ERDA: Go For An Operating Demonstration Fusion Plant In The Late 1980s

Following are excerpts from the statement of Dr. Robert L. Hirsch, ERDA Assistant Administrator for Solar, Geothermal and Advanced Energy, to the House Science and Technology subcommittee on Fossil and Nuclear Energy Research, Development and Demonstration, Feb. 25.

Recent Accomplishments

The year 1976 produced a number of fundamental advances in tokamak research. Improved plasma confinement and heating was obtained in the Princeton Large Torus (PLT) and the Oak Ridge Tokamak (ORMAK). In PLT operation at higher electrical currents produced better results that agreed with theoretical predictions. Both the density-confinement time product and impurity effects were found to be improved in the larger diameter plasmas produced in these machines. This is important because theory tells us that larger sizes are the key to practical systems...

In ORMAK the Oak Ridge research team produced a major advance by raising ion temperatures to 2 kilovolts (20,000,000 degrees C) by a technique known as neutral beam heating. While this temperature is below what is needed, this result unambiguously demonstrates that the required temperatures will be achieved when we finally invest in the necessary heater power.

But the most exciting event in tokamak confinement research happened only two weeks ago. The fusion research group at MIT reported that the Alcator machine produced a world record confinement value of 2×10^{13} cm^{-3} sec. This result, in higher magnetic fields (85 kilogauss) than any other tokamak, is double the previous achievement, and clearly carries us above the breakeven threshold. An important aspect of this result is that theory and experimental results remain in agreement as our machines are pushed to higher levels of performance.

The major alternate to the tokamak is the mirror, a straight system in which magnetic lines are squeezed at the ends so as to "mirror" reflect plasma particles that want to leak out the ends. As a consequence of important results in the 2X-IIB mirror at the Lawrence Livermore Laboratory, the mirror program has recently undergone a revolution in concept and direction. Temperatures of 230,000,000 degrees C were created while some bothersome small-scale instabilities in the plasma were simultaneously suppressed. Many of the major physics questions in the mirror program were resolved this past year so that we can begin to see a major new path to an efficient mirror reactor. With our present program we expect to have a sound technical basis for comparing tokamaks and mirrors as power reactors by 1981 or 1982.

These results and many others that did not make headlines have given fusion physicists and engineers worldwide increased confidence. Progress has been continuous, and we have found no law of nature that can prevent the achievement of practical fusion power....

'Fusion Could Be Considered The Enduring Solution To Energy Problems'

Following are excerpts from the statement on the Carter administration's proposed Fiscal Year 1978 budget authorization for the ERDA magnetic fusion program by Edwin E. Kintner, Director of ERDA's Division of Magnetic Fusion Energy.

I am especially pleased to make this initial presentation on the Magnetic Fusion Energy Program to this Committee as it commences its new responsibilities for the program. We believe fusion is an important and exciting challenge with great potential benefit for this nation, and I hope I can convey some of that belief to you....

As Dr. Hirsch has pointed out in discussing the Program Plan, the program could have a range of schedule objectives, since the rate of progress of development programs of this kind can be influenced, within limits, by the application of increased or reduced resources, or by acceptance of greater risks in making program decisions....

IX. Accomplishments in 1976

1976 was another year of important new advances in fusion. The effects of plasma size and current were measured at higher values in the PLT. The data obtained confirmed theoretical predictions of scaling as the square of the linear dimension of the plasma. These results were duplicated in the T-10 device in Moscow. Ion temperatures were raised to 2 keV (a factor of approximately three from minimum temperatures needed for ignition). For the first time, electron temperatures were raised with the ion temperatures, as predicted but not observed previously. The production of density times confinement time, was increased by 100 percent to a new world record high in Alcator at MIT to within a factor of three of the Lawson Criterion for "breakeven." Predicted advantages of elliptical and boublet plasmas were confirmed by direct experiment in Doublet IIA. Perhaps most important for the implications of tokamaks as practical power reactors, a general concensus developed during 1976, that on a theoretical basis, betas — the ratio of plasma to magnetic pressure — of up to 10 percent are achievable. Experiments to confirm these theoretical predictions are planned for the next two years. This prediction has great implication for reduced size, and therefore cost, of Tokamak power reactors.

There were also major forward steps in the Mirror concept. Ion temperatures were doubled to 23 keV, more than twice the values needed for ignition. Peak values of beta were doubled more than 200 percent resulting in densities up to 2 x 10^{14}. Two new ways were proposed to reduce end losses in mirrors, which if confirmed by future theory and experiment, would make this concept far more attractive as a power reactor.

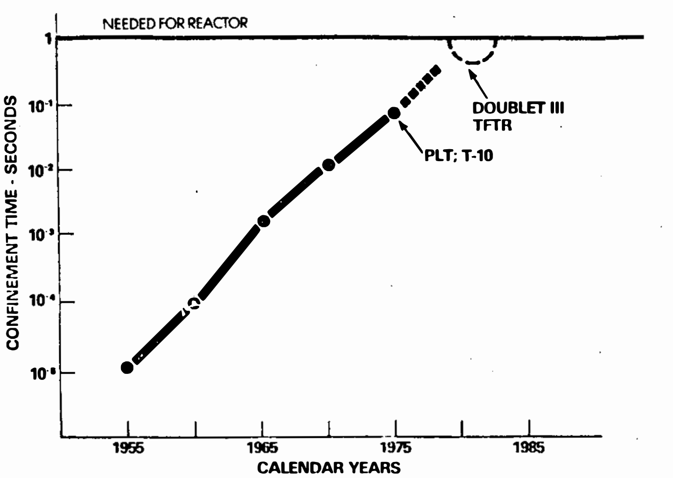
1976 was also a year of important advances in the technologies which are needed to support further experimentation in fusion plasma physics, and allow useful commercial application of the physics when developed. More powerful neutral beam heating sources were developed at ORNL, and the design of the neutral beam heaters for TFTR completed.

A national program plan for development of the critical materials for fusion was worked out, and steps taken to build a tritium systems test facility to study this important systems aspect of fusion. Three conceptual designs for superconducting magnets for the Large Coil Project were completed by industrial subcontractors. Perhaps most important, two laboratory-industry teams began conceptual studies of the next reasonable facility step in fusion development beyond the TFTR.

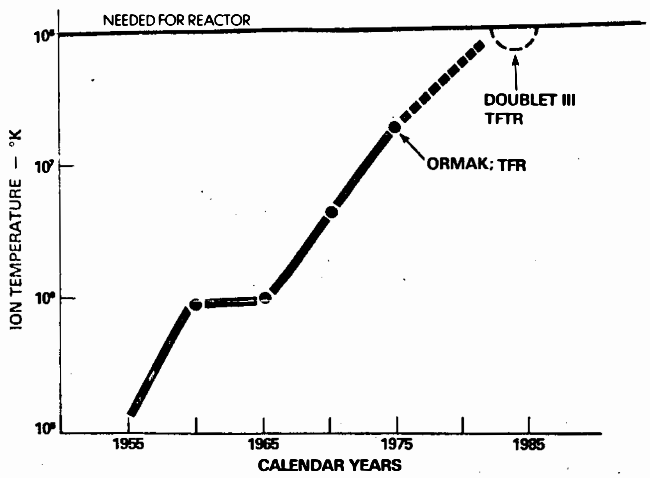
Because there is considerable controversy about the "feasibility" of fusion, I would like to take a moment to show you the steady, continued progress which has been made in world fusion research since its inception in the early 1950's.

These next four illustrations show the steady progress in our ability to generate, control and heat fusion plasmas since the earliest fusion research in the early 1950's, and they also show how near to the conditions needed to design power reactors we are today. (Slides 19, 20, 21, and 22.)

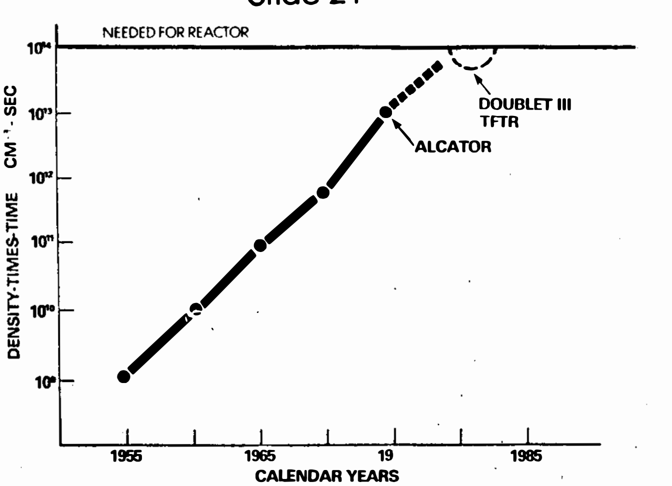
Slide 19



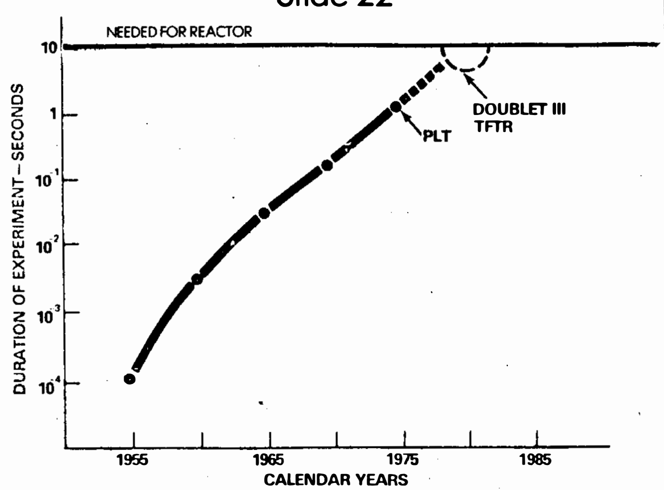
Slide 20



Slide 21



Slide 22



I believe these charts should cause those outside the fusion community to conclude, as those in the community already have, that success in no longer a question of whether, but of when, where and by whom...Now let me show progress in the U.S. on the earlier plot of quality of confinement versis temperature, (Slide 23). First, you will see where we stood in early tokamaks, theta pinches and mirrors as of the end of 1974. Then the solidly bounded areas indicate the significant progress which has been made in just slightly over two years. The areas bounded by dotted lines indicate the design characteristics of devices already designed, under construction or approved. Finally, the Prototype Experimental Power Reactor, now being conceptualized by two competing laboratory-industry teams, and shown in the upper right, and is intended to operate into the ignition region....

While the results of the hybrid studies must be regarded as preliminary, some broad conclusions may be drawn. One hybrid is neutronically capable of breeding sufficient fissile fuel to supply the needs of five to ten Light Water Reactors of a similar thermal rating while producing significant amounts of net electric power.

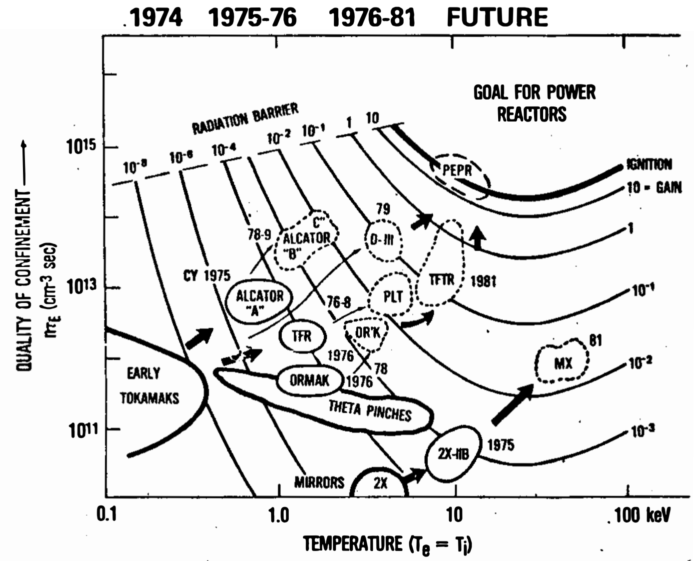
Physics requirements for successful hybrid operation are reduced by about a factor of four from those of pure fusion, particularly in the gain or plasma power amplification requirements. The reduced physics requirements may allow the physics conditions to be demonstrated by the early 1980's; they also allow confinement concepts to be considered for hybrids which would not be adaptable to pure fusion power production....

There are other alternative benefits from fusion research which could be pointed to in the technological spinoffs which will surely occur just as they have from fission and space research. For example, we have had to develop high voltage neutral beam power supplies and switch tubes which should have immediate usefulness in other high voltage applications. We are today developing for radiofrequency heating of plasmas, vacuum tubes of higher power and frequency than have previously been developed in the U.S. The work we are supporting on superconductors and magnets will push these technologies further than ever before in the U.S. with important implications for energy conservation in the electrical industry, as well as in new processes such as magneto forming of metals. Plasma research, itself, could well lead to applications not foreseen. We are exploring the frontiers of new science and new technology and it is to be expected that the insights gained will provide information and ideas useful beyond the primary objectives of the program....

XI. Pace of the Program

In recent years expenditures for fusion research have increased dramatically from $34.3M in 1970, to $56.3M in 1974. In FY 1978 $272.M in budget outlays is being requested. This rapid increase has raised the question in some minds as to what the proper pace for fusion should be. The program is projecting a need for continued support at the FY 1978 or higher levels for many years before practical results can be demonstrated. In fact, I know of no previous non-military development

Slide 23



program in which support at this level has been proposed for such a long time before payoff. This fact requres that the question of the proper pace for fusion development, given its great implications both in terms of benefits and costs, be examined carefully and with a special vision. Therefore, we in the program have tried to define the factors which we believe should be considered in reaching a conclusion as to proper pace in the fusion effort.

The Nation is faced by difficult decisions about the future prospects of long-range energy technologies. Decisions made now about future options can be irretrievable. Correct and wise decisions buy time and increased chances for success in a context where rewards and penalties can hardly be overstated. Resources applied now lessen the uncertainty of decisions which are unavoidable five or ten years from now. Unfortunately, decisionmakers receive no immediate rewards for wisdom and farsightedness in making difficult decisions whose results can not be assessed until decades in the future.

Decisions will have to be taken between competing technologies for power sources of the next century. These political decisions may be taken before many persons in the scientific community think they should be taken, and without the sound technical basis for them that further research and development could have produced. These choices will forclose options, divert scientific talent to other problems and increase our dependency on the concept selected for further development and demonstration. Success of any one avenue is not assured. Recovery from failure at the latter stages of experimentation before demonstration will be more difficult and costly.

So the question of proper pace for the Magnetic Fusion Energy program is a complex and important question — too important to be decided by default.

The decision as to pace in fusion is a decision between proceeding aggressively toward a series of stated goals to a payoff twenty to thirty years in the future, or proceeding slowing in the direction of a more distant, less distinct goal with fewer options in a program of narrowed scope. There is also, of course, the possibility of moving with greater speed to reach an earlier decision point, as in Logics III or IV of the Fusion Program Plan.

The value of fusion's development is not susceptible to precise cost-benefit analyses. It is, therefore, difficult to argue in an absolute sense the rightness of one pace versus another. The penalty for moving slowly, like the rewards for proceeding aggressively, will be felt much later by others, not by the people who make the decisions now. But there are a number of factors which can be identified now and should be a part of any evaluation to determine what pace — i.e., national commitment — is appropriate:

1. *The need for an inexhaustible, non-fossil, base energy source is clear.* Fossil fuels are becoming scarce and more expensive. It is vital to the U.S., expecially in view of its advanced industrialization and resultant dependency upon copious supplies of energy, to assure itself of major new energy resources. The choices for such new energy resources are limited. None is an obvious solution. All seem today to have significant problems of one form or another.

2. *Fusion has an excellent potential to satisfy that need.* Worldwide, fusion has been recognized as a vital objective for technological development. In the Soviet Union, in Western Europe and in Japan, the great potential for fusion energy has been recognized in major development programs. Fusion could be considered the enduring solution to energy problems. Its potential for satisfying the need for energy can be sensed if one imagines the effect on world economic and military affairs if fusion were now developed and available on a competitive economic basis.

3. *Major advances in fusion have been made despite a modest expenditure to this time.* As described above, major new and encouraging advances continue to be made in fusion research. These successes have established technical bases and opened new opportunities for accelerating the fusion program with reduced rusk. Within the fusion community, fusion is no longer looked on as a question of scientific feasibility, but only one of practicality and economics. Major problems and long, expensive development programs remain but it should be recognized that the total expenditure on fusion in this country through FY 1976 was only $800 million, all the results described above have been bought for that cost.

4. *Organization and plans are in place to proceed effectively.* Four national laboratories and one industrial laboratory have strong, experienced organizations working on fusion. The successes mentioned above demonstrate the capabilities of these organizations. In addition, a detailed technical program for magnetic fusion has been worked out and iterated over a span of several years to reflect a consensus of the best minds in the fusion community as to program logic and scope. We know what needs to be done next.

5. *Profound national and international effects will follow successes in fusion — even interim successes.* Fusion is one of the leading measures of national technological capability and achievement. The world looks to both the United States and the Soviet Union for leadership in the field. The Soviet effort is larger than our own. The implications of success in this field on both the economic and diplomatic strengths of the U.S. are great.

6. *It is important to determine the probabilities of fusion practicality at an early date.* A reasonable assurance that fusion can be made practical and economic, or an understanding that such a result is not likely, will have fundamental effects on energy-related policies, both in the Nation and abroad. Our own priorities with regard to conservation of fossil resources, and the urgency of development of alternate, long-range energy sources — solar and the breeder — would be significantly changed if an understanding of the probabilities of fusion success was in hand now. In this sense accelerated research in fusion could be looked on as providing the technical basis for sounder energy policy decisions.

7. *Development of any new energy source to large-scale economic use requires decades after the base technology is available.* Technological development of fusion to demonstration will take a long time — approximately a quarter of century — even on an aggressive schedule. But even after demonstration, years will be required to develop and deploy fusion commercially to a point where it provides significant economic benefit.

Based on consideration of these factors, we believe the pace of fusion development should be decided on the basis of the great social, political and economic implications of success or failure.