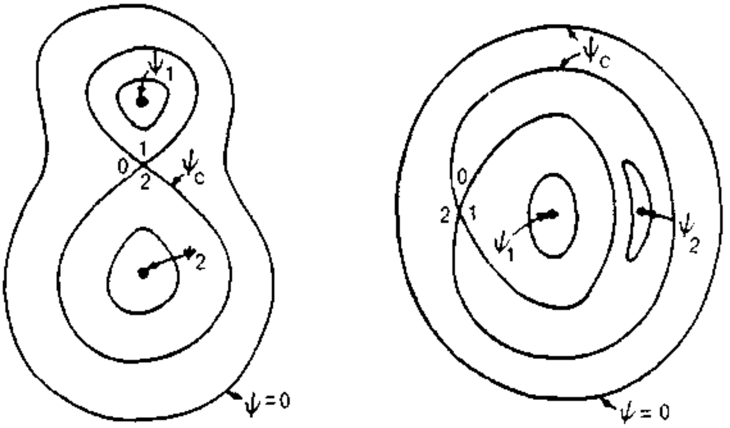
$$\begin{aligned} &\mathbf{B} = \nabla \Theta \times \nabla \psi + nB_\theta, \qquad n = r\nabla\Theta, \\ &p = p(\psi), \qquad rB_\theta = f(\psi), \\ &\Delta^*\psi \equiv r^2 \;\mathrm{div}(\nabla \psi / r^2) = -r^2\dot{p} - f\dot{f}. \end{aligned} \tag{3}$$

We are looking for a family of solutions of (2) or (3) which depends continuously on a parameter. If the parameter is contained in **given** functions $p(\psi, t)$ and $f(\psi, t)$ or in a varying domain or boundary condition [with $p(\psi)$ and $f(\psi)$ fixed], we have a classical elliptic problem with known criteria for uniqueness, bifurcation, etc. If the problem is **adiabatic**, the two given profiles $p(\psi)$ and $f(\psi)$ are replaced by two adiabatic constraints (profiles) which serve to determine p and f. For **diffusion**, there are additional evolutionary equations with time derivatives to govern the changing p and f profiles. In neither the adiabatic nor the diffusion problem is the formulation of a well-posed problem evident.

Both of these problems are very singularly related to the dynamical equations of motion of an ideal or resistive magneto-fluid. In broad outline, the ideal equations, linearized about a static equilibrium [viz. about a solution of (1)], can be related to a formally self-adjoint operator, L, whose eigenvalues, $\lambda = -\omega^2$, are the natural frequencies of oscillation. L is a sixth order operator whose spectrum contains continua, accumulation points, dense sets of points, and more pathology. With regard to the original question concerning a family of equilibria depending on a parameter, the significant feature is that if the origin, $\lambda = 0$, belongs to the spectrum of L for a given solution of (1), then no neighboring, linearly perturbed equilibrium exists. A **point eigenvalue**, $\lambda = 0$, in the linearized problem is a signal that there is a bifurcation of solutions in the nonlinear problem. Plausible arguments and examples can be given to show that a **continuum** which extends to the origin is a signal that the flux surface topology of the nonlinear solution is about to change, but (when correctly formulated), without any loss of uniqueness. There is also the possibility of simultaneous bifurcation and **islation** or **anislation** (appearance or disappearance of islands) for a point eigenvalue $\lambda = 0$ at the end of a continuum.

Flux conservation, implying invariance of all topological properties of field lines and flux surfaces, is a property common to both the linear and nonlinear ideal fluid equations of motion; it would therefore seem to be appropriate to adopt it as a constraint for any model of adiabatic variation. In particular, the value of the flux $\psi$ is constant at any critical point $\nabla \psi = 0$. This topological constraint prevents continuation of both the linear and nonlinear adiabatic models past a point at which a continuum touches the origin. We are therefore led to generalize the definition of adiabaticity, dropping the invariance of $\psi$ at a saddle point (the presumed mechanism is a dissipative boundary layer in the neighborhood of a separatrix). Simply allowing a change of topology would permit infinitely many continuations of the equilibrium. The situation is handled in a way which is analogous to the introduction of shocks in ideal fluid dynamics. Several (usually three) adiabatic regions which abut along a separatrix (cf. Figure 6) are joined by **jump conditions** analogous to the Hugoniot conditions across a shock wave. The matching conditions are suggested by appropriate conservation laws. This procedure turns out to specify the **unique** continuation of a **generalized adiabatic** process past a change in topology. As in shock theory, no properties of the dissipative mechanism need be known, so long as they are compatible with the hypothesis that the separatrix boundary layer is thin. Total flux is conserved but there is "breaking" and "reconnection" of magnetic lines; similarly, there is conservation of total mass, but redistribution among the regions.

**FIGURE 6**
ISLATED DOMAINS




**(a)** Doublet type

**(b)** Tearing type

Depending on whether appropriate regions grow or shrink, fluid and field can either mix or split; the two processes are evidently thermodynamically different. Problems of creation, growth, destruction, and interference of magnetic islands can be handled without overt reference to any dissipative mechanism. The evolutionary time scale is determined by the variation of the external constraints and not by a dissipative time scale.

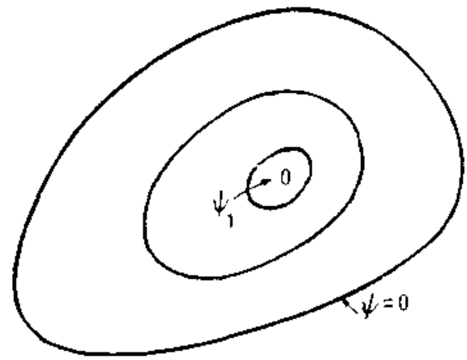
The present state of this subject comprises a modest number of proved mathematical theorems, somewhat more in the way of formal boundary layer and other expansions and examinations of singularities, and a large amount of empirical but accurate numerical confirmation of conjectured well-posed problems and of formal analytic singular and limiting behavior, the latter including, in particular, bifurcation, islation, and a host of boundary layers near the plasma edge, near a small emergent island and near the separatrix. The generalized adiabatic formulation with jumps has also been confirmed by numerical calculation of the resistive problem in the limit of small resistivity and, in a few cases, by comparison with plasma experiments.

If there is any point to this, one can also make an obvious connection with catastrophe theory.

## The Adiabatic Problem and Generalized Differential Equation in a Simple Geometry

After this qualitative introduction, we turn to a slightly more detailed exposition. To be precise, consider a simple two-dimensional domain as shown in Figure 7 and a solution of Eq. (2) with simple nested contours $\psi = \text{const}$ (as shown). For adiabatic constraints it is natural to introduce the volume $V(\psi)$ (area in two dimensions) within a flux surface (contour) $\psi = \text{const}$. The adiabatic constraints take the form

FIGURE 7
SIMPLE DOMAIN




$$p = \mu(\psi')^{\gamma}, \qquad ' = d/dV \qquad (\dot{} = d/d\psi).$$

$$f = \nu\psi', \tag{4}$$

where $\mu(\psi)$ and $\nu(\psi)$ are adiabatically invariant (given) profiles to replace p and f. Eliminating p and f in favor of $\mu$ and $\nu$ in Eq. (2) yields the rather opaque functional equation

$$\Delta\psi = -\dot{\mu}(\psi')^{\gamma} - \gamma\mu(\psi')^{\gamma-2}\psi'' - \nu\dot{\nu}(\psi')^2 - \nu^2\psi''. \tag{5}$$

To gain understanding we simplify by setting $\gamma = 2$, $\nu = 0$, $\mu = 1/2$, and obtain

$$\Delta\psi = -\psi'' \tag{6}$$

or for greater generality

$$\Delta\psi = F(V, \psi, \psi', \psi''). \tag{7}$$

This equation has been called a QDE (for "queer" differential equation or from the sequence O, P, Q) or, more prosaically, a GDE (generalized differential equation).

Even the simplest form (6) is rather unusual. On the left is an elementary elliptic operator on $\psi(x,y)$; on the right is the equivalent 1-D elliptic operator on $\psi(V)$; but the 1-D independent variable V is defined in terms of the contours, $\psi = \text{const}$, of the 2-D dependent variable. The fact that the 2-D and 1-D differential operators are the same order will be seen to be crucial.

To return to more familiar territory, consider the elliptic problem with $p(\psi)$ and $f(\psi)$ given. The correct elliptic boundary condition is to specify $\psi$, for example, $\psi = 0$ on the boundary. The value $\psi_1$ at the "center" (assuming that the topology happens to turn out as shown in Figure 7) is determined together with the rest of the solution. If the shape of the domain is altered, the value of $\psi_1$ will change. The adiabatic statement of flux conservation for the entire domain demands that $\psi_1$ be fixed under domain variation. In other words, to be physically appealing, the systems (5) or (6) [and, for mathematical reasons, also (7)] must allow specification of $\psi_1$ in addition to an elliptic boundary condition. This is not compatible with any known type of differential equation (it might result from an appropriate movable singularity at the "center," but this is not the correct description, and we are basically looking for smooth solutions).

The mathematical (heuristic, numerical, and probably rigorous) resolution of the question of well-posedness of Eqs. (5)-(7) becomes visible on taking the **microcanonical average** (volume weighted average, restricted to a flux surface) of the GDE. Without confusion [we have already used a common symbol for $\psi(x,y)$ and $\psi(V)$], we introduce $V(x,y)$ as well as $V(\psi)$, define

$$\langle\varphi\rangle = \oint \varphi(ds/|\nabla V|) = (d/dV) \int_{V(x,y)<V} \varphi\, dx\, dy, \tag{8}$$

where

$$\oint(ds/|\nabla V|) = 1, \qquad 1/\psi' = \oint(ds/|\nabla\psi|), \tag{9}$$

and verify

$$\langle\Delta\psi\rangle = (K\psi')', \tag{10}$$

where

$$K(V) = \langle|\nabla V|^2\rangle = \oint|\nabla V|\, ds$$
$$= (1/\psi')\oint(\partial\psi/\partial n)ds. \tag{11}$$

Let us detour for a moment with a few properties of the crucial geometrical quantity $K(V)$. First of all, K is fully determined by the family of contours, independent of the distribution of $\psi$ on the contours. Second, for the topology of Figure 7, $K(0) = 0$. For a shell $\psi_1 < \psi < \psi_2$, within which $\Delta\psi = 0$, the inductance of the shell is

$$L = \int_{V_1}^{V_2} dV/K. \tag{12}$$

It is easily shown that

$$K \geq \ell^2 \geq 4\pi V, \tag{13}$$

where $\ell$ is the length of the contour. The first relation comes from Schwarz's inequality, the second is the isoperimetric inequality (remember, V is area). The first relation is an inequality only if $|\nabla V| = \text{const}$ (equidistant curves), the second relation only for a circle. For a family of ellipses $x^2/a^2 + y^2/b^2 = \text{const}$, we have $K = 2\pi V(a/b + b/a)$. For an imminent change in topology at the origin, $\psi = x^2 + \epsilon y^2 + ay^4 + \cdots$, $K(V) \sim V^{2/3}$ $(\epsilon = 0)$. Briefly, K is minimized by a family of circles; it is large for elongated ellipses or corrugated contours; it is particularly large for elongated contours in the neighborhood of a degenerate critical point. These qualitative properties of K will allow qualitative estimates of physical behavior under adiabatic compression (and diffusion).

Returning to (10), $(K\psi')' = K\psi'' + K'\psi'$, we note that the center, $V = 0$, is singular for the averaged Laplace operator since the coefficient of $\psi''$ vanishes. The fact that no boundary condition can be specified at $V = 0$ for the 1-D averaged operator, $(K\psi')'$, is a reflection of the fact that $V = 0$ is an interior point of the domain for the original 2-D elliptic operator $\Delta$. For the full GDE (6), the coefficient of $\psi''$ in the averaged equation is $K + 1$, which does not vanish. The average of the GDE, (5) or (7), is always an ODE (taking K as given). Under appropriate (mild) restrictions, the center $V = 0$ is regular for this ODE, allowing $\psi(0) = \psi_1$ to be specified as a boundary condition.

From the above remarks, it is heuristically clear why specifying $\psi_1$ as well as an elliptic boundary condition is likely to be well-posed for the GDE. It is also clear that the **averaged GDE** must be used in any constructive approximation algorithm to allow the prescribed value of $\psi_1$ to enter the problem at all. The simplest algorithm (which turns out to converge remarkably well numerically) is to alternate between successive evaluations of the 2-D **contours**, $\psi$ = const, and the 1-D **profile**, $\psi(V)$. First, the GDE is used, with the right side treated as a given inhomogeneous term (or a given function of V or $\psi$) in order to determine a family of contours, thence $K(V)$; the values of $\psi$ are discarded. Next the averaged GDE is treated as a second-order ODE with $K(V)$ known and is solved (subject to obvious restrictions) taking $\psi = \psi_1$ at $V = 0$, as well as $\psi = 0$ at the outer boundary. The principal result is the solution of a specified equilibrium given **adiabatic constraints** $\mu$ and $\nu$ instead of elliptic constraints p and f; as a consequence, one also obtains a **family** of adiabatically related solutions as an external constraint is varied, keeping $\mu(\psi)$ and $\nu(\psi)$ fixed. This can be considered to be the solution of an appropriate (slowly varying) time-dependent problem.

The mathematical significance of the GDE is elucidated by embedding it in a sequence of problems:

$$\Delta\psi = F(\psi), \tag{14a}$$

$$\Delta\psi = F(V), \tag{14b}$$

$$\Delta\psi = F(\psi'), \tag{14c}$$

$$\Delta\psi = F(\psi''), \tag{14d}$$

In each case we suppose that the domain and the function F are suitably restricted to avoid complications. We now make a number of statements, some proved in restricted formulations, others apparently amenable to tools of conventional analysis but not yet attempted, still others in the form of supporting nonrigorous analytical and numerical evidence. Case (14a) is elliptic. Although (14b) is not elliptic (it is not a differential equation), it is **qualitatively** elliptic in that a solution is determined by an elliptic boundary condition, and the right-hand side is dominated by the left-hand side. The same is true of (14c). In other words, it is not the introduction of the highly implicit and nonlinear variable V that is important; it is the appearance of a second derivative $V''$ to interfere with the second-order Laplacian, as in (14d), which changes the nature of a well-posed problem.

**Islation and Generalized Adiabatic Constraints**

Now we turn to changes in topology resulting from a variation of constraints. In the elliptic formulation, a change in contour topology is not particularly significant, and the topology of the solution does not enter in existence proofs or practical methods of solution. For example, the elementary equation $\Delta\psi = 1$ will have simple contours in a convex domain, but the topology will change when the domain is sufficiently indented. Similarly, for the nonlinear $\Delta\psi = F(\psi)$, one can have a family of **unique** solutions in a family of domains with no singular behavior at a change in topology. The variational equation for the elliptic equation $\Delta\psi = F(\psi)$ in terms of $\varphi = \partial\psi/\partial t$ is

$$(\Delta - \dot{F})\varphi = 0. \qquad (15)$$

This equation makes bifurcation visible, but it exhibits no distinguishing features at a point of islation. The variational equation for the GDE (6) in terms of $\varphi = \partial\psi/\partial t$ is

$$(\Delta + \sigma)\varphi = -\langle\varphi\rangle'' - \sigma\langle\varphi\rangle, \qquad \sigma = \psi'''/\psi'. \qquad (16)$$

This equation **is** singular at a degenerate critical point such as $\psi = x^2 + y^4 + \cdots$, where $\psi'(0) = 0$, or on any **curve** where $\psi' = 0$ (this is topologically unstable). How to continue the solution past such a point is not obvious from the linear variational equation (which is a GDE, just as is the original nonlinear equation).

The correct treatment of islation (also of **anislation** — disappearance of an island) is necessarily nonlinear, but nonlinearity alone is insufficient for a change in topology. The matching conditions across a separatrix are, in principle, elementary, but they are subject to a number of subtleties such as differences between $\gamma = 2$ and $\gamma \neq 2$, differences between islands in 2-D and in axial symmetry, and different relationships toward conservation of energy and variational formulations in these various cases.

It is useful to point out that there are apparently **two** mathematically valid formulations of the equilibrium problem under adiabatic constraints ($\mu$ and $\nu$ formulation) in a complex geometry. One is to adopt **strict** flux conservation as suggested by the equations of motion, taking $\psi_c$ as well as $\psi_1$ and $\psi_2$ as given constraints (Fig. 6); $\mu_i(\psi)$ and $\nu_i(\psi)$ are given profiles for $i = 0,1,2$, the range of $\psi$ in each function being specified a priori. One can give heuristic iterative and variational arguments to indicate that this strict interpretation of flux conservation will frequently (e.g., in axial symmetry for $\gamma \neq 2$) lead to discontinuities of p, f, and $\psi'$ ($\psi$ is, of course, continuous) across the separatrix. This also implies the presence of a surface current, $[\nabla\psi] \neq 0$, at the separatrix. The local pressure balance

$$[p + \tfrac{1}{2}B^2] = 0 \tag{17}$$

will be maintained across the separatrix; this is merely the weak form of (1). Note that although this strict adiabatic formulation may allow one to solve for an equilibrium in a complex topology with strictly given adiabatic constraints $\mu_i$ and $\nu_i$, it does **not** allow the topology to change so long as the profiles $\mu_i(\psi)$, $\nu_i(\psi)$ are strictly preserved.

The surface current at the separatrix is one reason to expect nonconservation of the value of $\psi_c$ (Figure 6) since the separatrix is perpetually in a resistive boundary layer for any value of the resistivity, no matter how small. On the other hand, Faraday's law implies conservation of $\psi_1$ and $\psi_2$ even when they are separated by a resistive layer. Dropping the constraint on $\psi_c$ is the principal ingredient in the **generalized adiabatic** formulation in which **total** mass, flux, and volume are preserved, but not in the individual regions i = 0, 1, 2. The basic matching conditions are found to be

$$[p] = [f] = [\nabla\psi] = 0 \tag{18}$$

in addition to (17). Some of the most important auxiliary connection formulas are

$$1/\psi_0' = 1/\psi_1' + 1/\psi_2' \tag{19}$$

(involving conservation of volume and flux and absence of surface current). In the case of **mixing** (shrinking domain 0 in Figure 6(a) and growing domain 0 in Figure 6(b)), we have

$$\mu_0^{1/\gamma} = \mu_1^{1/\gamma} + \mu_2^{1/\gamma}\ , \tag{20}$$

while for **splitting** (growing domain 0 in Figure 6(a) and shrinking in Figure 6(b)),

$$\mu_1 = (\psi_0'/\psi_1')^\gamma \mu_0,$$

$$\mu_2 = (\psi_0'/\psi_2')^\gamma \mu_0. \tag{21}$$

The formulas (20) and (21) serve to extend the definition of the "given" function $\mu(\psi)$ when the domain of the independent variable $\psi$ changes.

**Resistive Diffusion**

In the limit of small resistivity, we expect diffusion to be a slow phenomenon compared to wave motion. A formal scaling can be given to decouple the two time scales (actually, two sets of time scales). The principal result is that the plasma is, at every instant, in static equilibrium, $\nabla p = \mathbf{J}\times\mathbf{B}$ ($\rho\, du/dt$ is dropped from the equation of motion). However, all other time derivatives such as $\partial B/\partial t$, $\partial p/\partial t$ are retained. In other words, the system of equations (which we exhibit later) describes a time-dependent (one-parameter family) of static equilibria, $\nabla p = \mathbf{J}\times\mathbf{B}$. Let us examine Ohm's law, which takes the form

$$\mathbf{E} + \mathbf{u}\times\mathbf{B} = \eta\mathbf{J}, \tag{22}$$

where $\eta$ is the resistivity. At a given instant we can consider $\mathbf{B}$ and $\mathbf{J}$ to be known, while **E** and **u** are not. In an infinite medium, $\eta\mathbf{J}$ can be arbitrarily split into two parts, **E** and $\mathbf{u} \times \mathbf{B}$, by choosing a moving frame. In a bounded domain, the resolution of $\eta\mathbf{J}$ into its two components is not arbitrary; this separation is, in fact, the object of the difficult and elaborate theory of diffusion. Consider first the two limiting cases, $\eta\mathbf{J} = \mathbf{E}$, $\eta\mathbf{J} = \mathbf{u} \times \mathbf{B}$.

If $\mathbf{E} = \eta\mathbf{J}$, from Maxwell's equations, $\partial B/\partial t + \text{curl}\,\mathbf{E} = 0$, and $\mathbf{J} = \text{curl}\,(\mathbf{B}/\mu_0)$, we obtain a simple diffusion equation:

$$\partial B/\partial t + \text{curl}(\eta/\mu_0 \text{curl}\,\mathbf{B}) = 0, \tag{23}$$

with diffusion coefficient $D_0 = \eta/\mu_0$. This equation describes the classical **skin effect** usually described as field or current penetration into a rigid conductor.

The other extreme, $\eta\mathbf{J} = \mathbf{u} \times \mathbf{B}$ is simplified by assuming that **u** is perpendicular to **B**; thus,

$$\mathbf{u} = -\eta(\mathbf{J} \times \mathbf{B})/B^2 = -\eta\,\nabla\,\mathbf{p}/B^2. \tag{24}$$

To further simplify, consider an isothermal case in which $\mathbf{p}/\varrho = \text{const.}$ Conservation of mass, $\partial p/\partial t + \text{div}(p\mathbf{u}) = 0$, therefore takes the form

$$\partial\,p/\partial t = \text{div}(\mathbf{p}/B^2 \quad \nabla\,p). \tag{25}$$

This is a somewhat unusual, nonlinear diffusion equation for p with a (linearized) diffusion coefficient $D_1 = \beta D_0$ (in laboratory plasmas $\beta$ can take values ranging from $10^{-8}$ to $10^2$). $D_1$ is frequently termed the "classical" coefficient to plasma diffusion.

One would be tempted to describe (23) as field diffusing through plasma and (25) as plasma diffusing through field. At first glance one might also think that only the **relative** diffusion matters. But in a domain with boundaries, **both** plasma and field diffuse (of course, not independently, since they are related by pressure balance). There are thus at least two phenomena, not always distinguishable, and the determination of **E** and **u** comes from the solution of a nonstandard global initial-boundary value problem.

The scaling referred to above, in which $\varrho du/dt$ was dropped, retains u in other equations of the system. For example, with a simplified treatment of energy conservation, the Grad-Hogan equations are

$$\nabla\,p = \text{curl}\,\mathbf{B} \times (\mathbf{B}/\mu_0),$$

$$\partial B/\partial t + \text{curl}(\mathbf{B} \times \mathbf{u}) + \text{curl}(\eta/\mu_0 \quad \text{curl}\,\mathbf{B}) = 0,$$

and

$$\partial p/\partial t + \text{div}(p\mathbf{u}) + (\gamma - 1)p \quad \text{div}\,\mathbf{u} = 0. \tag{26}$$

We have an isothermal or adiabatic plasma by setting $\gamma = 1$ or $\gamma > 1$, respectively. Evidently (assuming that the system makes sense), **u** must be determined at each instant in such a way that the time derivatives $\partial B/\partial t$, $\partial J/\partial t$, and $\partial p/\partial t$ are compatible with maintaining $\nabla\,p = \mathbf{J} \times \mathbf{B}$. In two dimensions and axial symmetry, $\mathbf{u} \cdot \nabla\,\psi$, the normal component of **u**, turns out to satisfy a linear GDE (with coefficients depending on the instantaneous equilibrium state). In a sense, this reduces the diffusion problem to one that is "known"; but this method of advancing the solution in time is much less efficient, numerically, than others which will be described later.

For purposes of illustration, we turn to a simpler model [which turns out to be the special case of (26) in two dimensions at low $\beta$, in which limit p and the two field components decouple]. Taking the flux function $\psi$ as in Eq. (2), the classical **skin effect** in a **solid conductor** is described by the diffusion equation

$$\partial \psi / \partial t = \eta \triangle \psi, \tag{27}$$

where $\eta$ is the (constant) resistivity; the current $\triangle \psi$ satisfies the same equation. In a **deformable plasma**, pressure balance requires identical flux and current contours,

$$\triangle \psi = F(\psi, t). \tag{28}$$

The two equations (27) and (28) are, in general, incompatible for any choice of F (the frequently solved cylindrical diffusion problem in which circular $\psi$ contours automatically coincide with $\triangle \psi$ contours is nonrepresentative). Since a plasma is deformable, we replace (27) by

$$\partial \psi / \partial t + \mathbf{u} \cdot \nabla \psi = \eta \triangle \psi, \tag{29}$$

and (as a result of scaling or merely for simplicity of the model), we also take

$$\langle \mathbf{u} \cdot \nabla \psi \rangle = 0 \tag{30}$$

on each flux contour (the deformation is incompressible, on the average). It is not immediately evident that the system (28)-(30) with an elliptic boundary condition (say $\psi = 0$ on a fixed or deforming boundary), plus initial specification of $F(\psi, 0)$ describes a well-posed problem and determines $\psi(x, y, t)$, $F(\psi, t)$, and $\mathbf{u} \cdot \nabla \psi$. (The relation to the GDE formulation is hidden is this model).

A heuristic indication that this dissipative system is well-posed (impractical numerically, but useful for analytic results) goes as follows.[17] Differentiating the constraint $\triangle \psi = F(\psi, t)$,

$$\Delta(\partial \psi / \partial t) = (dF/dt) + \dot{F}(\partial \psi / \partial t). \tag{31}$$

Taking G to be the inverse (Green's function) of the operator $\triangle - \dot{F}$,

$$\partial \psi / \partial t = G(dF/dt) + g, \tag{32}$$

where g is the contribution from the boundary. Averaging on a flux contour

$$\eta F = \langle G \rangle (dF/dt) + \langle g \rangle \tag{33}$$

or

$$dF/dt = \eta \langle G \rangle^{-1} F - \langle G \rangle^{-1} \langle g \rangle. \tag{34}$$

The Green's function $\langle G \rangle$ is averaged with respect to each pair of variables. For a "reasonable" set of contours, $\langle G \rangle^{-1}$ has been shown to be approximately a Sturm-Liouville operator.[20] Thus F evolves in time as the solution of a 1-D diffusion-type equation (34). After solving for F, $\psi$ is obtained from (28), and $\mathbf{u} \cdot \nabla \psi$ from (29).

The same averaged Green's function technique can be used to formally solve the linear variational equation (16) in the case of the adiabatic GDE

$$\varphi = -G(\langle\varphi\rangle'' + \sigma\langle\varphi\rangle). \tag{35}$$

Averaging and inverting $\langle G\rangle$ yields

$$\langle\varphi\rangle'' + \langle G\rangle^{-1}\langle\varphi\rangle = -\sigma\langle\varphi\rangle. \tag{36}$$

The principal part (ignoring bounded operators) is the sum of two Sturm-Liouville operators and is invertible with suitable restrictions.

An alternative solution scheme, which is very efficient numerically (though less tractable analytically), is to average (29), viz.

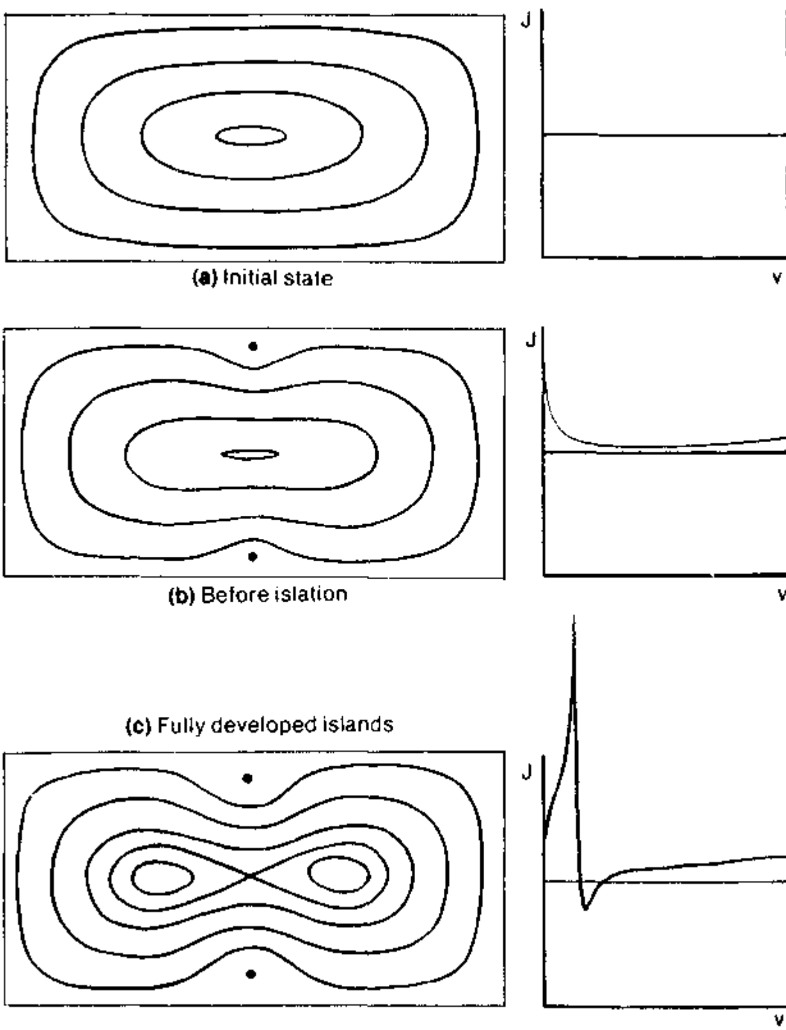
$$\psi_t = \eta(K\psi')', \qquad (K\psi')' \equiv F. \tag{37}$$

If the geometrical coefficient $K(V,t)$ were known, the profile $\psi(V,t)$ would be obtained as the solution of this conventional 1-D diffusion equation. The inductance, K, which reflects the 2-D origin of (37), is recalculated at appropriate time intervals by computing contours from $\triangle\psi = F$ where F is the end result of an interval of 1-D diffusion of (37). Since the geometry usually changes relatively slowly (compared to 1-D profiles which may contain boundary layers, etc.) this formulation successfully solves an unusual, implicit 2-D diffusion equation by the expenditure of not much more computing time than is appropriate for a 1-D standard diffusion problem.

It is instructive to compare (29) with its mean value (37). The convective term $\mathbf{u}\cdot\nabla\psi$ is highly implicit and nonintuitive since it arises in response to the changes in shape of the contours required to maintain $\triangle\psi$ constant on $\psi$ contours. The changes in shape are incorporated into (37) (which has no convection) through the inductance coefficient $K(V)$ which, after a little practice, becomes very intuitive.

For example, in Figure 8 are presented the results of a numerical solution of Eqs. (28)-(30), using the methods just indicated. The initial state, Figure 8(a), represents a belt pinch with $J(V) = \text{const}$ [the diagram on the right is $J(V)$]. A pair of coils in the vacuum region between the plasma and the rectangular box is used to compress the waist of the plasma. Figure 8(b) is just before islation, and Figure 8(c) some time later. The corrugation near the edge of the plasma increases the value of K and gives a small current increase toward the plasma edge (the boundary condition has been chosen to eliminate the skin current which would give a sharp negative peak). The elongation of the flux contours near the center just before islation gives the current peak at the center in Figure 8(b). The double peaked current layer near the separatrix in Figure 8(c) is a consequence of the fact that $K(V - V_c) \sim -\log|V - V_c|$ is a moving singularity. A simple analytic exercise, solving (37) explicitly with a given $K = K(V - ct)$ (containing the logarithmic singularity) for a steady state $\psi(V - ct)$ gives the same type of double peaked current. In particular, the exact solution of the diffusion equation gives a cusp at the positive peak and a rounded negative peak, as indicated by the

**FIGURE 8**
TRANSITION FROM BELT PINCH TO DOUBLET




numerical computation. It is worth remarking that the detail in the 1-D current profile is accurate on a much finer scale than the 2-D mesh size.

One crucial qualitative point can easily be missed. In a solid conductor, flux and current diffuse identically. In a deformable plasma, the flux $\psi(V,t)$ again satisfies a diffusion equation; changes at the boundary penetrate to the interior very slowly, via a boundary layer if $\eta$ is small. The current density does **not** satisfy a diffusion equation; viz., with $\eta = \text{const}$, $J \equiv (K\psi')'$,

$$J_t = \eta(KJ')' + (K_t\psi')'. \tag{38}$$

The additional term, involving $K_t$, is an **adiabatic** change in J caused by the change in shape and penetrates instantaneously. The common reference to "current skin penetration" is misleading. Numerical results very similar to

Figure 8 are obtained from the adiabatic GDE, using appropriate jump conditions across the separatrix (except for the current layer in Figure 8 very near the separatrix, which is resistance-dominated).

The relation of this diffusion problem in the limit of small $\eta$ to the appropriate (low $\beta$) adiabatic problem is illuminating. Setting $\eta = 0$ in Eq. (37), we obtain $\psi_t = 0$; the profile $\psi(V)$ is invariant as the geometry changes in time. The fact that $\psi(V)$ is invariant can also be deduced as a singular limit of the adiabatic GDE (5), given $\mu(\psi)$ and $\nu(\psi)$. The precise formulation of the **limiting** adiabatic problem [replacing (5)] is to determine the geometry given the profile $\psi(V)$ [this profile problem could be added as a fifth item in the list (14)]. Specifically, given a domain with volume $\overline{V}$ and a function $\psi(V)$, $0 < V < \overline{V}$, determine $\psi(x, y)$ such that $\Delta\psi = f(\psi)$ for some f. There are several effective numerical methods of solving this profile problem by interation. One is to introduce V(x, y) instead of $\psi(x, y)$ through

$$\Delta V = K' + (\psi''/\psi')(K - |\nabla V|^2). \tag{39}$$

Here $\varphi(V) = \psi''/\psi'$ is given and V(x,y) is to be found. More interesting is the following variational formulation. Take the conventional variational function (Dirichlet principle)

$$U = \int \tfrac{1}{2} |\nabla\psi|^2 dx\, dy, \tag{40}$$

where admissible functions $\psi(x,y)$, defined in a simple 2-D domain, are assumed to be compatible with a **given** $\psi(V)$. With this class of admissible functions $\psi(x,y)$, U turns out to be stationary for $\psi = \psi_0(x,y)$ such that $\psi_0 = \text{const}$ and $\Delta\psi_0 = \text{const}$ contours coincide. A member of the admissibility class can be alternatively described by specifying a family of curves which simply cover the domain as in Figure 7; given the curves one calculates V and assigns $\psi$ according to $\psi(V)$, then calculates $\psi(x,y)$, hence U. In this formulation the **geometry** and **profile** parts of the various formulations (14) are visibly distinct.

As a final comparison between the resistive and adiabatic problems we present a table of singular behavior near a separatrix:

| **Adiabatic** | **Resistive** |
|---|---|
| $J \sim V^{-1/3}$ (acute) | $J \sim 1$ |
| $K \sim 1$ | $K \sim \log\|V\|$ |
| $K' \sim V^{-1/3}$ (acute) | $K' \sim 1/V$ |
| $\psi' \sim 1$ | $\psi' \sim 0$ |

The statement $J \sim V^{-1/3}$ (acute) means that J has this unbounded behavior on the acute side of the separatrix, but is bounded on the obtuse side. Note that $J \sim 1$ near an acute corner automatically implies that $V \sim \psi \log \psi$; to have $\psi' \sim 1$ [for the adiabatic problem to be meaningful, $p = \mu(\psi')^\gamma$ cannot be zero], J must be unbounded. J is more singular in the adiabatic case. K in the resistive. There is evidently a very complicated transfer of singularities in the limit $\eta \to 0$ [e.g., for very small $\eta$ there is a transfer from $K' \sim V^{-1/3}$ (acute) near the separatrix to $K' \sim 1/V$ **very** near the separatrix].

One physical conclusion is worth repeating. The time scale for magnetic field line "breaking" and "reconnection" (which are terms used in astrophysics) is not related to that of any dissipative mechanism; compared to the latter, changes in topology occur arbitrarily rapidly. This has been clearly demonstrated, analytically and by adiabatic and resistive numerical calculations.[21-30]

## CONCLUSION

In a period of over twenty years, a new scientific discipline has arisen, based on the goal of magnetically confining a hot plasma under conditions appropriate for the release of energy by nuclear fusion. New experimental techniques have been developed and refined by orders of magnitude; new technologies have begun; new (as well as applications of old) physical and mathematical theories have appeared and in some cases reached a level of scientific maturity. Effective use is being made of large-scale computing for instantaneous and retrospective interpretation of the overwhelming quantity of experimental data; computer use as an imaginative supplement to theoretical analysis and as an independent source of physical and mathematical "experimental" information through mathematical modeling has already had significant impact on the understanding of plasma phenomena.

The almost incredible advances in the accuracy of physical measurements and in control of plasma, and to a lesser extent in externally visible plasma confinement landmarks and in plasma understanding are the most obvious indications that we shall successfully overcome the (still undefined) enormous difficulties that are certainly ahead. Equally important is the flexibility inherent in the almost unlimited variety of plausible options that are available; wherever one obstacle has appeared, three solutions have presented themselves. The present state-of-the-art is sufficiently fluid so that maximal orderly speed toward the goal of economical fusion power requires examination of as many carefully selected options as financial exigencies will permit, rather than arbitrarily closing down on the one or two that appear to be "most likely." In any event, there is no doubt that in the ultimate future, whatever the date, fusion will take the position as prime energy resource away from oil and coal (with fission as a necessary but unpleasant holding action, and solar, geothermal, etc. as supplemental resources, depending on local conditions).

Mathematically, the welding of numerical experimentation with traditional analysis to extend the power of mathematics in order to be able to act effectively on a practical time scale is only beginning to make itself felt. Since the digital computer is essentially a purely mathematical device (more precise and demanding of rigor than many mathematicians), this relationship is a natural one. Perhaps the most significant feature, practically, is that no conceivable future generation of computing machines is large enough to allow routine solution of plasma reactor problems; optimization, efficiency, and cleverness at the analysis-computing interface, however, have empirically been found to be worth many orders of magnitude (or generations of computer hardware).[31] In particular, to determine what is far in the future or what may soon be at hand is not a simple evaluation.

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