

**THE U.S. PROGRAM OF
FUSION ENERGY
RESEARCH AND DEVELOPMENT**



Report of the Fusion Review Panel

***The President's Committee of Advisors
on Science and Technology (PCAST)***

July 1995

EXECUTIVE OFFICE OF THE PRESIDENT
OFFICE OF SCIENCE AND TECHNOLOGY POLICY
WASHINGTON, D.C. 20500

July 11, 1995

The Honorable John H. Gibbons

Assistant to the President for Science and Technology

The Honorable John A. Young

Former President and CEO, Hewlett-Packard Co.

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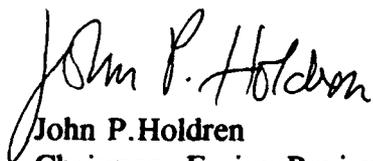
Dear Dr. Gibbons and Mr. Young:

We are pleased to transmit to you the enclosed report, "The U.S. Program of Fusion Energy Research and Development." This report was prepared by the PCAST Fusion Review Panel, which was constituted by Dr. Gibbons on March 22, 1995, to review the efforts of the Department of Energy (DOE) to develop fusion as an attractive energy source. The Panel was asked to clarify the technical and policy tradeoffs and budgetary requirements associated with different options for structuring the Department of Energy's magnetic fusion energy program and to recommend a preferred option.

Our report concludes that U.S. funding for research and development (R&D) on fusion energy is a valuable investment in the energy future of this country and the world, as well as sustaining a field of scientific research — plasma physics — that is important in its own right and has been highly productive of insights and techniques applicable in other fields of science and in industry. We conclude also that DOE's program plan for continuing this effort in the decade ahead represents a reasonable approach in pursuit of the National Energy Strategy goal of operating a demonstration fusion reactor by about 2025. Because it is now apparent that the budgets needed to carry out the DOE's program are unlikely to be made available, however, the Panel focused most of its attention on developing a budget-constrained fusion R&D strategy for the United States that could preserve the most indispensable elements of the U.S. fusion R&D effort, and the associated international collaboration, while spending about half as much over the next ten years as now planned by DOE.

The strategy recommended by the Panel entails hard choices, considerable pain, and difficult negotiations with our international partners in fusion energy R&D. We believe, however, that it is the best that can be done within budgets that are likely to be sustainable in the current climate, and the least that can responsibly be done to maintain a degree of momentum toward the goal of practical fusion energy and to sustain the highly productive field of plasma science. We very much hope that these recommendations will gain the support of the Administration.

Sincerely yours,



John P. Holdren
Chairman, Fusion Review Panel



Robert W. Conn
Vice Chairman, Fusion Review Panel

Enclosure

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EXECUTIVE SUMMARY

Funding for fusion energy R&D by the Federal government is an important investment in the development of an attractive and possibly essential new energy source for this country and the world in the middle of the next century and beyond. This funding also sustains an important field of scientific research — plasma science — in which the United States is the world leader and which has generated a panoply of insights and techniques widely applicable in other fields of science and in industry. And U.S. funding has been crucial to a productive, equitable, and durable international collaboration in fusion science and technology that represents the most important instance of international scientific cooperation in history as well as the best hope for timely commercialization of fusion energy at affordable cost. The private sector can not and will not bear much of the funding burden for fusion at this time because the development costs are too high and the potential economic returns too distant. But funding fusion is a bargain for society as a whole.

Based on the importance of developing energy sources adequate to meet the needs of the next century and the promise of fusion for this purpose, the benefits of fusion R&D in strengthening the national science and technology base, the impressive recent rates of progress in fusion research, the costs of the logical next steps, and the growing investments being made in fusion R&D in the European Union and Japan (which already total more than three times the corresponding investment here), we believe there is a strong case for the funding levels for fusion currently proposed by the U.S. Department of Energy (DOE) — increasing from \$366 million in FY1996 to about \$860 million in FY2002 and averaging \$645 million between FY1995 and FY2005.¹ These are actually the minimum amounts required to support full U.S. participation in the construction phase of the International Thermonuclear Experimental Reactor (ITER) as currently envisioned while maintaining a vigorous, complementary domestic program that (a) extracts the remaining scientific value from experimental facilities already in operation; (b) constructs the Tokamak Physics Experiment (TPX) to explore crucial issues not accessible in existing devices or in ITER, and to anchor the domestic experimental program in the next century; and (c) nourishes essential efforts in smaller experiments (including alternative concepts), theory, computing, technology development, and fusion-reactor materials. It almost certainly would not be possible to spend less and still meet the fusion timetable of the National Energy Strategy, developed during the last 2 years of the Bush Administration, which calls for operation of a fusion demonstration reactor by about 2025.

Although the program just described is reasonable and desirable, it does not appear to be realistic in the current climate of budgetary constraints; we therefore have devoted most of our effort to developing a budget-constrained U.S. fusion R&D strategy that, given level funding at about half of the average projected for the period FY1996 through FY2005 under the current DOE plan, would preserve what we believe to be the most indispensable elements of the U.S. fusion effort and associated international collaboration. This strategy would cost about \$320 million per year, \$46 million less than the U.S. fusion R&D budget in

¹ All dollar figures in this report are as-spent dollars unless otherwise noted.

FY1995. It would entail hard choices and considerable pain, including straining the patience of this country's collaborators in the international component of the fusion effort, forcing difficult trade-offs between even a reduced U.S. contribution to international collaboration and maintaining adequate strength in the domestic components of U.S. fusion R&D, shrinking the opportunities for involvement of U.S. industry in fusion technology development, and surrendering any realistic possibility of operating a demonstration fusion reactor by 2025. But we believe it is the best that can be done within budgets likely to be sustainable in the current climate, and the least that can responsibly be done to maintain a modicum of momentum toward the goal of practical fusion energy.

The key priorities around which our budget-constrained strategy is organized are as follows:

- **a strong domestic core program in plasma science and fusion technology, with funds to explore both advanced tokamak research and research on concepts alternative to the tokamak, leveraged where possible on related activities worldwide;**
- **a collaboratively funded international fusion experiment focused on the key next-step scientific issue of ignition and moderately sustained (circa 100 seconds) burn, at a cost about one-third that of ITER as currently planned; and**
- **an international program to develop practical low-activation fusion-reactor materials, highly desirable for economical reactor performance and environmental attractiveness.**

This budget-constrained, internationally integrated U.S. fusion R&D program would, more specifically:

- **preserve and somewhat enhance the U.S. core program in relation to its FY1995 level of about \$180 million per year, including a degree of remedy of the current program's neglect of confinement concepts other than the tokamak;**
- **continue to operate, within the core program, the medium-scale tokamaks at General Atomics (DIII-D) and MIT (Alcator C-MOD), upgrading DIII-D after Princeton's Tokamak Fusion Test Reactor (TFTR) is shut down, and continue modest funding to pursue energy applications of the inertial-confinement fusion effort being funded for stockpile stewardship purposes in the weapons budget;**
- **continue to operate TFTR for 3 years beyond its currently scheduled shutdown at the end of FY1995, at a somewhat reduced funding level of about \$50 million per year; and**
- **continue U.S. participation in the Engineering Design Activities phase of ITER at the current level (\$70 million per year), to which this country is committed through FY1998 under existing international agreements.**

Under this proposed budget-constrained program, the United States would also immediately open negotiations with its ITER partners to modify the post-FY1998 phase of international cooperation, seeking to

- **gain agreement for downsizing ITER construction and operation from a \$10-13 billion ignition-and-long-burn physics and reactor-technology development project to a not more than \$4 billion ignition-and-moderate-burn physics project, on a construction timetable delayed 3 years from the current plan;**
- **promote the possibility of significant international participation in the complementary next-generation fusion experiments hitherto planned as domestic projects (such as TPX in the United States); and**
- **add to the collaborative agenda a materials/blanket test facility, as part of the international, low-activation-materials and blanket-development program. The United States should be prepared to commit up to a total of \$200 million as its share of a project that achieves international consensus and begins construction in FY2000.**

The expectation of a successful outcome from this negotiation would depend on the United States bringing to the table a firm commitment, endorsed by the President, of a \$1.2 billion contribution to the next phase of the cooperation (cumulative over about 10 years). The negotiation would include the possibility of expanding the number of partners (to include, e.g., China, India, South Korea). It is possible that the outcome of the negotiation would be that the full-scale ITER was constructed despite the reduction of the U.S. contribution from what had been anticipated. This outcome would have the benefit of gaining, for the world, the additional science results and the technology-testing benefits associated with ITER as currently envisioned; but it would have the liability of sharply reducing the chance that money will be found within the international effort to fund the international materials test facility and to help pay for TPX (or another machine with a similar mission).

During the negotiation of the next phase of the ITER cooperation, construction of TPX (currently scheduled to begin in FY1996) would be delayed for 3 years. Thereafter, TPX construction would proceed *if*

- (a) **the outcome of the negotiation was such as to permit funding the (probably downsized) ITER ignition experiment, the materials test facility, *and* the TPX with a cumulative contribution of \$1.2 billion from the United States toward the total construction costs of these three facilities, the remainder to come from our international partners,**

or

- (b) **the outcome of the negotiation was such that the United States did not become a participant in an international ignition experiment, but an ignition experiment went forward somewhere under other auspices.**

If neither of these outcomes occurred, construction of TPX would not proceed unless and until a review of the new situation — with its lack of a commitment to an ignition experiment anywhere — concluded that proceeding with TPX was the most sensible next step for the United States in that situation.

Under some of the possible outcomes from the negotiation of the next phase of international collaboration, TPX would not be built. This would be extremely unfortunate. We consider TPX to be a well conceived and innovative advanced tokamak experiment, without which the United States will lack a large tokamak of its own after TFTR is shut down. We believe, nonetheless, that the highest priority should be given to preserving both (a) U.S. participation in a robust international collaboration that includes, above all, an ignition experiment and a materials test facility, and (b) a strong domestic core program of theory and smaller experiments. If negotiation of the next phase of international collaboration under a total U.S. contribution of \$1.2 billion does not produce an outcome that achieves these ends and TPX as well, then the loss of TPX will have to be considered a particularly dismaying consequence of constraining the overall U.S. fusion R&D program to \$320 million per year.

In addition to developing the strategy just described for a fusion R&D program funded at about \$320 million per year, we also have attempted to envision a program that could preserve key priorities at a still lower budget level of about \$200 million per year. We find that this cannot be done. Reducing the U.S. fusion R&D program to such a level would leave room for nothing beyond the core program of theory and medium-scale experiments described above — no contribution to an international ignition experiment or materials test facility, no TPX, little exploitation of the remaining scientific potential of TFTR, and little sense of progress toward a fusion energy goal. With complete U.S. withdrawal, international fusion collaboration might well collapse — to the great detriment of the prospects for commercializing fusion energy as well as the prospects for future U.S. participation in major scientific and technological collaborations of other kinds. These severe consequences — deeply damaging to an important and fruitful field of scientific and technological development, to the prospects for achieving practical fusion energy, and to international collaboration in science and technology more generally — are too high a price to pay for the budgetary savings involved.

We urge, therefore, that the Administration and the Congress commit themselves firmly to a U.S. fusion R&D program that is stable at not less than \$320 million per year.

* * * * *

Chapter 1 THE BENEFITS OF FUSION R&D

The principal objective of the U.S. program of fusion energy research and development is to provide this country and the world with an abundant, safe, environmentally attractive, and cost-competitive new energy source. Achieving this objective would bring large benefits almost irrespective of how the energy future unfolds; and achieving it could be crucial if society finds it necessary, for environmental or political reasons, to reduce sharply the currently dominant role of fossil fuels in world energy supply.

In the course of pursuing this energy goal, fusion R&D yields an immediate and continuous additional benefit by nourishing an important branch of basic science — plasma physics — and the technologies related to pursuing it. This field of research, for which nearly all of the funding comes from fusion energy R&D budgets, has been prolific in the production of insights and techniques with wide applications in other fields of science and in industry.

Finally, for a variety of reasons, fusion energy R&D has evolved a higher degree of international scientific and technological cooperation than any other field of scientific or technological research. This cooperation — entailing not only extensive exchanges of personnel and information but also full-fledged international collaboration in design, construction, and operation of some of the largest experiments — is in itself a valuable model and precedent for internationalization of R&D in other fields. Such cooperation is likely to become increasingly important as the costs of cutting-edge R&D continue to grow in relation to the capacities of individual nations to pay for it.

In the remainder of this chapter, we elaborate, in turn, on the potential benefits of fusion R&D for the future of energy supply and on its benefits for science and technology more generally. Chapter 2 summarizes the history and current status of fusion R&D programs in the United States and elsewhere, as well as existing plans for the continuation of these efforts. Chapter 3 evaluates the current U.S. program and the Department of Energy's plans for its future against a variety of criteria. Chapter 4 offers a strategy and specific recommendations for how best to pursue fusion energy R&D under the kinds of budget constraints likely to characterize the latter half of the 1990s and perhaps beyond.

1.1 Future Energy Demand and the Role of Fusion

Energy Demand in the Mid-Twenty-First Century

Future global energy demand will be determined by rates of population growth, rates of growth in economic activity per capita, and rates of reduction in energy intensity of economic activity ("energy conservation") in the world's various regions.¹ The combination of high population growth rates and the pursuit of rapid economic development in the less developed countries means that these countries, rather than the industrialized ones, are likely to account for

¹ If economic activity per capita is measured by gross domestic product (GDP) per person, then energy intensity is measured in energy per unit of GDP, and total energy use is governed by the identity,

$$\text{energy} = \text{population} \times \text{GDP/person} \times \text{energy/GDP}.$$

most of the growth of global energy demand over the next several decades. Although the details vary, most long-range projections of world energy demand show it reaching 2 to 3 times the 1990 level by the middle of the next century.² Getting by with the lower figure without widespread frustration of economic aspirations — and the likelihood of associated social tensions and political instability — would require assigning unprecedented priority to investments and policies that promote energy efficiency. More than a doubling of energy use between 1990 and 2050 may well be required to sustain global economic development, to foster international stability, and to facilitate investments that improve environmental quality.

Of the total primary energy supplied to civilization in 1990, nearly 30% was used to generate electricity and about 70% was used in nonelectric applications of fuels (for, e.g., heat and motive power). Some 80% of the world's nonelectric energy came from fossil fuels in 1990, with most of the rest coming from biomass fuels (fuelwood, charcoal, crop wastes, and dung); of world electricity generation, which in 1990 amounted to about 11 trillion kilowatt-hours, 62% came from fossil fuels, 19% from hydropower, 17% from nuclear fission, and a bit over 1% from the sum of biomass fuels, geothermal energy, wind power, and solar energy.³ The electric share of total energy use has been increasing: a doubling of energy use between 1990 and 2050 might well be associated with a tripling of electricity generation (to, say, 35 trillion kilowatt-hours).

Options for Meeting the Demand

The options available for meeting the world's demand for energy in 2050 and beyond are those already in use — fossil fuels, biomass energy, nuclear fission, hydropower, geothermal energy, wind energy, and solar energy — plus, potentially, nuclear fusion.⁴ Each of the options already in use is likely to play some role in 2050 and beyond; each has the potential for improvement in technical, economic, and environmental characteristics beyond those associated with these options today (and each deserves investment to achieve this potential); and, at the same time, each is subject to shortcomings and constraints that could limit its contribution. In what follows, we characterize the options briefly in descending order of their current contribution to total energy supply.

² See, e.g., Edmonds & Reilly (1985), WEC (1989), Holdren (1990), IPCC (1992), Johansson et al. (1992).

³ See, e.g., Johansson et al. (1992), Holdren (1990,1994). In the United States, the shares of electricity generation were: fossil fuels 68%, nuclear fission 19%, hydropower 10%, and the sum of biomass, geothermal, wind, and solar energy a bit under 3%. Nonelectric uses of energy in the United States were supplied 95% by fossil fuels and 5% by biomass.

⁴ The "hydrogen economy", which has attracted considerable interest in the context of energy for the 21st century, does not refer to an additional primary energy source but rather to a set of possibilities for using hydrogen as an energy carrier, analogous to electricity or gasoline. Just as gasoline must be made from petroleum (or, as "synthetic" gasoline, from coal) and electricity must be made from fossil fuel, uranium, falling water, or some other naturally occurring primary energy source, so also must one start with a primary (fossil, nuclear, geothermal, or renewable) energy source to make hydrogen; this can be done by thermochemical processing of hydrocarbon fuels or by the electrolysis or thermal decomposition of water.

Oil and natural gas combined accounted for 70% of all fossil-fuel use in 1990. Global resources of these convenient and versatile fuel forms are much smaller than those of coal, however, and the richest remaining oil and gas fields are very unevenly distributed. The shares of oil and gas in world energy supply — and even the absolute magnitude of their contribution -- are likely to shrink in the decades ahead. Dawdling in the task of finding supplements and replacements for them is a prescription not only for increased monetary costs associated with their increasing scarcity in relation to demand, but also for political tensions and perhaps even conflict associated with the circumstance that the largest and most valuable of these resources are concentrated in only a few regions.

Coal, which currently accounts for about 30% of all fossil-fuel use, is abundant enough to take over the burdens now borne by oil and gas — and even to expand significantly the total amount of energy derived from fossil fuel — for the entire 21st century and beyond. But coal is dirty, inconvenient, and costly to clean up or convert to liquid and gaseous synthetic fuels that could replace oil and natural gas in their main applications. Coal, oil, and gas all suffer the liability of releasing carbon dioxide to the atmosphere when they are burned, moreover, and coal is the worst of the three in this respect. (Conversion of coal to liquid or gaseous fuels aggravates this problem further, because in such conversions some of the coal's energy invariably is lost.) The possibility of unacceptable impacts on climate from accumulating atmospheric carbon dioxide might well constrain the rate at which society will want to use coal and other fossil fuels to much lower levels than would be dictated by fuel supply and costs of extraction and conversion — possibly to levels lower than today's.⁵

Energy from biomass, which currently accounts for nearly 20% of world nonelectric energy supply but less than 1% of electricity generation, has the advantage of making no net addition to atmospheric carbon dioxide as long as new growth replaces the plant materials being burned. Under current practices in many parts of the world, however, biomass use for energy is associated with deforestation, soil erosion, and acute air pollution with particulate matter and hydrocarbons. Improved practices could alleviate these problems and increase the efficiency with which biomass energy is used, but ultimately the amount of biomass energy used by civilization will be constrained by how much of the planet's land area and photosynthetic production can be spared from the requirements of providing food, fiber, biodiversity, and other services.

Hydroelectric power worldwide is half as important as biomass in total-energy terms; as a source of electricity, it is a third as important as fossil fuels and roughly equal to nuclear power. Untapped hydropower potential may equal 2 to 3 times what has been harnessed so far, but many of the untapped sites are farther from demand centers or otherwise costlier to develop than the hydro sites now in use, and there would be objections to developing many of them on grounds of environmental damages and impacts on local populations. It will be a remarkable accomplishment if hydropower is ever able to generate as much electricity as fossil fuels are generating today.

⁵ The question arises whether it is practical to use fossil fuels while sequestering the resulting carbon dioxide, rather than releasing it to the atmosphere. Carbon dioxide sequestration in depleted natural gas fields and deep aquifers, and in other ways, is one of many understudied areas of energy research. At present, the key questions of ultimate storage capacity, long-term retention of the carbon dioxide, and cost remain unanswered. Until much more work is done, it will not be possible to count on this particular technological finesse.

Nuclear fission contributed about a sixth of world electricity generation in 1990. Nearly a quadrupling of the size of the nuclear-fission enterprise worldwide would be needed in order to generate as much electricity as the world was generating from fossil fuels in 1990 — that is, this enterprise would need to grow from the equivalent of some 300 1,000-megawatt reactors today to the equivalent of more than a thousand such reactors, with associated fuel-cycle facilities. To generate half of the electricity likely to be used in 2050, nearly 3,000 large reactors would be needed. Leaving aside the demanding requirements for capital and infrastructure to implement such a scenario, whether it can be done at all will depend on whether electric-utility officials, government decisionmakers, and publics are convinced that the issues of reactor safety, nuclear-waste management, and proliferation risks can be satisfactorily managed for a nuclear-fission power complex of this scale and dispersal. This might happen, but it also might not.

Geothermal energy, windpower, and solar energy currently make very small contributions to world electric and nonelectric energy supplies, although each of these options could make larger contributions in the future.

- Geothermal energy in the forms currently harnessed is dependent on isolated, depletable deposits of hot water and steam. Using geothermal energy on a much larger scale would require tapping the hot, dry rock available everywhere at sufficient depth in the Earth's crust. The practicality and cost of doing this remain to be established.
- Windpower, which currently contributes less than a tenth of a percent of world electricity generation, is economically competitive or close to competitive with coal-fired and nuclear electricity generation at sites with good wind resources, and it could be greatly expanded. To make as much electricity as currently comes from fossil fuels would require about 10 million 300-kilowatt wind turbines (the size range that appears to be most economic).⁶
- The two solar energy technologies most likely to see large-scale use are solar-thermal and photovoltaic electricity generation. Both do best at sunny, desert sites. Photovoltaics are more versatile, as they can use the diffuse-beam solar radiation characteristic of cloudy days as well as the direct-beam radiation that most types of solar-thermal plants require; but photovoltaics are also much farther from economic competitiveness with conventional electricity sources. To generate with photovoltaic cells as much electricity as now comes from fossil fuels would require perhaps 80,000 square kilometers of photovoltaic installations, equal to about 5% of the currently urbanized area of the planet.⁷ Land requirements for typical solar-thermal plants would be perhaps half as great.

⁶ This calculation is based on a capacity factor of 25%, meaning the actual annual output is 25% of what would be obtained if the plant operated at full rated capacity 24 hours per day, 365 days per year. Such capacity factors are achievable by wind turbines at good sites, by no means everywhere.

⁷ This calculation is based on use of sunnier-than-average sites (annual average insolation of 200 watts per square meter, compared to the world mean of 175 watts per square meter), arrays that convert to electricity 10% of the sunlight incident on them, land area equal to twice the array area, and adequate grid-connectedness or diurnal storage to utilize all of the electricity produced.

Ocean thermal energy is another renewable energy resource of substantial magnitude, but the monetary and environmental costs of harnessing it on a significant scale are highly uncertain.

The foregoing considerations make clear that it will be an immense challenge to support a doubling or more of energy use — and, probably, a tripling of electricity use — in the middle of the next century in ways that are safe, economically affordable, environmentally tolerable, and politically acceptable. If stabilizing or even shrinking the use of fossil fuels proves to be required, as could happen for environmental or political reasons or a combination of these, the challenges posed to nonfossil sources will be all the greater.

Most of the major energy options, fossil and nonfossil alike, are subject to sharply rising costs of some kind — economic, environmental, social, political — when their scale of utilization passes a critical level. For example, hydropower, windpower, and solar energy become much costlier when it becomes necessary to resort to inferior sites; oil becomes much more dangerous politically when total demand grows so large as to require excessive dependence on the resources of unstable regions; fossil fuels altogether become much costlier environmentally when the scale of their emissions overwhelms the absorptive capacity of biogeophysical systems; nuclear fission will be problematic if it grows and spreads more rapidly than the managerial competence needed to operate it safely and to protect its fissile materials; and so on. Society's menu of energy options is also susceptible to sudden narrowing as a result of political mischief, unfavorable new knowledge, or other unforeseen developments.

In these circumstances, it should be obvious that there is great merit in the pursuit of diversity in energy options for the next century. There are not so many possibilities altogether. The greater the number of these that can be brought to the point of commercialization, the greater will be the chance that overall energy needs can be met without encountering excessive costs from or unmanageable burdens upon any one source. The potential value of developing fusion energy must be understood in this context. The possible costs of needing fusion at midcentury and beyond, but not having it, are very high.

What Could Fusion Offer?

Nuclear fusion has many attractive attributes: (a) The fuel supply is extractable from ordinary seawater (thus available to all countries) and is sufficient in quantity for millions to billions of years. (b) There are significant advantages over fission energy options with respect to possibilities for minimizing radiological hazards and links to nuclear weaponry, over fossil-fuel options with respect to emissions to the atmosphere, and over many forms of renewable energy with respect to impacts on ecological and geophysical processes. (c) The monetary costs of fusion could be comparable to those of other medium-term and long-term options. These points are discussed briefly in the following paragraphs.

Fuel supply. Extracting lithium from seawater until its concentration drops to half of today's value would yield at least 150 million terawatt-years of thermal energy in first-generation fusion reactors, based on the deuterium-tritium (D-T) reaction (where the tritium is obtained by splitting lithium). Extracting deuterium from seawater until its concentration drops to half of today's value would yield 250 billion terawatt-years of thermal energy in advanced fusion reactors,

based on the deuterium-deuterium (D-D) reaction. For comparison, world coal resources are estimated at 5,000 to 10,000 terawatt-years, and world energy use was about 13 terawatt-years per year in 1990.

Safety and environment. Fusion, like fission, requires attention to the full range of nuclear safety-and-environment issues: worker safety, minimizing routine exposures of workers and the public to radiation, preventing reactor accidents, safely managing radioactive wastes, and avoiding linkages between civilian and military nuclear programs. But the issues that are most difficult to handle for fission are likely to be less so for fusion. Specifically,

- with respect to reactor safety, if priority is given in the development of fusion to achieving its potential for reduced radiological hazards, it should be possible to achieve D-T fusion reactors in which "worst case" accidents would produce population exposures to radiation about 100 times smaller than those from "worst case" fission reactor accidents; use of advanced fuels could give even larger improvements over fission;
- with respect to radioactive-waste hazards, those of fusion (based on the most meaningful indices combining volume, radiotoxicity, and longevity) can be expected to be at least 100 times and perhaps 10,000 or more times smaller than those of fission;
- with respect to links to nuclear weaponry, electricity-supply systems based on fusion would be less likely than fission-energy systems to contribute to the acquisition of nuclear-weapons capabilities by subnational groups, and would be easier to safeguard against clandestine use for fissile-material production by governments.

In comparison with renewable energy sources, fusion would have no counterpart to the ecological problems associated with large-scale production of biomass for energy (heavy use of land, water, fertilizers, and pesticides, and loss of natural biodiversity). The ecological and geophysical impacts of fusion would be less severe than those of hydropower, ocean thermal energy, and (probably) geothermal energy. In addition, fusion's land-use requirements would be smaller than those of most forms of solar electricity generation.

Economics. The cost of the raw fuel for fusion -- lithium and deuterium extracted from seawater -- would be a very small fraction of the total cost of the electricity produced. The construction costs of the power plant would account for most of the total cost (as with nuclear fission and with most forms of renewable energy). The potentially higher costs of fusion plants compared to fission plants, which are associated with the complexity of fusion technology, could be substantially offset, for the safest designs, by savings resulting from easier siting and licensing and greater simplicity in waste disposal. Safer designs could also lead to reduced requirements for "nuclear-grade" certification of plant components. Comparing the costs of electricity from fusion with the costs of electricity from renewable energy is made complicated by the base-load character of fusion and the intermittent character of many renewables, neither of which is ideally matched to an independently varying electricity demand.

If positive results from vigorous pursuit of fusion energy were to lead to deployment of the first commercial fusion reactors around the middle of the 21st century, it would be possible

to imagine this source providing, by late in that century, an electricity contribution comparable to that from fossil fuels in 1990 -- about 7 trillion kilowatt-hours per year. This output would correspond to the output of nearly 600 fusion power plants of 2,000 megawatts capacity each, operating at an average capacity factor of 70%. Such plants, twice the size of today's typical coal-fired and nuclear-fission power plants, would typically be deployed on electricity grids substantially larger than today's and probably would be the largest power plants in the mix of facilities producing electricity. Contrary to most current expectations, however, fusion power plants might turn out to be cost-competitive at capacities significantly below 2,000 megawatts, in which case a 7 trillion kilowatt-hour contribution could come from a larger number of smaller plants. Smaller plants fit more easily into power grids of moderate size (which, in the mid-to-late 21st century might still characterize some developing regions), and the production of larger numbers of units provides greater opportunities for technological and institutional learning.

Because fission and fusion are both nuclear technologies and are, in some sense, competitors for the same "niche" in global energy supply — large-scale, capital-intensive, grid-connected electricity generation, with the possibility of contributing to fluid-fuel supplies by electrolytic production of hydrogen — the issue of comparisons between and possible interactions of these two technologies is particularly salient. In this context, two pathways entailing an important role for fusion can be envisioned:

- Along one path, first fission prospers, and then fusion prospers alongside it, perhaps eventually replacing it and perhaps co-existing with it indefinitely. In this scenario, the public becomes comfortable with nuclear fission before fusion is commercially available — probably in connection with reduced reactor and fuel-cycle costs, decades of trouble-free management of the nuclear fission enterprise worldwide, and decades of international diplomacy in which nuclear weapons play only a minor role. Investments in advanced fission technologies could increase the likelihood of this scenario, although they cannot guarantee it.
- Along a second path, fission does not prosper, but fusion prospers by being sufficiently different from fission in its nuclear characteristics so that either (a) these differences translate into a significant economic advantage for fusion (through, e.g., simplified safety systems, reduced regulatory requirements, and easier siting), so that fusion is economically attractive where fission was not, or (b) fusion is deemed politically acceptable where fission was not.

Pathways in which fission prospers but fusion does not, or in which neither prospers, can also be envisioned. It is not our role here to offer assessments of which of these outcomes is most likely. We want simply to point out that the possible shortcomings of other major options for the second half of the next century and beyond, the benefits of diversity in energy technologies, and the penalties attendant on inadequate energy supply are all such as to mandate continued intensive efforts to make both fusion and fission available in that time period in the most attractive forms possible.

Energy R&D Policy in Global Context

In sharp contrast to most of the countries in the world — industrialized and developing alike — the United States is blessed with abundant coal resources, oil reserves still able to supply half of domestic consumption, and gas reserves sufficient to provide the domestic fossil-fuel system with considerable flexibility. The United States also enjoys a relative abundance of land area that could provide the basis for large-scale deployment of solar-electric energy systems, biomass-energy plantations, and windfarms. Economic geography presents a much less promising energy prognosis for Japan and the countries of Western Europe, and this disparity goes far toward explaining the substantially lower levels of commitment of public resources to energy R&D per capita in the United States today, relative to the levels in these other countries of comparable prosperity.

The sense of complacency in the United States engendered by this country's relative energy-resource abundance is understandable, but it is not justified. Having suitable energy-supply options is far more a matter of having the right technology than of having the raw resources, and this will be even more true in the next century than it is today. There will be great economic benefit to the United States, moreover, if it is in a position to be an exporter of attractive energy technologies to the huge world market of the next century, and considerable economic cost if these must be imported from Japan, Europe, and elsewhere. There is particular, additional merit in exerting and maintaining leadership in nuclear energy technologies, because of the influence of choices about these technologies worldwide on the prospects for minimizing nuclear-weapons proliferation and for avoiding major nuclear accidents. This last point underlines a more general one: the United States is unlikely to be able to isolate itself from either the political turmoil or the large-scale environmental problems that will result if it turns out that adequate energy options for countries less well endowed than the United States geographically and technologically are simply not developed at all, by this country *or* by others.

Thus there is a powerful argument for the United States to invest adequately in a broad and deep program of energy research and development, to maintain a position of international leadership in this field, and to exert that leadership to foster and steer international collaboration in those aspects of energy R&D for which there are particular advantages in doing so. The U.S. energy R&D program needs to address energy efficiency and all of the major supply options and potential options, including fission and fusion; and, in the nuclear technologies especially, it needs to be closely coupled to the R&D programs of other countries. The difficulties experienced by the United States in developing and sustaining the sort of energy R&D policy that is required, and the importance of overcoming those difficulties, are treated at length in the recent report on energy R&D by the "Yergin committee" of the Secretary of Energy's Advisory Board (SEAB 1995). We hope that readers of our report will read that one, too.

1.2 Benefits of the Fusion Program Besides Energy

Advancement toward the goal of fusion power has required the birth and development of a new field of science — plasma physics. Plasmas are often referred to as the fourth state of matter (the first three being solids, liquids, and gases). It is now understood that 99% of the

known matter in the universe, although little of the matter on Earth, is in the plasma state. The birth of modern plasma physics occurred with the advent of fusion research in the 1950s, and the fusion program has been the dominant driver of plasma physics ever since. Conversely, the development of plasma physics has been the engine driving progress in fusion. This essential link between fusion energy and plasma science implies that the national benefits accruing from plasma science should be viewed not simply as a "spin-off" of an energy program.

Any assessment of the fusion research program should, therefore, recognize both classes of benefits of fusion R&D — the long-range development of an energy source and the more immediate gains from plasma science. In what follows we address the latter, in two categories: first, the value of this field as science and, second, the impact of plasma science on industry and technology.⁸

Contributions to Science

Plasma physics has uncovered a panoply of new phenomena. A plasma is a gas of charged particles, each of which interacts with all the other particles in the gas — not just with those very close to one another, as in an ordinary gas of neutral particles. Understanding the behavior of plasmas poses an enormous intellectual challenge. Insight into this important medium, obtained by the techniques of modern physics, reveals that a plasma is not simply an unpredictable assembly of motions. Basic principles have been developed to understand the rich array of plasma waves, instabilities, spontaneous magnetic phenomena, and turbulence. These emerging principles have advanced fundamental concepts in complex systems, an avenue of inquiry at the forefront in numerous areas of science.

Results from fusion plasma physics have fundamental and pervasive import for many other scientific fields. In astrophysics, plasma science has been employed to understand the behavior of the plasma and magnetic fields in the earth's magnetosphere, in the sun and other stars, and in galaxies. For example, plasma physics is required to understand magnetic storms observed on Earth, solar flares, shock waves in space, magnetic fields in stars and galaxies, pulsars, accretion disks of active galactic nuclei, black holes, and star formation. Fusion plasma physics has been at the forefront in the development of the new sciences of chaos and complexity and has forged new concepts in the area of turbulence, one of the great scientific problems of this century. In the area of large-scale scientific computing, fusion researchers have pioneered the use of supercomputers to solve complex problems. In particular, the fusion energy program was the first to employ time-sharing supercomputers serving a large scientific community.

One of the applications of plasma physics has been to the non-neutral particle beams of particle accelerators. Progress in understanding and controlling plasma instabilities has made possible all modern accelerators, such as the colliders RHIC (at Brookhaven), Tevatron (at Fermilab), and PEP II and the SLC (at Stanford).

⁸ This subject is treated in far greater detail in a National Academy of Sciences report (NRC 1995) entitled "Plasma Science: From Fundamental Research to Technological Applications", which was made available to us in page-proofs while our own report was in the final stages of preparation.

Contributions to Industry and Technology

The pursuit of fusion energy has laid the scientific foundation for, and has already contributed to, a number of technologies that have applications in manufacturing, materials, electronics, electric power, computing, and the defense industries.⁹

In manufacturing, the unique properties of plasmas have led to important applications in the processing of materials. Analysis of the fundamental chemical and physical processes occurring in plasmas has led to better understanding and resulted in the improved performance of industrial plasmas. Equipment and instruments developed by fusion physicists and engineers to produce, monitor, and control plasmas have wide use. Examples of such industrial application include:

- Plasma etching, deposition, and surface modification to manufacture integrated circuits. Plasma processing is a principal manufacturing technology for creating microelectronic devices on the very small (submicron) scale that is required for the advanced integrated circuits in computers, communications equipment, and consumer electronics products. This technology also reduces toxic wastes from microelectronic-circuit manufacturing.
- Plasma-assisted chemical vapor deposition to prepare diamond and superconducting films. Models to optimize film growth require knowledge of molecular dynamics and film microstructure that are a result of developments in plasma processing and materials characterization.
- Plasma-ion implantation to harden tools, to produce anticorrosion coatings, and to reduce wear by creating low-friction surfaces for both industrial and biomedical applications. Ion implantation treats the surface of metal parts, such as high-strength ball bearings, cam shafts for performance vehicles and military equipment, and prosthetic joints that are low-friction and biocompatible.

The needs of fusion research have provided a major stimulus for the development of superconducting magnets. In order to confine very hot plasmas, superconducting magnets of unprecedented size and power have been required. Applications of superconducting magnets are potentially extensive; they include energy storage, transportation, and rocket propulsion.

Several classes of advanced materials have been developed in the course of fusion and plasma research. Superconducting wire and cable configurations are now used in various industrial and medical applications. High-strength, nonmagnetic steels, composites capable of withstanding very high heat fluxes, new surface-cleaning methods, and new electron-beam welding techniques — all developed in fusion research — are finding numerous applications.

⁹ For additional detail, see, e.g., USDOE (1993).

An important contribution of fusion has been the advancement of pulsed-power technology, including capacitors, switches, and cables, to meet the high power needs of fusion devices. Applications include pulsed-power components and systems used for defense and commercial research and applications. Lightweight, compact, and reliable power supplies, initially used for tokamak plasma control, are being used in defense and in rail transport.

Many practical engineering computation and simulation techniques can be traced to the fusion program in computational physics. For example, both the computational methods to describe the magnetic field generated by complicated coils and the finite-element method of analysis of stress were developed for the design of magnetic fusion devices. These codes have been adapted by engineers working on magnet designs for electromagnetic launch and levitation systems. As another example, the computational solutions of the electromagnetic wave equation, developed in plasma heating studies, are being used in engineering applications ranging from antenna design to calculating radar cross sections.

The above examples are only illustrative. The influence of plasma science and fusion-related technology is growing, with many applications only now emerging. Already there are a considerable number of successful spin-off companies, which are transferring important technologies to several commercial sectors. A strong fusion program would continue to be an important driver in this area.

Chapter 2 HISTORY, STATUS, AND CURRENT PLANS

The U.S. fusion R&D program is best understood in terms of the history of fusion R&D, key issues that such R&D must address, status and plans of the current (FY1995) U.S. program, the U.S. funding picture, fusion R&D activity in other countries, the role of international cooperation in fusion R&D, and the pace of progress. In this chapter, we treat these topics in a largely descriptive way. Evaluation follows in Chapter 3.

2.1 History of Fusion R&D

The history of work on fusion energy, together with extensive descriptions of the various experimental fusion machines that have played important roles in the development of this field, can be found in more detail than is possible here in a number of recent major reviews of U.S. and world fusion programs.¹ What follows here is a capsule summary.

Fusion energy R&D began in 1951, in Britain, the United States, and the Soviet Union, as a spin-off of weapons work on the hydrogen bomb. The work was kept secret until 1958, when work on magnetic fusion energy (MFE) in all three countries was declassified by an agreement reached at the 2nd United Nations Conference on Peaceful Uses of Atomic Energy, held in Geneva. Since then, there has been excellent international cooperation in MFE; this cooperation has, in fact, been more extensive and important, in relation to the total amount of activity in the field, than collaboration in any other field in science or technology.

In the 1950s and 1960s, many different configurations for containing plasmas were considered. At that time, physicists realized that the development of a "magnetic bottle" was the primary subject (it still is), and they believed that a variety of approaches would be successful. Stellerators, mirror machines, Z-pinchs, q-pinchs, and other devices were studied. Slowly, a sophistication was developed, both experimentally and theoretically, through the development of a diagnostic ability that allowed careful measurement and through the development of the theory of plasmas. Physicists began to understand the subject of instabilities. Soon they were able to comprehend, and even predict, which devices would be subject to what type of instability and why.

During this period, the basic parameters needed to measure progress towards harnessing fusion energy were identified. These parameters comprise the ion temperature (T_i), the product of plasma density and confinement time ($n\tau$), and the power produced divided by the power put into the plasma (Q). Achieving a significant fusion reaction rate using the most reactive fusion fuel (a mixture of deuterium and tritium) requires T_i greater than 100 million degrees Celsius. Achieving a reaction rate sufficient to produce more energy than needed to heat the fuel ("energy break-even") requires, in addition, that the product $n\tau$ exceed 10^{14} seconds times fuel ions per cubic centimeter (called the Lawson criterion). Finally, Q must be considerably larger than unity to make up for energy lost to infrastructure (e.g., pumps, fans, lighting), to the inefficiency of the devices that heat the plasma (gyrotron tubes, neutral-beam injectors, etc.), and to the production of the magnetic fields of the "bottle" (resistive losses in the magnet coils and/or power for

¹ See, e.g., OTA (1987), NRC (1989), ERAB (1990), FEAC (1992), and OTA (1995).

refrigerators if the coils are cryogenic or superconducting). "Ignition" corresponds to infinite Q , but even a finite Q that is large compared to unity would be of interest for fusion energy.

In 1968, Soviet scientists announced that they had achieved very long confinement times of hot, dense plasmas in a tokamak device — called T-3 — which had been conceived by Sakharov and Tamm.² The fusion world was astounded, but skepticism was laid to rest when a British team, invited by the Soviets to independently make measurements of plasma parameters in T-3, confirmed the announced results. This event initiated a worldwide effort on tokamaks that blossomed in the 1970s and into the 1980s.

In the United States, this effort included the achievement of a record Lawson parameter on Alcator (MIT) in 1975 and the authorization of Doublet III (General Atomics) and the Tokamak Fusion Test Reactor (TFTR; Princeton) in the same year. Record temperatures were achieved in the Princeton Large Torus (PLT) in 1978. Meanwhile, work proceeded in a number of U.S. laboratories on fusion experiments in configurations other than tokamaks — mirror machines, pinches, and so on (discussed further below) — at a somewhat lower level of effort.

In 1980, the Magnetic Fusion Energy Engineering Act authorized a 20-year, \$20 billion effort, but it never came to pass. The annual appropriations needed to implement it simply did not materialize. Instead, throughout the 1980s, projects investigating confinement concepts other than the tokamak were terminated one after the other to make room, in declining budgets, for continuation of a vigorous effort on tokamak development. Stimulated in part by the combination of budgetary stringency and the escalating costs of proceeding further down the tokamak line of development, there emerged from the 1985 Reagan-Gorbachev summit an agreement to intensify international cooperation in fusion research in the form of a collaborative project to design an International Thermonuclear Experimental Reactor (ITER). That project, which is described in the next section, was proposed as a major tokamak that would address both physics and engineering issues.

Although a number of proposals to construct new U.S. tokamaks — such as the Compact Ignition Torus (CIT) and the Burning Plasma Experiment (BPX) — were submitted in the late 1980s, none was funded. The domestic experimental effort focused, in the 1980s and the first half of the 1990s, on operating and, in some cases, upgrading the tokamaks that had been authorized in the 1970s:

- In 1983, Alcator achieved $n\tau$ adequate for fusion, but the plasma temperature was too low for energy break-even. An upgrade of this machine was authorized in 1987 (Alcator C-MOD).
- Doublet III and its upgrade (DIII-D) produced many significant results, including demonstration of high-quality plasma-confinement regimes (suitable for advanced tokamaks), divertor concepts (of great importance to many fusion configurations), and high-power microwave heating.

² The term "tokamak" is a Russian acronym for words meaning toroidal chamber with magnetic coils.

- TFTR produced several significant accomplishments. Learning how to handle tritium and learning how to perform the everyday operation of the device were necessary. At the same time, sophisticated diagnostics, which now make this machine perhaps the best equipped fusion device in the world, were developed. In 1994, TFTR achieved the record fusion output power of 10.67 megawatts and $Q=0.3$. The detailed study of plasma-wall interactions, the self-sustaining bootstrap current, alpha-particle heating and removal, and the production of reversed shear (for enhanced confinement) have been of equal importance.

Investigation of the tokamak concept has also dominated the world's other major fusion R&D programs over the past two decades. In Russia, the T-3 was succeeded in the mid-seventies by the larger T-10 and later by the T-15.³ European researchers built the ASDEX tokamak in the seventies and Tore-Supre and Frascati Tokamak Upgrade (FT-U) tokamaks in the eighties; and, collectively, the European fusion programs completed the Joint European Tokamak (JET), a D-T-fueled machine somewhat larger than TFTR, in 1983. Japan established a vigorous fusion R&D program in the 1970s, leading to operation of a large tokamak called JT-60 in 1985. Russia, various European countries, and Japan also maintained smaller-scale investigations of magnetic-confinement concepts other than tokamaks, and the non-U.S. efforts in these alternative concepts remain stronger than those in the United States today. (Current U.S. and non-U.S. programs are reviewed in more detail in subsequent sections.)

The worldwide effort in MFE was paralleled, beginning in the 1960s, by a narrower effort along an alternative pathway to controlled fusion called inertial fusion energy (IFE). IFE entails using pulses of laser or particle-beam energy to compress and heat small pellets of fusion fuel to ignition conditions, whereupon the duration of the burn is limited by the rapid expansion of the reacting fuel, constrained only by inertia. Break-even occurs, as in MFE, when the energy yield exceeds the energy deposited on the pellet by the laser or particle beam.

In contrast to the case of MFE, the physics of IFE resembles in some respects the physics of thermonuclear weapons. Consequently, most of the U.S. effort on IFE has been directed toward the study of weapon physics and has been funded, accordingly, out of the defense budget rather than out of the energy budget, and much of the work has been classified. The largest IFE efforts outside of the United States have been in other nuclear-weapon states, notably the Soviet Union and France, and appear to have been similarly motivated. The possibility of harnessing this approach as an energy source is of considerable interest, however, and a small part of the Department of Energy (DOE) fusion energy budget is currently devoted to exploring IFE's energy applications.

Very substantial progress in IFE research has been made since the first encouraging results were obtained in 1969. Most of the effort has been based on use of powerful lasers as the source of the energy deposited on the fuel pellet, culminating with the very large Nova laser facility completed at the Lawrence Livermore Laboratory in 1985. In addition, an important set of "proof of concept" experiments was done using underground nuclear explosions as the energy source for

³ T-15, although completed, has not operated because of financial difficulties in the Russian program attendant on the economic changes ongoing in that country.

compressing and heating fuel pellets, but this approach is not relevant to reactor possibilities. Many IFE experts question whether lasers will lead to commercial reactors, either, and advocate exploring the use of heavy-ion beams instead. Such heavy-ion "drivers" for IFE have received relatively little funding to date, although a number of national reviews of IFE efforts have recommended increased emphasis on this approach.

The terms of reference of this review dictated a focus on the fusion R&D supported by the DOE Office of Fusion Energy (OFE) — hence mainly on MFE rather than on the inertial-fusion work funded from the defense budget — but we do give some attention to the value of continuing to explore inertial fusion's potential application as an alternative route to commercial energy.

2.2 Key Issues in Fusion R&D

Pursuit of the promise of commercial energy from magnetic fusion requires an R&D program embracing four key elements: core plasma science and fusion technology; ignition and burn; steady-state operation; and materials development. In the following subsections, we indicate briefly what each of these elements entails.

Core Plasma Science and Fusion Technology

A broad core research program is necessary to address the multitude of scientific issues and opportunities presented by the pursuit of fusion energy. The nature of the scientific issues requires a spectrum of devices; some scientific issues are best addressed in large experiments, some in small experiments. What is understood as the core program in the U.S. fusion community today is the part of the fusion R&D effort *not* associated with the largest experimental devices that are operating or are under design or construction — that is, at present, the program other than TFTR (operating), ITER (in design), and TPX (ready for construction).

The core program provides scientific underpinning for the fusion endeavor and for the generation of new ideas, which are essential to progress. It is also the source of much of the support for graduate teaching and postdoctoral training in plasma physics and fusion technology, which are essential to the future vitality of the fusion-energy effort. It includes research into improvements to the tokamak concept, exploration of alternatives to tokamaks, basic theory and computation, basic experimental studies, and fusion-technology research, as elaborated in the paragraphs that follow.

Tokamak improvements. The tokamak fusion concept is highly developed, to the point that a tokamak ignition experiment is feasible. The prospects for turning a tokamak into a commercial fusion reactor are hampered, however, by specific drawbacks relating to its economic attractiveness. Among these shortcomings of tokamaks are their vulnerability to disruptions (spontaneous events in which the plasma energy is rapidly lost to the wall), their need for electrical current in the plasma (to produce a portion of the magnetic field that confines the plasma), and their low power density (and consequent large physical size and high cost). These drawbacks may prove susceptible to alleviation through research. Techniques to control disruptions, and improved methods of current drive, can probably be investigated by combining medium or small tokamak experiments with theory, as part of the core program. Approaches to

achieving higher plasma pressure have been identified that would make use of a predicted alternative stability regime accessible through plasma shaping and sophisticated current control; these approaches can also be investigated to some extent through small- and medium-scale experiments in the core program, but they will ultimately require testing in larger devices capable, variously, of ignition and steady-state operation (discussed below).

Alternative concepts. Progress in fusion has been driven historically by a strong evolutionary process involving parallel exploration of various confinement concepts. In this way, new ideas arise, less favorable ideas die, concepts continually improve, several fusion concepts sometimes merge into a new approach, and fusion science advances. There is a nearly continuous spectrum of alternative magnetic-confinement concepts, ranging from those that are close relatives of the tokamak to those that are radically different. Today, those that are closer to the tokamak are often moderately developed, and those further from the tokamak tend to be much less highly developed. (The principal exception is the inertial-fusion alternative, which, as a nonmagnetic approach, is very far indeed from a tokamak but is quite highly developed, both theoretically and experimentally, because of work done in the defense program.) Research into a specific alternative fusion concept is usually motivated by a perception that the concept has a possibility of evolving into a more attractive reactor than a tokamak; in addition, alternative configurations often permit investigation of valuable fusion plasma physics under conditions not possible in a tokamak. Attractive features of alternative concepts currently under study worldwide include the absence of plasma current and the associated need for current drive (such as in the stellarator); reduction in the magnetic-field requirements for confinement (the reversed-field pinch — RFP — and spherical torus); and extreme compactness (the spheromak and field-reversed configuration). Each alternative concept has advantages and disadvantages, and each presents scientific issues that must be addressed before its reactor potential can be adequately assessed.

Basic theory and computation. Theoretical and computational studies of fusion plasma physics are needed to attack all aspects of the fusion problem, from understanding of basic processes, to development of new alternative concepts, to design support for large devices. Theory has enjoyed great success in many areas, including the development of predictive capability in macroscopic plasma stability, electromagnetic wave heating, and current drive. Such theoretical advances have been used to improve the tokamak and to evolve alternative concepts. Enormous challenges remain in many areas, such as plasma turbulence and transport. Advanced computational techniques, including plasma simulation, are needed to treat the complex, nonlinear equations that describe plasma behavior, and computational physics has taken on importance equal to that of analytical methods. Theoretical and computational studies are relatively low-cost activities with enormous impact on the fusion program.

Basic experimental studies. Basic experiments, focused on specific scientific issues, constitute a modest but valuable component of the core program. Such experiments are typically not performed in experimental configurations suitable for a reactor. Their purpose, for example, is to isolate a particular physics issue and optimize the experimental design for study of that issue, rather than to test a fusion-reactor concept.

Fusion technology research. A fusion reactor will strain the limits of current technology in several ways. As these technological limitations are addressed and gradually overcome, the

fusion-reactor concept itself may change. Three particularly important technological issues are fusion-reactor materials, plasma-control systems, and the interaction of high-temperature energy conversion and tritium management:

- The development of suitable materials for a fusion reactor — especially materials that resist damage and activation under bombardment by fusion neutrons ("low-activation materials") — is perhaps the dominant technological issue. Many aspects of materials studies, such as preliminary development and testing of new low-activation materials, can be carried out within the confines of the core program. Because materials testing will ultimately require a major facility for the generation of large quantities of neutrons, however, the materials effort must eventually outgrow the core program; this larger dimension of the materials issue is discussed separately below.
- Plasma-control systems include heating and current-drive systems (such as radiofrequency waves and neutral beams) and fueling systems (such as pellet injectors). Large experiments and reactors will require advances in these systems to function in steady state and at high output. In addition, a tokamak fusion reactor will require very large, state-of-the-art superconducting magnets.
- In a fusion power reactor fueled by the D-T reaction, a "blanket" located between the plasma core and the superconducting magnets will have the dual functions of (a) regenerating tritium by means of reactions between the fusion neutrons and lithium and (b) transferring the energy of the neutrons to a high-temperature fluid medium from which the energy can be converted, outside the blanket, to electricity. The tritium is radioactive and, as an isotope of hydrogen, diffuses readily through many metals at high temperatures. Combining the functions of high-temperature energy conversion and adequate tritium production and containment poses large technical challenges.

Another important element of fusion-technology research is systems studies. These studies conceptualize a full reactor plant and examine the effects of different features on reactor attractiveness. They are extremely valuable, not as precise cost predictors, but as guides to the research themes that might have significant impact on the reactor product.

Ignition and Burn

In all existing MFE experiments, the plasma is heated by external sources. The external energy input is required to overcome inevitable leakage of energy out of the plasma. Ignition denotes the condition in which fusion conditions are self-sustaining: The alpha particles produced in the fusion reaction deposit their energy back into the plasma at a rate sufficient to keep the plasma at a fixed temperature. At ignition, the external heating can be turned off and the plasma will undergo fusion "burn" continuously.

In an ignited plasma, the alpha particles can influence the plasma behavior in ways that are difficult to predict. The difficulty is that important aspects of plasma behavior depend on plasma waves and turbulence, the description of which is at the forefront of theoretical physics research. If the alpha particles interact with the plasma in a simple fashion (for example, without influencing

plasma turbulence), then simple, well known calculations will predict the behavior of an ignited plasma. If the alpha particles alter the turbulence, however, or if their interaction with the plasma is affected by the turbulence, then current theory cannot supply reliable predictions. The kinds of calculations that are currently practical provide insight into possible new effects, but not predictive capability.

Three related issues of great interest are alpha-particle heating, alpha-particle transport, and alpha-particle-generated instabilities. It is desired that the alpha particles effectively heat the plasma so as to achieve ignition (the heating problem), that they be confined long enough to do this (the transport problem), that they be confined briefly enough that they do not dilute the fuel (the transport problem again), and that they not generate new plasma turbulence that can degrade confinement (the instability problem). The only definitive way to determine the behavior of an ignited plasma is through an ignition experiment, aided by theory. Two-fold differences in outcomes can have a dramatic effect on the prospects for fusion.

The current experiments in TFTR provide a useful example. TFTR is far from ignition, but it is generating a population of alpha particles that is large enough to explore some of the effects that alpha particles will produce in fusion reactors. The alpha particles deposit their energy into the plasma exactly as predicted by simple theory, but the plasma confinement actually improves in the presence of alpha particles. There is about a 25% improvement in the confinement time of the plasma, a completely unanticipated result that is not yet understood.

Producing an ignited plasma will be a truly notable achievement for mankind and will capture the public's imagination. Resembling a burning star, the ignited plasma will demonstrate a capability with immense potential to improve human well-being. Ignition is analogous to the first airplane flight or the first vacuum-tube computer. As in those cases, the initial model need not resemble the one that is later commercialized; much of what would be learned in a tokamak ignition experiment would be applicable both to more advanced tokamak approaches and to other confinement concepts.

An unfortunate distinguishing feature of ignition, relative to analogous seminal demonstrations in other fields, is that it will be very expensive to achieve. The cost exceeds what the United States and, probably, other nations individually are able or willing to expend. All of the leading nations in fusion research have taken the position, however, that achieving ignition is of high importance. These circumstances underlie the international decision to pursue ignition in the collaborative ITER project.

Steady-State Operation

Existing large tokamaks produce transient plasmas. Typically, the plasma duration is in the range of 1 to 10 seconds (although the proposed upgrade of the Tore Supra tokamak in France may be capable of a 1,000-second pulse). A power plant will need to generate energy continuously. Thus, it is essential that the physics and technology of fusion plasmas be tested under steady-state conditions.

From a physics viewpoint, one can argue that the plasma duration need not be truly continuous, but only larger than the longest physical time scale of interest. Many plasma physics time scales of significance, such as instability time scales, are very short — less than one-thousandth of a second. The longest plasma physics time scale of interest is thought to be the time for the plasma current to equilibrate. This time can be in the range of 100 seconds for large experiments (depending on the parameters of the particular experiment). Seeking a factor-of-ten margin beyond this level, then, would entail a plasma duration of about 1,000 seconds. The duration of most present experiments exceeds all plasma physics time scales except the current equilibration time.

An important feature of fusion experiments that extends beyond pure plasma physics considerations, however, is the interaction of the plasma with the surrounding structure. The plasma energy deposition on material surfaces releases impurity atoms into the plasma. The impurity atoms can cool the plasma and alter its behavior. The time scale for the plasma and wall to reach an equilibrium is difficult to calculate. It depends on both plasma physics and solid-state physics in complex ways. Thus, a definitive investigation of plasma behavior for fusion application requires a steady-state experiment.

As the plasma duration becomes long, the combined physics and technology issue of handling the intense energy flux to the wall becomes critical. In transient plasmas, the total energy leaving the plasma can be handled with existing materials and energy-channeling (divertor) methods. At long duration or steady state, however, the energy flux challenges the capabilities of currently available materials. Definitive tests of power-handling techniques require plasmas of long duration.

Steady-state operation is, therefore, an important milestone for fusion research. The large stellarator under construction in Japan will operate in steady state and provide valuable information, much of it transferable to tokamaks. It is also critical to examine advanced tokamak scenarios under steady-state operation. Finally, from the viewpoint of the electric-utility industry — the ultimate customer — the credibility of fusion power requires a demonstration of steady-state operation; physics arguments relating to time scales are insufficient.

Materials Development

The materials from which the reactor structure is made are critical in several respects. Fusion is distinct from fission in that the products of the fusion reaction are not themselves radioactive. The surrounding reactor structure can become radioactive, however, as a result of "neutron activation" reactions caused by bombardment of the structural materials by the neutrons released in the reaction. Complete realization of the environmental advantage of fusion requires the development of materials that yield little long-term radioactivity from these neutron-activation reactions. Fortunately, even with current materials the radiological hazards posed by a fusion reactor would be smaller than those posed by a fission reactor of similar electrical output; but the opportunity exists to increase this advantage by orders of magnitude if new materials under consideration prove suitable.

In addition to considerations of hazard minimization, it is necessary that materials retain their structural integrity after being subjected to years of intense fluxes of energy and neutrons. Development of appropriate materials to meet these structural requirements, not to mention hazard-minimization goals, will eventually require a neutron source to simulate the conditions of a fusion reactor. This could take the form of either an accelerator-based neutron source for small component tests or a volume neutron source (a fusion plasma) for more realistic large component tests. A facility of either type would be large and expensive, and, accordingly, filling this materials-testing need has been under discussion as a potential focus for international collaboration.

2.3 Status and Plans of the Current (FY1995) U.S. Fusion Program

The current program, at an annual budget of \$365 million, is composed of seven components. In order of decreasing size, these components are: moderate- to large-scale tokamak experiments, ITER design, the Tokamak Physics Experiment (TPX) design, small-scale fusion physics experiments, technology development, theory and computation, heavy-ion inertial fusion, and other small activities. (See Table 1.)

Table 1. FY1995 Fusion Budget Breakdown

Program Component =====	Funding (million FY1995\$) =====	Percent of Budget =====
Moderate- to large-scale tokamaks	139	38
ITER	71	18
TPX	42	12
Small-scale experiments	24	7
Technology research	23	6
Theory	17	5
Inertial fusion energy	9	2
Other	39	11

The activity in moderate- to large-scale tokamaks (38% of the budget) is concentrated mainly in three experiments: TFTR at Princeton, DIII-D at General Atomics, and Alcator C-MOD at M.I.T.

- TFTR, which began operation in 1975, is a tokamak of circular plasma cross section operating with D-T fuel. Recent TFTR experiments have produced nearly 11 megawatts of fusion power for a duration of about 1 second per pulse, permitting study of the behavior of the reaction-product alpha particles in the plasma. These results have been widely celebrated and constitute a significant milestone for fusion research. TFTR is scheduled for shutdown by September 1995, although the TFTR group has described a possible several-year extension that would probably yield 20 megawatts of fusion power and, more importantly, would enable further study of the dynamics of plasmas under the influence of alpha particles. Operation of TFTR will cost \$66 million in FY1995.
- After the termination of the TFTR project, DIII-D will be the largest operating tokamak in the United States. Although not capable of D-T operation, it is of more modern design than TFTR and well suited for forefront research in the advanced tokamak physics that may lead to tokamak reactors with higher power densities, hence smaller size and lower cost for a given output. Issues being studied in DIII-D include pressure limits, energy and particle transport, disruptions, self-driven currents, current-profile control with radio-frequency current drive, and divertor studies. The DIII-D group proposes to upgrade the facility to further these studies. Operation of DIII-D will cost about \$40 million in FY1995.
- Alcator C-MOD is a compact, high-magnetic-field, high-power-density tokamak. It is the most recent of a sequence of experiments that have exploited high magnetic field to advance fusion plasmas to new parameter regimes. It began full operation within the past year. Its compact size yields a very high surface power flux; thus, the device is well suited to explore the physics and operating modes of divertors. Such studies are likely to have application not only to tokamaks but also to alternative concepts. In addition, external radiofrequency heating at high power is available on C-MOD, and this machine can also contribute to advanced tokamak studies through its profile-control capability. Operation of Alcator C-MOD will cost about \$16 million in FY1995.

As indicated above, we consider DIII-D and Alcator C-MOD to be part of the U.S. "core program" in plasma science and fusion technology, whereas TFTR — like the ITER and TPX projects we describe next — is treated as a large project outside the core.⁴

ITER is an international venture in which the costs, contributions, and benefits are shared among the European Union, Japan, Russia, and the United States. Currently in the engineering-design phase, it is a very large experiment aimed at testing, in an integrated fashion, the physics of ignited and sustained-burn plasmas as well as all the key technologies needed for a fusion power plant. It is the only experiment currently planned anywhere in the world that will attack the important physics issues of ignition and sustained fusion burning. It is being designed to produce about 1,500 megawatts of fusion power during a pulse length of 1,000 seconds, with the possibility of later upgrade to steady-state operation.

⁴ In some reviews, TFTR is included in what is called "the base program" and ITER and TPX are the only projects listed outside the base.

In the technology-testing phase that is to follow an initial period of physics experiments, ITER is expected to explore such critical technological issues as the performance of high-heat-flux components, the behavior of blankets that must recover the heat of the neutrons for conversion to electricity, the response of materials under neutron bombardment, the performance of large-scale superconducting magnet systems, and the operation of the complete fusion fuel cycle, including the reprocessing and recycling of tritium fuel. ITER will not be a power plant, however, and it is not intended to generate electricity. It is a physics and technology experiment aimed at testing all key aspects of a practical fusion energy system, intended in the DOE's program plans to be the step prior to a demonstration power plant. DOE also stresses the value of ITER as a pioneering model of international collaboration, from conception to construction, in large science and technology projects.

The ITER conceptual design is complete, and the 5-year Engineering Design Activity (EDA) is under way, with completion expected in 1998. The integrated total EDA cost is about \$1 billion, split four ways. The total project cost,⁵ including construction, has not yet been set but is anticipated to be \$10-13 billion, distributed over 8 to 10 years. This latter cost will not be split four ways but is to be negotiated among the four parties: the host probably will be paying greater than a one-quarter share of the total costs, with the other partners paying less than a one-quarter share. No decision has been made yet about ITER construction; the negotiations on this subject are expected to take place over the next 2 to 3 years. If the ITER partners decide to proceed with construction on the schedule currently envisioned, plasma operation will probably commence in about 2008. At present, ITER EDA activities constitute about 19% of the U.S. fusion program, although other components of the program also perform some work that supports ITER.

TPX, a national facility presently under design, is to focus on investigating advanced tokamak physics in steady state. Compared to ITER, which is a relatively conservative physics design that would extrapolate to a large and quite costly reactor, TPX is aimed at producing the understanding leading to a more compact, economically attractive tokamak power plant. Unlike all large tokamaks to date, which operate for only several seconds, TPX will be able to operate continuously. It will not, however, use tritium fuel, except in tests toward the end of its experimental lifetime. The steady-state feature will establish whether the advanced tokamak operating modes tested to date for several seconds will persist for long time periods. TPX is to be situated at Princeton, in TFTR's present location. As a national facility, however, its management and operation will be shared by institutions spanning the U.S. fusion community, as its design has been. Already, industrial participation has been significant and at a level not found in previous fusion efforts. In FY1995, TPX design constitutes 12% of the fusion budget. Its total project cost is estimated by the DOE at about \$750 million. The start of TPX construction awaits a determination of priorities for the future of the U.S. fusion program.

About 7% of the U.S. fusion R&D budget in FY1995 is allocated to small experiments, i.e., those with annual operating costs of less than \$5 million. This work mainly involves tokamak experiments studying basic physics processes critical to fusion, such as plasma transport, as well

⁵ The term "total project cost" means, in conventional usage in this field, the cost of completing the facility but not the cost of operating it.

as novel ideas to improve the tokamak, such as new current-drive techniques or transport-suppression techniques. Many of the small experiments are located at universities.

About 6% of the FY1995 budget is allocated to technology research, which encompasses work on the development of advanced materials suitable for a fusion reactor environment, plasma technologies (such as neutral-beam systems, radio-frequency sources, and fueling systems), fusion technologies (tritium handling, neutronics), and reactor-systems studies. A substantial amount of general fusion technology R&D is also under way as part of the ITER effort described above.

Theoretical studies of fusion plasma physics make up 5% of the U.S. program. These studies address all aspects of the fusion problem, from understanding of basic processes to development of advanced tokamak concepts to ITER support studies. The theoretical program involves national laboratories, universities, and industry.

Studies of IFE using heavy ions as the driver (long believed to be the most likely IFE concept to lead to a commercial fusion reactor) receive only 2% of the fusion energy R&D budget. Work on inertial-confinement fusion in the U.S. defense program, which is based mainly on laser drivers, was funded in FY1995 at \$176 million. This spending level will increase substantially if the National Ignition Facility (NIF), a next-generation laser/inertial-fusion experiment to be built at the Lawrence Livermore National Laboratory as part of the (weapon) Stockpile Stewardship Program, goes forward as currently planned. NIF will cost about \$1 billion to build, and construction is to start in FY1997.

2.4 Current U.S. Fusion Funding in Context

Expressed in constant dollars, U.S. outlays for fusion energy in the first half of the 1990s have averaged \$330 million (1992 dollars) per year, which in real terms is about half of the level of a decade earlier (see Figure 1). Measured in as-spent dollars, U.S. funding for fusion energy research was about \$30 million (as-spent dollars) per year from the late 1950s to the early 1970s, rose steeply to about \$450 million per year in the mid-1980s, then fell to about \$300 million per year in the late 1980s. U.S. government outlays for fusion energy R&D up to 1990 totaled about \$13 billion (1992 dollars). At the current spending level, the U.S. government's outlays for fusion R&D represent about 20% of total government spending for energy-supply R&D (fossil, renewable, geothermal, fission, fusion, and end-use efficiency combined⁶) and are equivalent to about 0.15% of U.S. electricity revenues in this country.

The current U.S. fusion program plan is based on the assumption of strong future budget growth. The allocation of significant design funds for ITER and TPX will reap a significant return only if the experiments are constructed and operated. The importance of these experiments is emphasized in the DOE budget request for FY1996. Significant differences between the FY1995 budget and FY1996 plans include the following: U.S. ITER design activity increases from \$70 million to \$85 million, TPX increases from \$42 million to \$62 million as it begins construction,

⁶ It is reasonable to include energy end-use efficiency with energy "supply", because a kilowatt-hour saved by increasing efficiency is fully as useful elsewhere in the economy as a kilowatt-hour generated by a power plant.

TFTR is terminated (with some \$40 million allocated to initiation of TFTR shutdown activities), and the small experiment program decreases from \$24 million to \$21 million.

Thus, the core research program is currently being limited so as to provide funds for TPX and ITER design. If the United States decides to proceed as a full partner in ITER, this country's annual outlays for this activity after 1998 will peak at about \$500 million. TPX expenditures for construction will reach about \$135 million annually. The DOE program plan to accommodate these two large experiments plus a core research program calls for annual fusion budgets increasing from \$366 million in FY1996 to \$860 million in FY2002 and then falling to \$700 million (all as-spent dollars) in 2005. The program-plan average for the 10 years from FY1996 to FY2005 is \$645 million per year. The strong probability that fusion R&D budgets of this magnitude will not actually be forthcoming is, of course, the primary reason that PCAST was asked to convene a panel to review the current fusion R&D program and plan, as well as less costly alternatives.

2.5 Current Status and Plans of Non-U.S. Fusion Programs

The other dominant players in fusion research are the European Union, Japan, and Russia. The European Union's fusion R&D program is now nearly twice as large as that of the United States, and Japan's program is also larger than ours; together, the European Union and Japan spend about three times as much on fusion as the United States does. These other programs also exceed that of the United States in breadth and, apparently, commitment to long-term stability in funding for fusion R&D.

The current funding level of fusion R&D in the European Union — which is also the level projected for the next 4 years — is the equivalent of about \$600 million per year. The lead tokamak in Europe, JET, is located in England. JET is larger than TFTR and contains more advanced tokamak features. Several years ago, tritium was introduced to JET to produce 2 megawatts of fusion power. In a few years, tritium operation will be extended to higher power. The main role of JET in future years, however, is to provide research support to ITER, for example, in divertor studies.

There are two additional large tokamaks in Europe: Tore Supra in France and ASDEX-U in Germany. Both experiments are in the size class of the U.S. DIII-D. Tore Supra is a circular-cross-section tokamak with superconducting coils. It is capable of 60-second operation — longer than other existing tokamaks but shorter than the planned TPX. Asdex-U is a short-pulse tokamak with moderate shaping, whose research program is focused on divertor and edge physics. About five other mid-size tokamaks are also in operation throughout Europe.

The European program differs from the U.S. program in that it involves a substantial effort in selected alternative concepts, constituting nearly 20% of the total effort. Two substantial alternative concept experiments are under way, namely, a stellarator and an RFP. A medium-scale stellarator, Wendelstein-7AS, is in operation in Germany, a small stellarator is beginning operation in Spain, and a next-step device (Wendelstein-7X) is in the planning stage in Germany. The stellarator is a toroidal concept that is similar to the tokamak, but with the distinguishing feature of being free of plasma current and, therefore, inherently steady state. A large RFP

experiment in the DIII-D size class is under way in Italy, and a small RFP is in operation in Sweden. The RFP is a toroidal configuration that is also similar to the tokamak, with the distinguishing feature that the magnetic field required is about 10 times smaller than that of the tokamak. On a smaller scale, low-aspect-ratio tokamaks are under investigation in England. The low-aspect-ratio tokamak is a "fat" tokamak with a small hole in the center of the torus, leading to a more compact reactor.

The European technology development program is active in nearly all of the ITER technology task areas, including superconducting magnets, heating and current drive, blankets, tritium processing, high-heat flux components, materials, remote handling, and safety. A parallel core technology program, of about the same size as the ITER effort, emphasizes longer range research, particularly in the areas of superconducting magnets, blankets, materials, and high-heat flux components. The entire technology program is about twice the size of the U.S. program, although it includes some redundant efforts in the different European countries.

The December 1994 report of the European Union Council of Ministers states that "for the period of 1994 to 1998, the priority objective is to establish the engineering design of the Next Step within the framework of the quadripartite (ITER-EDA) cooperation." Thus, the European Union is supporting the engineering design of ITER, while maintaining "...the option of proceeding towards a European Next Step should cooperation on ITER prove too difficult to continue....". The European Union has not yet made a commitment to participate in the construction of ITER.

The Japanese program is funded at a level about 30% greater than that of the United States (which, as a fraction of GDP represents more than twice the U.S. commitment). It is striking that, 15 years ago, Japan had only a fledgling fusion program; indeed, at that time, Japan was sending teams of physicists to the United States to participate in the forefront experiments. Today, Japan is moving toward a program with two large experiments in the TFTR/JET class. A contemporary of JET, the JT-60 experiment currently in operation is a large tokamak with advanced tokamak features. In addition, a new large stellarator, the Large Helical Device (LHD), is under construction. Its total construction cost is about \$1.5 billion, and operation is expected by about 1997. The LHD experiment will investigate stellarator confinement at plasma conditions beyond those obtained in present stellarators. LHD is similar to the new stellarator planned in Germany.

In Japan, fusion R&D is funded by two separate agencies: the Japan Atomic Energy Research Institute (JAERI) and the Ministry of Education (Monbusho). The JT-60 tokamak is funded by JAERI, and the LHD stellarator is funded by Monbusho. LHD is intended to be a national experiment involving researchers from universities across Japan. It will be housed in the new National Institute for Fusion Science. Medium-sized tokamaks and stellarators are also in operation in national laboratories and universities. There are various experiments in alternative concepts, including, in addition to the stellarator, a large tandem mirror and small efforts in the RFP, spheromak, and field-reversed configurations. The latter two belong to a class of configurations called compact tori, which have an aspect ratio of 1 (no hole in the center). More than a third of the total fusion effort in Japan is devoted to alternative concepts.

Japan also has a technology-development program of the same magnitude as that in Europe (almost double that of the United States). The Japanese are active in virtually all areas of ITER technology, and operate a similarly sized core technology program that emphasizes fundamental new technology studies and materials research in universities. Areas of particular strength include advanced superconductors, materials, remote handling, and neutral-beam heating.

The Japanese program is characterized by stability for relatively long periods of time into the future. There is a strong commitment to ITER, and an upgrade of the JT-60 tokamak (called JT-60 Super Upgrade or JT-60 SU) is being considered. JT-60 SU would be a very large steady-state advanced tokamak. Japanese industry is more intimately involved in the fusion program than U.S. industry is in this country's fusion program. Japanese industry plays a central role in the design and construction of fusion experiments, and many industrial scientists and engineers consider themselves to be fusion professionals.

The former Soviet Union was a pioneer in fusion research. Early theory and experiments in Russia led to development of the tokamak. The Soviets developed gyrotrons and were in the forefront of radio-frequency heating of plasmas (now widely used). In recent years, the difficult economic situation has greatly weakened the Russian effort, but medium-sized tokamaks and a stellarator are in operation. Despite the internal difficulties, Russia has contributed its full share of the manpower requirements for the ITER EDA.

2.6 Role of International Cooperation in Fusion R&D

The extraordinary extent of international cooperation in MFE R&D, now extending over nearly four decades, is attributable to a number of factors. Early in the effort, cooperation was facilitated by the recognition that MFE had no significant overlap with the science and technology of thermonuclear weapons, as well as by the recognition that commercial applications, with their attendant competitive pressures, were quite distant in time. Another important factor was the complementarity of different countries' relevant technical strengths at the time: theory in the Soviet Union, diagnostics and engineering in the West.

More recently, the most important driver for maintaining vigorous international cooperation in fusion R&D probably has been the realization that achieving commercialization of fusion is going to be much costlier than was imagined when the effort began. It has become apparent that not only the physics but also the technology of fusion energy pose problems of immense complexity and difficulty. Although some aspects of plasma science and fusion technology can be investigated in experiments of modest scale, it turns out that pushing the frontiers in relation to ignition, steady-state operation, power- and fuel-handling technologies, and materials development requires at least some facilities of very large scale and cost. The corresponding incentives to divide some of these missions among countries — and to build the very largest integrated facilities jointly — are obvious.

The cooperation in fusion R&D in its early years, immediately after the British measurements confirming the performance of the Soviet T-3 tokamak, was extensive but relatively informal. It included exchanges of scientists among the major fusion research centers in the United States, Europe, and the Soviet Union, as well as international conferences to share

and compare designs for next-generation experiments. These interactions were instrumental in generating coordination and complementarity among the research objectives pursued in the major facilities of the various countries — visible today as D-T operation in TFTR, noncircular cross section and high current in JET, the divertor-development and impurity-control focuses of JT-60, superconducting coils in T-15, and so on.

As the cooperation continued, many aspects were formalized in negotiated agreements, which currently include the following:

- International Energy Agency (IEA) agreements for cooperation in the medium- and large-scale tokamak experiments, as well as in RFPs and stellarators, in the United States, Europe, and Japan;
- U.S.-EURATOM collaboration on the Tore Supra tokamak in France, the FTU tokamak in Italy, and JET in England;
- an International Atomic Energy Agency agreement for cooperation in fusion-reactor safety studies; and
- bilateral agreements on various aspects of fusion R&D linking the United States with Japan, Russia, Canada, and China.

These collaborations involve, variously, short-term and long-term exchanges of research teams and equipment, coordinated experiments at different facilities, joint planning, and data exchange. Specific examples include:

- installation and operation, by a U.S. team, of a pellet fuel injector at JET; the injector, built at the Oak Ridge National Laboratory in the United States, led to discovery of improved operating regimes for JET;
- financing, by Japan, of the neutral-beam heating system on the Doublet-III machine in San Diego, and training of a Japanese team at the Doublet-III facility preparatory to the start-up of Japan's first large tokamak, the JT-60;
- design of the Japanese LHD stellarator now under construction based on studies conducted at Oak Ridge;
- participation by the European Union and Japan in the Tritium Systems Test Assembly at the Los Alamos National Laboratory; and
- successful completion, under an IEA agreement, of the Large Coil Project, in which six superconducting coils of different designs were built in the United States, Europe, and Japan and then assembled and tested at the Oak Ridge National Laboratory.

The most impressive international collaboration in fusion R&D to date is, of course, the ITER project, the technical features of which have already been described above. The ITER

effort is being conducted under formal agreements among the United States, the European Union, Japan, and Russia. In the conceptual-design phase, which ran from January 1988 to December 1990, all four parties contributed personnel and funding to the ITER design team based in Garching, Germany. In the engineering-design phase now under way, a Joint Central Design effort involving multinational teams at each of three sites — Garching, San Diego, and Naka, Japan — is complemented by efforts of Home Teams based on the territories of each of the four partners. About 1200 scientists and engineers are involved in the Joint Central Team and the Home Teams combined. The aim of the engineering-design effort is to produce "a detailed, complete, and fully integrated engineering design of ITER and all technical data necessary for future decisions on the construction of ITER." Proceeding with construction is not a foregone conclusion, however, and will require a further formal agreement that is to be negotiated on completion of the design; issues to be settled include which country will host the actual facility and how the costs of construction will be divided among the host country and the other partners.

2.7 The Pace of Progress in Fusion R&D

Progress in fusion can be gauged in two ways, both of which demonstrate the remarkable advances that have taken place in fusion research over the past decade. The first measure of progress involves the key figures of merit relating to plasma quality, such as density (n), temperature (T), energy confinement time (τ), the triple product of these parameters ($nT\tau$), the fusion power (P_f), and the fusion-power amplification factor ($Q = P_f$ divided by the input power to the plasma). These plasma-quality factors reflect both the state of understanding in plasma physics and the state of development of plasma technologies (magnets, plasma heating and fueling systems, and so on) needed to translate physics understanding into operational plasmas.

All of the plasma-quality indicators — including, above all, the integrating, "bottom line" indicators of fusion power and amplification factor — have increased over the years at rates that have been extraordinary in steadiness and magnitude. The increase in fusion power over the past 20 years has been a factor of 100 million, from 0.1 watt in 1975 to more than 10 million watts in 1995. (This progression is shown in Figure 2.) The amplification factor, Q , has increased similarly, to a point now very close to the "break-even" value of $Q=1$. (JET has obtained a projected Q value — that which would have been obtained if the fuel had been D-T — of 0.7) The rapid and steady growth of these performance parameters over time compares favorably with the growth rates exhibited by the performance of computer chips in the same time period.

The second measure of progress is the extent of the qualitative developments in plasma physics that, in fact, have underpinned the progress in the quantitative indicators just described. Illustrative of these qualitative developments and their impact are the improvements that have taken place in understanding of plasma pressure limits, of the self-generated "bootstrap" currents that occur in tokamaks, and of the H-mode of tokamak plasma operation.

- If a fusion reactor is to have suitably high power density, the plasma pressure (the product of density and temperature) must be high. Plasmas tend to become unstable beyond a threshold in pressure, however. Above this pressure limit, large waves develop that throw the plasma against the surrounding vessel, which cools the plasma and quenches the fusion reaction. A relatively complete theory of such processes, which are called macroscopic

instabilities, has been formulated within the framework of magnetohydrodynamics (MHD), a construct in which plasma is analyzed as a conducting fluid. The theory is able to predict the pressure limit and the dynamics of the instability, and has been confirmed in numerous experiments. In combination with large-scale computation, moreover, the theory can be used to modify tokamak design to increase the pressure limit, as well as to identify alternative configurations that might outperform tokamaks. Current advanced tokamak concepts arose from the use of this theory. It has also proven to be of great usefulness in space physics.

- Some 20 years ago, it was predicted that a tokamak plasma would spontaneously generate its own current, even in the absence of an electric field. This "bootstrap" current is driven by pressure gradients within the plasma, a subtle result of the complex particle orbits within a tokamak. It can reduce the necessity for external current drive systems, thereby simplifying and reducing the cost of a tokamak reactor. Long considered only a theoretical hope, or "too good to be true," the phenomenon now has been confirmed in a series of experiments, showing that millions of amps of plasma current can be sustained by plasma pressure gradients. Bootstrap currents, at the forefront of theoretical plasma physics just 10 years ago, today are used routinely by engineers designing improved reactor configurations.
- Some 10 years ago, it was discovered empirically that, under certain conditions the energy confinement time in tokamaks roughly doubled from its expected value. This H-mode of tokamak behavior (shorthand for high-energy-confinement mode) has since been observed in many different experiments, strongly indicating that the phenomenon is not an artifact of a particular device. In contrast to the bootstrap current story, where theory led to experiments that confirmed the result, in the H-mode case an experimental result led to development of a line of theoretical research. The still unfolding H-mode theory has shown that the plasma turbulence responsible for energy leakage from the plasma is reduced in the H-mode by strongly sheared rotational flow of plasma near the plasma edge. This theory is at the forefront of turbulence research, a notoriously challenging area in 20th century physics. It has, nonetheless, led already to practical techniques for controlling plasma turbulence to improve energy confinement.

These examples, which illustrate — although not exhaustively — the striking progress in understanding fusion plasmas, are all classic cases of the interaction of theory and experiment through which science normally progresses. In combination with other advances, these improvements in understanding have been responsible for the 10-million-fold improvement in Q and the 100-million-fold increase in fusion power achieved in MFE research during the past 25 years. Only a rather modest further extension of this progress is now required to reach the performance range needed for ignition.

Chapter 3 EVALUATION OF THE CURRENT U.S. PROGRAM AND PLAN

The reasonableness of a program of R&D can be judged on both "internal" and "external" criteria:

- The internal criteria have to do with whether the goal of the program is reasonable; with whether the program elements likely to be necessary to achieve the goal are all present, including redundancy appropriate to the level of uncertainty about the outcomes of particular elements and to the urgency of the goal; with whether the individual elements are suitably designed for their purposes; with whether they are suitably linked and phased; and with whether they are being funded at levels commensurate with the tasks involved, the opportunities available, the other (competing or cooperating) entities pursuing or likely to pursue the same goal, and the timing desired.
- The external criteria have to do with whether the funding allocated to the program can be justified in relation to the total resources available in the agency, or in the society, for similar activities, e.g., for all energy R&D or for all R&D in general; judgments in this category entail reaching conclusions about the importance and prospects of attainment of a program's goals in relation to its cost and in relation to the importance/prospects/cost combinations of other R&D programs.

Our conclusions here about the reasonableness of the U.S. fusion energy R&D program are necessarily confined mainly to the internal criteria. We had neither the mandate nor the resources nor the time to compare fusion R&D with other areas of energy R&D, not to mention with other areas of research such as, for example, high-energy physics, space, or global environmental change.

We do assert, relevant to the external criteria, that the absolute importance of having economically affordable, environmentally tolerable, and politically acceptable energy sources adequate to meet society's energy needs in the middle of the next century is high enough to warrant substantial R&D investments to increase the likelihood of this outcome, and that the arguments are compelling for attempting to develop all of the major, long-term energy options to the point that their potential to contribute can be confidently assessed. We also assert that, in light of the observation that successful high-technology enterprises typically invest 2-3 percent of gross revenues in R&D, and in light of U.S. gross electricity revenues' running currently in the range of \$200 billion per year, it would be perfectly reasonable for U.S. electricity-supply R&D to be spending \$4-6 billion per year. If half of that were spent on long-term options and a third of the half were spent on fusion, the result would be national fusion-energy R&D expenditures of \$0.7-1 billion per year. (This is without reference to the non-energy benefits of fusion R&D, such as advances in basic science and spinoffs into technologies for non-energy applications.)

As for our evaluation of the reasonableness of the DOE's program of fusion-energy R&D on the "internal" criteria, which is an important part of our charge, we have been able to draw upon a number of major reviews of the program that have been conducted by others in the period

since 1989¹; on analyses conducted for and embodied in the Energy Policy Act of 1992 (Public Law 102-486, 24 October 1992); and on an extensive series of briefings to our Panel, during the two and a half months of our study, by participants in all of the main elements of the U.S. program and by those responsible for overseeing it (in DOE) and for reviewing its budget (in the Office of Management and Budget).² Our conclusions about the reasonableness of the program on the "internal" criteria, based on these inputs and our own deliberations, follow.

3.1 Goal of the Program

The National Energy Strategy (NES) formulated during the last 2 years of the Bush Administration called for a vigorous program of fusion energy R&D aimed at operation of a demonstration reactor by about 2025 and operation of commercial fusion power plants by about 2040. This timetable has been taken seriously by DOE, and it has been the basis for DOE's choices about how to shape the program and for its plans and expectations about budgets.

Is it a reasonable goal? For an affirmative answer, the goal should be both desirable and potentially attainable. In view of the importance of having fusion available as a commercial energy source by the middle of the next century (Chapter 1), we believe the goal is clearly desirable. In view of the pace of progress in plasma physics and fusion technology over the past two decades (Chapter 2), we also believe there would be a reasonable probability of attaining the goal — that is, of meeting the NES timetable or something close to it — assuming that the requisite R&D funds were available.

The conclusion that trying to harness fusion energy on this time scale is reasonable is supported explicitly or implicitly in all of the recent reviews, as well as having been repeatedly reaffirmed by the U.S. Congress. Acceptance of this goal was also nearly unanimous among our briefers.

3.2 Presence of Needed Elements

The previous reviews and our own briefers were also in striking agreement on what elements need to be present in a sensible program to achieve the indicated goal: a strong and broad "core program" of theory and experiment in plasma science and fusion technology, plus reactor-systems studies linking these ingredients; early demonstration of ignition and reasonably prolonged burn of a fusion plasma; investigation and demonstration of the application of advances in confinement physics needed for steady-state operation and for the design of more compact, more economical reactors; development and demonstration, at near-commercial scale, of the fuel-management and power-handling technologies needed to turn a fusion plasma into an

¹ These reviews include the late 1989 report of the Committee on Magnetic Fusion in Energy Policy of the Energy Engineering Board of the National Research Council (NRC 1989), the September 1990 report of the Fusion Policy Advisory Committee to the Secretary of Energy (FPAC 1990), the September 1991 report of the Fusion Task Force of the Secretary of Energy Advisory Board (SEAB 1991), the September 1992 report of the Fusion Energy Advisory Committee to the Secretary of Energy (FEAC 1992), and the February 1995 report on the fusion energy program by the Office of Technology Assessment of the U.S. Congress (OTA 1995).

² The briefings received by the Panel are listed in an Appendix to this report.

electric power source; exploration, at a lower level of effort, of alternative confinement concepts that may be worthy of further development if the mainline (tokamak) concept falls short; development of advanced fusion-reactor materials that can reduce radioactivity burdens and enhance reactor performance; increased engagement of industry in the fusion development effort; and strong international cooperation to exploit diverse capabilities and share costs.

Concerning the actual presence of these elements in the U.S. program, it is clear from the discussion in Chapter 2 that:

- ITER is to provide the ignition demonstration together with a platform for development and demonstration of relevant fuel-management and power-handling technologies, in an international context; and
- TPX and, to a lesser extent, continued operation of the medium-sized tokamaks at GA and MIT are to provide the U.S. contribution to exploring applications of advanced confinement physics and the attainment of steady-state tokamak operation.

In addition, there is widespread agreement among the previous reviews and our briefers — and we also agree — that

- although a strong core program has existed and continues to exist within the U.S. effort, it is not as broad or as strong as is desirable, it is focusing increasingly on support of the ITER and TPX projects, and it is in danger of being squeezed down to inadequacy by the drain on the budget generated by these projects as they enter their construction phases;
- the effort on alternative concepts, which was practically eliminated in the series of project cancellations brought on by program budget cuts through the 1980s and into the early 1990s, is now wholly inadequate;
- the effort on advanced materials, although singled out as a priority by virtually every review, has never received the resources it requires and deserves — and might well not receive them in the future, even under the overall budget increases reflected in current DOE planning;
- the engagement of industry in the U.S. fusion effort has been growing, but needs to be further strengthened.

The necessary ingredients, then, although all are present, are not all adequate, and redundancy is weak. We elaborate on these points in the element-by-element evaluation of the program that follows.

3.3 Evaluation of Individual Elements

In what follows, we look more closely at the core program, TFTR, ITER, and TPX, in turn.

Core Program

The core program, which was described in some detail in Chapter 2, consists of work in tokamak improvements, alternative concepts, basic theory and computation, basic experimental studies, and fusion-technology research (including materials development). As noted in Chapter 2, the combination of a declining budget (relative to a decade ago) and increasing funds allocated to design of ITER and TPX has yielded a core program only a fraction of its former size. Its diminution has produced a widespread concern in the fusion community that the program has become dangerously out of balance — too large a sum is being gambled on future large experiments which may not even be built, at the expense of the essential core program of active research. Particularly troublesome constraints are being felt in alternative concepts, basic theory and computation, basic experiments, and materials development. These constraints clearly threaten progress in fusion.

The most glaring victim of the decline in emphasis on the core program has been research on alternative concepts. The disappearance of most of the alternative-concept work from the U.S. program over the past decade is chronicled in Table 2. Although we fully understand the character of the budgetary pressures that contributed to this result, we believe that the tiny fraction of U.S. fusion R&D funds currently being devoted to alternative concepts is wholly inadequate. It is clear to us that interesting ideas exist at present that deserve experimentation. Many of these ideas can be advanced, moreover, in small- and medium-sized experiments; they are not at the point in the development trajectory characterizing the tokamak, where large facilities are needed to gain increased understanding. We think scientific common sense requires sustaining a vigorous program in alternative concepts. Other recent reviews of the fusion program have consistently made the same point.

The only alternative concept of any real vitality in the U.S. fusion program at present is the nonmagnetic alternative — namely, the work on inertial-confinement fusion that has progressed not because of support from the fusion-energy program but because of support in the defense budget to pursue insights about nuclear weaponry through inertial confinement research (see Section 2.3). The small allocation for IFE studies in the current MFE budget is intended to explore aspects of IFE that would be important in reactor applications but are not being pursued in the defense-funded inertial confinement research. It is not clear that the amount is adequate even for that limited purpose.

TFTR

Princeton's TFTR is the lead tokamak in the United States and a peer of the JET tokamak in Europe and the JT-60 tokamak in Japan. It began operation in December 1982, with plans for two phases of operation: a deuterium phase to study the plasma physics of a large tokamak and a D-T phase to demonstrate fusion energy production on a pulsed basis. As indicated briefly in Chapter 2, TFTR has been a highly successful experiment: All of the objectives set for it prior to its operation have been met or exceeded within the anticipated budget, although the schedule has been constrained by annual funding rates.

Table 2. Fate of Alternative MFE Concepts in the United States Since 1985^a

Concept	Facility/ Location ^b	Construc- tion Cost (million \$)	Fate of Project
Mirror machine	MFTF-B/ LLNL	372	Closed in 1986 upon completion of construction.
Stellarator	ATF/ ORNL	19	Operated intermittently since opening in 1990, mothballed 1994.
Field-reversed configuration	LSX/ U of WA	14	Operated minimally upon completion in 1992; being relocated for use in tokamak fueling experiments.
Reversed-field pinch	CPRF/ LANL	58 ^c	Cancelled during construction, 1992.
Reversed-field pinch	MST/ U of WI	4	Operated at reduced budget since opening in 1988.
Spheromak	MS/ U of MD	4	Maryland spheromak phased out in 1992 without attaining expected performance.
Spheromak	S-1/ PPPL	9	Constructed in 1983, operated until 1987.

^a Source: OTA (1995), Table 4-4.

^b Abbreviations: MFTF-B/LLNL, Mirror Fusion Test Facility, B. Lawrence Livermore National Laboratory; ATF/ORNL, Advanced Toroidal Facility, Oak Ridge National Laboratory; LSX/U of WA, Large S Experiment, University of Washington; CPRF/LANL, Confinement Physics Research Facility, Los Alamos National Laboratory; MST/U of WI, Madison Symmetric Torus, University of Wisconsin; MS/U of MD, Maryland Spheromak, University of Maryland; S-1/PPPL, Spheromak-1, Princeton Plasma Physics Laboratory.

^c Unfinished

In its deuterium phase, TFTR achieved a record ion temperature of nearly 500 million degrees C, provided the first experimental demonstration of the bootstrap current in a tokamak, and demonstrated how to attain very high plasma pressures using current-profile control. In its D-T phase, TFTR attained $P_f = 10.7$ megawatts and $Q = 0.3$, as mentioned above, as well as producing a range of alpha-particle physics results of great interest.³ The TFTR D-T experiments, which involve extensive tritium handling, have been performed with an excellent safety record — a significant engineering and management success. Although TFTR is scheduled to shut down at the end of FY1995, the TFTR group has developed plausible plans for operating it longer to extract additional benefit from its unique capabilities, if funds are available.

ITER

Previous reviews of the U.S. program in general — and of ITER in particular — have emphasized, and we agree, that

- ITER's primary mission of ignition and sustained burn, and its secondary mission of utilizing its burning plasma as the core of a test-bed for development and testing of key components of fusion-reactor technology, are essential milestones on the path toward a demonstration reactor;
- the current design of ITER represents an intelligent approach to accomplishing the primary and secondary missions with high confidence; that it is a large, complex, costly tokamak based on relatively conservative confinement physics was the natural result of the desire of all of the partners (i) to cover enough ground with ITER to make design and construction of a demo possible at the next step (thus making it conceivable that a demo could operate by 2025 or so) while (ii) minimizing the chance that ITER would fail to ignite (which, obviously, would be a huge setback for progress toward fusion energy);
- given the magnitude of this project, international cooperation in financing it and carrying it out not only makes excellent sense, it is practically a necessity; and
- the international cooperation on ITER so far, through the conceptual-design phase and well into the engineering-design phase, has been very successful (notwithstanding the predictable difficulties of conducting international negotiations about every step) not only in cost-sharing but also in building highly effective multinational technical collaborations; this success has made the ITER project a highly visible example of the potential for international cooperation on complex and costly science-and-technology projects.

At the same time, certain reservations about the ITER project have emerged from prior reviews and, as evidenced by the briefings and letters provided to our Panel by the U.S. fusion community,

³ First-time measurements have been performed on alpha-particle slowing down, alpha-particle confinement, and a range of other issues relevant to the influence of alpha particles on plasma behavior and the prospects of ignition. (See also the subsection on ignition in Chapter 2.)

are being felt with increasing urgency at a time of severe constraints on funding for R&D in general and fusion in particular. Specifically:

- even divided four ways (among the United States, the European Union, Japan, and Russia), and even before the construction phase begins, the costs of ITER have imposed a significant drain on the U.S. fusion budget, severely constraining the availability of funds for the existing major U.S. tokamak experiments, for alternative concepts, and for other ingredients of the core program including plasma theory and materials development;
- the higher budgets that must follow if ITER is to proceed to the construction phase will be even more problematic from the standpoint of the ability of the United States to maintain needed depth and diversity in its domestic program of fusion research; and a science-and-technology project this costly — \$10-13 billion for design, construction, and operation — is vulnerable to budgetary pressures arising in ANY of the partners; thus ITER may amount to putting too many of fusion's eggs in a very fragile international basket;
- by relying on a conservative approach to confinement physics, ITER will (at best) end up demonstrating an approach to fusion that is unlikely to be extrapolatable to an attractive commercial reactor — the power density in this approach would be low, the physical size of the plant and hence the cost of construction and maintenance would be high, and the minimum practical unit size would be so large as to restrict interest in such plants to only the largest electric utilities; a demonstration of this approach, even if completely successful in physics and technology-development terms, could be a setback for fusion's prospects of commercialization unless accompanied by parallel demonstrations of approaches likely to lead to more compact, more economical reactors.

Our own synthesis of the attractions and liabilities of the ITER project is that

- ITER as currently construed is a reasonable approach to achieving, in one device and on a time scale commensurate with operation of a demo around 2025, the key ignition, prolonged-burn, and technology-development steps essential to such a timetable;
- in order to avoid both the appearance and the possible reality of locking fusion development onto a pathway toward a suboptimal reactor, the currently programmed ITER would need to be accompanied by other domestic or international projects to demonstrate directions of tokamak evolution, or development of alternative approaches, that are more likely than ITER *per se* to lead to an attractive reactor; and
- if U.S. (and/or other) fusion R&D budgets are not likely to permit pursuing the multi-pronged approach just described, then ITER and the rest of the international fusion collaboration will need to be somewhat restructured; a set of recommendations for how to proceed in this direction, in a budget-constrained case, is presented in Chapter 4.

TPX

TPX, as discussed in Chapter 2, would be a national facility located at Princeton and would study advanced tokamak plasmas under steady-state conditions. The origin of the project can be traced to the 1991 recommendation of a Secretary of Energy Advisory Board (SEAB) Task Force to cancel BPX, then under design, for fiscal reasons. The SEAB Task Force recommended that a less costly machine — in the \$0.5 billion range — be developed instead, and the TPX focus and design then resulted from a national process established to determine the best way to proceed within the indicated fiscal constraint.

The TPX steady-state experiment is complementary to ITER (or any other ignition experiment): As indicated in the discussions of ignition and steady-state operation in Chapter 2, both missions are critical to achieving practical fusion energy. An experiment of either type can be justified either in the presence or in the absence of the other. Tokamak physics is sufficiently advanced to permit construction of an ignition machine with a high probability of success; but attainment of ignition will not, in itself, establish that commercially attractive fusion reactors are feasible. This demonstration may well depend on the sorts of advanced tokamak physics that TPX has been designed to test and that offer the possibility of more compact, more economical fusion reactors.

TPX is now ready to begin construction. The design produced by the nationally constituted project has been extensively and favorably reviewed; it appears to do an excellent job of reconciling physics goals and engineering constraints, and would lead to a machine costing about \$750 million. The degree of industrial involvement in the project that has been envisioned would be a significant benefit, providing industry with experience in fusion technology ranging from construction of superconducting magnets to systems integration. We believe that proceeding with TPX would be highly desirable, given funding for U.S. fusion R&D in the range planned for by DOE in connection with the 2025 demonstration-reactor goal.

3.4 Linking and Phasing of Program Elements

The elements of the current U.S. program are, for the most part, complementary and well coordinated with each other, with fusion R&D in other countries, and with the international collaborative effort in fusion energy. This positive situation is the result of several factors: the high degree of communication and interaction within the U.S. fusion community and between it and the world fusion community; the conscious decisions made by the program's managers in the DOE, and their counterparts in other countries, to devote different machines to different key tasks in fusion development, avoiding excessive duplication of effort; the exceptional frequency and diligence of the high-level reviews of the direction and priorities of the U.S. fusion energy R&D program (possibly the most reviewed science and technology program in history) and in the ITER project; and, less positively, the overwhelming dominance of the tokamak line of development within the U.S. program, which has ensured that even most of the basic theory and basic experimental work in the core program relate to tokamaks. Most of the specifics that add up to this high degree of coordination and complementarity have already been described in earlier sections.

Probably the strongest criticism that can be leveled at the U.S. and world programs with respect to the phasing of their elements relates to the argument, mentioned above, that the ITER experiment in ignition, sustained burn, and associated technology testing might benefit greatly from the kinds of developments only now emerging in advanced tokamak physics. From this standpoint, it might have been better if ITER had come later, in that physics advances being demonstrated in current-generation and next-generation machines at a scale smaller than ITER could have allowed the ITER goals to be attained in a more compact, less costly device that better pointed the way to an attractive reactor. It can be argued on the other side, however, that the timing of ITER has been dictated by the desire to operate a demonstration reactor by about 2025 and, perhaps even more relevant, the desire to achieve ignition and sustained burn within the lifetimes of some of the scientists whose careers have been devoted to fusion energy and some the legislators voting for fusion budgets.

A somewhat related criticism of the phasing of program elements is that development of advanced materials for fusion reactors has not proceeded at a sufficient pace to enable ITER to employ such materials in its principal components (although it may be used in its technology-testing phase to test components made of such materials). Materials research, despite much lip service, simply has not yet been given the priority or the funding that will be required for it to affect choices for next-generation machines. So far this is really more a problem of omission than of phasing.

3.5 Adequacy of Funding

The last decade's decline in U.S. government spending on fusion R&D was not the result of diminished capacity to spend the money effectively, or of diminished prospects for success in reaching the energy goal, or of informed conviction that other energy sources will suffice to meet the energy needs of the next century in economically affordable, environmentally tolerable, and politically acceptable ways. Quite the contrary, by all of these indicators spending on fusion should have increased, as it has in Europe and Japan. Rather, the U.S. decline was a consequence of pressures on the Federal budget generally, and on energy R&D budgets particularly, occasioned in part by the belief that much of what the Federal government has been doing could be done more efficiently by state and local governments or by the private sector.

In the case of fusion, however, any expectation that a substantial part of the funding for current and near-term-future fusion energy R&D will come from the private sector is not realistic. The investments are too large, and the possibility of economic returns is too distant, to elicit significant funding from the private sector at this stage of fusion-energy development. Because fusion is not of particularly greater relevance to one state or region than to another, and also because of the size of the investments required, it is not realistic to expect significant investment in fusion energy R&D from states, either. Indeed, the investments required to pursue fusion energy development to its logical next steps are so large as to threaten to exceed what even the richest industrial nations are prepared to invest in individual projects; as noted above, this situation constitutes one of the primary incentives for international collaboration on these steps. Thus, the characteristics of fusion energy R&D — very large potential benefits for society as a whole, large investments required to secure these benefits, commercial application decades away

— constitute a classic case for bearing the funding burden at the level of the Federal government and, indeed, at the level of a consortium of governments.

It is important to understand that the relatively high cost of fusion energy R&D is not a consequence of profligacy on the part of fusion researchers or mismanagement on the part of DOE. It is a consequence of inherent properties of the most promising approaches to harnessing fusion that have been discovered so far, for which the energetics and scaling are such that ignition can only be approached, attained, and studied in devices of great size and technological sophistication. This is in sharp contrast with many other branches of energy research, which can be productively pursued at much smaller scales and lower costs.⁴ That does not mean, however, that only these other energy options, and not fusion, deserve to be pursued. We have argued in Chapter 1 that civilization is likely to need, in the middle of the next century and beyond, all of the safe, affordable, environmentally tolerable, politically acceptable energy it can get. If some of the options that can meet these criteria are costlier to develop than others, they nonetheless *should* be developed as long as their prospective benefits are much higher than their development costs.⁵ We believe that fusion R&D meets this test and that, therefore, prior decisions to provide substantial funding for fusion R&D were not wrong.

The funding provided, in fact, has generally not been as great as the major reviews of the program have recommended. For example:

- A National Research Council review of the MFE program completed in 1989 concluded that funding would have to be increased by 20% each year in the early 1990s and by larger amounts in the late 1990s to allow the United States to proceed with CIT and with ITER construction, and it recommended that this be done (NRC 1989).
- The Fusion Policy Advisory Committee of the Energy Research Advisory Board concluded in 1990 that appropriate progress of the MFE program toward the objective of a demonstration reactor by 2025 would require increasing the budget from \$316 million in FY1990 to more than \$600 million (FY1990 dollars) in FY1996; even FPAC's "constrained" program called for the MFE budget to increase to \$470 million (FY1990 dollars) by FY1996 (FPAC 1990).
- Even after the cancellation of the BPX (into which CIT had evolved), the SEAB Task Force on Energy Research Priorities concluded in September 1991 that "The Task Force

⁴ We note also that this property of fusion science — the requirement for devices of great size, sophistication, and cost in order to make major advances — is *not* in contrast with, but in fact very much resembles, the situation in other branches of science (such as nuclear physics and high-energy physics) with which plasma physics could be considered comparable in importance *as* science, irrespective of its energy applications.

⁵ One member of the Panel (not from the fusion community) formulated this proposition, in the course of our discussions, in a way that bears repeating here: If you are offered three investments, each with a net present expected value of \$100, and two of the investments cost \$1 while the third costs \$10, the correct strategy is not to make just the first two investments but to make all three (assuming that there are no other \$100 gains to be had at the \$1 price, and assuming that the difference between ending up with \$200 and ending up with \$300 is important to you).

believes that funding for the magnetic fusion program must increase at a modest rate (e.g., 5% real growth per year)...". (SEAB 1991).

- The Fusion Energy Advisory Committee for DOE's Office of Fusion Energy concluded in September 1992 that MFE budgets would have to increase by at least 5% per year in real terms over the FY1993 level of \$331 million, reaching \$420 million (in FY1993 dollars) in FY1998, with a further increment thereafter for ITER construction, to be plausibly consistent with the 2025 target date for operation of a demonstration reactor (FEAC 1992).

None of these recommended increases materialized. The U.S. MFE budget has remained essentially flat in real terms since FY1990. No one has relieved DOE's Office of Fusion Energy of the burden of pursuing the NES goal of a demonstration reactor in 2025, but neither has the government seen fit to provide the money that every review has concluded would be needed to meet that goal.

Accordingly, the OFE has continued to develop program plans, and corresponding budget projections, that are plausibly consistent with the NES timetable. As we noted above, the current OFE program plan calls for a budget that rises steadily from \$366 million in FY1996 to \$860 million in FY2002 before falling to about \$700 million in FY2005 (all as-spent dollars). The average for the 10-year period from FY1996 to FY2005 would be \$645 million per year. We believe these budgets are reasonable. Indeed, they are the minimum amounts required to support full U.S. participation in the construction phase of ITER as currently envisioned while maintaining a vigorous, complementary domestic program that (a) extracts the remaining scientific value from experimental facilities already in operation; (b) constructs the TPX to explore crucial issues not accessible in existing devices or in ITER, and to anchor the domestic experimental program in the next century; and (c) nourishes essential efforts in smaller experiments (including alternative concepts), theory, computing, technology development, and fusion-reactor materials. It almost certainly would not be possible to spend less and still meet the NES fusion timetable. The indicated amount also can hardly be said to be beyond the financial means of the United States; \$645 million per year could be raised with a 0.3% tax on current U.S. electricity sales.

None of this changes the political reality of the current budget-cutting climate, however, and that reality indicates that fusion funding at levels even approaching those in the OFE's current plan will not be forthcoming. We devote most of the next chapter, therefore, to an analysis of how the most important priorities within fusion energy R&D could be preserved at lower budget levels.

Chapter 4 RECAPITULATION, STRATEGY, AND RECOMMENDED PROGRAM

In this chapter, we recapitulate the main findings from Chapters 1-3, describe the principles and priorities appropriate to managing the U.S. fusion energy R&D program in a budget-constrained environment, and present and explain our recommendations about specific elements of a suitable budget-constrained program.

4.1 Recapitulation

Funding for fusion energy R&D by the Federal government is an important investment in the development of an attractive and possibly essential new energy source for this country and the world in the middle of the next century and beyond. This funding also sustains an important field of scientific research — plasma science — in which the United States is the world leader and which has generated a panoply of insights and techniques widely applicable in other fields of science and in industry. And U.S. funding has been crucial to a productive, equitable, and durable international collaboration in fusion science and technology that represents the most important instance of international scientific cooperation in history as well as the best hope for timely commercialization of fusion energy at affordable cost.

World energy demand in 2050 is likely to be at least twice as large as it was in 1990, electricity demand at least three times as large as in 1990. Failure to make sufficient electricity and other energy forms available in safe, economically affordable, environmentally tolerable, and politically acceptable ways will be a prescription for widespread frustration of economic aspirations, accompanied by social tensions, political instability, and environmental deterioration. Other than nuclear fusion, the options potentially available for meeting the energy and electricity demands of the next century are fossil fuels, renewable energy sources, and nuclear fission. Each of these options is likely to be playing a role in world energy supply in 2050 and beyond; each has the potential for improvement over its technical, economic, and environmental characteristics of today, and deserves investment to achieve this potential; and each, unfortunately, is subject to shortcomings and constraints that could limit its contribution.

Fusion energy offers (a) fuel extractable from ordinary seawater (thus available to all countries) and sufficient in quantity for millions to billions of years; (b) significant advantages over fission-energy options with respect to possibilities of minimizing radiological hazards and links to nuclear weaponry, over fossil-fuel options with respect to emissions to the atmosphere, and over many forms of renewable energy with respect to impacts on ecological and geophysical processes; and (c) the prospect of monetary costs comparable to those of other medium-term and long-term energy options. Diversity in the menu of energy options for the future is essential. There are not so many possibilities altogether. The more of these that can be made usable, the greater will be the chance that overall energy needs can be met without encountering excessive costs from or unmanageable burdens on any one source. The potential value of developing fusion energy should be understood in this context. The possible costs of needing fusion at midcentury and beyond, but not having it, are very high.

Progress in fusion energy R&D has been enormous over the past two decades by almost any standard, but there is still far to go. The physics and engineering of fusion are complicated,

and, although aspects of plasma science and fusion technology can be demonstrated at small to modest scale, the inherent properties of the most promising approaches to harnessing fusion that have been discovered so far are such that taking the next logical steps requires devices of great size and technological sophistication. The high costs of such steps, combined with the uncertainty of success and the long time scale before there is the possibility of a return on the investment, explain why governments, and not the private sector, must bear the main burden of funding fusion R&D at this stage of the technology's development. Indeed, the costs of the next steps are high enough to constitute a considerable incentive for governments to join forces in international collaborations toward these ends.

Based on the importance of developing energy sources adequate to meet the needs of the next century and the promise of fusion for this purpose, the benefits of fusion R&D in strengthening the national science and technology base, the impressive recent rates of progress in fusion research, the costs of the logical next steps, and the growing investments being made in fusion R&D in the European Union and Japan (which already total more than three times the corresponding investment here), we believe there is a strong case for the funding levels for fusion currently proposed by DOE — increasing from \$366 million in FY1996 to about \$860 million in FY2002 and averaging \$645 million between FY1995 and FY2005 (all in as-spent dollars). Spending less would drastically reduce the chance of meeting the National Energy Strategy goal of operating a fusion demonstration reactor by about 2025.

4.2 A Budget-Constrained Strategy for Fusion R&D

Although DOE's program plan and associated budgets are reasonable and desirable, they do not appear to be realistic in the current climate of budgetary constraints. We therefore have devoted most of our effort in this study to developing a budget-constrained U.S. fusion R&D strategy that, given level funding at about half of the average projected for the period FY1996 through FY2005 under the current DOE plan, would preserve what we believe to be the most indispensable elements of the U.S. fusion effort and associated international collaboration. This strategy would cost about \$320 million per year, \$46 million less than the U.S. fusion R&D budget in FY1995.

Embracing this strategy would entail hard choices and considerable pain, including straining the patience of this country's collaborators in the international component of the fusion effort, forcing difficult trade-offs between even a reduced U.S. contribution to international collaboration and maintaining adequate strength in the domestic components of U.S. fusion R&D, shrinking the opportunities for involvement of U.S. industry in fusion technology development, and surrendering any realistic possibility of operating a demonstration fusion reactor by 2025. But we believe it is the best that can be done within budgets likely to be sustainable in the current climate, and the least that can responsibly be done to maintain a modicum of momentum toward the goal of practical fusion energy. The rest of this chapter is devoted to describing and explaining this budget-constrained overall strategy and our specific recommendations for implementing it.

The strategy we recommend is aimed at ensuring maximum benefit from the investment of public funds, at promoting a logical progression of steps directed toward the ultimate goal of

commercialization of fusion energy, and at sustaining a significant and stable scientific infrastructure (people and facilities) capable of implementing our program and of capitalizing on future opportunities. Consistent with these aims, we believe such a strategy must:

- ensure the vitality of the plasma-science and fusion-technology communities (including universities, national laboratories, and industry);
- use and leverage the facilities and nuclear expertise that exist in all three of these sectors;
- expand the partnerships among these sectors;
- conduct the program in a way that ensures U.S. industrial capability and competitiveness;
- emphasize international cooperation and joint international projects.

Combining these objectives with consideration of the possibilities presented by the current state of development of fusion energy and by the existing elements in the fusion R&D programs of the United States and other countries has led us to conclude that the key priorities of a budget-constrained U.S. program should be as follows:

- a strong domestic core program in plasma science and fusion technology, with funds to explore both advanced tokamak research and research on concepts alternative to the tokamak, leveraged where possible on related activities worldwide;
- a collaboratively funded international fusion experiment focused on the key next-step scientific issue of ignition and moderately sustained (circa 100 seconds) burn, at a cost about one-third that of ITER as currently planned; and
- an international program to develop practical low-activation fusion-reactor materials, highly desirable for economical reactor performance and environmental attractiveness.

Pursuit of improved understanding of advanced tokamak physics, including steady-state operation, in TPX is also a very important goal. For reasons given below, however, we believe that, in a budget-constrained program it must be assigned a lower priority than the preceding elements.

4.3 Specific Recommendations, With Explanations

The recommendations and findings that follow represent the Panel's considered, unanimous judgment about how this country's fusion energy R&D program, in a time of great fiscal stringency, can most effectively make progress toward the goal of practical fusion energy, sustain the important field of scientific research that plasma science has become, and maximize the efficiency with which the public's funds are used. In what follows, our recommendations and findings are presented in **boldface** type, with wording identical to that in the Executive Summary, and explanations and elaborations of the recommendations and findings are interspersed in *italic* type.

This budget-constrained, internationally integrated U.S. fusion R&D program would, more specifically:

- **preserve and somewhat enhance the U.S. core program in relation to its FY1995 level of about \$180 million per year, including a degree of remedy of the current program's neglect of confinement concepts other than the tokamak;**
- **continue to operate, within the core program, the medium-scale tokamaks at General Atomics (DIII-D) and MIT (Alcator C-MOD), upgrading DIII-D after Princeton's Tokamak Fusion Test Reactor (TFTR) is shut down, and continue modest funding to pursue energy applications of the inertial-confinement fusion effort being funded for stockpile stewardship purposes in the weapons budget;**

The core program includes small- and medium-scale plasma-confinement experiments (varying in construction cost from under \$1 million to the range of \$200 million), research investigating improvements to the tokamak concept and concepts alternative to the tokamak, basic fusion technology (including materials development), and plasma theory. Chapters 2 and 3 have described the content of these elements of the core program, their critical importance to the viability of the U.S. fusion-energy R&D effort and to the prospects for attaining fusion's highest potential as a commercial energy source, and the constraints they have increasingly suffered as a result of budgetary stringency and rising expenditures on the big devices outside the core program. We believe that a vigorous core program, containing all of the elements just mentioned, must be preserved whatever the overall fusion-energy R&D budget. In the budget-constrained scenario with fusion-energy R&D spending level at \$320 million per year (as-spent dollars), we think the core program could and should be funded initially at its FY1995 level of about \$180 million per year, with modest increases possible over the 10-year period we have considered. A stable or slowly growing core program would, of course, entail the continual termination of experiments as they complete their useful lives and the continual start-up of new experiments, as needed to address current issues.

As noted in Chapters 2 and 3, alternative confinement concepts constitute an element of the core program that has been particularly badly constricted by the way DOE's program has evolved over the past decade. Alternative concepts have a number of features that might lead to marked improvements in the attractiveness of fusion as an energy source; at their current state of development, moreover, a number of concepts that deserve further investigation can be pursued at relatively modest cost. The Panel recommends that research on alternative concepts be expanded, even in the context of a level or only slowly rising core program. This effort should include (but not be limited to) support for exploring the energy applications of inertial-confinement fusion, especially the development of the heavy-ion drivers likely to be needed for energy applications but not being funded in the defense programs that support most of this country's inertial-confinement fusion research.

Notwithstanding the need to expand research on alternative concepts, materials, and basic theory and experiments, it is also essential that evolution of the tokamak continue. Ensuring that it does will require that tokamak research remain a substantial part of the core program, and that this work remain connected to — and contribute to — the worldwide effort

in tokamak research. The two medium-scale tokamaks in the U.S. core program, DIII-D and Alcator C-MOD, have important roles to play in this connection. As indicated in Chapter 2, they are at productive stages in their operating lives, and their designs suit them for exploring a number of the important issues in advanced tokamak physics. These devices cannot do everything in advanced tokamak physics that TPX could do — most importantly they cannot explore the physics of steady-state operation. But they can both be operated for considerably less money than it would take to construct TPX. Even under an overall fusion-energy R&D budget constrained at \$320 million per year, we believe that continuing to operate both DIII-D and Alcator C-MOD is highly desirable. The proposed DIII-D upgrade, which would increase this machine's capabilities in some of the forefront areas in tokamak physics research, is also desirable, although our constrained-budget case would require that the upgrade be delayed 3 years (to coincide with the shutdown date we recommend below for TFTR). Smaller tokamaks than DIII-D and Alcator C-MOD are also needed to help address a variety of critical fusion issues. Examples of critical issues that can be investigated fruitfully at small scale include disruptions, current drive, and transport.

- **continue to operate TFTR for 3 years beyond its currently scheduled shutdown at the end of FY1995, at a somewhat reduced funding level of about \$50 million per year;**

This recommendation is aimed at reaping additional scientific benefits from continued D-T operation in TFTR and supports the key priority of ignition and sustained burn as the major next-step scientific issue. TFTR is unique in the world program in its capability for extended D-T operation. The past year of TFTR experimentation has advanced the frontier of fusion research with regard to alpha-particle physics, the further understanding of which is a key motivation behind the international ignition and sustained-burn experiment. Results obtained in TFTR over the next 3 years will yield new information on alpha-particle effects and will influence the physics plan for the ignition experiment.

The results from TFTR already have partly confirmed several theoretical predictions, but an unexpected positive result has also materialized: an improvement in plasma confinement apparently resulting from the presence of alpha particles. The TFTR team has developed an excellent plan that will creatively and significantly extend the device's capabilities. Through enhancement of the available plasma-control techniques, TFTR's fusion power output can be increased from 10 megawatts to 20 megawatts; this increase will double the population of alpha particles, enhance the alpha-particle heating of the plasma, and approach some predicted limits for alpha-particle excitation of instabilities. In addition, novel techniques to control alpha-particle heating of the plasma and alpha-particle transport will be tested for the first time.

These experiments will not require any major upgrades of the TFTR systems. Accumulated operation by the end of the 3-year extension will not exceed that for which the device was designed, and the experiment is already fitted with outstanding and specialized diagnostic instrumentation to measure alpha-particle dynamics. Given the uniqueness of the TFTR capability, the importance of alpha-particle physics, and the promising plans for the next 3 years, we believe that continuation is highly desirable. In the constrained \$50 million annual budget for the operation of this machine at Princeton (the FY1995 budget is about \$62 million),

TFTR can probably operate effectively, but the full 3-year extension will be required to exploit its capabilities adequately.

- **continue U.S. participation in the Engineering Design Activities phase of ITER at the current level (\$70 million per year), to which this country is committed through FY1998 under existing international agreements.**

Under this proposed, budget-constrained program, the United States would also immediately open negotiations with its ITER partners to modify the post-FY1998 phase of international cooperation, seeking to

- **gain agreement for downsizing ITER construction and operation from a \$10-13 billion ignition-and-long-burn physics and reactor-technology development project to a not more than \$4 billion ignition-and-moderate-burn physics project, on a construction timetable delayed 3 years from the current plan;**
- **promote the possibility of significant international participation in the complementary next-generation fusion experiments hitherto planned as domestic projects (such as TPX in the United States); and**
- **add to the collaborative agenda a materials/blanket test facility, as part of the international, low-activation-materials and blanket-development program. The United States should be prepared to commit up to a total of \$200 million as its share of a project that achieves international consensus and begins construction in FY2000.**

The expectation of a successful outcome from this negotiation would depend on the United States bringing to the table a firm commitment, endorsed by the President, of a \$1.2 billion contribution to the next phase of the cooperation (cumulative over about 10 years). The negotiation would include the possibility of expanding the number of partners (to include, e.g., China, India, South Korea). It is possible that the outcome of the negotiation would be that the full-scale ITER was constructed despite the reduction of the U.S. contribution from what had been anticipated. This outcome would have the benefit of gaining, for the world, the additional science results and the technology-testing benefits associated with ITER as currently envisioned; but it would have the liability of sharply reducing the chance that money will be found within the international effort to fund the international materials test facility and to help pay for TPX (or another machine with a similar mission).

The \$1.2 billion figure represents, nominally, \$1 billion for a one-quarter U.S. share of a \$4-billion ignition-and-moderate-burn experiment plus \$200 million for the U.S. share of an international materials/blanket test facility. In the event that a suitable ignition experiment could be agreed and designed for less than \$4 billion, or in the event that the international pool of funds for the collaboration was larger than $4 \times \$1.2 \text{ billion} = \4.8 billion despite the limitation of the U.S. contribution to \$1.2 billion (as could happen, for example, if additional partners were recruited), then it might become possible to fund TPX or a similar device from this international pool along with the ignition experiment and materials/blanket test facility.

Achievement of ignition and sustained burn of a plasma confined by a magnetic field has been for many years the preeminent goal of the world fusion community. Scientifically, it is the largest single physics unknown for a tokamak power system. In an ignited plasma, external heating can be turned off and the plasma will undergo fusion burn continuously. Sustaining the burn is also critically important in order to understand issues that might influence either the length of the burn or the optimal means of controlling the power- production level during sustained burn. Once ignited, the plasma will follow as yet unexplored paths into new high-temperature plasma regimes. The alpha particles will influence the plasma behavior in ways that are difficult to predict, especially since important aspects of plasma behavior depend upon plasma waves and turbulence, the description of which is at the forefront of theoretical physics research.

The Panel views ignition and sustained burn as the most important endeavor to pursue in a single major new experiment and believes that it should be undertaken internationally. In the ITER project as currently envisioned, however, combining an ignition experiment with a burn as long as 1,000 seconds — and with a technology-testing phase intended to demonstrate most of the technological features of a reactor — has led to a project with a price-tag that may be higher than even an international consortium is willing to pay. Under any assumptions about future U.S. fusion R&D budgets consistent with the current budget-cutting climate, the United States will not be able to afford participation in the construction phase of the currently envisioned ITER as a full partner. It is also possible that the project will ultimately prove too costly for some of the other partners, even if the United States professes its willingness to continue with it at the initially intended scale. We think the prospects for actually carrying out an international ignition experiment in a timely way would be improved by downsizing the ITER project now, retaining for it the crucial mission of ignition and moderately sustained burn and separating out, to be accomplished in devices to be built elsewhere (and, in some cases, later), the long-burn and technology-testing missions. This is therefore the aim with which we think the United States should enter negotiations with its partners about the future of ITER.

The figure of \$4 billion for a downsized ITER ignition-and-moderate-burn experiment is not based on a specific design; undertaking such a design in even a preliminary way would have been far beyond the scope of this Panel. The cost figure emerges, rather, from very rough considerations of the savings likely to be associated with eliminating the technology-testing components of ITER and with scaling down the physics experiment in both power and duration of pulse. Aiming for a pulse length of 50 to 100 seconds rather than a 1000-second burn would probably permit operating with copper coils (probably nitrogen cooled) instead of with superconducting coils. This would reduce the need for shielding of the coils and would permit a smaller device. That a machine considerably smaller than ITER would still be able to achieve ignition is suggested by the PPPL designs for the Compact Ignition Tokamak (CIT) and Burning Plasma Experiment (BPX). The BPX had an estimated total project cost (in 1992) of \$1.7 billion (in as-spent dollars). Although the objective of BPX was operation at Q of 5 or more and a pulse length of about 5 seconds, the margin between the BPX cost figure and the \$4 billion mentioned above allows some room for a design to be developed that would inspire high confidence in its capacity to achieve the dual goals of ignition and moderately sustained burn. The \$4 billion figure has the further attraction of permitting the United States to play something like a 25%-partnership role in the project within an overall contribution to international collaborative efforts of about \$1 billion. We believe this figure is the largest amount compatible

with the constrained U.S. fusion R&D budgets now in prospect. The actual cost of a downsized ITER and the details of a design for it, of course, would have to emerge from negotiation among the partners and an associated design process.

As the last two of the four bulleted recommendations just above indicate, what we are proposing here really amounts to a restructuring of the international fusion collaboration as a whole, with the aim of optimizing the capacity of collaborative and domestic programs together to cover the combination of ignition-and-moderate-burn demonstration, advanced tokamak physics demonstrations, alternative-concepts research, and materials development that will be needed to progress toward an attractive reactor. We believe that if the United States brings to the negotiating table a firm commitment of a contribution of \$1.2 billion to the next decade of fusion collaboration — and here the "firmness", underpinned by a Presidential endorsement, is likely to be essential — there is a good chance of an outcome in which the downsized ITER goes forward and it is also possible to build internationally a materials/blanket test facility and to fund, on an international basis, the construction of TPX or another steady-state advanced-physics tokamak. Such an outcome would become still more likely if the number of partners could be increased, which might be made possible by the growing interest in fusion in other major countries not now participating in ITER.

As noted above, it is also possible that the other ITER participants will decide — despite the efforts of the United States in the new negotiation and despite the reduction of the U.S. contribution from what had been expected — to proceed with construction of the full ITER as currently envisioned. As also noted, if this effort succeeded it would have the obvious merit of gaining the scientific and technological benefits toward which the current ITER has been designed; but proceeding in this direction would have the liability of absorbing so high a proportion of the total funds available in the world for fusion R&D that the chances of building either an international materials/blanket test facility or a steady-state, advanced-physics tokamak would be considerably reduced. That is why we think the U.S. preference should be for a downsized ITER. We did not think it would be fruitful or even feasible, however, for our Panel to try to anticipate all the twists and turns such a negotiation might take, and to make recommendations about what the U.S. position should be in each eventuality. Clearly, some flexibility as well as ingenuity will be required. We hope, though, that the sense of priorities we have tried to convey here will be useful to the U.S. side as the negotiation evolves.

We do consider it essential, in the \$320 million per year scenario, that the United States continue to honor its formal commitment to participate fully in the ITER engineering- design phase that ends in FY1998; we also believe that \$70 million per year meets that commitment. If the United States opens the discussion of downsizing the next phase of ITER by announcing that this country is withdrawing from its official commitment to complete the current phase, it loses credibility as a negotiating partner and potentially reliable participant in whatever next phase might be agreed. Although a \$70 million per year "entry fee" to these discussions may seem high, the money being spent in ITER engineering design is producing insights about fusion technology that will be valuable whether or not ITER goes forward in the form currently planned. We recommend that, as part of the negotiation about the mission and scale of ITER, the United States should take the position that some of engineering design funds be spent exploring how to redesign ITER for a downsized role.

The rationale for adding to the collaborative agenda a materials/blanket test facility, as part of the international low-activation-materials and blanket-development program, is that (a) achieving fusion's highest potential requires a strong materials and blanket component development program and (b) progress will be made more rapidly if this program is international in scope, is well-coordinated, and has as a primary feature the construction of a large-scale facility as an international partnership in the spirit of the ITER agreement. The world fusion program has long studied the design of a small-volume, high-flux, accelerator-based fusion neutron source facility (the materials test facility or MTF) for small-sample irradiation testing. It has also described a larger volume neutron source (VNS) facility for the development and testing of blankets and other major fusion-plant core components. The Panel has not had the time or the charge to investigate the design viability of such specific facilities. We are convinced, however, that the development of low-activation materials and of critical fusion-power core components such as blankets is crucial to fusion's long term attractiveness. Thus, the Panel's recommendation is that the U.S. participate as a key international partner in a materials/blanket test facility and that it be prepared to commit up to a total of \$200 million as its share of such a project that achieves international consensus and begins construction in FY2000.

During the negotiation of the next phase of the ITER cooperation, construction of TPX (currently scheduled to begin in FY1996) would be delayed for 3 years. Thereafter, TPX construction would proceed if

- (a) the outcome of the negotiation was such as to permit funding the (probably downsized) ITER ignition experiment, the materials test facility, and the TPX with a cumulative contribution of \$1.2 billion from the United States toward the total construction costs of these three facilities, with the remainder to come from our international partners,**

or

- (b) the outcome of the negotiation was such that the United States did not become a participant in an international ignition experiment, but an ignition experiment went forward somewhere under other auspices.**

If neither of these outcomes occurred, construction of TPX would not proceed unless and until a review of the new situation — with its lack of a commitment to an ignition experiment anywhere — concluded that proceeding with TPX was the most sensible next step for the United States in that situation.

Under some of the possible outcomes from the negotiation of the next phase of international collaboration, TPX would not be built. This would be extremely unfortunate. We consider TPX to be a well conceived and innovative advanced tokamak experiment, without which the United States will lack a large tokamak of its own after TFTR is shut down. We believe, nonetheless, that the highest priority should be given to preserving both (a) U.S. participation in a robust international collaboration that includes, above all, an ignition experiment and a materials test facility, and (b) a strong domestic core program of theory and smaller experiments. If negotiation of the next phase of international

collaboration under a total U.S. contribution of \$1.2 billion does not produce an outcome that achieves these ends and TPX as well, then the loss of TPX will have to be considered a particularly dismaying consequence of constraining the overall U.S. fusion R&D program to \$320 million per year.

The steady-state advanced tokamak concept is embodied in the TPX experiment. The investigation of steady-state plasmas is a crucial step in both the physics and technology development for fusion. The development of the advanced tokamak concept is also likely to be essential if the tokamak is to evolve into an attractive reactor. The integration of steady state and advanced tokamak goals into one experiment is sensible, because a complete test of advanced tokamak ideas requires their demonstration in steady state. We believe that TPX represents an exciting and important fusion physics experiment, well suited to be the lead domestic experiment in the United States.

The cost of TPX would be roughly equal to that of a U.S. share in an international experiment focused on ignition physics. An aggressive fusion program, which we would endorse, would support both experiments. They are complementary to each other, and both are essential in pursuit of attractive fusion reactors for the middle of the next century. If, however, the budgets for U.S. fusion R&D become constrained to a degree that allows only for the core activities plus one large experiment, it will be necessary to decide which of the two experiments — the international ignition experiment or the domestic steady-state experiment — deserves higher priority. Although, as discussed above, it may be possible to finesse having to face this choice under some combinations of domestic budget stringency and reshaped international collaboration, if forced to face it we would assign the international ignition experiment higher priority than the domestic, steady-state, advanced tokamak.

We reach this conclusion regarding relative priorities while fully aware of three counterarguments:

- *It would be useful to explore ways to make tokamaks more compact and economical before pursuing ignition. In this way, the cost of the ignition device might be reduced, and it would be more likely to resemble an attractive reactor.*
- *Without TPX, the United States would have no cutting-edge tokamak of its own after TFTR shuts down. This would be a significant liability for the domestic program.*
- *TPX is ready to enter the construction phase now. An international ignition device, by contrast, is unlikely to go on line until at least seven years after the time TPX could begin operation.*

All of these are reasonable arguments, but we find the arguments in favor of pursuing ignition and in favor of U.S. participation in international collaborative approaches to be more compelling. It is also germane that, although none of the other existing or proposed experiments (and no combination of these) can fully perform the TPX mission, the other experiments can perform parts of it. The ignition mission, by contrast, cannot be addressed even approximately by any existing machine.

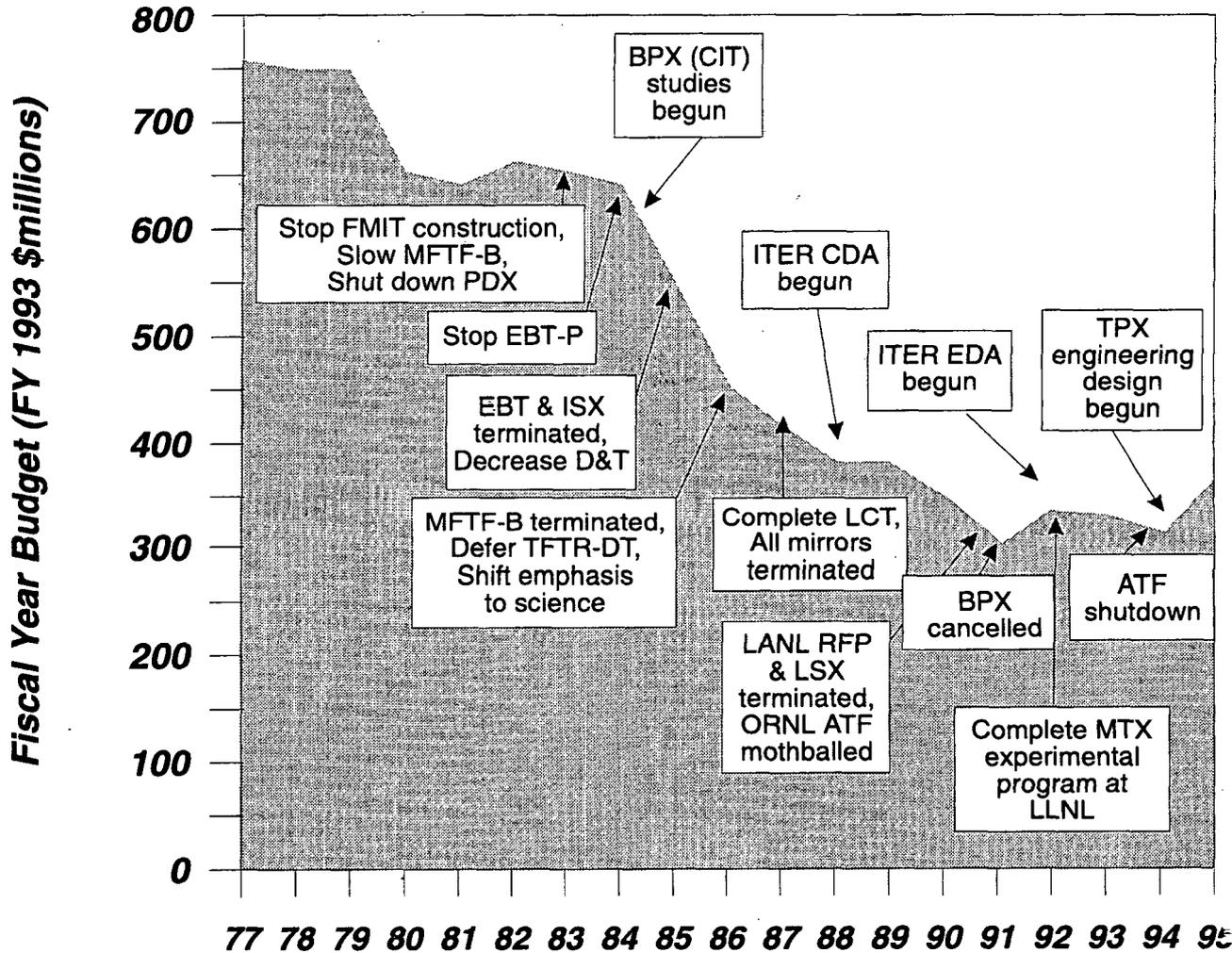
Although TPX would only be constructed, in our \$320 million per year scenario, under the subset of possible outcomes of international negotiations indicated in the boldfaced text just above, we recommend that a reduced TPX R&D effort by the core national TPX team continue during the international negotiation phase. The purposes of the continued effort are to move forward in the TPX design, to continue the intellectual leadership provided by the TPX project in advanced tokamak research, and to maintain a state of readiness for construction in the event that the outcome of the negotiation makes it feasible to proceed. Industrial involvement in the effort should continue, at a level appropriate to the reduced design effort.

In addition to developing the strategy just described for a fusion R&D program funded at about \$320 million per year, we also have attempted to envision a program that could preserve key priorities at a still lower budget level of about \$200 million per year. We find that this cannot be done. Reducing the U.S. fusion R&D program to such a level would leave room for nothing beyond the core program of theory and medium-scale experiments described above — no contribution to an international ignition experiment or materials test facility, no TPX, little exploitation of the remaining scientific potential of TFTR, and little sense of progress toward a fusion energy goal. With complete U.S. withdrawal, international fusion collaboration might well collapse — to the great detriment of the prospects for commercializing fusion energy as well as the prospects for future U.S. participation in major scientific and technological collaborations of other kinds. These severe consequences — deeply damaging to an important and fruitful field of scientific and technological development, to the prospects for achieving practical fusion energy, and to international collaboration in science and technology more generally — are too high a price to pay for the budgetary savings involved.

We urge, therefore, that the Administration and the Congress commit themselves firmly to a U.S. fusion R&D program that is stable at not less than \$320 million per year.

* * * * *

U.S. MAGNETIC FUSION BUDGET AND PROJECT HISTORY FISCAL YEARS 1977-95 (FY 1993 dollars in millions)

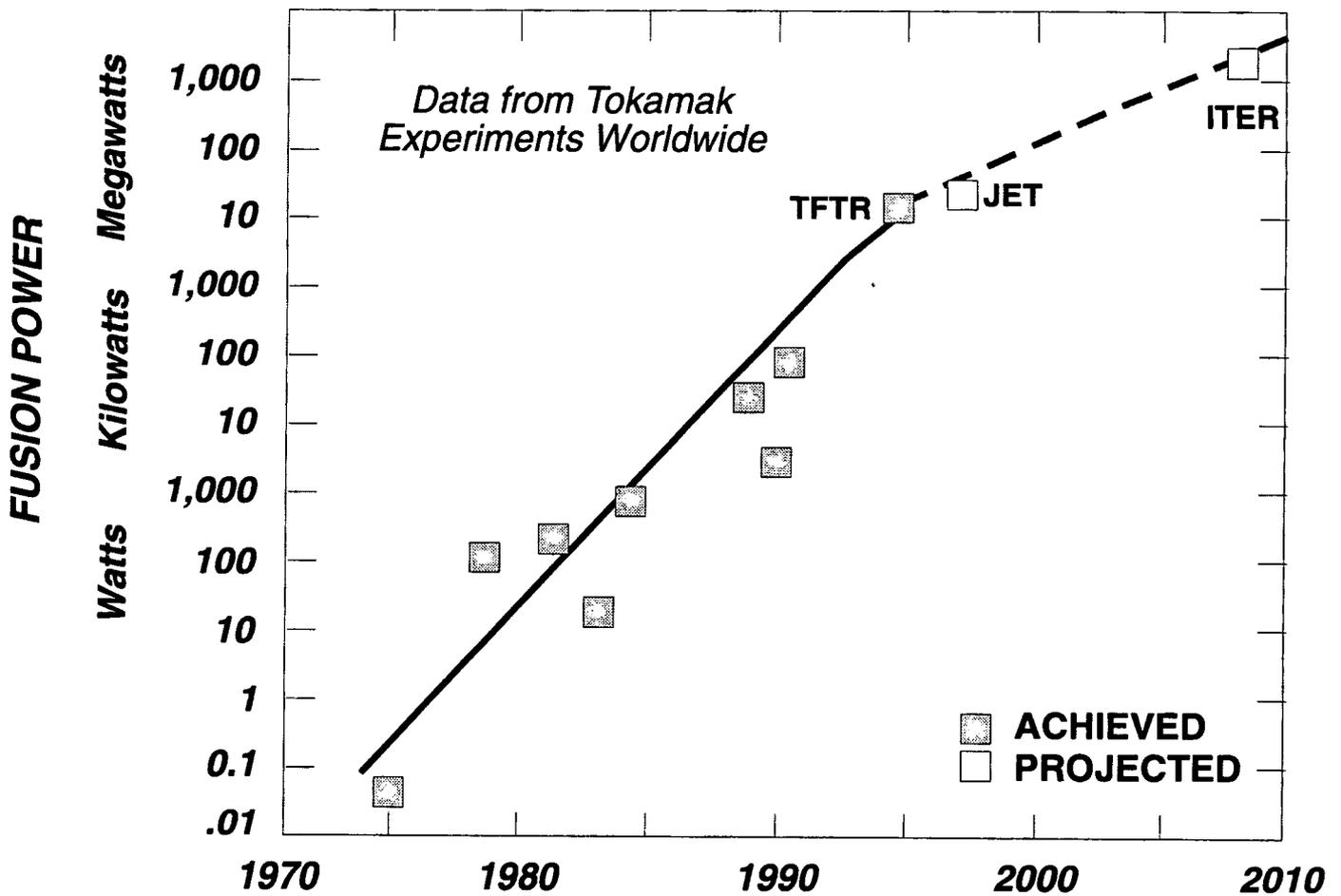


- KEY:**
 BPX/CIT = Burning Plasma Experiment/Compact Ignition Tokamak
 DT = Deuterium-Tritium
 EBT = Elmo Bumpy Torus
 EBT-P = Elmo Bumpy Torus-P
 FMIT = Fusion Materials Irradiation Test Facility
 ISX = Impurity Studies Experiment (a tokamak)
 ITER CDA = International Thermonuclear Experimental Reactor Conceptual Design Activities
 ITER EDA = International Thermonuclear Experimental Reactor Engineering Design Activities
 LANL RFP = Los Alamos National Laboratory Reverse Field Pinch
 LCT = Large Coil Test Facility (superconducting magnets)
 LLNL = Lawrence Livermore National Laboratory
 LSX = Large S Experiment (a field-reversed compact toroid device)
 MFTF-B = Mirror Fusion Test Facility-B
 MTX = Microwave Tokamak Experiment
 ORNL ATF = Oak Ridge National Laboratory Advanced Toroidal Facility (a stellarator)
 PDX = Princeton Divertor Experiment
 TFTR = Tokamak Fusion Test Reactor
 TPX = Tokamak Physics Experiment

SOURCE: U.S. Department of Energy, 1994.
 Modified from Office of Technology Assessment Report of 2/95.

Figure 1

PROGRESS IN MAGNETIC FUSION POWER



TFTR Tokamak Fusion Test Reactor
 JET Joint European Torus
 ITER International Thermonuclear Experimental Reactor

Updated from Fusion Policy Advisory Committee Report of 9/90.

Figure 2

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PCAST REVIEW OF THE DOE MAGNETIC FUSION PROGRAM: CHARGE TO THE PANEL

The Department of Energy's magnetic fusion energy program has the stated goals of (i) developing a demonstration fusion power plant by approximately 2025 and (ii) facilitating the operation of a commercial fusion power plant by 2040. The research and development program that has been formulated by the DOE to meet these goals, funded in FY1995 at \$368 million per year, would require very substantial increases in budget over the next decade; this will be problematic if Federal expenditures for energy research and development of all kinds remain constant or decline in this period, as is widely expected. The conference report for the Fiscal Year 1995 Energy and Water Appropriations Act points out this potential difficulty and calls for a review of the DOE's magnetic fusion energy program by the President's Committee of Advisors on Science and Technology (PCAST).

In response to this request, the Office of Science and Technology Policy has appointed a fusion review Panel, under the auspices of PCAST, consisting of four members of PCAST and five other members chosen for their diverse perspectives and expertise. The task of the Panel is to clarify the technical and policy tradeoffs and budgetary requirements associated with -- and recommend preferred alternatives among -- various possible trajectories for the magnetic fusion energy program, including, (a) the trajectory currently programmed, (b) an alternative in which expenditures would increase in a similar manner but would be allocated differently, (c) an alternative in which expenditures would remain approximately constant, (d) an alternative in which expenditures would decrease moderately, and (e) an alternative in which expenditures would decrease sharply.

Because of the large role, in the budgets for the currently programmed trajectory, of two major construction projects -- the Tokamak Physics Experiment (TPX), scheduled to be completed at Princeton in 2001, and the International Thermonuclear Experimental Reactor (ITER), scheduled to be completed in 2008 at a site not yet determined -- consideration of alternatives must include the possibilities of downsizing, deferral, or elimination of one or both of these experiments. Also important in the evaluation of alternative scenarios will be the scope they provide for vigorous efforts in theoretical and experimental plasma physics independent of the large machines, for a suitable program to develop the special materials that fusion reactors will require, for efforts to identify and investigate reactor concepts other than the tokamak approach being pursued in TPX and ITER, and for continuation of the high degree of international collaboration that has been a hallmark of the global fusion effort for nearly 40 years.

The PCAST Panel is to report to the Office of Science and Technology Policy at the end of June 1995, in time to influence Congressional action on the Fiscal Year 1996 budget and to be used in the Administration's Fiscal Year 1997 budget decisions. This schedule does not permit including, in the Panel's mandate, any substantial review of the related program in inertial confinement fusion, which has been funded mainly under the DOE's defense programs for its applications to the study of nuclear-weapons physics. The Panel's review of programs exploring alternatives to the tokamak will include, however, a review of the modest effort funded on the energy side of DOE's budget to explore applications of inertial fusion technology as an energy source.

The Panel will draw upon, but will not be limited by, the findings of reviews of the DOE's magnetic fusion energy program conducted over the past few years by the Department's Fusion Power Advisory Committee (FPAC) and Fusion Energy Advisory Committee (FEAC), by the National Research Council, and by the Congressional Office of Technology Assessment, and will take cognizance of the parallel review of the full range of the DOE's strategic energy research and development programs being conducted by the "Yergin Committee" of the Secretary of Energy Advisory Board.

Appendix B

List of Principal Briefings

March 29, 1995: Washington, D.C.

John H. Gibbons, Assistant to the President for Science and Technology
Martha A. Krebs, Director, Office of Energy Research, DOE
Anne Davies, Associate Director for Fusion Energy, DOE
Robert Aymar, Director, ITER Project
Robert J. Goldston, Chief Scientist, Tokamak Physics Experiment, Princeton, N.J.

April 13-14: San Diego, California

Masaji Yoshikawa, Executive Director, Japan Atomic Energy Research Institute
Charles Maissoner, Director, Fusion Program, European Commission
Evgenii Velikhov, President, Kurchatov Institute, Russian Federation
Robert Aymar, Director, ITER Project
Charles C. Baker, ITER U.S. Home Team Leader, San Diego, California
Ben Carreas, Oak Ridge National Laboratory, Tennessee
Thomas C. Simonen, Director, D-III Program, General Atomics, San Diego, California
David E. Baldwin, Associate Director, Lawrence Livermore National Laboratory

April 24: Washington, D.C.

T.J. Glauthier, Associate Director, Office of Management and Budget
Stephen O. Dean, President, Fusion Power Associates

April 25: Princeton, N.J.

Ronald C. Davidson, Director, Princeton Plasma Physics Laboratory
Dale M. Meade, Deputy Director, Princeton Plasma Physics Laboratory
Gerald A. Navratil, Professor of Applied Physics, Columbia University

May 17-18, 1995: Washington, D.C.

John P. Boright, Deputy Associate Director, OSTP
Michael Campbell, Associate Director, Lawrence Livermore National Laboratory
Bruno Coppi, Massachusetts Institute of Technology (MIT)
Miklos Porkolab, Director, Plasma Fusion Center, MIT
Stephen L. Rosen, Director, Industry Relations, Houston Lighting & Power Co., Texas
Marshall Rosenbluth, Chief Scientist, ITER Project

June 12-14, 1995: San Francisco, California - Executive Session

Communications Received by the Panel

The PCAST Fusion Panel wishes to acknowledge with appreciation the letters and memorandums received from the following scientists and engineers who offered views and relevant information on the future of the fusion energy research and development program:

Aamodt, Richard E., Lodestar Research Corporation, Boulder, Colorado

Baldwin, David E., Senior Vice President for Fusion, General Atomics, San Diego, California

Berk, Herbert L., Professor of Physics, University of Texas, Austin, Texas

Bradbury, James, Los Alamos National Laboratory, New Mexico

Campbell, Michael, Associate Director, Laser Programs, Lawrence Livermore National Laboratory, California

Cary, John R., Professor and Chair, Astrophysical, Planetary, and Atmospheric Sciences, University of Colorado, Boulder, Colorado

Davidson, Ronald C., Director, Princeton Plasma Physics Laboratory, New Jersey

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GLOSSARY OF FUSION TERMS AND ACRONYMS

Alcator-C	MIT, dense plasmas, high magnetic field; C-MOD= upgrade; exploring divertor solutions.
ASDEX	Axisymmetric Divertor Experiment, Garching, Germany; enhanced tokamak confinement and current drive.
ATF	Advanced Toroidal Facility, Oak Ridge National Laboratory (ORNL) stellarator, shut down 1994.
Beta	The ratio of the outward pressure exerted by the plasma to the inward pressure that the magnetic confinement field is capable of exerting.
Blanket	The structure surrounding the plasma in a fusion reactor, within which the fusion-produced neutrons are slowed down, heat is transferred to a primary coolant, and tritium is bred from lithium.
Break-even	The point at which the fusion power generated in a plasma equals the amount of heating power that must be added to the plasma to sustain its temperature (corresponds to $Q=1$).
Burning plasma	A plasma in which the fusion reactions supply a significant fraction of the energy needed to sustain the plasma.
BPX	Burning Plasma Experiment (also called CIT), cancelled 1992.
CDX	Current Drive Experiment, Princeton.
CIT	Compact Ignition Tokamak, same as BPX.
Confinement	Restraint of plasma within a designated volume. In magnetic confinement, this restraint is accomplished with magnetic fields.
CPRF	Confinement Physics Research Facility, reversed field pinch, Los Alamos National Laboratory (LANL), same as ZT-H, terminated 1991, during construction.
D-D reaction	A fusion reaction in which one nucleus of deuterium fuses with another.
DEMO	Demonstration Fusion Power Reactor, target 2025 in National Energy Strategy.
Deuterium	A naturally occurring heavy isotope of hydrogen containing one proton and one neutron in its nucleus.

Divertor	A component of a toroidal fusion device used to shape the magnetic field near the plasma edge so that particles near the edge are captured before they can strike the walls and generate secondary particles that would contaminate and cool the plasma.
DIII-D	Doublet III-D, General Atomics, Beta=10%.
D-T reaction	A fusion reaction in which a nucleus of deuterium fuses with a nucleus of tritium. The D-T reaction is the most reactive fusion reaction.
EBT	Elmo Bumpy Torus, cancelled 1985.
ETF	Engineering Test Facility (inertial fusion), pre-DEMO.
ETR	Engineering Test Reactor (US generic term for ITER-scale device).
Field-reversed configuration	A magnetic confinement concept with no toroidal field, in which the plasma is essentially cylindrical in shape.
Fission	The process by which a neutron strikes a nucleus and splits it into fragments; during the process of nuclear fission, neutrons are released at high speed, and heat and radiation are released.
FMIT	Fusion Materials Irradiation Test Facility, halted 1983.
FT-U	Frascati Tokamak Upgrade, Italy, high density, high current \$150 million class.
Fusion	The process by which the nuclei of light elements combine, or fuse, to form heavier nuclei, releasing energy.
HLT	High-performance Long-pulse Tokamak, European pre-conceptual design, copper coils.
Ignition	The point at which a fusion reaction becomes self-sustaining. At ignition, fusion self-heating is sufficient to compensate for all energy losses; external sources of heating power are no longer necessary to sustain the reaction.
ILSE	Induction Linac Systems Experiment, heavy-ion driver for inertial fusion, \$50 million.
Inertial confinement	An approach to fusion in which intense beams of light or particles are used to compress and heat tiny particles of fusion fuel so rapidly that fusion reactions occur before the pellet has a chance to expand.
ISX	Impurity Studies Experiment, tokamak, ORNL, terminated 1985.
ITER	International Thermonuclear Experimental Reactor, 2005.
JET	Joint European Torus, Culham, UK, biggest tokamak, Q= 0.7.

JT-60	Japan, tokamak; U= upgrade (approaches JET); SU= superupgrade.
Lawson parameter	The product of plasma density and confinement time that, along with temperature, determines the ratio between power produced by the plasma and power input to the plasma.
LHD	Large Helical Device, Japan, stellarator, \$1 billion.
LMF	Laboratory Microfusion Facility, 400-MJ class inertial fusion energy facility, possibly combined with ETF.
Low-activation materials	Materials that, under neutron irradiation, do not generate intensely radioactive, long-lived radioactive isotopes and produce less afterheat following a reactor shutdown than more conventional materials.
LSX	Large S Experiment (field-reversed compact toroid).
Magnetic confinement	Any means of containing and isolating a hot plasma from its surroundings by using magnetic fields.
Magnetic fusion energy	Energy released by a thermonuclear reaction in the fuel of a magnetically confined plasma.
Magnetic mirror	A generally axial magnetic field that has regions of increased intensity at each end where the magnetic fields converge.
MFTF-B	Mirror Fusion Test Facility, B, Lawrence Livermore National Laboratory (LLNL), shut down 1986.
MST	Madison Symmetric Torus, reversed-field pinch, U of Wisconsin.
MTX	Microwave Tokamak Experiment, LLNL.
NIF	National Ignition Facility, LLNL, laser fusion (planned), yield 1-20 MJ.
$nT\tau$	The product of the key figures of merit relating to plasma quality: density (n) times temperature (T) times energy confinement time (τ).
PBX-M	Princeton Beta Experiment (M = modification), approached second stability.
PDX	Princeton Divertor Experiment, shut down 1983.
Plasma	An electrically neutral gas of charged particles.
PLT	Princeton Large Torus, predecessor of TFTR.
Q	The ratio of fusion power (P_f) generated in the plasma to the heating power that must be added to the plasma to sustain its temperature.

Reversed field pinch	A closed magnetic confinement concept having toroidal and poloidal magnetic fields that are approximately equal in strength, and in which the direction of the toroidal field at the outside of the plasma is opposite from the direction at the plasma center.
RFX	Reversed-Field (pinch) Experiment, Italy, operation 1991.
SBX	Steady Burn Experiment, potential upgrade from SS/AT.
Spheromak	A compact toroidal magnetic confinement concept in which a large fraction of the confining magnetic fields is generated by currents within the plasma.
SS/AT	Steady State Advanced Tokamak, Princeton, same as TPX.
Stellarator	A toroidal magnetic confinement device in which the confining magnetic fields are generated entirely by external magnets.
T-10	Russia, tokamak, 2 MW electron-cyclotron heating power.
T-15	Russia, Kurchatov, large superconducting tokamak.
TEXT	Texas Experimental Tokamak, U of Texas, Austin.
TEXTOR	Torus Experiment for Technology-Oriented Research, Julich, Germany.
TFTR	Tokamak Fusion Test Reactor, Princeton.
Tokamak	A magnetic confinement concept whose principal confining magnetic field, generated by external magnets, is in the toroidal direction but that also contains a poloidal magnetic field that is generated by electric currents running within the plasma.
Tore Supra	Superconducting toroidal field coil tokamak, Cadarache, France, 1988.
Toroidal	In the shape of a torus (i.e., doughnut-shaped).
TPX	Tokamak Physics Experiment, Princeton, same as SS/AT, low-budget replacement for BPX.
Tritium	A radioisotope of hydrogen that has one proton and two neutrons in its nucleus. In combination with deuterium, tritium is the most reactive fusion fuel.
TSTA	Tritium Systems Test Assembly, LANL.
VNS	Volume Neutron Source, pre-conceptual alternative to blanket-testing.
Wendelstein-7AS/X	Wendelstein stellarators, Garching, Germany.