Magnetic Confinement Fusion

H. P. FURTH

Fusion plasmas with reactorlike temperatures have been confined in magnetic-field configurations of the tokamak type. The measured rate of heat leakage from these plasmas is sufficiently small to be compatible with the requirements of a full-sized fusion power reactor. Improvements in other aspects of reactor performance are still needed, however, and the high cost of reactor-development steps has become an obstacle on the path to practical fusion power.

BEFORE JOINING PROJECT SHERWOOD AS A GRADUATE student in 1956, I had only a rough idea of what I was getting into. In those days, any kind of fusion research was classified—which tended to slow down the flow of scientific gossip. Both the grandeur of Project Sherwood's goals and the modesty of its experimental achievements (*1*) came as a surprise to me.

Progress on the theoretical side of fusion research (*2, 3*) was already striking in 1956. There were detailed visions of "toroidal magnetic bottles" holding hundred-million-degree fusion plasmas, which have meanwhile turned out to be quite accurate. As to how long the harnessing of fusion power was going to take, opinions were divided. In those salad days of nuclear energy development, some eminent optimists thought that 20 years was long enough to do just about anything. Others felt that the confinement of high-temperature plasmas for fusion power generation might well exceed the skills of 20th-century science—and their pessimism was deepened by the experimental failures of the 1960s. As I write this introduction, I am enjoying thoughts of how the typical optimist and the typical pessimist of 1956 would have reacted to a preprint of the present article.

Fusion experiments are now releasing up to 40 kW of nuclear power from deuterium plasmas (Fig. 1), by means of the fusion reactions

$$\begin{aligned} D + D &\rightarrow T\ (1\ \text{MeV}) + p\ (3\ \text{MeV}) \\ &\rightarrow {}^3\text{He}\ (0.8\ \text{MeV}) + n\ (2.5\ \text{MeV}) \end{aligned} \qquad (1)$$

These experiments make use of the tokamak configuration (Fig. 2), which was first developed at the I. V. Kurchatov Institute in Moscow (*2*). Substituting a deuterium-tritium (D-T) plasma mixture and using the much livelier reaction

$$D + T = {}^4\text{He}\ (3.5\ \text{MeV}) + n\ (14\ \text{MeV}) \qquad (2)$$

will give about 300 times greater projected yield from the existing deuterium plasma regimes—corresponding to peak fusion-power releases above 10 MW. Figure 1 illustrates that the generation of the fusion power output P_F in tokamak experiments to date has depended on the presence of a power input P_H that maintains the plasma temperature against heat loss to the cold surroundings. The magnitude of the ratio $Q = P_F/P_H$ is seen to have improved more than a factor of 10^4 during the past 15 years.

The Tokamak Fusion Test Reactor (TFTR) in the United States and the Joint European Torus (JET) in England are carrying out plasma confinement studies (Fig. 3) directly in the 100-million-degree reactor-plasma regime (*4, 5*). Both have good technical prospects of reaching break-even in the D-T burning phase, where about 30 MW of heating power input will produce another 30 MW of fusion power release, for a total heat output of about 60 MW. Raising the Q-value beyond the break-even level will be made easier by the energetic α-particles (the ^{4}He nuclei) of the D-T reaction, which will take over a growing share of the plasma-heating function. There is now general agreement about the feasibility of a next-step tokamak device with fusion-power generation in the 100- to 1000-MW range and a Q-value of the magnitude desired for a practical reactor ($Q \sim 30$). Reading such good news, the optimist of 1956 would probably claim to be at least partly vindicated, while the pessimist might confess to a sense of agreeable surprise. Both would imagine that an intensive effort must now be under way to profit from the unlocking of the fusion energy source—and both would be surprised by the rest of this article.

Fusion Power Development

Because the present expectation of mankind is that our standard of living as well as our total number will continue to rise during the next century, various disorders of our environment are also likely to be on the rise. Disorderly processes can be arrested, or even reversed, by the expenditure of sufficient energy, but once the generation of energy itself becomes a prime contributor to environmental disorder, the outlook is bleak.

A recent National Research Council report (*6*) cites the environmental issue as a persuasive reason for pursuing the fusion energy option. Fusion reactors, like coal burners and fission breeders, lend themselves to the economical production of base-load electric power. A general environmental advantage of nuclear power plants (fission or fusion) is the minimization of mining requirements and noxious effluents. A further advantage of fusion, relative to fission, is the absence of melt-down dangers and long-lived radioactive wastes (*7*).

The only unavoidable "ash" of the D-T fusion reaction is ordinary helium gas. What sort of secondary nuclear reactions are caused by the accompanying 14-MeV neutrons depends on the engineering choices that are made for the reactor blanket and structural materials. Progress in materials science could lead to still shorter time scales for radioactive decay (months rather than decades). A recent review of the materials science aspects of magnetic fusion is provided by Holdren *et al.* (*7*). Further progress in the physics of plasma confinement may allow the burning of fusion fuels with higher atomic numbers, such as ^{3}He or even ^{6}Li, which have fusion

The author is at the Princeton Plasma Physics Laboratory, Princeton, NJ 08543.

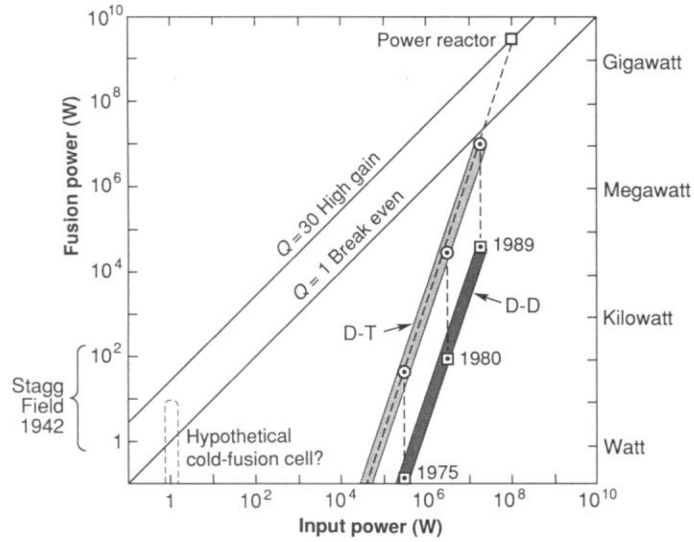

Fig. 1. The ratio $Q = P_F/P_H$ of fusion output power (P_F) to heating input power (P_H) needs to be about 30 for an economical power reactor. The Q-value of magnetic fusion experiments has been advancing steadily, but the associated power levels have become inconveniently high. For comparison, the output power of the Stagg-Field fission reactor experiment is shown, along with the hypothetical performance of a "cold fusion cell."

reactions that release most of their energy in charged particles rather than neutrons. The potential for long-term perfection through the evolution of technical expertise is one of the attractive features of the "fusion energy path."

Turning to the special obstacles in the way of the development of controlled fusion, the historical challenge was to show that hot-plasma confinement could be made to work at all. Initial results were discouraging, but high-powered experiments of recent years seem to have laid to rest the issue of basic scientific feasibility: The quality of plasma confinement currently being measured at reactor-level temperatures can be extrapolated to a full-scale power reactor of typical present-day power rating.

The true obstacle to the development of fusion is that high-powered (and correspondingly expensive) research facilities are needed at each step of the reactor development path. The present experimental trend points to a D-T break-even threshold around $P_F = P_H = 30$ MW (Fig. 1). By way of contrast, the figure also shows the output range of the first power-producing fission reactor at Stagg Field (0.5 to 200 W). In the case of fission, the initial demonstration of net power production had to be followed by a gradual advance towards useful power levels. Fusion will have the advantage of demonstrating net power production at levels that are directly in the reactor-relevant range—but, in the meantime, the optimization of fusion reactor concepts and technologies is greatly impeded by the required size and cost of each new experiment.

Just how much the popularity of fusion energy would benefit from a cheaper development path was brought out in a semi-serious way by last year's "cold fusion" events (*8*). The instant enthusiasm for "small versus large" did not relate to ultimate fusion power stations: Electric plants of the same output ratings tend to have similar overall size—and the prospect of building power stations out of billions of palladium cells, instead of a few tokamaks, would not be a clear winner. The instantly appealing feature of cold fusion lay in the development area: the reactor principle could be tested and optimized at very modest cost and on a relatively short time scale.

The absence (thus far) of a magical short cut to the development of fusion power does not diminish the attractiveness of the goal and should not discourage the hope that some real-world breakthroughs may yet occur. The following sections of this article discuss some key areas of scientific and programmatic opportunity for the next phase of fusion research. The discussion focuses on the tokamak as the leading representative of magnetic confinement fusion. Some alternate solutions of the toroidal confinement problem, such as the stellarator and reversed-field pinch (*4*), are showing significant promise of their own, as well as contributing to a common physics understanding. There is also a major U.S. effort in inertial confinement fusion (*4*), which is aimed at defense objectives in the near term, but provides a possible alternative fusion-power source that depends on basically different technological developments.

Improving the Tokamak Concept

The 1950 proposal by Igor Tamm and Andrei Sakharov for a power-producing tokamak reactor (*2*) is doubly fascinating: (i) it foresees much of the basic physics of tokamaks, including their special promise, and (ii) it describes a power source of such huge physical size and patently low cost-effectiveness as to invite immediate rejection of the whole tokamak idea. In the United States, Lyman Spitzer's stellarator proposal had similar characteristics. Remarkably, the birth of controlled fusion research was inspired by dreams of football-field-sized D-T burning monsters (*9*) with unit power outputs in the range 1 to 10 gigawatts electric (GWe).

Proponents of advanced energy sources are faced with a basic dilemma: whether to assume tomorrow's technology at the risk of seeming frivolous, or to assume today's technology at the cost of making unattractive proposals. The general historical record supports the realism of the first approach—and this has also proved to be true for magnetic fusion. The advent of high-field superconductors during the 1960s and '70s made prior fusion reactor studies obsolete, and the recent excitement about higher temperature superconductors has served to remind us that magnet technology is still on the move. The apparent slowness of fusion research in delivering a useful product has given rise to the conjecture that the quality of the reactor goal must be deteriorating as well (*10*)—but in the case of the tokamak the opposite has actually been happening. Recent reactor design studies (*11, 12*) have held to a unit power rating of about 1 GWe, while advancing monotonically towards more attractive reactor features, such as higher power density and quasi–steady-state operation.

The density of fusion power generation is roughly proportional to the square of the plasma thermal pressure: $(nT)^2$ where n is particle density and T is temperature. To maximize power output for a given investment in magnet coils and supporting structure, one would generally like to increase the ratio of average plasma pressure to magnetic field pressure $\beta = 8\pi\, nT/B^2$. Fortunately, the ability to predict gross magnetohydrodynamic (MHD) properties—such as the stability limit for the plasma β-value—has advanced greatly over the years. During the 1950s, there still were many puzzling anomalies, but the inclusion of small, vital effects, such as the finiteness of the electrical conductivity and of the ion gyro-radius, has led to a close correspondence between theory and experiment (*13*). The availability of high-powered computers has allowed this basic physical understanding to be applied to complex three-dimensional magnetic-field configurations and nonlinear instability phenomena (*4, 14*).

Parenthetically, one should note that there have been substantial corollary benefits as well. The past three decades of intensive worldwide research on the stability of magnetically confined plasmas have led, in turn, to major advances in modern computer science, as well as sparking a revitalization of the mature scientific field of classical mechanics. Areas in which fusion research has made notable contributions include the theory of ergodicity, with its associated topics of "magnetic island" formation and transition to chaos (*15*). The advances in classical mechanics have had a direct impact on our understanding of space plasmas. There have also been some notable high-technology spin-offs from the international fusion research effort—including new sources of x-rays and ultraviolet light, as well as gyrotrons and free-electron lasers.

The target β-value for fusion experimentalists used to be "at least 5%"—to which the cautionary note was commonly added that the tokamak configuration would have a hard time meeting this goal. By following the guidelines of MHD theory (see for example Fig. 2B), a number of tokamaks have now exceeded the 5% mark, with the DIII-D device at General Atomics (*16*) achieving a 10% β-value. Characteristically, the coming of experimental success has encouraged the imposition of more stringent requirements in the latest tokamak reactor studies (*12*), which look toward reactor plasmas of larger toroidal aspect ratio R/a (see for example Fig. 2C) and lower plasma current than would be compatible with the geometry of the present-day high-β experiments. On the hopeful side, MHD theory points to opportunities for meeting these more stringent geometric demands while reaching still higher β-values. A leading prospect is the so-called "second stability regime" of the tokamak (*17*), which has been likened to a Chinese finger-trap: The harder the plasma tries to get away, the more firmly it is gripped by the magnetic field lines (as indicated by the outward-shifted poloidal flux surfaces in Fig. 2C). It remains to be verified that a regime of second stability exists and that energy transport in such a regime does not grow unacceptably large.

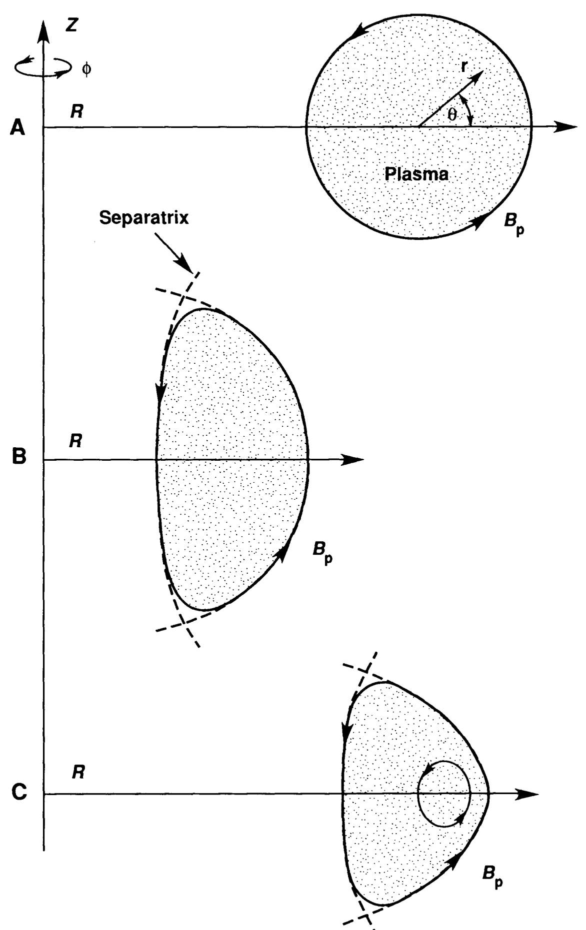

Fig. 2. The classical tokamak (**A**) is a toroid of round minor cross section, with a strong toroidal (ϕ-directed) magnetic field component B_t and a weaker poloidal (θ-directed) component B_p, which is generated by a toroidal plasma current. Vertical elongation (**B**) helps to raise the MHD stability limit governing the plasma β-value, and is accompanied by a magnetic separatrix that can be useful for guiding the plasma outflow into a "divertor" pumping system. Reactor geometry (**C**) calls for the achievement of high β-values at large aspect ratio R/a.

The desire of reactor designers to minimize the tokamak plasma current springs from a number of practical engineering considerations, which become particularly compelling if the goal is true steady-state reactor operation. Present-day tokamak experiments typically use an air-core transformer to induce the desired toroidal current—an approach that provides good electrical efficiency but implies limited pulse duration. The largest present-day tokamaks of the transformer-driven type have current-pulse durations that are only in the 10-second range; whereas reactor-scale tokamak plasmas would be limited to multihour-long burn pulses. Various types of noninductive, quasi–steady-state current drive have also been demonstrated, with the most notable parameters being achieved by microwave techniques in Japan (*4*): The JT-60 has driven plasma currents up to 2 MA in this way, and the smaller TRIAM device, which has superconducting external magnet coils, has recently achieved tokamak plasma durations that exceed 1 hour.

The main drawback of such noninductive current-drive techniques is a loss of electrical efficiency, with associated input-power requirements that provide the reactor plasma with a needlessly large source of auxiliary heating, thus depressing the Q-value. A possible solution is provided by transport theory, which has long predicted that the collisional outward diffusion of a hot tokamak plasma could generate a major spontaneous contribution to the tokamak current by the so-called bootstrap effect (*18*). Recent experiments on large tokamaks have verified the reality of the bootstrap current (*4, 5*), thus providing hope for a steady-state tokamak solution consistent with the desired goal of $Q \sim 30$. For this purpose, the ability to operate at high β-values and relatively low plasma currents would be particularly helpful.

Any kind of long-pulse or steady-state fusion-reactor operation depends, of course, on control of the plasma fuel mixture. Effective techniques for injecting fresh fuel—for example, in the form of high-velocity frozen D-T pellets—have already been demonstrated. The use of pellet injection on full-scale reactors would require higher velocities than the 2 km/s commonly achieved today. The exhaust of the helium ash and the avoidance of heavy impurity-ion influx seem to present even more challenging problems. A key role could be played by the establishment of external control over various subtleties of the plasma-transport process—which can act either to concentrate or dilute non-hydrogenic ions.

Understanding Plasma Transport

Further reductions in the gross magnitude of plasma transport will not have such a direct impact on the projected cost of D-T fusion energy as raising the tokamak β-value or the burn-pulse duration while preserving a high energy-multiplication factor Q. The minimum D-T tokamak reactor size tends to be set by constraints beyond plasma physics, such as the desired neutron flux at the first wall and the minimum thicknesses of the reactor blanket, shielding, and superconducting magnet windings. There is little question, however, that a more sophisticated understanding and control of plasma transport will continue to play a central role in bringing about the advances desired in regard to the β-value, plasma purity, and other aspects of tokamak performance.

For the near term, a simple reduction in the magnitude of transport coefficients would tend to have a strong impact on the economics of fusion development: D-T burning experiments aiming at high Q-values will have to minimize size and cost to break

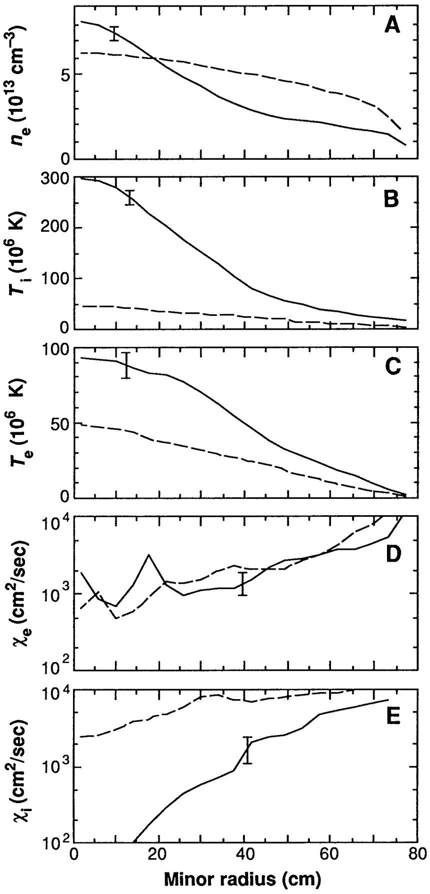

Fig. 3. Well-confined TFTR plasmas (solid-line profiles) have average ion temperatures T_i in the 100-million-degree range, along with heat-transport coefficients χ_i and χ_e in the range $<10^4$ cm^2/s, as desired for a practical fusion reactor. A modest change in the density profile n_e near the plasma edge (dashed-line profiles) can give rise to a more poorly confined plasma regime (*L*-mode), in which the ion temperature drops significantly. The profiles are shown here as functions of minor radius r after computer-averaging with respect to the poloidal angle θ. [Experiment carried out by M. Zarnstorff *et al.* (*4*).]

through the present "affordability barrier." In the longer term, major improvements in the quality of confinement would still be valuable in opening the door to the use of non–D-T fusion reactions. (*19*). Aside from these obvious benefits to fusion research, the understanding of high-temperature plasma transport phenomena ranks as one of the truly fundamental challenges of modern physics—with broad relevance to problems in near-space, astrophysics, and advanced technologies.

At present, tokamak designs are based on scaling laws of plasma transport that are basically empirical. High-powered experimental research on transport is one of the main activities in large tokamak facilities such as TFTR (see for example Fig. 3). Time-dependent minor-radius profiles of temperature, density and other relevant plasma parameters are measured and confirmed by multiple diagnostics (*20*). The data are processed (partly on-line) by computer programs (*21*) that deduce the plasma transport coefficients, as well as various atomic and nuclear phenomena. Theories about the physics of transport can be tested by introducing local perturbations in the heating or fueling source terms and by resolving the microscopic structure of allegedly transport-inducing plasma fluctuations.

In the absence of any magnetic field—or in a grossly unstable magnetic field configuration—one would expect to see a 100-million-degree deuterium plasma vanish at the thermal velocity $v_{th} \sim 10^8$ cm/s. The magnetic-confinement experiments of the 1960s were characterized by anomalous transport velocities of order $v_{Bohm} \propto v_{th}(\rho/a)$, where ρ/a is the ratio of the gyro-radius to the plasma minor radius. Transport coefficients of order $D_{Bohm} \propto a\, v_{Bohm}$ had been "predicted" by David Bohm in the 1940s, but remained inexplicable in terms of known theoretical transport mechanisms—as well as being several orders of magnitude too large for practical reactor purposes. By comparison, present-day plasmas have typical transport velocities scaling like $v_{th}(\rho/a)^2$ that are only of order 10^2 cm/s. The associated transport coefficients are of order 10^4 cm^2/s—which is good enough for a 1-GWe reactor.

The reduction of plasma transport to a second-order effect in ρ/a has brought it within the reach of drift-wave theory (*22*). The observed transport scaling lends itself naturally to explanation in terms of drift-instabilities, as do the relative magnitudes of various transport coefficients (including some not shown in Fig. 3, such as the particle and impurity-ion diffusion rates and the plasma viscosity). The fluctuation measurements also exhibit microscopic features that are congenial to drift-wave theory, such as the observed dependencies of fluctuation amplitudes and frequencies on the wave vectors parallel and perpendicular to **B**. In these respects, drift-wave transport theory does seem to hold considerable promise—but the experiments have also shown an impressive ability to knock down detailed transport models about as quickly as they can be set up. In addition, there has been great difficulty in coping with some quite general anomalies of tokamak transport, such as the tendency for the amplitude of transport coefficients to increase rather than decrease towards the cold plasma edge. A plausible interpretation is that residual MHD phenomena may become dominant near the edge—partly because of the rise in electrical resistivity and perhaps partly because of the appearance of atomic effects and the loss of ideal geometric symmetry at the interface of the hot plasma with the surrounding world. Explanation of the observed edge anomalies would take care of the old Bohm anomaly as well: The small plasmas of the 1960s were simply "all edge," whereas present-day plasmas have Bohm-like edges enclosing relatively well-behaved and increasingly large hot-plasma interiors.

Given the present intensity of theoretical-experimental confrontation, the outlook for improved understanding of the physics of tokamak transport appears to me to be quite good. Fusion research seems, furthermore, to resemble other areas of applied fluid dynamics in its ability to make effective use of empirical scaling relations: The lower bound (*L*-mode) formula for tokamak plasma confinement (*23*), which was proposed well before the start of high-powered experiments, in TFTR and JET, has turned out to be remarkably accurate. The prospects of improving confinement also look favorable, precisely because transport at the plasma edge plays such an important role, and because the edge plasma is relatively accessible for modification. Improvement factors of order 2 to 3 in global energy confinement are typically achieved by present-day techniques for optimizing the interaction of the hot-plasma edge with the surrounding wall. The magnitude and shape of the electrostatic potential near the plasma edge appears to be particularly important, suggesting a role for externally imposed potential-control techniques to enhance bulk-plasma confinement while excluding impurity ions.

Burning-Plasma Experiments

If all the energy of fusion reactions were released in neutrons, the plasma physics and nuclear physics aspects of fusion research would be entirely decoupled. The plasma effects caused by the D-T α-particles, however, are potentially very important (*24*): (i) the pressure of the α-particle minority population can excite new types of collective modes—or help stabilize familiar ones; and (ii) the 20% of the D-T fusion power that goes into the α-particles can take over the plasma-heating function (that is, can "ignite" the plasma) when P_F equals five times the rate of plasma heat loss—while introducing new problems of thermal stability as $Q = P_F/P_H \rightarrow \infty$. Practical magnetic fusion reactors are designed to operate somewhat below the ignition point, typically around $Q \sim 30$.

The most effective way to approach the study of α-particle collective phenomena is to use a hot non-Maxwellian D-T plasma whose energetic-ion tail makes the principal contribution to the fusion-reaction rate. This non-Maxwellian approach (*25*) maximizes the α-particle source strength relative to the α-thermalization rate, thus reaching reactorlike levels of the α-particle minority population. The 1976 TFTR Project Plan (*26*) called specifically for the enhancement of fusion power by means of the non-Maxwellian effect, and established goals for plasma performance and fusion yields (notably the release of 1 to 10 MJ of D-T energy per pulse) that have meanwhile proved to be fully realistic (*5*). Neutral-beam injection up to the 30-MW level to provide the desired high-energy ion tail has already produced deuterium-deuterium event rates of $7.6 \times 10^{16}\ s^{-1}$ in TFTR, while projecting to the release of over 10 MJ per pulse in a D-T plasma, and to a D-T Q-value of about 0.5. During the past year, the JET experiment has used a similar technique to approach the TFTR D-D reaction rate, while raising the projected D-T Q-value to 0.7. Current program schedules call for TFTR to enter D-T operation in 1993–94, followed by JET in 1995–96.

Plans for the transition from hydrogen and deuterium plasma-confinement studies into the D-T burning phase of fusion research have slipped by almost 10 years during the past 10 years—notably in the case of the U.S. program. As suggested by Fig. 1, meaningful D-T burning experiments require large capital investments. The associated rise in machine activation also makes experimental research more expensive and cumbersome. A cost-effective D-T burning program that leads step-by-step to prototypical electric power generation will require the establishment of a U.S. fusion development plan with a basic coherence time that reaches several decades into the future. Such a program plan was envisaged in the Magnetic Fusion Energy Engineering Act of 1980, but the projected doubling of fusion budgets during the course of the 1980s proved unrealistic from the outset: Meanwhile, the real dollar funding for magnetic fusion research has not been doubled, but cut in half (*27*).

To ensure sufficiently strong and stable support for the realization of a sequence of D-T burning steps leading to a reactor, the U.S. national fusion effort aims to become part of a well-integrated international D-T burning program. The present schedule calls for TFTR to lead the way to break-even and α-particle physics, and JET to build on the TFTR results. The U.S. fusion community has developed plans for a Compact Ignition Tokamak (*4*) (CIT) experiment that aims at exploring fully reactorlike plasmas up to the thermal stability limit $Q = \infty$, during multisecond pulsed operation. The basic idea is to use high-field copper coils, along with other confinement-improving techniques, to keep the CIT size similar to that of TFTR. Various other burning-plasma-physics experiments have been proposed in Europe, but the main thrust of the European, Japanese, and Soviet national programs has been towards considerably larger sized national projects: engineering test reactors (ETRs) made with superconducting magnet coils and that use long-pulse operating techniques (high-Q burn pulses lasting hundreds of seconds or longer) to begin the testing of fusion-reactor engineering systems. The present U.S. plan is to seek participation in some form of international ETR project and provide the CIT burning-plasma-physics results in support of preparations for the D-T operating phase of the ETR.

The leading ETR candidate is the International Thermonuclear Experimental Reactor (*4*) (ITER), for which a design is being developed by a joint team of scientists and engineers from the United States, Europe, Japan, and the U.S.S.R. The probable date for the start of ITER construction would be in the mid-1990s, and the most probable siting would be in Central Europe. The ITER D-T operating phase is to begin after 2005, and may include the demonstration of prototypical electric power generation. A power-generating reactor prototype of more advanced design could be constructed nationally or internationally for operation around 2020.

Concluding Remarks

Fusion pioneers of the 1950s saw the confinement of 100-million-degree plasmas as the one formidable obstacle to the release of fusion energy, and launched a brilliant and ultimately successful attack on it. They failed to guess that scientific success might have no direct consequences.

The current problem is that the development of fusion reactor technology and performance at reactorlike energy multiplication factors implies experiments at nearly full-scale reactor power output levels and correspondingly high capital costs. Relative to the total investment in new electric power plants that will have to be made by the United States. during the first half of the next century, the cost of the individual experimental steps leading to a fusion power demonstration is moderately small (of order 10^{-4} to 10^{-3})—but in the context of current U.S. energy research budgets, effective entry into the D-T burning phase of fusion research presents a daunting challenge.

The present U.S. approach to overcoming the funding obstacle is twofold: (i) seek to reduce the cost of the development path by raising the quality of plasma confinement somewhat beyond the needs of the D-T power-reactor product; and (ii) seek to establish a constructive pattern of international cost-sharing to bring the price of reactor experiments within realistic national budgetary guidelines. Present proposals along the latter line grow naturally out of the long-standing international collaboration in fusion, but surpass the magnitude of any joint worldwide research project that has ever been undertaken. Such a large-scale effort in scientific collaboration might give rise to further problems on the road to fusion power—and might also yield further benefits to mankind.

REFERENCES AND NOTES

1. S. Glasstone and R. H. Lovberg, *Controlled Thermonuclear Reactions* (Van Nostrand, Princeton, 1960).
2. I. E. Tamm and A. D. Sakharov, in *Plasma Physics and the Problem of Controlled Thermonuclear Reactions*, M. A. Leontovich, Ed. (Pergamon Press, New York, 1961), vol. 1.
3. *Phys. Fluids* **1**, 1 (1958).
4. *Plasma Physics and Controlled Nuclear Fusion Research 1988*, Nice, France, 12 to 19 October 1988 (International Atomic Energy Agency, Vienna, 1989), three volumes.
5. H. P. Furth, *Plasma Phys. Controlled Fusion* **31**, 1497 (1989).
6. *Pacing the U.S. Magnetic Fusion Program* (National Academy Press, Washington, DC 1989).
7. J. P. Holdren *et al.*, "Report of the Senior Committee on Environmental, Safety, and Economic Aspects of Magnetic Fusion Energy" [Lawrence Livermore National Laboratory report UCRL-53766, 25 September 1989 (ESECOM)].
8. *Cold Fusion Research*, DOE Report S-0073 (United States Department of Energy, Washington, DC, November 1989).
9. L. Spitzer *et al.*, *Problems of the Stellarator as a Useful Power Source*, Model D Report NYO-6047 (U.S. Atomic Energy Commission, Washington, DC, 1954).
10. J. Horgan, *Sci. Am.* **260**, 25 (February 1989).
11. *Starfire—A Commercial Tokamak Fusion Power Plant Study*, Report ANL/FPP-80-1 (Argonne National Laboratory, Argonne, IL, 1980).
12. F. Najmabadi, R. W. Conn, and the ARIES Team, *The ARIES Tokamak Fusion Reactor Study*, presented at the IEEE 13th Symposium on Fusion Energy, Knoxville, Tennessee, 26 October 1989.

13. H. P. Furth, *Phys. Fluids* **28**, 1595 (1985).
14. D. A. Monticello, W. Park, R. Izzo, K. McGuire, *Comp. Phys. Commun.* **43**, 57 (1986); H. R. Hicks, B. A. Carreras, J. A. Holmes, *J. Comp. Phys.* **60**, 558 (1985).
15. R. A. Sagdeev, G. M. Zaslavsky, D. A. Usikov, *Nonlinear Physics: From the Pendulum to Turbulence and Chaos* (Harwood, New York, 1988).
16. J. R. Ferron *et al.*, *Bull. Am. Phys. Soc.* **34**, 1911 (1989).
17. B. Coppi *et al.*, *Phys. Rev. Lett.* **44**, 990 (1980); A. B. Michailovskii and V. D. Shafranov, *JETP* **39**, 88 (1973).
18. R. J. Bickerton *et al.*, *Nature* **229**, 110 (1971).
19. G. A. Emmert *et al.*, *Nucl. Fusion* **29**, 1427 (1989).
20. I. H. Hutchinson, *Principles of Plasma Diagnostics* (Cambridge Univ. Press, Cambridge, England, 1987).
21. R. J. Goldston *et al.*, *J. Comp. Phys.* **43**, 61 (1981).
22. P. Liewer, *Nucl. Fusion* **25**, 543 (1985).
23. R. J. Goldston, *Plasma Phys. Controlled Fusion* **26**, 37 (1984).
24. D. J. Sigmar, *Phys. Scr.* **16**, 6 (1987).
25. J. M. Dawson *et al.*, *Phys. Rev. Lett.* **26**, 1156 (1971).
26. *TFTR Project Management Plan*, Report TFTR-PAO-0001 (United States Department of Energy, Washington, DC, March 1976).
27. *Starpower*, OTA-E-338 (Office of Technology Assessment, Washington, DC, 1987).
28. I thank Dr. Rush D. Holt for his help in editing this article. This work was supported by United States Department of Energy Contract no. DE-AC02-76-CHO3073.

New Methods of Drug Delivery

ROBERT LANGER*

Conventional forms of drug administration generally rely on pills, eye drops, ointments, and intravenous solutions. Recently, a number of novel drug delivery approaches have been developed. These approaches include drug modification by chemical means, drug entrapment in small vesicles that are injected into the bloodstream, and drug entrapment within pumps or polymeric materials that are placed in desired bodily compartments (for example, the eye or beneath the skin). These techniques have already led to delivery systems that improve human health, and continued research may revolutionize the way many drugs are delivered.

IN THE LAST FEW YEARS, WE HAVE WITNESSED AN EXPLOSION in research aimed at creating new drug delivery systems. This research has been fueled by several developments. (i) Many drugs, both old pharmaceutical products and new molecular entities, can be administered in ways that not only improve safety and efficacy but, in some cases, permit new therapies. (ii) Newer and complex drugs such as proteins are becoming available through genetic engineering; the delivery of these drugs is often more complicated than that of more conventional drugs, necessitating novel delivery systems. (iii) There is an increasing awareness that drug release patterns (continuous versus pulsatile) significantly affect therapeutic responses. (iv) The overall expense to create a pharmaceutical that is a new molecular entity is at least \$150 million; the lower cost to improve the delivery of an existing drug is sometimes seen as a better investment. This issue is exacerbated because drug patents expire after 17 years, and a new drug delivery system may permit continued benefits for the company producing it. (v) Advances in materials science and biotechnology are permitting the development of new physical and chemical methods of drug delivery. In this article, some of the methods being studied to deliver drugs are discussed.

The author is in the Department of Chemical Engineering, Harvard-MIT Division of Health Sciences and Technology, and Whitaker College of Health Sciences, Massachusetts Institute of Technology, Cambridge, MA 02139, and Department of Surgery, Children's Hospital, Boston, MA 02115. He is Kenneth J. Germeshausen Professor of Chemical and Biochemical Engineering at MIT.

*To whom correspondence should be addressed at the Massachusetts Institute of Technology, E25-342, Cambridge, MA 02139.

Chemical Modification

A drug may be chemically modified to selectively alter such properties as biodistribution, pharmacokinetics, solubility, or antigenicity. One example is drugs that are designed to cross a normally impermeable barrier. The blood brain barrier, which contains tight endothelial cell junctions and prevents most molecules from entering the central nervous system, has been the target of considerable research. Several experimental approaches have been developed, in which drugs are complexed to agents that enable them to cross this barrier (for example, by rendering the drug more lipophilic or coupling it to a molecule that has a specific transport mechanism) (*1*).

Drugs have also been attached to soluble macromolecules such as proteins, polysaccharides, or synthetic polymers via degradable linkages. This process alters the drug's size and other properties, resulting in different pharmacokinetics and biodistribution. One example involves coupling the antitumor agent neocarzinostatin to styrene–maleic acid copolymers (*2*). When this complex was injected intra-arterially into patients with hepatocellular carcinoma, decreases in α-fetoprotein levels and tumor size were observed. In animals, antitumor agents such as doxorubicin coupled to *N*-(2-hydroxypropyl) methacrylamide copolymers showed radically altered pharmacokinetics, resulting in reduced toxicity. The half-life of the drug in plasma and the drug levels in the tumor were increased while the concentrations in the periphery decreased (*3*).

An exciting approach for "targeting" drugs to specific cells involves linkage of a bioactive agent (drug, radioisotope, or toxin) to a monoclonal antibody. Antibody conjugates are now being studied in the treatment of cancer, septic shock, and acquired immunodeficiency syndrome (AIDS). There are several critical issues in the use of antibodies. With mouse antibodies, anaphylactic reactions frequently occur with repeated administration. Thus, ongoing research is directed toward producing human monoclonal