ces of reactor performance. In a sense, it tells one how much power one is going to be able to get out of a reactor or to give an investment in the system.

A tokamak reactor needs to operate with a beta above 4½ to 5 percent. Beta values well over 2 percent, perhaps approaching 2½ percent, have been achieved in Doublet III, utilizing only the first of three neutral beam heaters scheduled for installation.

Thus, these preliminary results are very encouraging, and we are confident that the additional heating will allow us to reach reactor-like beta values.

Preparations for phase III of the Doublet III program have begun. In phase III, Doublet III's existing vacuum vessel will be replaced by a larger one in late fiscal 1984. This will be a relatively easy task, since Doubtlet III was designed from the beginning to accommodate a new vacuum vessel with a minimum of disassembly and assembly effort. Its larger, D-shaped plasma should have even better confinement properties and should more closely resemble a reactor plasma in size and operating conditions.

A new vessel will allow the high beta plasma results to be extended to a regime of higher temperature characteristic of fusing plasmas. Plasma engineering problems for reactor design can then be addressed directly.

In summary, nature has been kind to us lately. The experimental results have been quite favorable. We have been quite fortunate at General Atomic in that we have made our eight program milestones which were set up for us during the last year.

We are proceeding with preparations to install a new vacuum vessel in Doublet III, and we are thus taking advantage of a unique opportunity to build and operate a device that fulfills the program's need for a large D-shaped high current, high beta plasma.

Because of its engineering power and similarity in design philosophy to plans for first generation tokamak reactors, the modified Doublet III can be the primary program resource for elucidating the properties of our fusion reactor core. Moreover, it will provide an early test bed for future fusion engineering technologies.

I want to emphasize that this will be fully complementary to Princeton's tokamak fusion test reactor, TFTR, which will probe the engineering and physics of a reacting fuel. Together, these two experiments can provide the basis for confident operation of tokamak reactors.

The fiscal 1983 budget contains funding for the new Doublet III, which represents a highly cost-effective means of constructing a vitally needed tokamak research facility without buying a new operating mortgage. I hope that this committee, whose interest in fusion has historically been keen and well founded, shares General Atomic's belief in fusion's importance to the Nation.

Although I have been asked to speak about Doublet III, I wish to express my personal hope that you will continue to be strong supporters of the fusion program as a whole.

Thank you very much.

Mr. YOUNG. Thank you, Dr. Gilleland.

[The prepared statement of Dr. Gilleland follows:]

STATEMENT OF DR. JOHN GILLELAND, DIRECTOR OF DOUBLET III

Madame Chairman, General Atomic was the first private industry to engage in fusion research, beginning in 1957. Our fusion program has been supported in recent years primarily through the Department of Energy, but from the beginning it has also received support from the utility industries and from the company itself. This breadth of support is both a result and a reflection of GA's unique position in fusion energy development. We are participants in the very forefront of fusion research, but as an industrial company we concentrate on and are a bridge to the commercialization of fusion power. The cornerstone of our program is Doublet III, currently the largest operating tokamak—toroidal magnetic confinement machine—in the world. Doublet III has the goal of approximating the physical conditions in a confined plasma that will be necessary for a fusion reactor.

Two years ago, the Japan Atomic Energy Research Institute joined the Department of Energy in cooperative research on Doublet III. This five-year cooperation represents a strong international endorsement of our program and provides funding for much of the basic hardware fundamental to our long range research plans. This cooperative venture has also resulted in an enhancement of scientific productivity because of the unusual mix of scientific methods and manpower represented by the combination of Japanese and General Atomic scientists. Their collective efforts are resulting in the experimental verification of several theoretical propositions that are important for the future of the tokamak as a viable reactor.

Doublet III is a large tokamak capable of accommodating large, highly-shaped plasmas the size of those planned for Princeton's TFTR and Japan's JT-60. Doublet III is unique in that it combines technical ruggedness with an unusually high degree of technical sohpistication and experimental flexibility.

These features of the machine are fully utilized in Doublet III's long-range experimental program plan. This plan may be visualized as having three overlapping, but distinctly discernible phases. Phase I began in 1978 after the completion of Doublet III slightly ahead of a schedule first set in 1974. Experiments were conducted which emphasized plasma shaping and control in ohmically-heated discharges. During this first phase we verified our ability to make and retain a variety of plasma shapes, confirmed predictions about how well the magnetic bottle will contain plasma heat as a function of its shape and size, and demonstrated methods for prevention of plasma poisoning by impurities. These results added greatly to our confidence in the choice of dee-shaped plasmas for the first-generation tokamak reactor, gave experimental validity to the simplified impurity control schemes adopted for fusion reactor designs, and provided the technical basis for the final phases of experimentation on Doublet III.

The second phase of Doublet III operation recently began in January of 1982 and will continue well into fiscal 1984. During this period increasing levels of neutral beam heating power will be installed, thus allowing us to heat the plasma well beyond the temperatures attained in Phase I. Initial tests of rf systems for plasma heating and plasma current drive will also be conducted. Particular emphasis will be placed on maximizing beta, a term connoting the relative power density of the plasma. More formally, beta is defined as the ratio of plasma pressure to magnetic field pressure in a fusion device.

Beta values well over 2 percent have been achieved in Doublet III utilizing only the first of the three neutral beam heaters scheduled for installation. A tokamak reactor is expected to operate with beta values of about 5 percent. Thus these preliminary results are encouraging, and we believe the additional heating will allow us to reach reactor-like beta values.

Preparations for Phase III have begun. Doublet III's existing vacuum vessel will be replaced by a larger one in late 1984. This will be a relatively easy task, since Doublet III was designed from the beginning to accommodate a new vacuum vessel with a minimum of disassembly and assembly effort. It's larger, dee-shaped plasma should have even better confinement properties and should more closely resemble a reactor plasma in size and operating conditions. A new vessel will allow the high-beta plasma results to be extended to a regime of higher temperature characteristic of a fusing plasma. Engineering problems for reactor design can then be addressed directly.

We thus have a unique opportunity to build and operate a device that fills the need for a large, shaped, high-current, high-beta plasma. Owing to the flexibility of the Doublet III facility, and especially to the demountability of the toroidal field coils, this new stage can be initiated by a straightforward replacement of the present vacuum vessel with a large, dee-shaped vessel. Because of its engineering power and similarity in design philosophy to plans for first-generation tokamak reactors, the modified Doublet III can be the primary program resource for elucidating the properties of a fusion reactor core. Moreover, it will provide an early test bed for future fusion engineering technologies. It is fully complementary to Princeton's Tokamak Fusion Test Reactor, TFTR, which will probe the engineering and physics of a reacting fuel. Together, these two experiments can provide the basis for confident construction and operation of tokamak reactors.

The fiscal 1983 budget contains funding for the new vessel. It represents a highly cost-effective means of constructing a vitally needed tokamak research facility without buying a new operating mortgage.

I hope that this committee, whose interest in the fusion effort has historically been keen and well-informed, shares GA's belief in fusion's importance to the nation. I hope you will continue to be strong supporters of fusion energy. I thank you all, and we welcome any questions you may have.

Mr. YOUNG. Dr. Repici, would you care to proceed with your testimony?

STATEMENT OF DOMINIC J. REPICI, DIRECTOR OF CORPORATE DEVELOPMENT, INTERNATIONAL NUCLEAR ENERGY SYSTEMS

Dr. REPICI. Mr. Chairman, thank you.

Thank you very much, members of the committee. Dr. Bussard has asked me to send his apologies for not appearing here today. This does, however, give me the chance to appear and make some comments and depart somewhat from the record.

Let me say that it is a pleasure to be on this side of Pennsylvania Avenue and to shed some of the anonymity that covered what we have done prior to this appearance. It is a pleasure to be here and to state publicly that we support the goals of the act which this committee and the Congress passed 2 years ago, the McCormack Fusion Engineering Act.

I would like to depart from the general framework of the DOE program and make comments today about what INESCO is doing. We are a privately funded company, and we are outside the ambit of Federal funding. I would like to show some of the relationships that we have to the DOE program and to show where we are going.

The view from OMB and watching the transition between administration presents for me some unique impressions. There are issues of scope of the Federal involvement, the role of private industry, the relationships of private industry to the Federal fusion program, and the schedule of that program. I think those are probably best covered with this committee staff, and I would be glad to do that.

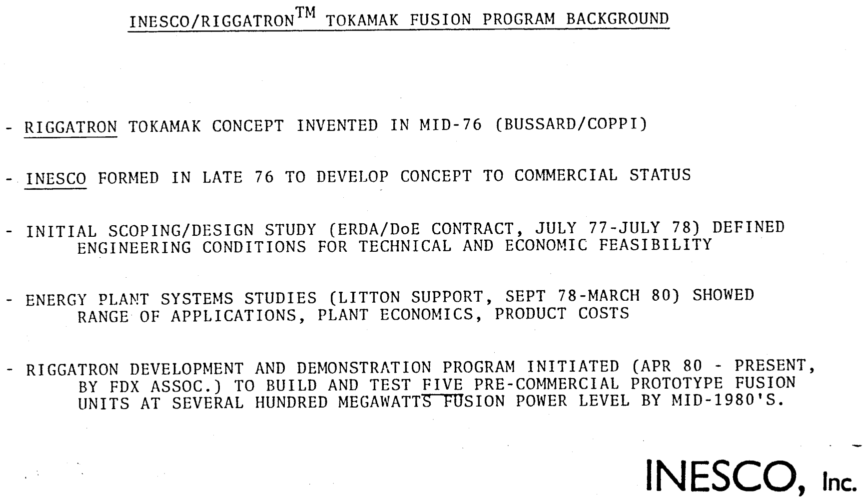
So I would like to confine my comments today to the INESCO program. There are four pieces which I would like to cover. Namely, I would like to say who we are and what our goals are. I want to say that practical fusion power, in our view, means that we must have small machines in some fashion. I would like to talk about the development program in which we hope, in 5 to 6 years, to actually have a burning plasma in a small demonstrated machine.

Then, briefly, I would like to touch upon the economic impacts that this would have on this country and on INESCO.

First, I think, if I may start with who we are, and just briefly a little bit of history. INESCO was founded in 1976. Dr. Bruno Coppi from MIT and Dr. Robert Bussard essentially each had half of an interesting idea. Dr. Coppi recognized that one could achieve ignition in small machines, and Dr. Bussard recognized that if one inverted the geometry by putting the neutron-absorbing mechanisms outside of the metal container, the copper magnets, one could extract energy.

At any rate, INESCO made a proposal to what was then the ERDA, and ERDA urged them to pursue the idea and funded this program for approximately 1 year.

I would like to use some viewgraphs so that I can skip through what is rather lengthy testimony and keep it to the confined time. I would like to start with slide 1.



INESCO/RIGGATRON™ TOKAMAK FUSION PROGRAM BACKGROUND

- RIGGATRON TOKAMAK CONCEPT INVENTED IN MID-76 (BUSSARD/COPPI)
- INESCO FORMED IN LATE 76 TO DEVELOP CONCEPT TO COMMERCIAL STATUS
- INITIAL SCOPING/DESIGN STUDY (ERDA/DoE CONTRACT, JULY 77-JULY 78) DEFINED ENGINEERING CONDITIONS FOR TECHNICAL AND ECONOMIC FEASIBILITY
- ENERGY PLANT SYSTEMS STUDIES (LITTON SUPPORT, SEPT 78-MARCH 80) SHOWED RANGE OF APPLICATIONS, PLANT ECONOMICS, PRODUCT COSTS
- RIGGATRON DEVELOPMENT AND DEMONSTRATION PROGRAM INITIATED (APR 80 - PRESENT, BY FDX ASSOC.) TO BUILD AND TEST FIVE PRE-COMMERCIAL PROTOTYPE FUSION UNITS AT SEVERAL HUNDRED MEGAWATTS FUSION POWER LEVEL BY MID-1980'S.

INESCO, Inc.

SLIDE 1

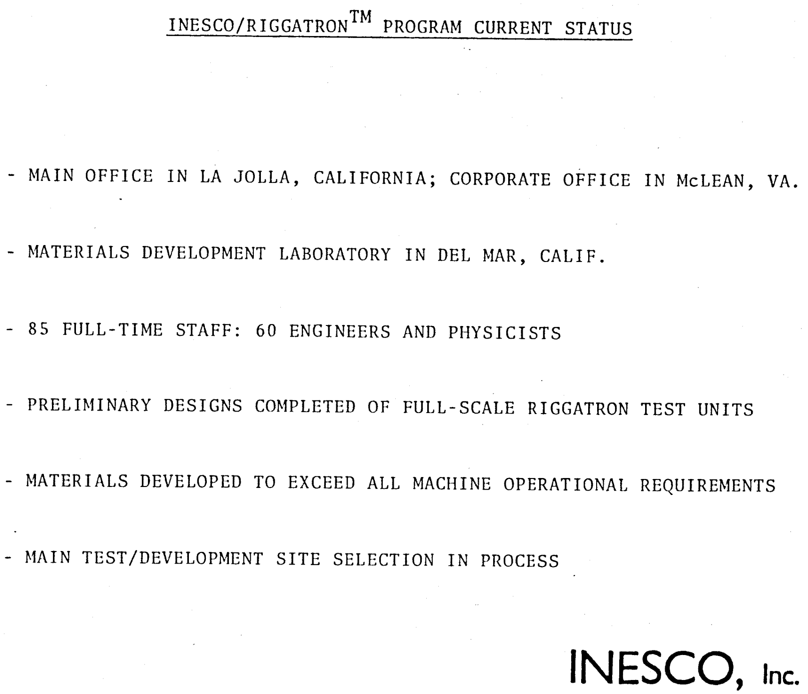
Essentially, the initial scoping/design study sponsored by ERDA showed the conditions that would be required for technical and economic feasibility of this device.

In the fall of 1978, after the completion of the ERDA work, Litton Industries supported INESCO for 18 months to conduct studies which were aimed at looking at what the markets would be for such a device if in fact we could build a device. So, in effect, it was a market scoping endeavor.

Some very phenomenal numbers come out of that, which I will share with you today, with the appropriate caveats.

In April of 1980, the RIGGATRON tokamak of the INESCO program was supported by FDX Associates, a Delaware limited partnership, which is funding INESCO at $102 million over a 5½ year period to literally build and test five of these precommercial prototype units.

The small units which we hope to build will have a fusion thermal power, of approximately 200 megawatts. It is a very substantial device. Next slide, please (slide 2).



INESCO/RIGGATRON™ PROGRAM CURRENT STATUS

- MAIN OFFICE IN LA JOLLA, CALIFORNIA; CORPORATE OFFICE IN McLEAN, VA.
- MATERIALS DEVELOPMENT LABORATORY IN DEL MAR, CALIF.
- 85 FULL-TIME STAFF: 60 ENGINEERS AND PHYSICISTS
- PRELIMINARY DESIGNS COMPLETED OF FULL-SCALE RIGGATRON TEST UNITS
- MATERIALS DEVELOPED TO EXCEED ALL MACHINE OPERATIONAL REQUIREMENTS
- MAIN TEST/DEVELOPMENT SITE SELECTION IN PROCESS

INESCO, Inc.

Slide 2

The work conducted to date has resulted in a detailed design for our first fusion reactor. We have that in hand. We have successfully developed the basic materials needed to achieve that device. We have worked with the Brush-Wellman Co., Cabot-Berylco, and the International Copper Research Association to develop the coppers. The key uncertainty associated with this device is whether or not one can build the magnets.

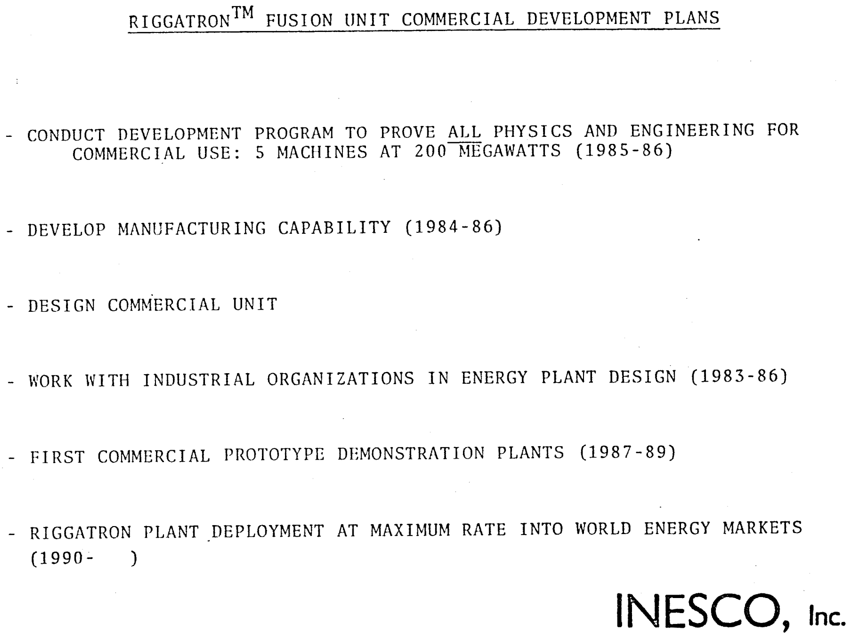
We have the materials in hand, and we are now engaged in the final site selection process. You should note that our work is based solidly on the world's tokamak research. It really represents an outgrowth of the MIT program, the Alcator program and the Frascati program. Quite candidly, we would not be here today were it not for the investment that this committee and this Congress have made in the U.S. fusion program.

We have not generated a new engineering technology, nor have we generated new confinement physics. Rather, we have taken the existing physics concepts and reengineered them, if you will, pushed them to the limits which the theory indicates they will go. In effect, we have reduced the size of tokamaks by a factor of about 1,000 in volume.

There remains a question of uncertainty, quite candidly, and that is whether or not tokamak confinement laws will still hold at the conditions needed for power reactors. That is a question, however, that is relevant to all tokamak research, not just what we are conducting.

We only have to go about a factor of two or three from the results shown in the fusion program to get to that ignition condition, so optimism is high.

Now, if some wholly unexpected or new phenomenon causes our tokamaks to fail, then, unfortunately, it is likely that all tokamaks would fail. This provides, from my perspective, a rationale for the breadth that the DOE program has at the present time. Next slide, please (slide 3).



RIGGATRON^{TM} FUSION UNIT COMMERCIAL DEVELOPMENT PLANS

- CONDUCT DEVELOPMENT PROGRAM TO PROVE ALL PHYSICS AND ENGINEERING FOR COMMERCIAL USE: 5 MACHINES AT 200 MEGAWATTS (1985-86)
- DEVELOP MANUFACTURING CAPABILITY (1984-86)
- DESIGN COMMERCIAL UNIT
- WORK WITH INDUSTRIAL ORGANIZATIONS IN ENERGY PLANT DESIGN (1983-86)
- FIRST COMMERCIAL PROTOTYPE DEMONSTRATION PLANTS (1987-89)
- RIGGATRON PLANT DEPLOYMENT AT MAXIMUM RATE INTO WORLD ENERGY MARKETS (1990-)

INESCO, Inc.

SLIDE 3

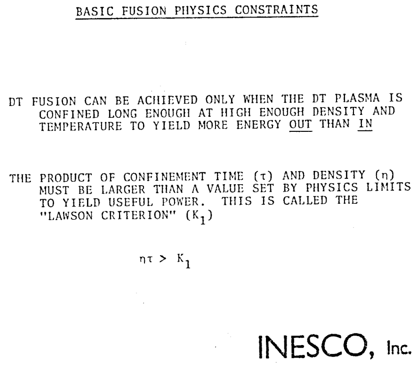
We are also engaged in the commercial development of this device. Clearly, we want to make a profit at this, and we are going to need manufacturing capability, and we hope to work with the major industrial organizations.

Next I would like to cover the basic concepts, why we must have small machines to be successful, to be economically successful. I will not go through the reassessment of what happens when fusion occurs, but the key point is that there are two criterea. We of course have to invest enough energy to heat the plasma so that the coulomb repulsion is overcome; literally, so that there is enough energy so that the ions can bump into each other and hence "stick" or fuse, if you will excuse the use of the vernacular.

Then, also, one has to achieve the Lawson criterion, which is essentially a statement that says the product of density and confinement time must equal some constant.

The amount of energy stored in the plasma is really a function of the number of particles per unit volume, and you need to have a magnetic field to put those particles in a bottle, quite literally. But, to control that plasma, you then have to invest energy in the magnetic field.

Could I have the next slide, please (slide 4)?



BASIC FUSION PHYSICS CONSTRAINTS

DT FUSION CAN BE ACHIEVED ONLY WHEN THE DT PLASMA IS CONFINED LONG ENOUGH AT HIGH ENOUGH DENSITY AND TEMPERATURE TO YIELD MORE ENERGY OUT THAN IN

THE PRODUCT OF CONFINEMENT TIME (τ) AND DENSITY (n) MUST BE LARGER THAN A VALUE SET BY PHYSICS LIMITS TO YIELD USEFUL POWER. THIS IS CALLED THE "LAWSON CRITERION" (K_1)

$$n\tau > K_1$$

INESCO, Inc.

SLIDE 4

The question then arises, what happens if we are limited in the strength of the magnetic field which we can produce? Well, the density that you can achieve is proportional to the magnetic field. Consequently, if you cannot have the necessary magnetic field, then you are limited as to what density you can achieve and, hence, you must have a larger resident time in the active region, that is, a larger confinement time, and, in effect, you must make the device larger.

Well, where does all this lead? May I have the next slide, please (slide 5)?

BASIC FUSION PHYSICS CONSTRAINTS

WITH SUPERCONDUCTING MAGNETS THE ATTAINABLE FIELDS (B) ARE TOO SMALL TO ALLOW SMALL MACHINES. THUS,

SUPERCONDUCTING MAGNET MACHINES MUST BE LARGE

LARGE SIZE MEANS LARGE COSTS AND LONG DEVELOPMENT TIMES
THIS IS THE SO-CALLED "MAINLINE" APPROACH

WITH COPPER MAGNETS, HIGH FIELDS CAN BE REACHED WHICH ALLOW SMALL SIZE, THUS

COPPER MAGNET MACHINES CAN BE SMALL

THIS IS THE INESCO/RIGGATRON TOKAMAK APPROACH

INESCO, Inc.

SLIDE 5

It says quite candidly, if you are going to use superconducting magnets, then the plasma region must be large, and if it is large, it is expensive and time consuming to develop and it generates a long-time scale for development.

The alternative upon which our program is based is to use copper magnets. They are smaller, they are less expensive, the development time is less; but there is a problem. You have to steal power from someplace to run those copper magnets. So there is really a question of power balance and whether or not you can physically build such magnet. These factors mitigate against copper. The alternative however, is superconductors; they are very complex. They are a new technology in many ways, and they must have large size.

What we are contending is that it is the small size—which is what our studies have shown, and it is quite straightforward—that it is the small size that generates the attractiveness of cost.

Let me now switch to the RIGGATRON tokamak development program and tell you what we are going to do. There are two pieces of fundamental engineering innovation that INESCO has produced. One of them is the fact that we are using water-cooled copper alloy conductors for the magnets. We feel we can build these magnets.

The second piece is that the blanket is outside of the magnets and, hence, the restrictions on blanket configuration are less severe. Our technical approach is shown in the next slide (slide 6.)

INESCO TECHNICAL APPROACH

DEVELOP VERY SMALL TOKAMAK FUSION MACHINES

USE WATER-COOLED, HIGH STRENGTH, COPPER ALLOY MAGNETS

USE PLASMA OHMIC HEATING AS PRINCIPAL IGNITION MEANS

DESIGN DOMINATED BY ENGINEERING LIMITS

DRIVE THESE UNITS VERY HARD, WITH LIMITED LIFE, BUT AT LOW UNIT COST/POWER PRODUCED

PERFORMANCE DETERMINED BY SUCCESSIVE TESTING OF MULTIPLE INEXPENSIVE MACHINES

CONSTRUCT POWER PLANTS FROM MODULAR ARRAYS OF FPC UNITS

DISPOSE/RECYCLE USED UNITS

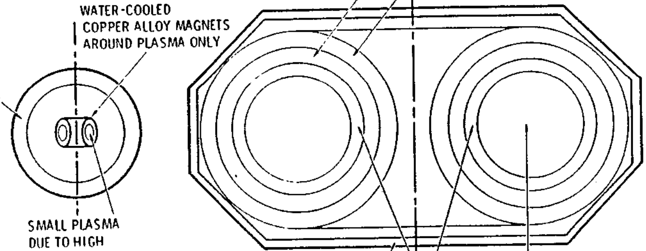
LOW UNIT COST YIELDS ATTRACTIVE ECONOMICS FOR CENTRAL STATION POWER PLANTS

INESCO, Inc.

SLIDE 6

The design philosophy that we are using is to design these machines to their engineering limits. They are small, about 8 tons' worth of copper. You can literally build, break and test them, and in effect you can push them to the limit, something you cannot do with larger machines. In a sense, we can reiterate the data and we can approach upon the problem literally by testing our way forward. Next slide, please (slide 7).

COMPARISON OF GEOMETRIC AND STRUCTURAL CHARACTERISTICS OF TOKAMAK FUSION OPTIONS



RIGGATRON TOKAMAKS
SMALL; SIMPLE GEOMETRY AND STRUCTURE

MAINLINE TOKAMAKS
LARGE; COMPLEX GEOMETRY AND STRUCTURE

LARGE VOLUME INTERNAL RADIATION SHIELD TO PROTECT MAGNETS

CRYOGENICALLY COOLED (L He) SUPERCONDUCTING MAGNETS EXTERNAL TO COMPLETE SYSTEM

SMALL VOLUME SPHERICAL OR CYLINDRICAL BLANKETS EXTERNAL TO COMPLETE SYSTEM

WATER-COOLED COPPER ALLOY MAGNETS AROUND PLASMA ONLY

SMALL PLASMA DUE TO HIGH MAGNETIC FIELD AND SMALL MACHINE SIZE

MASSIVE STRUCTURAL SPACE FRAME FOR MAGNET SUPPORT

LARGE PLASMA DUE TO LOW MAGNETIC FIELD AND LARGE MACHINE SIZE

NO MAGNET SHIELD REQUIRED
NO MAGNET STRUCTURAL SPACE FRAME REQUIRED

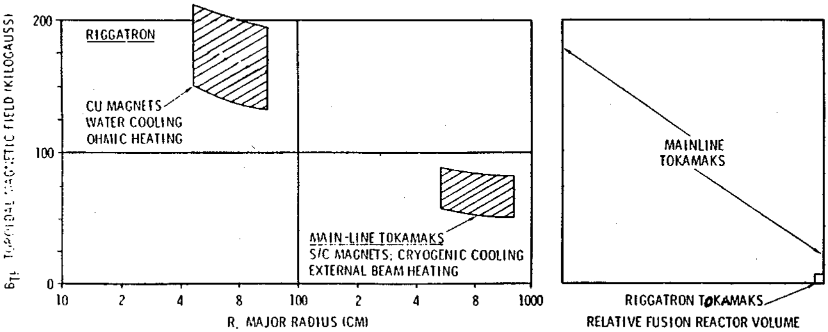
LARGE VOLUME TOROIDAL BLANKET INTERNAL TO MAGNETS

SLIDE 7

This is a comparison of the copper concepts with superconducting concepts. If you use the INESCO copper magnet concepts, you then can put the blanket outside of the device. If you are forced to use superconducting concepts, then you must have a large superconductor simply because of the magnetic field limitations on a superconductor.

You must then protect the superconductor. You must put the blanket or the mechanism by which you extract the neutrons within the superconductor, and that quite frankly makes for a difficult engineering problem and it makes for a big machine.

TECH APPROACHES TO TOKAMAK REACTORS RIGGATRON & MAINLINE TOKAMAKS



ALLOWABLE REGIMES FOR TOKAMAK OPERATION

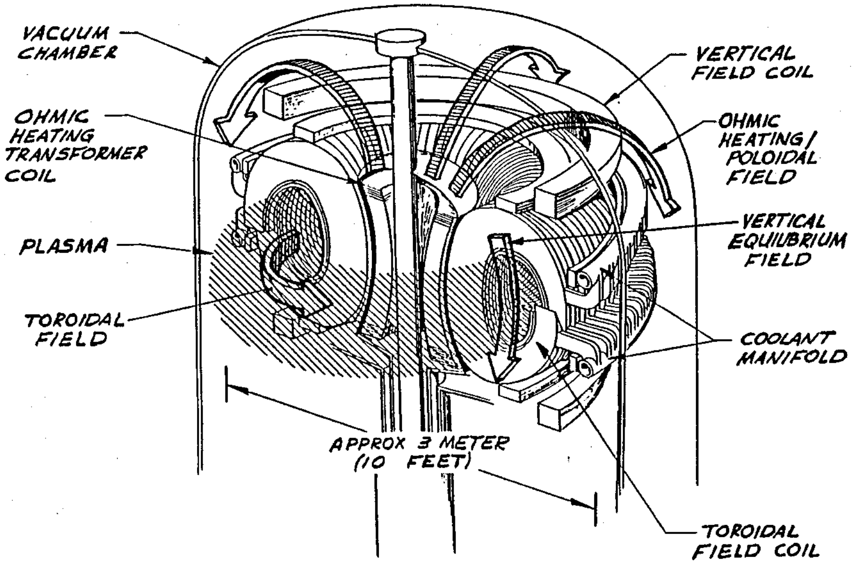
RELATIVE FUSION REACTOR VOLUME
COST ~ VOLUME

COMPARISON OF TECHNOLOGICAL FEATURES OF TOKAMAK FUSION OPTIONS

SLIDE 8

The next slide (slide 8) is just simply a quantification of what that means, just the different places where we are operating. Next slide, please (slide 9).

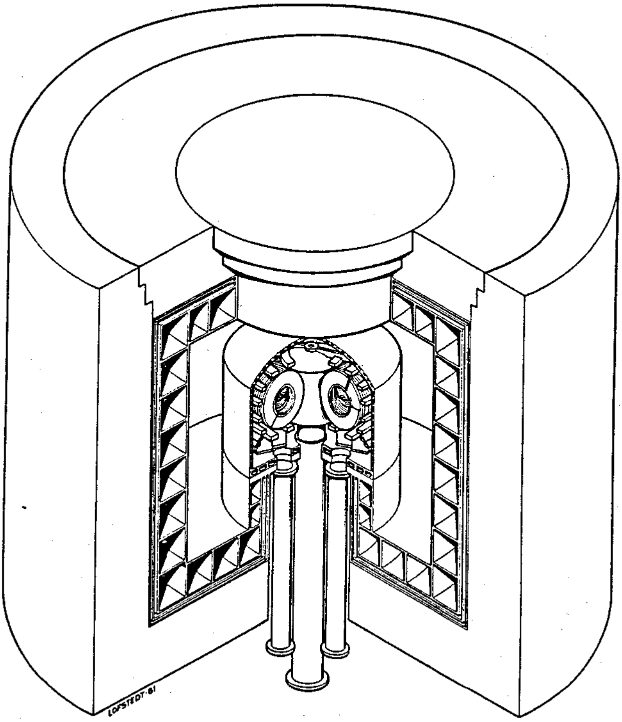
RIGGATRON^{tm} FUSION POWER CORE



SLIDE 9

What does our test device look like? I think we have said enough about comparisons and the reasons pro and con. Effectively, this is our device. The diameter is 40 to 60 centimeters. The whole device is perhaps 10 feet across. It is enclosed in a vacuum envelope and as a commercial device it would operate at 1,000 to 1,200 megawatts of fusion power. Next slide, please (slide 10).

RIGGATRON^{tm} FUSION POWER CORE IN BLANKET AND SHIELD CELL



SLIDE 10

Larger powerplants could be made from a modular array of these, and here I have shown the external blanket assembly, which would represent the fixed in-place equipment, and then you would literally plug in and plug out the fusion core, much as you would with a light bulb surrounded by a lamp shade.

A modular array of these in a powerplant, of course, would have a high utilization factor simply because an extra blanket assembly would be redundant without having to duplicate the balance of plant.

Our test site that would be required to conduct this program, we feel, would need about 10 acres. We would need little tritium because the system is relatively small, and we hope to conduct tests on these five machines in an 18- to 24-month period. First, we would use normal hydrogen, deuterium, and then deuterium-tritium, much like the plan for the DOE programs.

We hope that the device will operate. Our anticipation is that it will operate for a period of several seconds. Next slide, please (slide 11).

RIGGATRON TOKAMAK EXPERIMENTAL PROGRAM

DESIGN, BUILD AND TEST FIVE RIGGATRON TOKAMAKS IN 1985/86

INITIAL TESTS WITH H, NEXT WITH D, LAST WITH DT

CONTROLLED IGNITION AND STABLE BURN FOR SEVERAL SECONDS

WILL PROVE ALL PHYSICS AND ENGINEERING FEATURES FOR REACTOR OPERATION EXCEPT BURN CYCLE LENGTH AND MATERIALS LIFETIME

INESCO, Inc.

SLIDE 11

If we are successful, we will have proved all the physics and engineering needed for commercial units except for the length of the fuel burn cycle and the lifetime of the coil materials.

Now, we do have available data, and that data indicates that the lifetime of the coils would be of the order 1,000 hours and that the burn cycle would be of several minutes duration, and that the inter-pulse "off" time might be less than 3 seconds.

I would now like to comment very briefly about the RIGGATRON energy plant applications and just simply say that there are some spectacular numbers. Return on gross investment is of the order of 100 to 150 percent per year. These are astonishingly large. They result from two features that are inherent in the RIGGATRON.

First of all, the RIGGATRON acts much like a consumable fuel. We are burning copper, is really what it comes down to, and the balance of plant is relatively small.

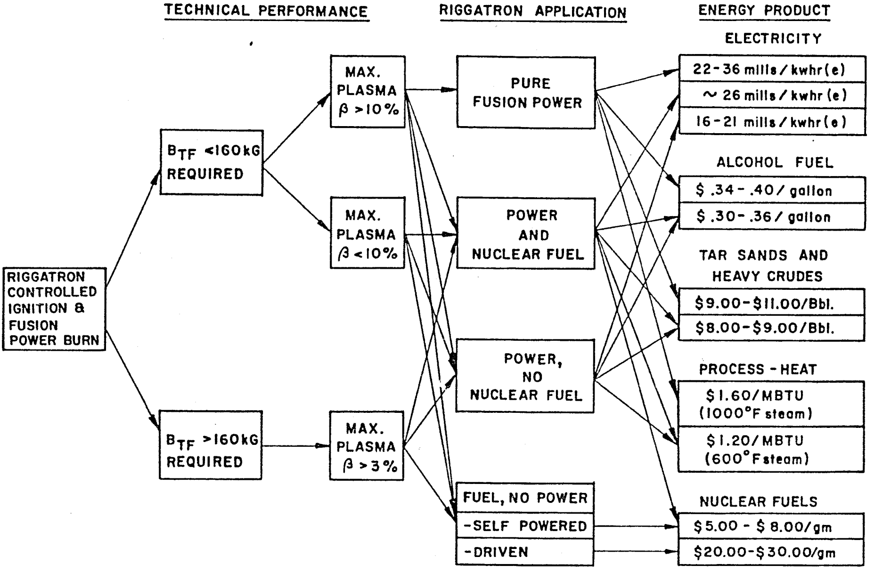
Some of the numbers indicated are fantastic—14 cents per 1,000 cubic feet of natural gas, $1.60 per million Btu of steam, 35 cents a gallon for ethanol. Now, I recognize this is a paper reactor, and it always looks better when it is on paper and then things happen when you go from concept to reality and suddenly the rules change. However, the point I want to make is not the inviolability of these numbers but the fact that they are very much below conventional prices, and that is I am off by 100, 200, 400 percent, there is room here and there is cushion here for a very economical system. I mean to say no more.

If we are successful, though, clearly this will have profound economic impact on where we are going.

The second feature which drives the economics is the fact that the capital equipment associated with the fusion energy source is small. Ninety percent of any utilization plant would be in the steam thermal conversion to electricity, and 10 percent would be in the RIGGATRON blanket.

The costs will predominately lie in proven standard technologies, and therefore, retrofitting might very well be possible.

In effect, the economics are dependent upon cheap steam and cheap neutrons. Next slide, please (slide 12).

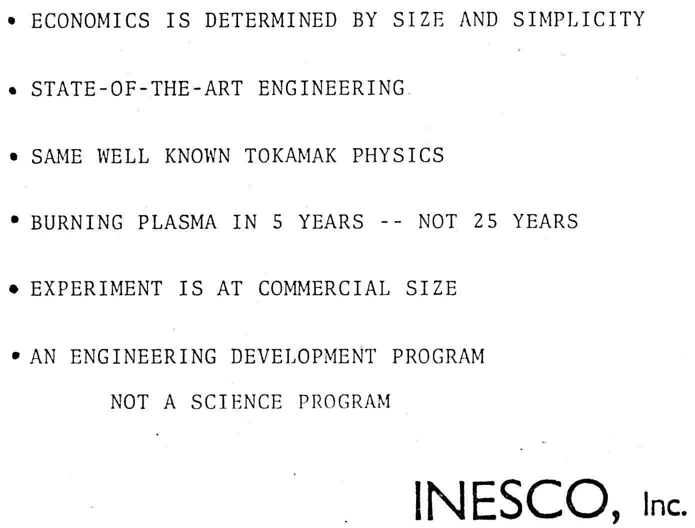


RIGGATRON TOKAMAK TECHNICAL PERFORMANCE AND TYPICAL ENERGY APPLICATIONS

SLIDE 12

This is a very complicated slide which essentially says, if I can make the device ignite by all those apparently complicated paths, I can make money at it. I will leave that discussion to the testimony submitted for the record. It is attractive, and there are various physics constraints which say some routes may work and some routes may not work, but nonetheless, what it says is that if it ignites, it will make money.

Finally, Mr. Chairman, I really have to ask the rhetorical question, why am I here? I don't have any Government funding; I am not looking for any Government funding. May I have the last slide, please (slide 13)?



- ECONOMICS IS DETERMINED BY SIZE AND SIMPLICITY
- STATE-OF-THE-ART ENGINEERING
- SAME WELL KNOWN TOKAMAK PHYSICS
- BURNING PLASMA IN 5 YEARS -- NOT 25 YEARS
- EXPERIMENT IS AT COMMERCIAL SIZE
- AN ENGINEERING DEVELOPMENT PROGRAM

  NOT A SCIENCE PROGRAM

INESCO, Inc.

SLIDE 13

I would like to just simply summarize what this all means. Let me just simply read that slide: Economics is determined by size and simplicity, not exotic engineering. We are at the state of the art in engineering. We have the same well known tokamak physics as the U.S. program is using.

We hope to have a burning plasma in 5 years, not 25. The experiment is already at commercial scale, and the program is an engineering program, not a science program.

Why are we in this program? We are in it for three reasons: the sheer joy of being in it and running a good race with the U.S. program; the profits that we may obtain from the success; and, after all, the rather patriotic reason that if it is successful, it has benefits to society in general.

Mr. Chairman, 2 years ago, this committee, under Chairman McCormack's leadership, passed the Fusion Engineering Act. Despite the budget problems and the issues that I have not touched today, that act in effect said, we want fusion to happen. What we at INESCO are saying is that we believe that INESCO today may represent an excellent chance for achieving that goal in the near term.

But I also want to say, Mr. Chairman, that we need that U.S. program. Thank you very much.

Mr. YOUNG. Thank you very much, Dr. Repici. We appreciate your testimony. The testimony of all four of our witnesses this morning has been very, very important to this committee.

Without objection, I would like to insert into the record a statement from Dr. John B. Yasinsky, general manager of the advanced power systems divisions of the Westinghouse Electric Corp.; a letter to Chairman Bouquard from the University Fusion Association; a statement from the Fusion Energy Foundation; as well as the full text of the prepared statements of our panel members today.

[The prepared text of Dr. Bussard follows:]

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Testimony to the House Science and Technology Committee Subcommittee on Energy Research and Production, Chairman, Congresswoman Marilyn Bouquard

Statement of Dr. Robert W. Bussard, President
International Nuclear Energy Systems (INESCO), Inc.
March 24, 1982

INESCO, Inc.

Madam Chairman and Members of the Committee:

It is indeed a pleasure to appear before you today. I thank you for this opportunity. I am Dr. Robert W. Bussard, the President and Chairman of International Nuclear Energy Systems Company (INESCO), Inc. which is a privately-funded fusion development corporation. Our offices and laboratory are located in La Jolla and Del Mar, California. We currently have a staff of about 85 people. I hold a Ph.D. in plasma physics from Princeton University, and am a former Associate Director for Development and Technology of the Atomic Energy Commission's Magnetic Fusion Program.

I come here today to tell you about the scientific and technological progress which INESCO has achieved, and of the promise of our work. I hope this information will be useful to you in your deliberations, and the fact that you are familiar with our program may be beneficial to us.

In today's testimony I wish breifly to explain:

1. Who we are and what are our company's goals
2. Some basic concepts which dictate that practical fusion machines must be small
3. The RIGGATRON tokamak and our private development program to produce commercial fusion power units in 5 to 6 years

and finally,

4. The economic impact which this program will initiate.

1. INESCO and the RIGGATRON Tokamak Program

Dr. Bruno Coppi and I invented the idea of the RIGGATRON tokamak in mid-1976 as a possible means of achieving controlled fusion ignition and power burn, for a practical small scale reactor, attainable on a short time scale. We filed the initial patents and founded International Nuclear Energy Systems Company (INESCO), Inc. in late 1976 for the express purpose of carrying this idea to fruition and to commercial status. Dr. Robert L. Hirsch, then Associate Administrator for Solar, Geothermal and Advanced Energy Systems (of the USERDA) urged us to pursue the idea, and we submitted a proposal for scoping studies to the USERDA. This was funded in July 1977, and carried to conclusion in July 1978. Results of this study showed the conditions required for technical and economic feasibility of the concept.

In the Fall of 1978 we sought private funding and were supported by Litton Industries, who were interested in the possible energy plant applications of RIGGATRON units. These studies, conducted over 18 months, showed an astonishing applicability to a wide variety of energy plant types, with very large return-on-investment and large profit margins from most of the useful applications. These ranged from cheap steam for electric power production, for chemical process heat, for alcohol production, and for heavy oil stimulation and recovery, to cheap neutrons for hybrid fusion power generating for fuel production for nuclear reactors, and for nuclear waste "burning" by transmutation.

In April of 1980 our main RIGGATRON tokamak design and development program was initiated with private funds, from FDX Associates L.P. (a Delaware investment partnership), to build and test five pre-commercial prototype fusion power units by the mid-1980's. These units are planned to operate at steady burn power levels of over

200 megawatts (thermal), and will prove much of the engineering and physics necessary to allow construction, test, and manufacturing of prototype units for commercial plant applications. This privately-funded program will cost in excess of $100M over this period. Figure 1 summarizes this history of our work.

Currently we employ about 85 people full-time, with 60 full-time engineers and physicists on our staff. Our main offices are in La Jolla, California, and our materials development laboratory is in Del Mar, California. We maintain a corporate liaison office in McLean, Virginia. Work conducted to date in our main effort has led to detailed design for our first fusion test units, and to the successful development of all basic materials needed to assure the engineering integrity of these units. Our materials development has been conducted privately in concert with Brush-Wellman, Cabot-Berylco, and the International Copper Research Association. Over the past year we have improved the strength and conductivity of commercially available alloys to the extent that these materials now significantly exceed the requirements of our most under-designed fusion machines. We have also defined our test power supply and test facility needs, and are engaged in final site selection for our five machine demonstration testing. Figure 2 summarizes our current status.

It is important to note that the RIGGATRON tokamak design is based solidly on all the physics knowledge derived from all of the world's tokamak research conducted since its invention (in late 1960's) by Soviet Academician Lev Artsimovich. The high field tokamak research programs at MIT (the Alcator program) and Frascati,

FIGURE 1

INESCO/RIGGATRON^{TM} TOKAMAK FUSION PROGRAM BACKGROUND

- RIGGATRON TOKAMAK CONCEPT INVENTED IN MID-76 (BUSSARD/COPPI)

- INESCO FORMED IN LATE 76 TO DEVELOP CONCEPT TO COMMERCIAL STATUS

- INITIAL SCOPING/DESIGN STUDY (ERDA/DoE CONTRACT, JULY 77-JULY 78) DEFINED ENGINEERING CONDITIONS FOR TECHNICAL AND ECONOMIC FEASIBILITY

- ENERGY PLANT SYSTEMS STUDIES (LITTON SUPPORT, SEPT 78-MARCH 80) SHOWED RANGE OF APPLICATIONS, PLANT ECONOMICS, PRODUCT COSTS

- RIGGATRON DEVELOPMENT AND DEMONSTRATION PROGRAM INITIATED (APR 80 - PRESENT, BY FDX ASSOC.) TO BUILD AND TEST FIVE PRE-COMMERCIAL PROTOTYPE FUSION UNITS AT SEVERAL HUNDRED MEGAWATTS FUSION POWER LEVEL BY MID-1980'S.

FIGURE 2

INESCO/RIGGATRON^{TM} PROGRAM CURRENT STATUS

- MAIN OFFICE IN LA JOLLA, CALIFORNIA; CORPORATE OFFICE IN McLEAN, VA.
- MATERIALS DEVELOPMENT LABORATORY IN DEL MAR, CALIF.
- 85 FULL-TIME STAFF: 60 ENGINEERS AND PHYSICISTS
- PRELIMINARY DESIGNS COMPLETED OF FULL-SCALE RIGGATRON TEST UNITS
- MATERIALS DEVELOPED TO EXCEED ALL MACHINE OPERATIONAL REQUIREMENTS
- MAIN TEST/DEVELOPMENT SITE SELECTION IN PROCESS

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Italy (the FT tokamak) have been of particular value in this regard. Our program utilizes no new engineering technologies and no new confinement physics; we have simply taken an engineering approach of high power density design (characteristic of the aerospace industry, rocket and jet engines) to reduce tokamaks by a factor of 1000 to sizes suitable for practical development and use.

The question remains if current well-proven tokamak confinement laws will still hold at the conditions needed for power reactor operation, which our RIGGATRON machines hope to achieve. This is the only unknown issue in our program. It is the same issue which confronts all the world's fusion programs; will current proven confinement laws still hold at higher levels? We have reason to believe this is likely when we note that the parameters of heating and confinement in tokamaks have been increased by a factor of over 10,000 since the tokamak concept was first conceived. The RIGGATRON unit must go only another factor of about 2 to be successful. We intend to prove this by our full scale RIGGATRON machine experiments. If some wholly unexpected new phenomenon causes our tokamak to fail, then it is likely that all tokamaks will fail. Generally, however, plasma physicists throughout the world believe that the tokamak scaling laws will extrapolate successfully.

In Figure 3 our commercial development plans are outlined. We intend to commercialize as rapidly as possible after success of our five machine tests in 1985-86. To do so we will be developing a manufacturing capability as we progress, will design prototype commercial units in 1986-87, and hope to work with major industrial

FIGURE 3

RIGGATRON^{TM} FUSION UNIT COMMERCIAL DEVELOPMENT PLANS

- CONDUCT DEVELOPMENT PROGRAM TO PROVE ALL PHYSICS AND ENGINEERING FOR COMMERCIAL USE: 5 MACHINES AT 200 MEGAWATTS (1985-86)

- DEVELOP MANUFACTURING CAPABILITY (1984-86)

- DESIGN COMMERCIAL UNIT

- WORK WITH INDUSTRIAL ORGANIZATIONS IN ENERGY PLANT DESIGN (1983-86)

- FIRST COMMERCIAL PROTOTYPE DEMONSTRATION PLANTS (1987-89)

- RIGGATRON PLANT DEPLOYMENT AT MAXIMUM RATE INTO WORLD ENERGY MARKETS (1990-   )

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organizations prior to 1986 to define RIGGATRON-using energy plant designs and develop energy plant sub-systems to acceptable status for commercial use in 1987-89. After the first few demonstration plants are running in 1989-90, we intend to deploy into world commercial energy plant markets at the maximum possible rate.

2. Basic Concepts and the Need for Small Machines

You will recall from various DoE technical presentations that fusion is the process whereby we "fuse" or bring together two lighter elements to form a heavier nucleus accompanied by the release of energy. The specific process of interest is fusion of the two heavy isotopes of hydrogen, deuterium and tritium (D and T), to form helium and a neutron. The majority of the reaction energy is carried away by the neutron.

To initiate a fusion reaction in a controlled manner, a gaseous mixture of deuterium and tritium is ionized to form a plasma, i.e., sufficient external energy is added to the D and T gas micture such that the electrons are stripped from the D and T ions and form a "gas" of charged particles known as a "plasma". We then have a highly energized gas or plasma of dissociated ions and electrons. Although the bulk plasma has no net electric charge, the dissociated ions and electrons have a charge and hence can be confined or held together on magnetic field lines. If these field lines are generated by magnet coils of the proper shape to form a "magnetic bottle", we will be able to hold the mass of plasma away from the walls of this "bottle".

To achieve fusion enough energy must be invested into the plasma so that the D and T ions become so energetic that they have sufficient energy to overcome the electrical forces of repulsion which exist between ions of similar charge. When sufficient energy is introduced, this coulomb repulsion is overcome, so that the D and T ions can actually collide and "stick" together, i.e., to fuse.

This energy investment expressed in scientific terms means that the plasma must have a minimum temperature T. In addition to this threshold temperature there is an additional threshold condition known as the Lawson criterion. This criterion requires that the plasma have a certain density and a certain interaction time or confinement time whose product defines the fusion reaction threshold. To the extent that the density (η) is large, the interaction or confinement time (τ) can be small, or, to the extent that the interaction time is large, the density can be small. The criterion is conceptually logical: It says that a sufficient number of particles must be present for long enough in an interacting or confinement region for the fusion process to occur. This is summzrized in Figure 4.

If we now look at the magnetic bottle which holds the plasma together to form a confinement or interaction region, we see that the physical size of that bottle is determined by the strength of the confining magnetic field (B) which must overcome or control the kinetic or heat energy invested in the plasma ion particles. Now, the energy contained in a unit volume of the plasma is determined by the number of particles in that unit volume which is just the

FIGURE 4

BASIC FUSION PHYSICS CONSTRAINTS

DT FUSION CAN BE ACHIEVED ONLY WHEN THE DT PLASMA IS CONFINED LONG ENOUGH AT HIGH ENOUGH DENSITY AND TEMPERATURE TO YIELD MORE ENERGY OUT THAN IN

THE PRODUCT OF CONFINEMENT TIME (τ) AND DENSITY (η) MUST BE LARGER THAN A VALUE SET BY PHYSICS LIMITS TO YIELD USEFUL POWER. THIS IS CALLED THE "LAWSON CRITERION" (K_1)

$$\eta\tau > K_1$$

plasma density. This means that if we wish the plasma to be more dense then we must increase the strength of the magnetic field.

However, if we are limited by the strength of the magnetic field which we can produce, then that magnetic field limitation defines the maximum plasma density at which we can operate. This maximum attainable density then defines the needed confinement time that must be achieved if the Lawson criterion is to be satisfied.

This confinement time, the time an ion spends in the interaction region, can be made larger simply by making the interactive region larger.

Where does this all lead us? As shown in Figure 5, it says quite simply that if you choose to use superconducting magnets, which have limited field strength capabilities to confine the plasma, then the plasma region must be very large. This is what the mainline fusion program has done. There is a minimum size for a superconducting machine and it is very large and therefore very expensive to build, and very expensive and time-consuming to develop.

The alternative is to use copper magnets whose field strengths can be made very large, and hence the plasma made very small but dense. Consequently, with a high strength small magnet the capital expense is made small, and the development time and costs are made small. This is what our RIGGATRON fusion program is all about.

In summary, both we and the Department of Energy recognize that the tokamak is the only known and proven successful scheme for fusion plasma confinement. Although there may be valid reasons for

FIGURE 5

BASIC FUSION PHYSICS CONSTRAINTS

WITH SUPERCONDUCTING MAGNETS THE ATTAINABLE FIELDS (B) ARE TOO SMALL TO ALLOW SMALL MACHINES. THUS,

SUPERCONDUCTING MAGNET MACHINES MUST BE LARGE

LARGE SIZE MEANS LARGE COSTS AND LONG DEVELOPMENT TIMES THIS IS THE SO-CALLED "MAINLINE" APPROACH

WITH COPPER MAGNETS, HIGH FIELDS CAN BE REACHED WHICH ALLOW SMALL SIZE, THUS

COPPER MAGNET MACHINES CAN BE SMALL

THIS IS THE INESCO/RIGGATRON TOKAMAK APPROACH developing the large low density devices, we feel that small size is required for low-cost, short development time, and practical plant applications.

3. The RIGGATRON Tokamak Development Program

The fundamental engineering innovation which we have introduced in fusion reactor design was nothing more or less than the use of water-cooled copper alloy conductors for high magnetic fields in a tokamak of very small size.

Our technical approach as shown in Figure 6 is to design machines to their engineering limits, and then experimentally test to those limits, to determine with precision the actual regime for operational success. This is possible only because the machines, themselves, are relatively inexpensive and easily replicated.* With this approach it is possible to conduct our development program in the classically successful mode of learning-by-doing, by successive testing of many machines.

Figure 7 illustrates the features of RIGGATRON small size in comparison with the "mainline" concepts for superconducting magnet machines to utilize shields and lithium blankets (required by breeding T) inside the main toroidal magnets. This limits useful field strength and requires large and complex machines. In contrast, the copper magnets of the RIGGATRON are placed directly around the fusion plamsa region, allowing the required lithium blanket to be placed external to the fusion unit, and physically separate from it.

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*Less than 10% of our total program cost is due to the cost of the five machines planned for demonstration testing.

FIGURE 6

INESCO TECHNICAL APPROACH

DEVELOP VERY SMALL TOKAMAK FUSION MACHINES

USE WATER-COOLED, HIGH STRENGTH, COPPER ALLOY MAGNETS

USE PLASMA OHMIC HEATING AS PRINCIPAL IGNITION MEANS

DESIGN DOMINATED BY ENGINEERING LIMITS

DRIVE THESE UNITS VERY HARD, WITH LIMITED LIFE, BUT AT LOW UNIT COST/POWER PRODUCED

PERFORMANCE DETERMINED BY SUCCESSIVE TESTING OF MULTIPLE INEXPENSIVE MACHINES

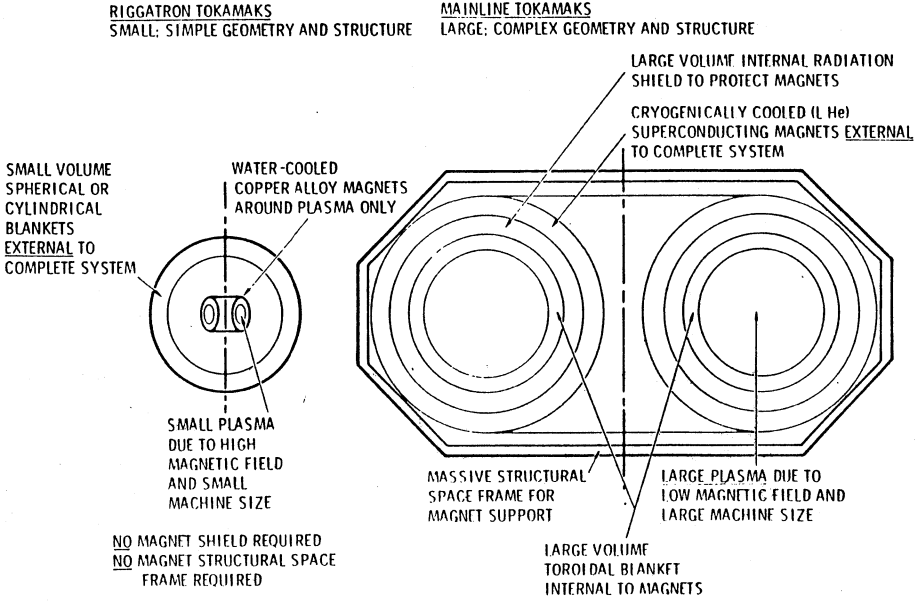
CONSTRUCT POWER PLANTS FROM MODULAR ARRAYS OF FPC UNITS

DISPOSE/RECYCLE USED UNITS

LOW UNIT COST YIELDS ATTRACTIVE ECONOMICS FOR CENTRAL STATION POWER PLANTS

FIGURE 7

COMPARISON OF GEOMETRIC AND STRUCTURAL CHARACTERISTICS OF TOKAMAK FUSION OPTIONS



The blanket remains - like a lampshade - while the internal fusion "light bulb" is removed and replaced when it wears out.

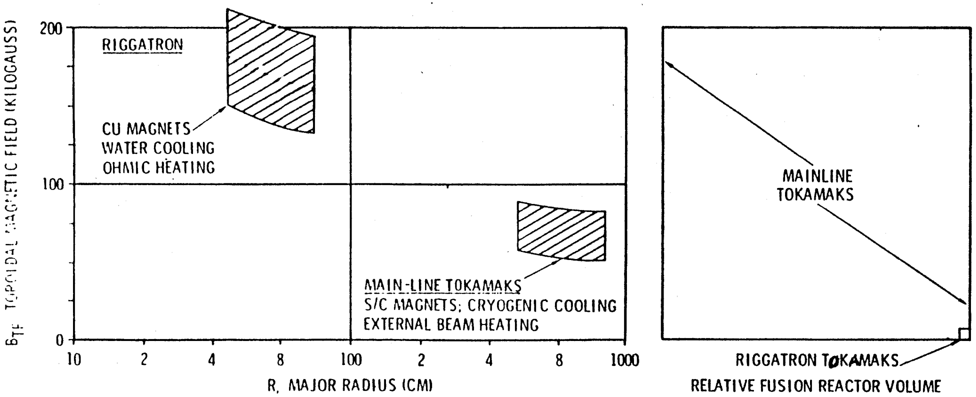
As Figure 8 indicates the "design space" for RIGGATRON machines is at a higher field but at a tenth smaller size than is possible for superconducting "mainline" tokamaks. Note that the RIGGATRON is only a few feet in radius. This ten-fold reduction in size is a thousand-fold reduction in plasma volume. Since machine cost varies roughly as the volume,and since copper alloys cost less than superconductors, RIGGATRON machines inherently will cost 1000 to 4000 times less than their larger superconducting tokamak cousins.

What does our test unit look like? Figure 9 shows an artists sketch of a canned "light bulb-like" RIGGATRON tokamak. Here we see that the entire fusion unit is enclosed in a vacuum envelope, much as a filiment in a conventional light bulb. Such a unit assembly will be placed inside a shielded room whose walls are lithium tanks to serve as the T breeding blanket, as indicated in Figure 10. Each such assembly (blanket plus RIGGATRON unit) would be capable of operating at 1000-1200 megawatts of fusion-generated power. Heat from the blanket and fusion unit would then be used to generate 1000°F steam which can be converted to 250-350 megawatts of electric power output. The steam output can, of course, be used for other purposes (e.g., as process heat at 1000°F).

Larger power plants can be made from a modular array of such systems. In <u>any</u> RIGGATRON plant, a high utilization factor can be obtained by providing one or two "extra" blanket/RIGGATRON units in the plant. When one unit is shut down for fusion core unit replacement, the "spare" unit is turned on so that there is no plant down

FIGURE 8

TECH APPROACHES TO TOKAMAK REACTORS RIGGATRON & MAINLINE TOKAMAKS



ALLOWABLE REGIMES FOR TOKAMAK OPERATION

RELATIVE FUSION REACTOR VOLUME
COST ~ VOLUME

COMPARISON OF TECHNOLOGICAL FEATURES OF TOKAMAK FUSION OPTIONS

FIGURE 9

RIGGATRON^{tm} FUSION POWER CORE

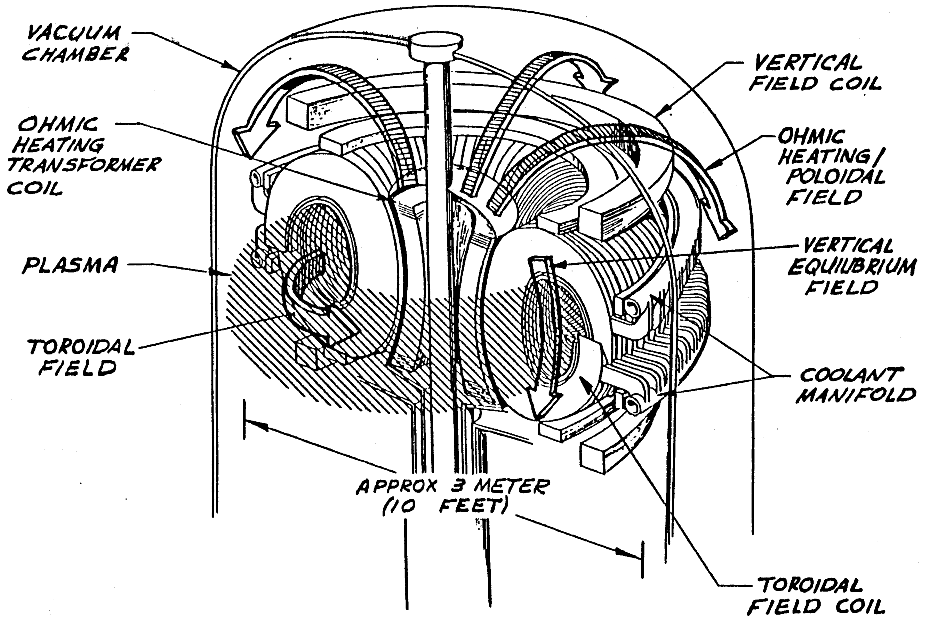
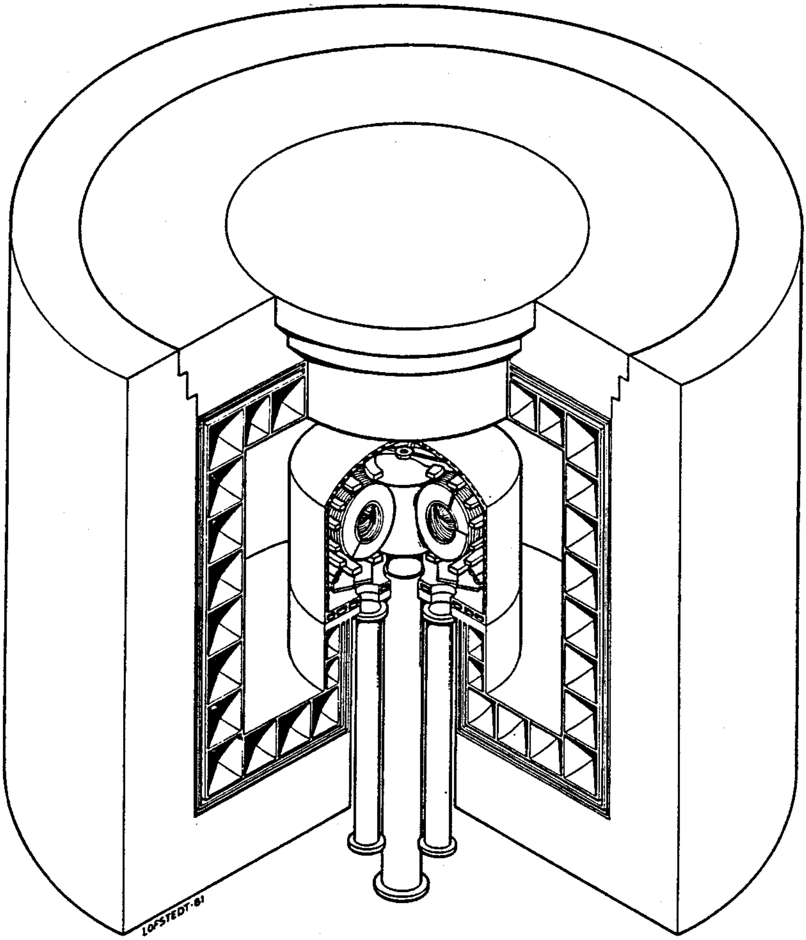


FIGURE 10

RIGGATRON^{tm} FUSION POWER CORE IN BLANKET AND SHIELD CELL



time due to unit failure or replacement. Replacement time can be less than 8 hours, because the unit is a canned assembly and is easily disconnected, removed, and replaced. This "plug in" quick-disconnect approach was pioneered and proven in the national nuclear rocket (Rova) program over 20 years ago.

The test site required for our five demonstration test machines requires only about 10 acres of land for site safety and compliance with all regulatory and hazard requirements. Very little tritium is on-site at any one time because the system is so small. We plan to carry out tests over a period of 18-24 months with parallel test cells. Initial tests will ve made with normal hydrogen, later with deuterium alone, and finally - for ignition/burn experiment - with D&T mixtures. These final tests will operate at full, steady, controlled burn for periods of several seconds; long compared to the energy confinement time of the plasma in the machines. Such tests will prove all the physics and engineering features needed for prototype commercial units except the maximum attainable length of the fuel burn cycle and the radiation damage lifetime of the coil materials.

Available data on copper alloys suggests that the lifetime may be approximately 1000 hours at operating conditions appropriate to commercial plant use. Our analysis also suggests that burn cycle length may be as large as several minutes before machine pump down is required. Inter-pulse "off" time could be less than three seconds, due to the small volume requiring pumping. This test program is summarized briefly in Figure 11.

FIGURE 11

, RIGGATRON TOKAMAK EXPERIMENTAL PROGRAM

DESIGN, BUILD AND TEST FIVE RIGGATRON TOKAMAKS IN 1985/86

INITIAL TESTS WITH H, NEXT WITH D, LAST WITH DT

CONTROLLED IGNITION AND STABLE BURN FOR SEVERAL SECONDS

WILL PROVE ALL PHYSICS AND ENGINEERING FEATURES FOR REACTOR OPERATION EXCEPT BURN CYCLE LENGTH AND MATERIALS LIFETIME

4. RIGGATRON Unit Energy Plant Applications and Economic Prospects

Over the past 3½ years we have made studies of the potential application of RIGGATRON tokamaks to a wide variety of energy plant and process steam uses. Results of these studies show that the return-on-gross-assets (ROGA) is 100%-150% per year for the range of plants which can benefit from RIGGATRON fusion. These astonishingly large values (by comparison with conventional energy plant economics) are a natural result of two features inherent to the RIGGATRON tokamak concept and to the means for its plant employment.

First and foremost of these is that the RIGGATRON device acts, economically, like a consumable fuel in an energy plant. It is not a long-lived, fixed depreciable asset but is used up over a period of about 1000 hours*. Its cost thus appears as an operating cost (analogous to fuel cost in oil-burning plants) rather than as a capital cost. Its attractiveness stems from the fact that the unit energy cost of RIGGATRON units or "fuel" calculate to be only about $.50/bbl. of oil equivalent, or $0.14 MCF for natural gas. Compare these to today's market prices at approximately $30/bbl. of oil and $5/MCF gas. I grant you that the RIGGATRON, today, is only a "paper reactor". However, even with the inevitable cost escalations which invariably occur as concept becomes reality, the cost advantage "cushion" is still extraordinarily attractive.

If successful, the deployment of RIGGATRON-driven plants in the early 1990's would have a significant effect on world market prices of oil and natural gas and, to a lesser degree, coal.

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*1000 hours in pure fusion plants; 5000+ hours in "hybrid" fusion-driven plants.

The second feature of RIGGATRON fusion unit applications to energy plants which augers for dramatically lower costs, or for large profit margins, is that the capital equipment associated with the fusion energy source portion of the plant is only a small fraction of the total plant cost. Our studies show that the capital cost of a RIGGATRON fusion-electric power plant is split approximately 90% to conventional equipment (for thermal conversion to electricity) and to balance of plant, and only 10% (or less) to the relatively small scale RIGGATRON-unit-related equipment. The RIGGATRON steam/electric plant costs will be predominantly in proven, standard plant equipment. This means that RIGGATRON units can be retro-fitted to many current oil and gas burning energy plants, at acceptably low costs for such conversion. Inexpensive retro-fitting for RIGGATRON fusion could be a cheap and fast way to remove oil and gas from our country's power plants. This does not mean that new plants will not be needed; quite the contrary. RIGGATRON retro-fitting will lead to lower energy costs but at the price of a 20% reduction in plant output. This, coupled with the continued growth which our society requires, leads to a need for new plant construction. The total capital cost of new plants constructed for RIGGATRON use will be significantly less than for conventional nuclear reactor plants, and will be about the same as current fossil-fuel-fired plants, but the on-going "fuel" costs will be virtually zero.

We estimate that the cost of steam from RIGGATRON steam plants to be about $1.60/MBtu (1980 $) at 1000°F, or $1.20/MBtu at 600°F, considerably less than the current cost of oil or of natural gas at the wellhead. Steam at these prices can displace most other sources of steam for process heat, in a wide variety of industrial applications. Since over 90% of energy in the U.S. is used for thermal non-electric applications, the RIGGATRON can have a profound impact on these industrial sectors as well as on the electric power utility business.

As one example of this impact we have considered the production of ethyl alcohol. Ethyl alcohol, or ethanol, is an excellent fuel capable of powering automobiles and jet aircraft, and for other heating purposes. Today it is too expensive to compete with gasoline except in a price-regulated economy, such as that in Brazil where one million cars currently are running on ethanol. Ethanol is made by the fermentation and subsequent distillation of biomass feedstocks, at medium temperatures (up to 500°F). Alcohol for beverages is made from corn, potatoes, sugar beets, rye, and sugar cane, among other feedstocks. The sugar can process involves crushing and squeezing the cane to extract the juice, and use of the crushed pith and husk to burn as fuel to heat the fermentation and distillation process for conversion of the juice to ethanol.

With an external energy source such as fusion, via the RIGGATRON unit, the pith also may be fermented, the husk removed more carefully for wood byproduct use, and the resultant cost of anhydrous fuel-grade ethanol is found to be about $0.35/gallon (current ethanol costs for conventional plants are over $1.60/gallon). At this cost ethanol can compete well in the market place with gasoline for automotive use. One RIGGATRON-unit-driven plant can produce about 5000 tons/day of ethanol from a high yield cane field about 18 miles square. The cost of such a plant is estimated to be $400-500M (1980 $).

Two hundred such plants could fuel all the cars in the United States, and we could stop using gasoline, to the benefit of our air pollution problems as well as to our freedom from fossil fuel dependence. Sugar cane is a solar-energy-grown crop, completely renewable, and the fusion fuel which drives the plant is inexhaustible. Some years ago the American Petroleum Institute (API) commissioned a study which showed that the energy required to produce alcohol exceeded the energy available from the alcohol thus produced, and concluded that alcohol production was <u>not</u> a fruitful way to attempt to solve the nation's oil problem. This conclusion is valid <u>only</u> for alcohol plants which run on conventional fuels; it is <u>not</u> true for RIGGATRON fusion unit plants in which the energy required is produced at extremely low cost. The point here is that the use of RIGGATRON fusion-produced steam yields low-cost alcohol both because the required process heat (steam) is very cheap <u>and</u> because the operating characteristics of the plant are changed.

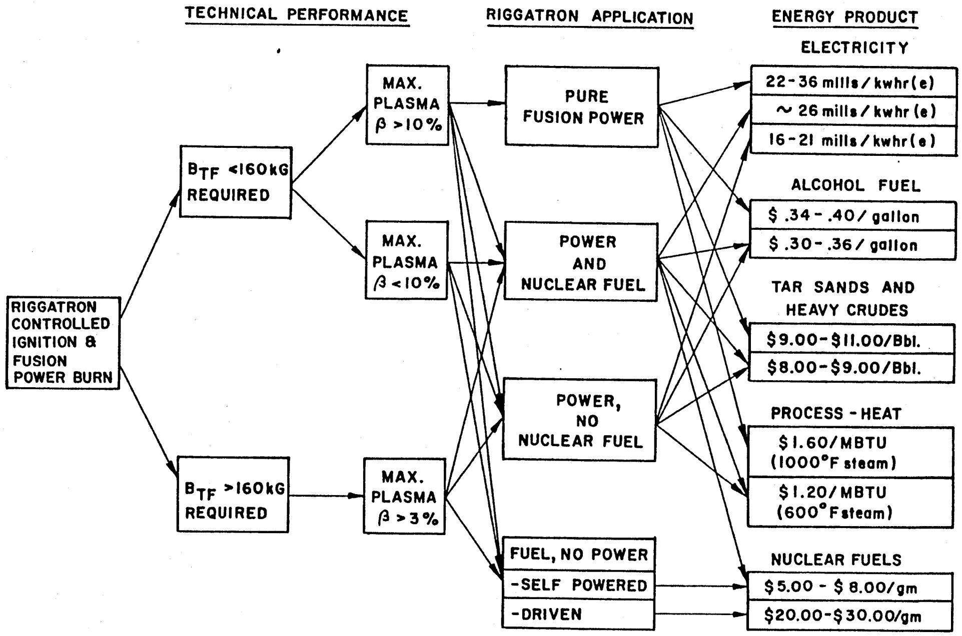
Other plant applications which appear of global significance are the production of agricultural water from sea water at $1.00/thousand gallons at the bottom end of a steam-electric plant with electricity produced at ca. 30 mills/kw_ehr at the busbar, steam stimulation of "heavy" oil wells and oil extraction from in-situ tar sands with savings of $3-6/bbl produced, generation of safe reactor-grade nuclear fuel at 1/3 to 1/2 of current world market prices, and the effective "burn up" of radioactive wastes from the world's nuclear reactors, with subsequent storage times reduced to less than one hundred years, rather than tens of thousands of years as at present.

Figure 12 schematically shows our total program from demonstration-experiment to commercial plant uses, by all of the possible technical routes. It is important to note that nearly all of the wide array of commercial plant applications can be achived no matter what levels of plasma performance are found to work successfully, provided only that controlled ignition and burn is achieved in the RIGGATRON demonstration experiments planned in our program.

All of these potential commercial applications, and others not mentioned here, come about because the RIGGATRON unit is small and cheap, and inherently can operate in an inexpensive manner in nearly all plant applications. All can be achieved if the RIGGATRON device successfully reaches ignition and controlled burn. RIGGATRON fusion energy can renew the United States' great strength, and can re-vitalize the world, giving mankind a new breadth of life and a new vision for our future.

Finally, Madam Chairman, you and your committee may well ask, "Why is Dr. Bussard telling us all this? We do not have government funding, and we are not seeking government funding. However, I believe it is important that you should know what we are doing and what we plan to do. Such knowledge should be helpful to you, as you plan the directions of the country's federally-financed energy developments. And it should be beneficial to us, as you act on that

FIGURE 12



RIGGATRON TOKAMAK TECHNICAL PERFORMANCE AND TYPICAL ENERGY APPLICATIONS knowledge, to help prepare the way for the most rapid and efficient industrial and commercial acceptance of our machines, as these reach commercial product status in the late 1980's.

If our machines work, we will have successfully controlled the fusion thermonuclear reaction at extremely high power levels, of hundreds of megawatts, in a small device which is easy to build and deploy commercially. In the economic sphere, this event will have a more profound and pervasive impact than the first successful <u>un</u>controlled thermonuclear reaction had some 30 years ago in another sphere. This is not a 30-year promise. This event will happen within 5-6 years, and you should know of our program.

Madam Chairman, we are here today to represent our corporation's best interests. We are in this program for three reasons important to us. One is just the sheer joy of winning, with a better technological idea than all the others. Another is the desire to make large profits from our success, without any federal subsidy, in a realistic time frame. This, we believe, is the essence of private enterprise in a capitalistic society. And finally, we believe that this machine, properly employed, can forever change the world for the better and alter the balance of industrial and economic power on the planet.
If we are correct, and the RIGGATRON systems perform as analyzed, the very nature of the industrialized world will be shifted dramatically from fossil fuels to fusion, not because the RIGGATRON device is a clever engineering achievement, but because it offers an economic miracle, a return to cheap energy - forever - in a framework which fits nearly all current industrial infrastructures. There is no OPEC to control the fusion fuels in the oceans, lakes, and rivers of the world.

If this works it will work within the next 5 to 6 years, and the national policy issues which this will generate must be discussed in this time frame if we are to exploit it properly. We do not have the liesure which a 30-year scientific program implies.

Madam Chairman, two years ago, this committee and this Congress, under Chairman McCormack's leadership, passed the Fusion Engineering Act of 1980. The objective of the McCormack Fusion Act was to "make fusion happen". We believe that INESCO, today, represents the best chance for achieving that goal. If INESCO is successful as we anticipate, then private enterprise, building upon the basic research investment made by the government, will have again demonstrated its ability to efficiently solve technology problems and make a profit.

Thank you.

Mr. YOUNG. At this time I would like to recognize Mr. Lowery, who would, I am sure, want to welcome some of the people from southern California who were such great hosts to us in January. Mr. Lowery?

Mr. LOWERY. Thank you, Mr. Chairman.

I hope the panelists will excuse my bouncing in and out of the committee meeting and try to understand that I have three committee meetings going simultaneously right now, so please understand.

I do want to welcome all of the panelists, this panel and the following panel. I am looking forward to all the testimony and enjoyed the testimony that we just heard.

I just made a couple of notes during Tihiro's testimony, on how pessimists talk about fusion being 50 to 70 years out. I am convinced that if we make a commitment as a nation, it will certainly be here within 25 years. I see such parallels to where we were in the fifties when men and women of vision would suggest that we could put a man on the Moon within 20 years, and they were probably looked upon as if they were smoking some kind of exotic rope.

But it was possible. We did make that commitment. I think of where we are today in the depletion of the traditional fuels we have used, the fossil fuels, and where we are going to be as we move into the next century, which is really just around the corner.

I have concerns about air quality. We are just now beginning to get into such subjects as acid rain. Look at air pollution and what we are doing to foul the air in our urban areas from traditional fuel sources of coal and oil.

Admittedly, I am not a scientist and I don't have the technical background in engineering or in scientific efforts, but I think I have a general understanding of the concepts and where America needs to be.

I guess I view the fusion program as possibly being, just possibly being, the most important body of research that we are doing in the country today in terms of maintaining in the next century the quality of life that we as Americans now know and the high standard of living we have had.

I guess there are few areas where we can have such a symbiotic relationship between environmental and economic concerns, but it is all brought together in fusion, not to mention a sense of energy self-sufficiency in the next century unlike we are able to enjoy currently, and all of the foreign policy applications that that has.

I have analogized the space program. It just strikes me where we are today is a matter of making the commitment, of making it happen, and doing it with various technologies, some of which will work, some of which will not, but doing it in a concurrent manner rather than stretching it out. Because if we had taken that same approach through NASA in the sixties, we would not have put a man on the Moon in the early seventies; we would still be fooling around and thinking about it without having various courses that we are taking to arrive at the goal.

We do not know which path is going to pay off for us, but we need to fund them all. That, Mr. Chairman, is kind of an overview. It just gives you one little old Congressman's overview of where I think we are and where I think we could go.

Mr. YOUNG. Thank you very much, Mr. Lowery.

We will now proceed with questions of the panel. The first question I would like to ask Mr. Matson. How can the producer and user communities of future fusion devices play a role in the program at this early stage of development?

Mr. MATSON. Mr. Chairman, I might give you one perspective from the utility point of view. We at Public Service Electric & Gas in New Jersey have been involved in the fusion program through work at and with Princeton University since about 1957. We have a research and development subsidiary, and we have taken a rather strong role, you might say, for a utility in fusion development. We have actively sought out funded contracts from bodies such as the DOE and Electric Power Research Institute. We have been actively trying to inject into those studies and ongoing programs the utility or end user perspective of what is needed for a program to succeed in that it be used or usable by a utility, a prospective end user.

In that respect, I think it is important that people such as utilities, the end user, are involved to look at what the prospective siting and safety issues are. I think we have learned a great deal by our very direct involvement in the fission program, and I think we are able to bring that over into the fusion program and, hopefully, add that perspective.

Mr. YOUNG. Thank you very much.

The figures that were cited by Dr. Repici are pretty interesting when we take a look at where we are. We have two homes, one in St. Louis with natural gas and one up here with electric, and those cost estimates for fusion power look pretty good.

Dr. Ohkawa, I understand that General Atomic is developing, along with Phillips Petroleum, the OHTE device, which has been described to me as a combination of reversed-field-pinch, and a stellarator device. Is this a correct description, and could you please describe this device in some detail to the subcommittee?

Dr. OHKAWA. The OHTE concept is a new type of magnetic bottle, toroidal variety but high beta. It uses, as you said, the pinch-like plasma. Also, it uses helical winding, which is somewhat like stellarators but with confinement physics. It is a new variety of magnetic bottle.

We have built an experimental device, and we have a 3-year research agreement with Phillips Petroleum. We have gone through the first year of experimentation. So far, the results obtained look very encouraging.

Mr. YOUNG. How much private capital has gone into the development of the OHTE device? How much more may be required to assess whether or not the device will work?

Dr. OHKAWA. The first phase is on the order of $20 million investment, $10 million from General Atomic and $10 million from Phillips Petroleum, to assess the physics of this new device in 3 years. With success, we are planning to have a second phase which might involve a much larger investment.

Mr. YOUNG. Does General Atomic's private investment in the OHTE mean that General Atomic does not believe that the tokamak and mirror devices being developed by DOE will evolve into viable commercial reactors?

Dr. OHKAWA. No, this is not mutually exclusive. It is the research and development portfolio, just like a financial investment portfolio. You have a spectrum of investments in several areas. With the high-risk, high-reward situation, you invest a smaller sum compared to the low-risk approach.

So we consider we are doing a balanced R&D portfolio.

Mr. YOUNG. Thank you very much.

Dr. Repici, in contrast to the large devices being developed by DOE which require auxiliary heating of some sort, the RIGGATRON is expected to achieve ignition by ohmic heating alone. Can you explain in greater detail why you think you can generate high enough temperatures in this manner?

Dr. REPICI. Mr. Chairman, the temperatures that you can generate are a function of the physics and the power supply and the engineering.

The contribution that Dr. Coppi made to the high-density concept, which evolved into the RIGGATRON concept, was that he recognized, that when one gets near ignition, by ohmic heating you generate significant alpha particles. He coined the phrase—I believe he is responsible for the phrase, at least—of alpha particle "bootstrapping." This is the effect by which the deposition of energy into the plasma by the alpha particles which you produce, further raises the temperature of the plasma and lets you go the extra distance into a fully ignited region.

The other method of heating besides ohmic heating—it is a small device, so it is not amenable to neutral beam heating, is RF heating which is an appropriate mechanism for heating a small device. So we and our technical people feel very confident that the combination of ohmic heating and, if necessary, RF heating will put us into the ignition region.

Mr. YOUNG. To Dr. Ohkawa and to Dr. Repici, this will be my final question. Are there similarities between your in-house corporate programs in fusion and the DOE program which can help advance the generic fusion effort?

Dr. Ohkawa, would you want to take that first?

Dr. OHKAWA. I have to correct the impression on the OHTE. This is not entirely private. We do use some Government equipment. In that sense, it is partly Government, in a small part.

Yes, I admire the entrepreneurship of INESCO, that they are trying to do the high-risk development with private funds. The whole spectrum of the approach should be taken. When it comes to Government versus private, the long-range program should be, of course, financed by Government. But if, as the INESCO approach indicates, there is a very high risk and high reward in the short term, there is a possibility the private funds might support it.

Mr. YOUNG. Dr. Repici?

Dr. REPICI. I have to point out the similarities which both Tihiro and I have are that both of our devices are relatively small, and I think that fact permits a nearer term device; also, if it is going to be nearer term, it must be something from which we can achieve some economic gain, some potential.

With respect to your question, Mr. Chairman, about the relationship between private industry and the DOE, we are not a DOE contractor. In fact, our backers are concerned that we should have any contract whatsoever so as not to endanger our patent position. So we have no Government support whatever.

Now, the people that we have, in all candor, come from the U.S. program, talk to their colleagues, and there is a certain low level cooperation. Contrary to the comments made yesterday, there is very little that the Department is giving to us, even though they would like to give us more. When the AEC was set up there was never any thought given to the possibility of private industry doing fusion on its own. Despite the best efforts of the OER management, we are really not able to have access, free access to computer time, to machines, to the diagnostics, simply because we are not a DOE contractor. If I had a $10,000 contract with the DOE, we could cure it, but I cannot. That is something that we are working with the Department on.

Mr. YOUNG. Thank you very much. Mrs. Bouquard?

Mrs. BOUQUARD. Thank you very much, Mr. Young.

I certainly appreciate our panel today. They have certainly given us some very valuable insight.

Mr. Matson, I appreciate your statement that we should move on to the goal of commercialization. Without this goal in front of us, and proceeding in piecemeal fashion, I am afraid that we are not really being too realistic with what we expect from our fusion program as directed by the Congress under the leadership of Congressman McCormack.

Also, I share with you your doubts of the wisdom of delaying the alternative confinement systems relative to our tokamak developments.

Last year, a panel of EPRI experts concluded that the fission-fusion hybrids appeared to be the most practical first application of fusion. They contended also that the hybrids offered a technology that is easier and potentially more competitive new energy option than fusion electricity generation.

Would you concur with that assessment?

Mr. MATSON. We at Public Service, in particular, are rather positive on the fusion-fission hybrid. We do feel it is a very logical step that could be taken to get a practical earlier use for fusion than the longer term fusion stand-alone electricity producer.

It is, we feel, a very positive way to use the neutrons and in fact to have a machine, the hybrid machine, that could produce energy and also be a net producer of fuel and that could support other fission reactors that the utilities have and will have in the future.

So we are positive as far as the fusion-fission hybrid. I might add that EPRI is taking a more positive view, I believe, on the fusion-fission hybrid, also.

Mrs. BOUQUARD. So you feel it will be a more competitive new energy option?

Mr. MATSON. Well, I think it has a possibility to be. There are those who look upon the hybrid as a competitor to, for instance, a breeder. We don't look at it so much that way, from the utility perspective. I think we are looking at it more as a means to enhance our capability to generate additional fuel for fission reactors and also a positive way to introduce the fusion concept at an earlier time.

Mrs. BOUQUARD. What do you envision as the first commercially viable application of fusion energy?

Mr. MATSON. That is a good question. I think it depends upon what may be the Nation's biggest need. The way we are going with the fission program it possibly will not be fissionable fuel. There is a possibility, of course, that if we are severely impacted by, say, by the Middle East on liquid fuels, petroleum, the application, we may have to go to, and hopefully not in a crash kind of fashion, but we may have to go toward hydrogen, producing hydrogen and then, downstream, liquid fuels.

In the utilities right now, we are still looking upon it as an electricity producer, electric energy.

Mrs. BOUQUARD. Let's say we are able to reasonably fund our fusion programs. What is your guesstimate of when we can make fusion viable, commercially viable, as a source of energy?

Mr. MATSON. Commercially viable. Now, I think maybe we have to define the concept of commercially viable.

Mrs. BOUQUARD. When can it enter the marketplace competitively?

Mr. MATSON. Entering the marketplace competitively; I am hopeful that that would be within the first quarter of the next century, a real commercially competitive kind of objective.

That would require, I think, looking at and meeting the goals in the intent of the Fusion Energy Act, which as I say, are viable. We think those are very viable goals and objectives. There is some perturbation, obviously, in the funding, and I think we have to roll with those punches, But those goals and objectives, I think, still stand out in our minds.

Mrs. BOUQUARD. That is why I say if we had a reasonable level of funding.

Mr. MATSON. Right. With a reasonable level of funding, I think we are in that scenario of, hopefully, the first quarter of the next century.

Mrs. BOUQUARD. Thank you very much.

Dr. Ohkawa or Dr. Gilleland, to what extent does the modification of the Doublet III vacuum vessel depend upon negotiation of the extension of the present United States-Japan agreement?

Dr. OHKAWA. When we were in Japan in February of this year, they were very interested in participating in relation to the $70 million 5-year agreement between DOE and JAERI. They have not gone through the formal government channels, but they are trying to—not trying to, they will—put on some funding in the next Japanese fiscal budget.

Mrs. BOUQUARD. When do you expect the modified device to be ready for research operations?

Dr. GILLELAND. We would install it in late fiscal 1984. It could be done earlier, but given the funding profiles as they are set now, I would say late fiscal 1984. It would take us about a year to be on the air again doing research, so it would be 1985.

Mrs. BOUQUARD. Do you think in any way the results obtained from the modified device would be preempted by the JET device?

Dr. GILLELAND. Not really. If we are funded to go according to that schedule, then because we are using hardware which has been proven, the heating systems, diagnostic systems, and we are using a basically proven device, the actual physics will probably come out of Doublet III first.

Also, there are complementary aspects to the two projects, particularly with regard to impurity control. So, no, I do not think we will be.

Mrs. BOUQUARD. What are the parallels with the JET device?

Dr. GILLELAND. They are both large plasmas. They both have large D-shapes to the plasma. In fact, I think they have been our biggest fans because we have done a great deal to prove their design.

Again, we are hoping, however, that we will be—I don't know whether they will be our fans or not—but we will be a few years earlier in the actual research in the big D, which of course is a machine which has a plasma which is about halfway between the size of the current machines and the actual reactor.

I define the actual reactor as those designs which are represented by FED or the Japanese FER. In a recent survey there was a discussion of the most important machines in the eighties to the United States and Japanese programs, and certainly in the large tokamak arena, the United States and Japan both stated that the TFTR, JT-60, and Doublet III were all very important machines to the program.

Do you want to add anything to that, Tihiro?

Dr. OHKAWA. No, thank you.

Mrs. BOUQUARD. Thank you very much.

Dr. Repici, in Dr. Bussard's prepared statement, he says—and this is kind of refreshing, something we don't hear very much—that you really don't want any Government funding. If this is the case, how can we help you here? [Laughter.]

Dr. REPICI. Thank you, Madam Chairman.

Mrs. BOUQUARD. Then why are you here?

Dr. REPICI. I tried to address that. The one reason I am here is to say that if we are successful, then you have decisions to make, and we relate to those decisions. I think, because I am an outsider with no Federal money and because of my personal experience at the OMB with the DOE fusion program, that I can say things that are positive about the U.S. fusion program and the goals of this act without bias or alternative agenda. There is a certain amount of credibility, quite candidly, attached to my statements.

There is also a self-serving role as to why I am here. There are certain policies within the U.S. Government that simply do not let private companies have access to taxpayer paid information. I have raised that issue with the staff, and it is not necessary to go into detail here, but you have funded the U.S. fusion program, and I think the goal of the program is not that we have DOE fusion but that we have a fusion success and somehow that that technology be made available for all of us.

Now, both Tihiro and I have a private enterprise dimension to all of this, he, in part of his project and for me in my entire project. If we are going to do well for America, I think, quite candidly, the investment that the taxpayers have made should be open to any credible American company.

But that is an issue that takes time to work. That is not done by fiat, and I recognize that.

Mrs. BOUQUARD. Well, if you are successful within the next 5 to 6 years, what impact do you see that this will have on the U.S. fusion program?

Dr. REPICI. Quite a substantial impact. What I am hoping is that we do not make the mistake that we made when the fission program was successful. Well, my heavens, if private industry is doing it now, we can shut down the AEC's activities in the fission work, except for the Admiral's work.

The complexion of the DOE fusion program will change. It is not clear that a tokamak would be the best mechanism to generate fusion power. We believe it is the one that will happen soonest in the parameters that are achievable with today's technology.

The complexion of the DOE would change and, hopefully, it would exploit the success achieved by INESCO and would also look into some of the alternatives that might make better products in the longer run.

There is a key element, though, Mrs. Bouquard, and that is one of the other reasons for my being here. The U.S. program should recognize, and I think the participants do, that we exist and we are part of the U.S. program and that it truly is a U.S. program, not just a DOE program.

Mrs. BOUQUARD. How would you sum up the way that we could best benefit from your success?

Dr. REPICI. Well, I think it is very clear how you could benefit from the success.

Mrs. BOUQUARD. Our overall program.

Dr. REPICI. There is a tremendous amount of work that has yet to be done on blankets. The program I outlined to you today, Mrs. Bouquard, was essentially to generate neutrons. We have not touched yet how we are going to use those neutrons. They have to be thermalized in some blanket. The power has to be extracted.

You asked some questions as to whether hybrid is best. I have alluded to alcohol production, to synthetic fuel production, to cheap steam. Those are all technology issues which fit very nicely into the making-fusion-happen aspect of the Fusion Engineering Act. I see quite a role for the U.S. program.

Mrs. BOUQUARD. Thank you very much.

Thank you, Mr. Chairman.

Mr. YOUNG. Mr. Lowery?

Mr. LOWERY. Thank you, Mr. Chairman.

Dr. Ohkawa, the Magnetic Fusion Energy Engineering Act of 1980 calls for the operation of a magnetic fusion engineering device by not later than 1990 and the operation of a demonstration plant by the year 2000. The level of funding for magnetic fusion in 1982 did not have the 25-percent increase as outlined in Public Law 96-386. The increase, in reality, was less than 5 percent. The $444 million budgeted by the administration for magnetic fusion for fiscal year 1983 is 11 percent below the 1982 dollars, and that is in constant dollars.

Now, the cutback has put the Elmo Bumpy Torus-P program in jeopardy and could cause a stretchout of the MFTF-B program. My question to you is, Do you feel that it is appropriate or prudent to, so to speak, put all our eggs in one basket with the funding emphasis on the development of tokamak, and do you feel that the development of this technology alone will attain our goals that we have set in the 1980 legislation?

Dr. OHKAWA. Well, as I said in my testimony, the program is a well balanced one. We are not putting all our eggs in one tokamak basket. You have a mirror program, and there is the bumpy torus operating. There are pinches at Los Alamos. We have a balanced program.

To get back to the accelerated fusion development program, we have to have about, roughly speaking, a 20- to 25-percent a year increase in budget.

Across the ocean, the Japanese Government is in the same situation. They too have a tight budget. Despite that, the Japanese program fusion budget increased 15 to 20 percent this fiscal year, starting in April. So that shows the Japanese are very serious about the development of fusion. They are planning to have a fusion experimental reactor in the 1992-93 period of time.

Many of us were there in February, including DOE people. I do not like to hear the joke that probably we will end up importing fusion reactors from Japan. So I strongly urge that the fusion program be expanded so that we can catch up lost ground in the past year or two.

Mr. LOWERY. And meet our goals.

Dr. Ohkawa, will it be possible for the modified Doublet III device to achieve ignition with deuterium-tritium plasma?

Dr. OHKAWA. No. The Doublet III is designed to use only ordinary hydrogen to save cost and time. It will simulate the conditions with ordinary hydrogen, but we have no plan for putting in deuterium and tritium. That experiment will be done in TFTR.

Mr. LOWERY. So you are not anticipating that it will serve the role of a miniature fusion engineering device? You do not see that happening with Doublet III?

Dr. OHKAWA. No.

Mr. LOWERY. Could it fill that role?

Dr. OHKAWA. Yes. This will simulate the core of a fusion engineering device so that we can learn how to control, how to handle these hot plasmas which are carrying the large plasma current. So that will be sort of a simulator of the FED core.

Mr. LOWERY. Is that the limit to which you think Doublet III will go, that simulation?

Dr. OHKAWA. Obviously, the simulation, but also, we are planning to have the tokamak improvement program in there so that the tokamak will eventually become steady state, not pulsed. Those experiments must be done in the kind of plasma which will be used in the FED core.

Mr. LOWERY. From your answers to Mr. Young's questions on the OHTE, it is obvious that you are committed to the reversed field pinch concept with the private sector investment that has been made.

What, specifically, are your goals with that program and within what time frame would you hope to achieve them?

Dr. OHKAWA. The first phase for the program is 3 years. In each different geometry, such as pinches, we have to establish empirically how well the plasma is confined and how much plasma will be confined; in other words, beta and confinement time.

So, in 3 years, we will do the physics experiments, and if the physics experiments indicate, by scaling them up slightly not in physical size but in plasma current, if we can reach the reactor-like plasma, that will be our plan for the second phase.

But since we just finished the first year of the experiments, we are not going to have the decision whether to go to the second phase or not until about a year and a half from now.

Mr. LOWERY. A good question to all of you. If you were king, and I guess my definition of king is you are the Congress and the President all rolled into one, and if you were to approve additional funds to restore some of the projects for the fusion program, what would your order of priorities be? John, why don't we start with you and go down the table?

Dr. GILLELAND. I don't pretend to possess that studied judgment in that I am a man who spends all his time in the laboratory at LaJolla.

Mr. LOWERY. You approach it from a micro——

Dr. GILLELAND. Yes. I happen to believe that the tokamak will evolve into a relatively simple and elegant reactor. I spent a year working for John Clarke, more or less, as executive director for the FED activities, and so, of course, my order of priority would be to move fusion truly into the engineering phase, and for that there is a requirement for a large device such as FED. FED as presently designed may not be the ultimate answer, but I found that one can design a device which will generate sustained large amounts of fusion power and that one can design a device which can accommodate innovations as they come; for example, the current drive possibilities which allow one to push the current around in the tokamak without an ohmic heating system.

This is a wordy way of saying that there are very promising bits of data now which say that we can eliminate systems from the tokamak and even further enhance its elegance. And so I think it is not premature to push ahead now with an initial reactor based on the tokamak concept, and that is where I would personally put my priority.

I could go down the list, but I think I would be in trouble as I got into lower levels because I cannot differentiate the promise of one system from another too well. I think they are all important. It is important to have a balanced program because some of these bottles will indeed someday be more appropriate than others as reactors. I think the main thing to say about fusion is that the results across the board are encouraging; the studies are encouraging; there is still this ultimate promise which I fix my dream upon, and that dream is that someday there will be an energy source which is highly acceptable environmentally and has almost no fuel cost and, although it is a high technology system, it is simple and it is elegant.

That is my dream, and that is how I am spending my life.

Mr. LOWERY. And creating the sun.

Dr. OHKAWA. As I said before, the present program is a balanced one; it is broad based. But that does not necessarily mean that we don't have a focused approach in each element. For example, in the tokamak area, most of our experts around the country agree that we are focusing on the improvement of the tokamak, such as to steady state.

So I would keep the similar balance if the funding is increased.

Mr. LOWERY. I forgot, in my definition of king, it is not only the Congress and the President but it is also the Director of OMB. [Laughter.]

Mr. MATSON. Well, now that I have all this power, hopefully not to be an autocratic king and knowing that there are other needs of the kingdom, I think I would look upon the action taken by the Congress as being rather astute in the overwhelming passage of the Fusion Energy Act and in the goals and the objectives that were included in that act.

I would probably want to have, as king, a pretty well developed plan as to how to get from here to there, basically, the there being what the act would provide the Nation in the form of essentially inexhaustible energy.

I agree with the previous comments on the balanced program. In particular, I think we don't want to center on the tokamak alone. As was mentioned in my testimony, the mirror, EBT, some of these other programs have been somewhat behind, and it is acknowledged that if one of these alternatives is better, we would certainly want to know about it. So I think I would want to have, as king, enough funding to be able to continue a balanced program to realistically evaluate the alternatives and be able to select downstream what would be the best approach to meet the objectives of the act in a reasonable amount of time as the act, I think, would like us to do. I think that is the way I would approach it.

Dr. REPICI. Mr. Chairman, since I am apparently much closer to engineering problems, if we are successful, the questions for me are ones of material data. It is most critical, as I go from the stage of creating the light bulb to making the lampshade, which uses that radiation, that the Fusion Materials Irradiation Test facility, the FMIT, be available. The FMIT, or similar device, would be important since it would supply the kinds of engineering data that I will need when I, hopefully, get there.

Mr. LOWERY. One final question. Mr. Matson, in the last months, utilities have stopped construction of quite a few nuclear powerplants largely because of the costs, the regulatory problems, and lower forecasts in demand. In fact, I would say construction of the powerplants in general, including nonnuclear, have at least slowed down.

It seems that the utilities are favoring smaller scale, lower cost facilities. Other than your comment in your testimony about making a national commitment to commercialization of fusion, are there other steps that we should be taking to make sure that the fusion program develops low-cost, economical systems?

Mr. MATSON. I think, as far as—you mentioned size of units. I think that is going to be, in the future, when utilities are looking for power options, an important factor. The items you pointed out regarding the current state of utilities are very true.

Mr. LOWERY. Do you think it is temporary?

Mr. MATSON. I think it is temporary, yes. I think that is going to turn around. I really feel that, as we have in the past, our Nation's capacity will surface. We will again revert to a strong industrialized base. I think we have in the past and I do not see any reason why we should not in the future.

In order to do that, I think we are going to have to have the capacity in electrical power to serve that need. So I think that will come back.

So it is a matter, I think, of the energy options that we have to look toward in the future, notwithstanding the fission reactor problem. I think that, in comparison, the fusion program has a great deal of advantage that the utility can look toward as an energy option downstream, and that could be—I emphasize "could be"—regardless of size because there are some utilities, who would want the smaller size fusion option; there are those who are going to want the larger size option, depending upon their particular needs and system characteristics.

So I think that to us looking at it from the utility perspective, it is still the attractive, inexhaustible fuel supply option that we would like to see downstream.

Mr. LOWERY. Energy abundance instead of managing scarcity.

Mr. MATSON. Right.

Mr. LOWERY. Thank you, Mr. Chairman.

Mr. YOUNG. Gentlemen, on behalf of the chairman and the members of the subcommittee, we want to thank you very much for your participation this morning and your organizations for the fine testimony that we have been able to put in the record. We look forward to having you back again next year, and that we will move on into the development of this fantastic power source. So I want to thank you very much for your attendance this morning.

Thank you.

I will now call our second panel of witnesses. This panel will consist of the Honorable Mike McCormack, Mr. Ed Kintner, and Dr. Stephen Dean.

Mike McCormack, as I am sure everyone here today knows, is a former chairman of this subcommittee and was the author of the

Magnetic Fusion Energy Engineering Act of 1980. Mike, it is always a pleasure to see you and we will be looking forward to hearing your provocative testimony.

Mr. Ed Kintner is another familiar face to members of this subcommittee, having been Associate Director of the Office of Fusion Energy for a number of years. Ed, as we are aware, felt it was necessary to resign his position a few months ago because of policy differences with the administration with the magnetic fusion budget. We will be interested in hearing his perspective of the program.

Finally, I would like to welcome back Dr. Steve Dean, president of the Fusion Power Associates and one of the real leaders of the fusion community.

Welcome, gentlemen. As I understand it each of you has a statement and then Mike will want to make some summary statements before we commence with the questions. So, Mike, it is our pleasure to have you this morning and please proceed.

STATEMENT OF MIKE McCORMACK, ENERGY CONSULTANT

Mr. McCormack. Mr. Chairman, good morning to you and Mrs. Bouquard and Mr. Lowery and Mr. Hollenbeck. It is certainly a pleasure for me to be back with the subcommittee, even on this side of the witness table.

With your permission, Mr. Chairman, I would like to submit my prepared testimony and additional material for the record.

Mr. Young. Without objection it is so ordered.

Mr. McCormack. First of all, I want to express my appreciation to you and to the members of the subcommittee for inviting me to discuss this Nation's policies and programs for magnetic fusion. It is a matter, as you know, of great concern to me, and also to Mr. Kintner and Dr. Dean who join with me today in presenting this testimony.

As you have said, Mr. Chairman, I will make a brief presentation, followed by Mr. Kintner and Dr. Dean, and then I will have a few summary remarks.

We will be brief and we are, of course, anxious to respond to questions that you or the other members of the subcommittee may have.

I think, Mr. Chairman, a bit of background may be appropriate at this time. For those of you who were not involved, this whole campaign to move fusion from a research concept to a program for power development started from many places at many times. For me it started when I was appointed to the Joint Committee on Atomic Energy in 1973, about the same time that Dr. Robert Hirsch became director of the fusion program for what was then the Atomic Energy Commission.

Dr. Hirsch and I set out on a campaign to expand the program. We agreed, and the Congress supported us in our belief, that this Nation could convert the concept of magnetic fusion energy into reality within perhaps 25 years with an adequately funded and coherent program of research, development, and demonstration, which obviously includes both research and engineering of materials testing.

When the Department of Energy was formed in 1977, Dr. Hirsch resigned, and Mr. Kintner replaced him as director of our fusion program. They had already been working together with me and the members of this committee and with other committees of the House and Senate to increase the funding levels for magnetic fusion research from about $30 million in 1973 when we started, to about $400 million in 1979.

Our goal was always the demonstration of magnetic fusion electricity by about the year 2000. During the late 1970's this subcommittee committed itself to eliminating unnecessary projects in the fusion program. Some of you were here when we entered into a protracted debate about how we were going to convert the program away from just a research-only concept into a goal-oriented program for energy production at the earliest realistic date.

The Fusion Advisory Panel was formed in 1979, chaired by Dr. Hirsch and composed of some of this Nation's outstanding fusion scientists, along with equally brilliant engineers and industrial executives. The report of the Fusion Advisory Panel in 1980 led us in this subcommittee to draft the Magnetic Fusion Energy Engineering Act in 1980. As a result of our introducing this legislation and advising the world that we were serious about enacting it, the Department of Energy created its own study panel under the Energy Research Advisory Board, directed by Dr. Sol Buchsbaum.

I think it is important for all members of the House Committee on Science and Technology today, and all the Members of Congress who are not familiar with it, to recognize that three committees; first of all the Foster committee, a Department of Energy Committee which preceded the Hirsch committee, the Hirsch committee, and the Buchsbaum committee—all made up of some of the Nation's most outstanding scientists, engineers, and corporate executives—agreed that the time had come for this country to move forward with a program of fusion engineering development and materials testing. And, the Hirsch panel and the Buchsbaum panel, which were still functioning at the time we were working on the legislation, each agreed that it should be possible to have a fusion electric demonstration plant on the line by about the year 2000. It was this confidence, expressed in these reports, and the concurrent success during that period of time of our plasma physics research in our various research laboratories that convinced us that we should go ahead and enact this legislation with the confidence that we would demonstrate scientific feasibility in our existing laboratories with machines under construction in the late 1983 to 1985 period.

It is also important to remember that this legislation was passed with only seven dissenting votes in the House, and by unanimous voice vote in the Senate. It had strong bipartisan support from both bodies and it was based on the conviction that we should not wait until after we had scientific feasibility demonstrated, but that we should start now with our engineering program so there would not be a 5-year gap after we had scientific feasibility, that we would not stand around in 1983 or 1984 or 1985 and say, "gee, what should we do now."

It is significant to note that within only a few weeks after the passage of the Magnetic Fusion Act, Japan, Russia, and the European Economic Community each initiated reviews of their fusion programs, and within a few months all three decided to accelerate their programs in quite the same way that we in the U.S. Congress, that you in this subcommittee and I had agreed to accelerate ours.

Unfortunately, the Department of Energy and the Office of Management and Budget have decided to fundamentally ignore the law that we enacted, and to revert back to a "research only" policy for fusion, delaying the engineering intiatives called for in this act. Because of this unauthorized attempt at policy reversal, the United States is already falling behind in this all-important area of energy engineering development.

I have several copies of Energy Daily here. One points out that we may be buying our fusion machines from Japan because they are already moving more aggressively in this direction.

I submit, Mr. Chairman, that the time has come for the Congress to insist that the spirit of the fusion law be followed and that at least a small part of the funding planned for fusion engineering development be approved for fiscal year 1983.

Mr. Kintner and Dr. Dean and I are joining today in encouraging this subcommittee to increase the authorization level for magnetic fusion research and development and demonstration for fiscal year 1983 by 10 percent over the current level for 1982. This would mean increasing funding from $455 million, as it is today, to about $501 million for fiscal year 1983. You will recognize that this really only compensates for inflation. The additional $56 million over the administration's 1983 request would, we recommend, be apportioned as follows:

For the Center for Fusion Engineering, $10 million in addition to the money that is already there; although there is no line item for it in the budget, there is some money there.

For the FMIT which is absolutely critical for any program whatsoever in the Federal program, $15 million.

For the Elmos Bumpy Torus, $21 million. Contracts, as you know, have already been let and that has been zeroed out in this budget.

And, for the MFTF–B, $10 million additional to put it back on track.

Each of these projects is critical to having a coherent program. The first two are absolutely essential for engineering programs; the bottom two are essential for filling out our research program.

This, in a sense, Mr. Lowery, responds to the question you asked the previous panel.

I recognize that there are those that may hesitate to provide adequate funding at this time to proceed at the levels we recommend. I should like to point out that the law which the Congress enacted was built on very careful consideration of what was necessary to move forward with a successful program of fusion engineering development and materials testing, and the partially completed FMIT is an example of that.

The bill called for a funding level for fiscal year 1982 of 25 percent above 1981. At this level, it would have been $525 million this year instead of the $455 million we are spending now. And, it called a 25-percent increase above that $525 million for this coming fiscal year. So, if the law itself were followed, the funding level for

1983 would be $656 million. We are suggesting $501 million, which means that we are already below the recommended funding level for 1983 by $155 million, not counting inflation.

Now, we certainly sympathize with the budgetary problems that you and other Members of Congress face. Nevertheless, we insist that the funding levels that we are suggesting are essential to maintain the program and to give the program meaning. If we are going to be spending $444 million it is essential that we spend the rest to give the program meaning at all.

The increase that we recommend, to put it in context, is equal to what this country spends every 6 hours on imported oil. Our proposed funding for magnetic fusion for fiscal year 1983, a total of about $500 million, is equal to what we spend in 2½ days on imported oil. The entire fusion program for the next 20 years—the $20 billion that we propose for the entire program—is less than 2 percent of what we will spend for imported oil in that time.

As I sat here listening to previous testimony I looked at these beautiful pictures around the wall and I thought to myself that for about one-third or about one-fourth of what was spent on these space programs, this country can demonstrate the fusion program and provide an absolutely unlimited source of cheap and clean energy for all mankind for all time.

We are really not talking about a money problem; we are really talking about a priority problem. This is, in fact, what I think must be understood.

Now, Mr. Kintner and Dr. Dean will discuss the status of our fusion program as it now exists and what needs to be done in the near future to move forward into a program of engineering development.

Before they do, I have one additional thought I should like to make at this time. The Congress and most of you committed this Nation to one of the most important projects in its history when we passed the Magnetic Fusion Act of 1980. It was not a trivial thing. Most of the Members of Congress recognized it when they approved a 20 year, $20 billion program. Most of them comprehended the extraordinary value to this country—and to mankind—of developing this ultimate energy source, at the earliest possible date, for the people of this country and the world.

Since that time, several individuals—only a few—in the administration, and none of them directly responsible to the Congress or to the people of this country, and none of them appearing before this committee, but each of them established in a strategic position in the administration, have taken it upon themselves to undo the fusion engineering program that we in the Congress, supported by scientists and other experts in the Government and industry to recognized as being essential. What has happened is that this scant handful of individuals is attempting to abrogate the law that you wrote, and to totally disregard what the Congress has directed be done. The law that you passed has been ridiculed, and administration spokesmen, none of whom have appeared before this committee, have stated that fusion is just a research program; that that fusion power is perhaps 70 years away. Some of them claim, without any appropriate background, to be qualified to ignore and reject the deliberations of some of the world's outstanding scientists, as well as of the Congress itself.

I urge the members of this subcommittee, in spite of your difficulties with the budget, to remember the leadership that you provided in the past, and to remember that the people of this country are looking to you now for that same leadership. What you do on this subject during the next few days can have a profound effect upon the world in which your children will live.

Thank you very much.

[The prepared statement submitted by Mr. McCormack follows:]

STATEMENT OF MR. McCORMACK ON MAGNETIC FUSION ENERGY RESEARCH, DEVOLOPMENT AND DEMONSTRATION

Good morning, Mr. Chairman.

I want to express my appreciation to you and to the members of the Subcommittee on Energy Research and Production for inviting me to discuss our nation's policies and programs for magnetic fusion research, development and demonstration. This is a matter of great concern to me, and to Mr. Edwin Kintner and Dr. Stephen Dean who join me today in presenting testimony. With your permission, Mr. Chairman, I will make a brief presentation, followed by Mr. Kintner and Dr. Dean, and then I will summarize. We will be brief; and we are, of course, anxious to respond to questions you and the other members of the Subcommittee may have.

Mr. Chairman, there is a bit of background which may be appropriate at this time. I was appointed to the Joint Committee on Atomic Energy in 1973, at about the same time that Dr. Robert Hirsch became director of the fusion program for the Atomic Energy Commission. Dr. Hirch and I set out on a campaign to expand the magnetic fusion program. We agreed, and the Congress supported us in our belief, that this nation could convert the concept of magnetic fusion energy into reality within perhaps 25 years with an adequately funded coherent program of research, development and demonstration.

When the Department of Energy was formed in 1977, Dr. Hirsch resigned, and Mr. Kintner replaced him as director of our fusion program. They worked together with me and the members of this Committee—past and present—and other Committees of the House and Senate to increase funding for magnetic fusion research from about $30 million in 1973 to about $400 million by 1979. Our goal was always the demonstration of magnetic fusion electricity by about the year 2000. During the late 1970s this Subcommittee committed itself to eliminating unnecessary projects in the fusion program and directing the fusion budget toward the goal of electric energy production at the earliest realistic date.

The Fusion Advisory Panel was formed in 1979, chaired by Dr. Hirsch and composed of some of this nation's outstanding fusion scientists, along with equally brilliant engineers and industrial executives. The report of the Fusion Advisory Panel in 1980 led us in this Subcommittee to draft the Magnetic Fusion Energy Engineering Act of 1980. As a result, the Department of Energy's Energy Research Advisory Board ordered a special study of our bill, directed by Dr. Sol Buchsbaum.

It is, I think, important for all members of the House Committee on Science and Technology today, whether they were members at that time or not, to recognize that three committees, each made up of some of the nation's outstanding scientists, engineers and corporate executives agreed that the time had come for this country to move forward with a program of fusion engineering development and materials testing, and the Hirsch Panel and the Buchsbaum Committee agreed that it should be possible to have a fusion electric demonstration plant on the line by about the year 2000.

It was the confidence, expressed in these reports, and the concurrent successes in plasma physics research in our research laboratories (at MIT and Princeton and Oak Ridge and Los Alamos and Livermore) that convinced us that we should enact this legislation. It is important also to remember that the legislation was passed with only 7 dissenting votes in the House, and by unanimous voice vote in the Senate. It had strong bi-partisan support in both bodies.

It is significant to note that within only a few weeks after the passage of the Magnetic Fusion Act, Japan, Russia and the European Economic Community initiated reviews of their fusion programs, and within a few months all decided to accelerate their programs in quite the same way that we in the United Stated Congress agreed to accelerate ours.

Unfortunately, the Department of Energy and the Office of Management and Budget have decided to ignore the law that we enacted, and to revert back to a "research only" policy for fusion; delaying the engineering initiatives called for in this Act. Because of this unauthorized attempt at policy reversal, the United States is already falling behind in this all-important area of energy engineering development.

I submit, Mr. Chairman, that the time has come for the Congress to insist that the spirit of the fusion law be followed; and that at least a small part of the funding planned for fusion engineering development be approved for fiscal year 1983.

Mr. Kinter and Dr. Dean and I are today joining in encouraging this subcommittee to increase the authorization level for magnetic fusion research development and demonstration for fiscal year 1983 by 10 percent over the current level for 1982. This would mean increasing funding from $455 million for this year to $501 million for fiscal year 1983. This, you will recognize, really only compensates for inflation. The additional $56 million would, we recommend, be apportioned as follows:

	Millions
Center for fusion engineering	$10
FMIT	15
Elmo Bumpy Torus	21
MFTF-B	10
Total increase	56

If there are those who hesitate to provide adequate funding for the magnetic fusion engineering program to proceed at the levels we recommend, I should like to point out that the law which the Congress enacted was built on careful consideration of what was necessary to move forward with a successful program of fusion engineering development and materials testing. It called for a funding level for fiscal year 1982 of $525 million rather than the $455 we are spending now; and it called for a 25 percent increase above the $525 for fiscal year 1983. Thus, if the law itself were followed, the funding level for fiscal year 1983 would be $656 million. We are suggesting $501 million, which means that we are already below the recommended funding level for fiscal year 1983 by $155 million, not counting inflation.

The increase we recommend is equal to what this country spends every six hours for imported oil. Our proposed funding for magnetic fusion for fiscal year 1983 is equal to about 2½ days of imported oil; and the entire $20 billion we project for fusion research, development and demonstration between now and the year 2000 is less that 2 percent of what we will spend for imported oil during that time.

Mr. Kintner and Dr. Dean will discuss the status of our fusion program as it now exists, and what needs to be done in the near future to move forward into a program of engineering development. Before they do I have one additional thought: Congress committed this nation to one of the most important projects in its history when it passed the Magnetic Fusion Act of 1980. Most of the Members of Congress recognized this when they approved a 20 year, $20 billion program. Most of them comprehended the extraordinary value to this country—and to mankind—of developing this ultimate energy source, at the earliest possible date, for the people of this country and the world.

Since that time, several individuals in the Administration, none of them directly responsible to the Congress or to the people of this country, but each of them established in a strategic position in the Administration, have taken it upon themselves to undo the fusion engineering program that we in the Congress, supported by scientists and other experts in government and industry recognized as essential. What has happened is that this scant handful of individuals is attempting to abrogate the law that you wrote, and to totally disregard what the Congress has directed be done. The law which you passed has been ridiculed, and Administration spokesmen have stated that fusion is just a research program; that that fusion power is perhaps seventy years away. Some of them claim, without appropriate background, to be qualified to ignore and reject the deliberations of some of the world's outstanding scientists, as well as of the Congress itself.

I urge the members of this subcommittee to remember the leadership that you provided in the past, and to remember that the people of this country are looking to you, now, for the same leadership. What you do on this subject during the next few days can have a profound effect upon the world in which your children will live.

Mr. YOUNG. Thank you very much, Mike.

Mr. Kintner?

STATEMENT OF EDWIN E. KINTNER

Mr. KINTNER. Thank you very much, Mr. Young.

I have a prepared statement which I would like to offer for the record and I would like to summarize.

Mr. YOUNG. Without objection, it will be entered as a part of the record.

Mr. KINTNER. For 5 years, as head of the U.S. magnetic fusion program, I came before this committee. Every year the committee showed its support for the program; either added some funds to the requested budget, or kept alive projects which had been recommended for recision. Then, under the leadership of this committee and its chairman, Mr. McCormack, the Congress enacted the Magnetic Fusion Act of 1980.

I proceed in my discussions from a basic point of view that energy is the most important secular question facing the future of the Nation. Its cost is already having a measurable effect on the economy, not only of the United States but of the Western World. Its geographical location and assurance of its availability dominates our diplomatic and military actions and plans.

Even so, the present circumstances with regard to energy are tenuous. Mr. Stark described it yesterday as "a house of cards." The energy problems are not solved using all available energy options. There is only one remaining option which can be added and that is fusion. If fusion were added, the future of energy would certainly be different. It is from that context from which I speak.

My purpose is to identify that two major programmatic decisions have been made during the several stages of the fiscal year 1982 and 1983 budget cycles which have changed, in a fundamental way, the character, direction and expectation of the U.S. program.

Whatever one thinks of the prospects for and importance of success, whatever one judges to be the importance of reducing Federal expenditures, these decisions will have long-term implications.

Fusion has been worked on for about 30 years. It is often said that we have made very little progress in 30 years. I think it is important to remember what Mr. McCormack said, that very little money was spent, the effort was very small, on a laboratory scale until about 8 years ago.

The T-10 and the Princeton Large Torus, the two machines on which the present confidence with regard to fusion rests, have been operating less than 6 years. The TMX experiment at Livermore, which gives us confidence that there are more than one way in which plasmas can be confined at power reactor conditions, has operated only a little over 2 years.

So, progress has, indeed, been rapid and significant. As a result of this progress there have been three high level reviews which Mr. McCormack referred to, I would like to quote very selectively from the reports of those reviews. First, the Foster committee said:

> There are now in view only a few essentially inexhaustible energy sources which can in principle contribute to the solution of the long-range world energy problem. A program based on fossil hydrocarbon fuels, though necessary for the near-term, provides only a limited solution.
>
> Since fusion energy is one of a severely limited number of possible alternatives, the first objective of the program must be to determine the highest potential of fusion as a practical source of energy. . .

> It is too risky at this time to concentrate just on the physics experiments and delay to a later time considerations of downstream engineering problems.
>
> There must be a major increase on engineering problems to provide the basis for proceeding to engineering development and the subsequent choice of an engineered prototype reactor.

This report to the Foster committee was translated into a policy statement for fusion energy by the Department of Energy, which said:

> There is no lead time to spare: if fusion energy is to be available when it is needed—the research and development program, rigorously directed toward the goal of commercial utility, must be undertaken now.

That report also established the milestone date of 1985 by which the alternate concepts, mirrors, elmo bumpy toruses, could be compared in some sort of a meaningful way with tokomaks before proceeding further with development of fusion energy.

Then, in late 1979 and early 1980, Mr. McCormack and this committee established a group of experts chaired by Dr. Robert Hirsch to review the program. They concluded as follows:

> The time is now for an engineering thrust centering on an engineering test facility.
>
> Our panel believes that fusion can be made commercial by the year 2000 if a national commitment is made soon.

Then, later in 1980, the Department initiated a further review by a very distinguished panel of experts, perhaps the strongest panel of scientific experts assembled for review of a program of this kind, the so-called Buchsbaum Panel. Its major recommendation was:

> The magnetic fusion program can, and should, embark on the next logical phase toward its goal of achieving economic feasibility of magnetic fusion. To this end a broad program of engineering experimentation and analysis should be undertaken under the aegis of a Center for Fusion Engineering.

Following that, both Europe and Japan carried out reviews which, essentially, supported the basic principle that fusion could now be assumed to be scientifically feasible and it was time to begin seriously the engineering development phase.

Congress then passed the Magnetic Fusion Energy Engineering Act of 1980 which said:

> The energy policy of the United States must be designed to insure that energy technologies using essentially inexhaustible resources are commercially available at a time prior to serious depletion of conventional resources.
>
> To insure the timely commercialization of magnetic fusion energy systems, the United States must demonstrate at an early date the engineering feasibility of magnetic fusion energy.
>
> It is, therefore, declared to be the policy of the United States and the purpose of this Act to accelerate the national effort in research, development and demonstration activities related to magnetic fusion energy systems.

The act and the ERAB recommendations were essentially in consonance with regard to the basic timetable and the basic methods of proceeding. The strategy was to embark on the engineering technology phase, its centerpiece being a fusion engineering device. It should be completed by the year 1990 so that a formal assessment could be made and, if things were going well, by the year 2000 a demonstration of practical usefulness could be concluded.

That, as you have heard from several other members of the panel before us, seemed a logical timetable and seemed a reasonable expectation for a well-organized, effective program. It is now an irony of history that this planning, this consensus that was achieved over a period of years, has now been turned back.

The expenditures for fusion energy have been sharply reduced; the fiscal year 1983 budget before you is 24 percent below the 1977 budget in real terms, 1977 was the peak budget year from the standpoint of program buying power.

Now, I would like to point out where the two fundamental decisions were made. The first one was to defer for an undetermined period, the intentions and actions required by the act of 1980. The second one was to delay, again for an indefinite period, the Mirror Fusion Test Facility which would have provided the basis for comparison in the mideighties of the mirror system with the tokamak and other toroidal systems.

Further, the plan to increase industry involvement which was a centerpiece of the Magnetic Fusion Engineering Act—to increase the industrial involvement in fusion development in a deliberate way—that will be postponed indefinitely, and the industrial and economic benefits of the high-technology development which would be involved in that aspect of the program would be delayed as well. But, the most important single factor, in my judgment—having been there when it happened—is an intention implicit in the fiscal year 1982 and 1983 budgets—to revert the program back to a science-oriented mode.

The persons who have shaped that policy have stated their purpose publicly and privately and their actions support their words. The orientation toward making fusion practical and useful which began 8 years ago is now being rescinded. Thus, what is happening goes well beyond budget policy or budget levels alone.

It goes beyond budget levels even with regard to the Office of Energy Research itself because both the high energy physics programs and the basic energy science programs have received increases, whereas magnetic fusion—the other large program in that budget—has been decreased. The root cause of this, as I see it, is a lack of perception of the true significance of energy and the problems of energy in the moderate and long term to which fusion must make a measurable contribution.

All of this could be easily understood if the program were failing technically or organizationally, but these recent actions have taken place, as you heard yesterday and today, in a circumstance in which significant progress is being made in almost every aspect of U.S. fusion program as well as in the rest of the world.

It is difficult to understand why responsible officials would want to turn away from a highly successful program looking toward making the last, the ultimate energy resource in the universe available. It is especially difficult in view of the fact that there are continuing difficulties developing in other energy sources and, for the first time, we see hard evidence of two threatening problems of the use of any fossil fuel: acid rain and carbon dioxide in the atmosphere.

In any case, $440 million is a great deal to spend on any purely scientific program. If, in fact—and I repeat, if, in fact—this is to be a science-oriented program without a mission, without a goal, without some practical social usefulness, then that is far too much money. If, on the other hand, the program has a purpose and a goal, and is working successfully toward that goal in the present circumstances, then in my judgment, $440 million is not sufficient, notwithstanding the budgetary difficulties that this Nation faces.

In an important sense, the real question is not budget levels, but intent and commitment. That is what the act gave us for the first time: a purpose, a plan, and a national commitment. That is what we have now lost by these two actions. The program is now a group of projects. They will spend the money. It is, as I have said, a little bit like a chicken from which all the bones have been removed—it weighs just as much but it does not fly very well. That is exactly what has happened to the fusion program.

What are the effects of this decision not to proceed deliberately and hopefully?

The first one is that the date on which fusion can be counted on to mitigate the many and increasingly intractable problems of energy will be postponed at least year for year. We have already lost 2 years, based on the testimony of DOE officials yesterday. I conclude that 2 more years are going to be lost before a decision is going to be reached as to whether to proceed or not.

The second loss is that the consensus strategy which has been achieved and which was a part of the act is also in grave danger of being lost. Such consensus does not happen overnight. It took many years to put together the program as it existed and then gain the support that it had, including congressional support. When that is withdrawn, even for a year, then the likelihood of the program coming apart into individual pieces is high. In other words, this is not simply a matter of postponing the program for a year or two; it is a matter of a threatened loss of the whole perspective.

Third, U.S. world leadership in fusion, which has allowed us to work effectively within the total world program to strengthen those world programs in a cooperative effort, will be weakened.

Finally, the development side, the technological side, in which we were pushing the state of the art on many fronts, will now be slowed, and the possibility that that effort would inspire industrial innovation will be lost. This is especially true since the engineering technology program would have been carried out mainly in industry.

Thus, within less than a year, without internal or external reviews of any consequence, on the basis of judgments of individuals with no previous experience or commitment to fusion, the consensus judgment of the Congress, and three high-level reviews has been overridden.

It is an irony of history—in my view a tragic one—that the United States finds itself, having arrived at recognized world leadership in an important technology with great implications both for this Nation and the rest of the world, unable to find the resources, either organizational or financial, to carry through with previously characteristic American will and vitality, a program which has been carefully laid out over the last 8 years.

Sidney Harris has said,

> An idealist believes the short run doesn't count. A cynic believes the long run doesn't matter. A realist believes that what is done or left undone in the short run determines the long run.

Nowhere is that going to be more true than in energy policy.

It may really be that this Nation cannot afford to carry out, in a deliberate and hopeful way, the consensus plans laid out for fusion development; but no one should doubt for a moment that this will not be without lasting consequences. The future will appear different in 10 years and it will be different within 25. Where there is no vision the people perish.

Thank you very much, Mr. Chairman.

[The prepared testimony of Mr. Kintner follows:]

STATEMENT OF
EDWIN E. KINTNER
TO THE
SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION
OF THE
HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY
MARCH 24, 1982

My purpose is to identify as clearly as possible that two major programmatic decisions have been made during the several stages of the FY 1982 and FY 1983 budget cycles. These decisions which have changed, in fundamental ways, the character, direction and expectation of the U.S. magnetic fusion energy program. Whatever one thinks of the prospects for and importance of success in fusion development and whatever one judges to be the importance of reducing federal expenditures these decisions will have important long-term implications. They deserve to be thoroughly understood as to their origins and effects by those parts of the Government which have responsibility for such matters, and by the public generally.

To properly appreciate the events of this last year, it is necessary to understand the general history of fusion development. Magnetic fusion originated as a science research program in the early 1950's. It worked along slowly at funding levels of about $30 million a year for many years, accruing preliminary understanding of a new scientific subject--plasma physics. At the time of the energy crisis in 1973, it had made sufficient progress to justify a major role in the plan for new energy research and development known as Project Independence. That plan envisioned increased financial support for fusion, a broadening of the physics investigations, beginning steps towards development of the engineering technology required to make fusion practical, and a goal orientation and schedule. Its impetus resulted in budget levels for magnetic fusion increasing to $316 million in FY 1977.

Since that time, real budgets through FY 1983, after adjustment for inflation, have decreased 24 percent. Nevertheless, with support of these resources, the United States established laboratories, facilities, and program strategy which gave it world leadership in this field--a leadership which had been exerted previously by the Soviet Union. The experimental results from the strengthened program gave greatly increased confidence that the fusion process hitherto known only in the Sun and stars, could, in fact, be made useful on Earth.

Thus, although fusion research is 30 years old, only in the last eight years has it been of significant scope. The medium scale experiments, T-10 in the Soviet Union and the Princeton Large Torus (PLT) have been operating only six years. During that time the program was searching intensively for a strategy suited to it, one which recognized its uniquely long-term, broadly based nature, and its lack of military motivation or early financial return.

As a result of recognition in recent years of increasing problems of energy supply and environmental effects, the impressive scientific progress, and the generally held judgment that scientific feasibility was now assured, three high level scientific reviews were convened to help establish the appropriate future course for fusion development.

In 1978, the new Department of Energy as one of its first acts established an Ad Hoc Experts Group on Fusion, chaired by Dr. John Foster, former head of Research and Development in the Department of Defense, and made up of a number of distinguished scientists and engineers, to review the program and report its findings and recommendations. The Experts Group arrived at a number of important conclusions; in particular: (Emphasis from here on is the author's).

> "*There are now in view only a few essentially inexhaustible energy sources which can in principle contribute to the solution of the long-range world energy problem*. These include fusion, fission (with some type of breeder), and solar power. *A program based on fossil hydrocarbon fuels, though necessary for the near-term provides only a limited solution.*
>
> "Since *fusion energy is one of a severely limited number of possible alternatives, the first objective of the program must be to determine the highest potential of fusion as a practical source of energy ...*
>
> "*...There is an urgent need to answer the questions concerning feasibility and the momentum of the program is an asset and should be maintained.*
>
> "*In our judgment the proper strategy should optimize the chances of determining the highest potential for a commercial fusion energy source at the earliest practical date.*
>
> "*...it is too risky at this time to concentrate just on the physics experiments and delay to a later time considerations of downstream engineering problems.*
>
> "*...there must be a major increase in emphasis on engineering problems to provide the basis for proceeding to engineering development and the subsequent choice of an engineered prototype reactor.*"

The Ad Hoc Experts Group recommended that the Department of Energy:

> "Lay the groundwork for one or more engineering test facilities (ETF) to be committed as soon as practical after the potentially competitive designs are identified.

Based on the recommendations of the Ad Hoc Experts Group, the Department issued a Policy Statement for Fusion Energy, which stated:

> "...Successful commercialization of fusion could provide an energy resource whose ultimate fuel (deuterium extracted from water) is cheap and essentially unlimited, and whose byproducts would pose much reduced environmental problems compared to coal and fission power. But there is no lead time to spare: if fusion energy is to be available when it is needed--the research and development program, rigorously directed toward the goal of commercial utility, must be undertaken now.
>
> "...The goal of fusion research in the Department of Energy is to develop the highest potential for employment of fusion energy."

In early 1980, the Chairman of the House Subcommittee on Energy Research and Production, House Science and Technology Committee, Congressman Mike McCormack of the State of Washington, organized a Fusion Advisory Panel to hold special hearings and advise him and his Committee on the status of the U.S. fusion program. The Panel was made up of a number of distinguished scientists both from within and without the fusion program. Their conclusions were, in part:

> "The Fusion Advisory Panel reaffirms its previous position:
>
> "- There has been very significant recent technology progress in fusion research.
>
> "- The time is now for an engineering thrust centering on an engineering test facility.
>
> "- Our panel supports the present DOE balanced research and development program.
>
> "- Our panel believes that fusion can be made commercial before 2000 if a national commitment is made soon."

In 1980, the Department chartered another thorough scientific review of the magnetic fusion program. This panel was headed by the Chairman of the Energy Rsearch Advisory Board, Dr. Solomon Buchsbaum, and its members were selected from a broadly experienced group of scientists and engineers. The Buchsbaum Panel has been characterized as the most powerful group of its kind every assembled for a scientific program review.

The Buchsbaum Panel spent a total of eleven days in plenary session in Washington and the major fusion laboratories, and its members visited the other magnetic fusion activities and received testimony from the general public. Its conclusions as a Panel were presented to, and unanimously agreed with by, the entire ERAB in August 1980. The major recommendation was:

> "1. The magnetic fusion program can, and should, embark on the next logical phase toward its goal of achieving economic feasibility of magnetic fusion. To this end a broad program of engineering experimentation and anslysis should be undertaken under the aegis of a Center for Fusion Engineering (CFE).
>
> "A key element of the program should be a device containing a burning plasma, and incorporating in its construction those technological features which can serve as a focus for the development of future reactor technology..."

This recommendation, and most of the other recommendations of the Panel, were incorporated promptly into the program planning of the Department.

Following the report of the Buchsbaum Panel, a number of complementary actions took place. A similar panel was established in the European Community and was chaired by the Vice President of Research and Development for Siemens Corp. Dr. Heinz Beckurts. After six months of review, it made a positive report on the European program recommending budgetary increases especially in engineering technology areas and looking to initiation of a device closely paralleling the ETF in objectives, following successful operation of the Joint European Tokamak (JET) at the Culham Laboratory in Great Britain.

In Japan, a Fusion Research Council review of its program recommended that Japan embark on a broad plan leading to an Engineering Test Reactor in the mid-1990's. This device would have considerably broader objectives than those recommended by the Buchsbaum Panel for the FED, including the production of some electricity. That plan has been accepted by the Japan Atomic Energy Commission and is now in process of being endorsed as a national policy. When Japan embarks on a course of this type as a national commitment, the probability of its carrying through to a conclusion is high.

In the United States, the Congress, acting on the basis of the ERAB Panel recommendations, enacted Public Law 96-386 on October 7, 1980. This Bill was approved in the House by a vote of 365-7 and by unanimous voice vote in the Senate. The findings and policy statement of that Act stated in part:

> "Sec.2.(a) The Congress hereby finds that--
>
> "...(2) the energy policy of the United States must be designed to ensure that energy technologies using essentially inexhaustible resources are commercially available at a time prior to serious depletion of conventional resources:
>
> "(3) fusion energy is one of the few known energy sources which are essentially inexhaustible, and thus constitutes a long-term energy option;...
>
> "(6) to ensure the timely commercialization of magnetic fusion energy systems, the United States must demonstrate at an early date the engineering feasibility of magnetic fusion energy systems...
>
> "Sec.2.(b) It is therefore declared to be the policy of the United States and the purpose of this Act to accelerate the national effort in research, development and demonstration activities related to magnetic fusion energy systems. Further, it is declared to be the policy of the United States and the purpose of this Act that the objectives of such program shall be --
>
> "...(2) to establish a national goal of demonstrating the engineering feasibility of magnetic fusion by the early 1990's.
>
> "(3) to achieve at the earliest practicable time, but not later than the year 1990, operation of a magnetic fusion engineering device based on the best available confinement concept;
>
> "(4) to establish as a national goal the operation of a magnetic fusion demonstratoin plant at the turn of the twenty-first century;...
>
> "...(7) to continue international cooperation in magnetic fusion research for the benefit of all nations.
>
> "(8) to promote greater public understanding of magnetic fusion;
>
> "(9) to maintain the United States as the world leader in magnetic fusion."

The Act and the ERAB recommendations are basically in consonance both as to the strategy and the schedule for carrying fusion forward; first to establish its technical feasibility as well as its scientific feasibility by the early 1990's, and then to demonstrate its commercial usefulness by the turn of the reviews, and an Act of Congress, the fusion program seemed to have arrived finally at a consensus strategy and timetable and a statement of national intent.

It is an irony of history that this planned broadening and acceleration of fusion research has now run directly into the perceived need to sharply reduce all non-defense spending. In the United States the expenditures for magnetic fusion have been reduced sharply from the levels required to carry out the program outlined by the Buchsbaum Panel and authorized by the Act of 1980. The proposed FY 1983 budget is 24 percent below the FY 1977 budget in real buying power, and all the initiatives designed to carry out the recommendations of the Panel are cancelled or postponed indefinitely. The completion of the Mirror Fusion Test Facility (MFTF-B), which was to have made possible an informed comparison between toroidal and linear confinement concepts by the mid-80's, has been postponed up to 3 years. The program is in imminent danger of being returned to a "science only" orientation. The Secretary of Energy has testified that his advisors tell him fusion is 30, 40 or even 50 years away, and that it should be science oriented with only a minimum of engineering effort.

During all but the last six weeks of the period during which the FY 1982 and FY 1983 budget actions were taking place, the decision-making positions in the Department of Energy, with the exception of the Secretary and Deputy Secretary, were filled with temporary appointments of career civil servants. The Examiner of the Office of Management and Budget while the FY 1983 budget was under development, was a temporary employee.

Now you will see where the two decisions referred to at the beginning have been made. The first was to forego, for an undetermined but extended period, the strategy and thrust of the ERAB Panel recommendations and the provisions of the Magnetic Fusion Energy Engineering Act of 1980. (The Act was characterized at senior levels in the Department as "permissive" and "a silly piece of paper".) The second was to delay the major facility in mirror system development (MFTF-B) by up to three years and, therefore, to forego an opportunity to make an informed judgment between toroidal and linear systems mid-decade as proposed by the DOE Policy Statement of 1978. Those two decisions leave the program without the strategic backbone of either the Foster or Buchsbaum review recommendations--a collection of individual projects and activities but without a mission or a timetable.

The plan to increase industry involvement in fusion development in a deliberate way will be postponed indefinitely, and the industrial and economic benefits of high technology development (spinoffs) which would have become increasingly important with an accelerated fusion engineering technology program, will be lost.

However, implicit in the FY 1982 and FY 1983 budget actions described above is an even more damaging intent--that of redirecting fusion research back to science objectives only and away from engineering technology developments looking to practical ends. The persons who have shaped this new policy have stated their purpose publicly and privately, and their actions support their words. The orientation toward making fusion practical and useful which began eight years ago is now being rescinded. Thus, what is happening goes well beyond budget policy or budget levels alone.

Why this sudden reversal in the national policy toward fusion? The obvious and immediate answer is that proceeding as planned required mortgages on the future at a time when the overriding Administration objective was to balance the budget. But the new Administation's policy statements as forwarded to the Congress by the President in the National Energy Plan included the following:

". There is an appropriate Federal role in certain long term research and development.

". The goal of long term R&D is to develop promising technological innovations to the point where private enterprise can reasonably assess their risks.

". <u>The Federal Government recognizes a direct responsibility to demonstrate the scientific and engineering feasibility of fusion."</u>

(emphasis added)

Moreover, considering the historic implications of fusion and the potential for return on investment from the technological development activities, the incremental cost which would have required doubling the present budget from $400 million to about $800 million annually over a five-to-seven year period seems a reasonable investment notwithstanding other budget pressures. And these incremental costs can be compared with the trillion dollars expected to be invested in energy facilities during this decade, or the $60 billion annually exported for oil.

The Administration's recommendations for the other large programs in the Energy Research activities of the Department of Energy (High Energy Physics and Basic Energy Sciences) were exempted from the cuts which were made in the magnetic fusion budget. Therefore, I conclude that there were other factors than budget restrictions alone at work.

To some degree, supporters of fission energy, and particularly the breeder reactor, have worried that if fusion is worked on hopefully and aggressively there will be a reduction in enthusiasm and support with which fission and the breeder are pursued.

There is also a question in any energy development scenario as to eventual competition with all other energy options, including specifically fossil reserves. This seems especially shortsighted regarding fusion since in a very real sense, "it is not an option". The world will be different within fifty years in the case where fusion is developed as compared to the one if it is not. In that sense the development of fusion is a black and white issue.

But I believe the root cause of this abrupt turning back from a national commitment to carry on with U.S. leadership in fusion development is a failure to perceive, on the part of responsible officials in the Government and by the public generally, the total significance of energy to national security and economic strength and the extreme importance of developing and exploiting whatever potentials exist in any of the energy "options". As a result it is especially difficult to support any sort of intensity of effort on a program whose practical payoff is at least two decades in the future.

All of this would be more readily understandable if the magnetic fusion program were failing technically or organizationally, but these recent actions have taken place despite continued impressive technical advances throughout the program. In the last several years these have been of a kind which have not created publicly understandable headlines, but whose implications for achievement of practical fusion energy have been significant to a greater extent than during any similar period in the past. These advances represent valuable return on the investment which the United States has made since 1973.

Recent conceptual designs of fusion power reactors, taking advantage of the many advances already accomplished or now foreseen, provide increased confidence that one or more of the plasma confinement concepts can, in fact, be developed into useful, practical, economic power producers. In short, this has been a rich period of experimental and theoretical advances. Certainly, there has been no indication of major setbacks to the steady advances in magnetic fusion over the last decade.

Let me now make what I think to be a vital point in this matter. Fusion is a difficult undertaking--probably the most lengthy and difficult technical development ever undertaken as a single organized, non-military development activity. *It is vitally important, therefore, to have a strategic framework and schedule on which to make internal program decisions and judge progress.* But fusion has no precedent as a research and development program, not only because it is so long term, but also because it is different in kind from its large predecessors, such as fission energy or space exploration. Therefore, and because of the long time constants associated with developments of this kind (at least ten years minimum for each major step), it seems vital to determine at the earliest practicable date whether fusion can be counted on as a part of long range energy policy. Obtaining an answer to that question may be more important if the answer is negative than if it is positive. Those working in the fusion field are increasingly confident the answer will be positive, but in either case, it seem vital to have an informed answer.

For these reasons, the Department proposed, and the Congress endorsed as the major near term program goal, the confirmation of *both* the scientific and technical feasibility of fusion by the early 1990's.

The fusion community worldwide is almost unanimous in concluding that scientific feasibility is assured and will be confirmed during the next five years in the Tokamak Fusion Test Reactor at Princeton, in the Joint European Torus in Great Britain, in the JT-60 in Japan, and in T-15 in the Soviet Union. What will not be done unless further steps are taken is sufficient engineering insights in plasma physics which inevitably will be obtained over the next decade in experimental devices now in operation or already under construction, an informed assessment of fusion's potential. That additional effort is what the ERAB Panel recommended and the Magnetic Fusion Energy Engineering Act translated into law.

It is difficult to understand why responsible officials of this Government would want to turn away from a highly successful program plan looking toward making fusion useful. It is especially difficult in view of the retreat from fission energy as seen in the reduction in new nuclear plant commitments by utilities. And it is especially difficult in view of recent hard evidence of the two threatening problems of fossil fuels--acid rain and climatic changes due to CO_2 in the atmosphere.

In any case, $444 million is a great deal to spend for purely scientific investigations of plasma physics without a practical mission objective. At the FY 1983 budget level of $444 million, progress will be made, new scientific results will be obtained and budgets in the fusion laboratories will be relatively comfortable. But the vital programmatic sense of the parts of a major development program fitting and working together towards a common goal will be lost. The program will be vulnerable to errors in judgment and further restrictions on its resources because it has no strong strategic logic from which to make decisions or justify support.

However, in an important sense, the first question is not budget levels but intent and commitment. Plans, strategies, schedules, attempts to establish goals and milestones for fusion have been made almost entirely from within the program. The first assistance from outside in this regard came from the Foster Committee Report which led to the Policy Statement on Fusion in 1978. Further progress in the program then led to the Buchsbaum Report and the Magnetic Fusion Energy Engineering Act of 1980. Finally, with that Act, there was an official policy stated strongly from outside the program--one which made good sense from the standpoint of the technical complexity of fusion and its historical implications, and one which, it seemed to many, was fiscally responsible--i.e., total expenditure estimated to be required was spread over two decades and was never as much as present and continuing budgets for fission development or space.

What is still missing is a national intent, an expressed purpose within the mechanisms of Government that this Nation should, as an investment in its future security and econommic health, proceed in a deliberate, persistent and technically honest way to make fusion energy available in a practical sense. A restatement of such an intent is more important in the first instance than increased budgets, because such an intent, properly expressed, contributes greatly to more effective use of whatever resources are supplied.

What are the programmatic effects of this decision not to proceed deliberately and hopefully with fusion development?

1) The date on which fusion can be counted on to mitigate the many and increasingly intractable problems of energy will be postponed at least year-for-year.

2) A consensus strategy based on high level review and Congressional support will be lost. It is not likely a similar consensus on a new strategy can be established in a short time. In other words, this is not simply a matter of increasing following year budgets "when the economy recovers".

3) U.S. world leadership in fusion and the ability of the U.S. to work effectively within cooperative arrangements to strengthen world programs as a whole will be weakened.

4) The potential of developments on the technology side of fusion which were pushing the state-of-the-art in a number of technological areas, and thus to inspire U.S. industrial innovations, will be lost. This is especially true since the majority of the engineering technology program would have been carried out in industry.

Thus, within less than a year, without internal or external programmatic review of any consequence, and on the basis of judgments of individuals with no previous experience or commitment to fusion, the consensus judgments of the Congress and three high level scientific reviews, as well as of the professionals in the fusion program who have put their reputations and careers on the line--all have been overridden.

If, indeed, it is considered that fusion is "30, 40 or 50 years away", and if the program is to be essentially a pure science program without practical goals and milestones, without a mission and without a strategy on which to base base decisions and measure progress, then the present budget of $444.1 million is sufficient. If on the other hand, the Congress is willing to maintain the fusion program at a steady level; i.e., provide for 10% inflation between the FY 1982 and FY 1983 budgets, then the whole thrust of the mission orientation intended by the Congressional Act could be maintained, albeit at a slower pace. That would require adding $56 million to the $444 million FY 1983 budget request (i.e., $44 million above the $456 million FY 82 budget) as follows: $10 million to establish a Center for Fusion Engineering and commence serious conceptual design of a Fusion Engineering Device; $15 million to commence construction of the Fusion Materials Irradiation Test, the only machine planned anywhere in the world which would create a neutron environment similar to that of a fusion reactor; $21 million to make a reasonable beginning on the Elmo Bumpy Torus Proof-of-Principle experiment, the first project for which industry will have system responsiblity under McDonnell Douglas; and $10 million with $5 million more from other sources within the program to put the Mirror Fusion Test Facility back on its original schedule of completion in 1985.

It is an irony of history--in my view a tragic one--that the United States finds itself, having arrived at recognized world leadership in an important technology with great implications both for this nation and the rest of the world, unable to find the resources--both organizational and financial--to carry through with previously characteristic American will and vitality the program which had been carefully laid out over the last four years.

As I watched, from my position as program director, the events affecting the program over the last five years, I often asked myself whether the program could be carried to a successful conclusion in the present social and political climate which emphasizes shorter term payoffs and goals. After the events of this past year, I have concluded, reluctantly, that there is grave doubt that fusion can be carried out as an intelligently organized, mission-oriented program, unless, perhaps, if driven by crisis. Because of the inherent time constraints, that may be too late!

Sidney Harris has said,

> "An idealist believes the short run doesn't count. A cynic believes the long run doesn't matter. A realist believes that what is done or left undone in the short run determines the long run."

It may be that this administration cannot afford to carry out, in a deliberate hopeful way, the consensus plans laid out for fusion development; but no one should assume that not doing so will be without lasting consequences. The future will appear different within 10 years, and it will be different within 25.

BIOGRAPHY

EDWIN E. KINTNER

Edwin E. Kintner was Associate Director for Fusion Energy, within the Office of Energy Research, U.S. Department of Energy (DOE) until January 9, 1982. Prior to September 1979, he served in this capacity within DOE's Office of Energy Technology, having assumed the post upon the creation of the Department in October 1977.

Mr. Kintner served in a similar position within the Division of Magnetic Fusion, U.S. Energy Research and Development Administration (ERDA), from April 1976 until the merger of ERDA with DOE. From January 1975 until April 1976, he served as Deputy Director of the Division.

Prior to service with ERDA, Mr. Kintner was assigned to the former Atomic Energy Commission (AEC). He joined the AEC in 1965, serving simultaneously as Assistant Director for Reactor Engineering and Deputy Director of Reactor Research and Development. In January 1975, and the absorption of fusion research functions by the newly-established ERDA, he transferred to that agency.

From 1963 to 1965, Mr. Kintner was President and General Manager of South Portland Engineering Company, South Portland, Maine, where he was engaged in the manufacture of large components for submarines and in building medium-sized ships.

Mr. Kintner served with the U.S. Navy from 1941 until he retired in 1963. Of his 22 years of service with the Navy, 14 years were devoted to assignments within the Naval Reactors Program. During this period, he served as Projects Officer for the USS Nautilus Project; Head, Advanced Design Group, Naval Reactors Branch; first Nuclear Power Superintendent, Mare Island Naval Shipyard; and Assistant Manager for Operations at the Pittsburgh Naval Reactors office. He was awarded the Navy Commendation Medal for his contribution to the Naval Nuclear Propulsion Program from 1950 to 1963.

Mr. Kintner graduated from the U.S. Naval Academy in 1942. He received an M.S. degree in naval construction and marine engineering in 1946 and an M.S. degree in nuclear physics in 1950. Both advanced degrees were awarded by the Massachusetts Institute of Technology.

Mr. YOUNG. Thank you very much, Mr. Kintner.

Dr. Dean?

STATEMENT OF DR. STEPHEN O. DEAN, PRESIDENT, FUSION POWER ASSOCIATES

Dr. DEAN. Thank you. It is a pleasure to be back.

I resigned from the Department of Energy 3 years ago, although it did not attract the same amount of public attention as Ed Kintner's did. I was working for Ed at the time. I left the Department to form Fusion Power Associates because I recognized that unless we started to mobilize the industry and the private sector, we were not going to take this program from a scientific research mode to something that would develop practical products.

Two individuals joined me to form the initial board of directors to incorporate Fusion Power Associates. In addition to myself there was Dr. Alvin Trivelpiece who testified yesterday and who is now the Director of Research at the Department of Energy and Dr. Nicholas Krall, who is the vice president of Jaycor and who is now the chairman of our board.

We started 3 years ago with 10 corporate members; today we have 30 corporate members and 20 affiliates and, I believe, we are making effective progress in mobilizing the industry and the private sector in bringing to bear the kind of perspective that is needed in the fusion program.

I would like to enter my prepared remarks into the record and make a few summary comments.

Mr. YOUNG. Without objection, so ordered.

Ed Kintner and Mike McCormack have both mentioned this question of scientific research versus engineering development. I would like to just comment that while it is very important to try to understand all the different physics' constraints and all the kinds of problems and solutions that the physicists are inventing and solving every day, it is also very important to keep our eye on the fact that we are developing this program in order to benefit society and not just to publish papers in scientific journals.

[Slide 1 shown.]

ENERGY FROM FUSION WILL BENEFIT SOCIETY

BY:

- PRODUCTION OF ELECTRICITY
- PRODUCTION OF HYDROGEN FOR SYNTHETIC FUELS AND TRANSPORTATION
- PRODUCTION OF FISSILE MATERIAL FOR FISSION REACTORS AND DEFENSE
- INDUSTRIAL PROCESS APPLICATIONS
- SPACE VEHICLE PROPULSION

FUSION USE WOULD HELP TO RELIEVE ONE OF THE PRIMARY CAUSES OF WORLD TENSION--THE UNEVEN DISTRIBUTION OF PRIMARY FUEL RESOURCES.

A FUSION ECONOMY WOULD PROVIDE A STABLE ENERGY BASE FOR SUSTAINED WORLD ECONOMIC GROWTH.

SLIDE 1

Fusion is one of the unique forms of energy in the sense that, not only can it be used to make electricity, but it probably is the only source that has so many different potentially useful applications. Other energy sources have unique potential for one or the other of these applications but fusion is probably the only one that has the potential, in fact, to do all of the various things that are listed. In addition, the use of fusion would help to relieve one of the primary sources of world tension—uneven distribution of primary fuel resources, because the fuel for fusion, as you know, comes from water.

[Slide 2 shown.]

FUSION ENERGY SHOULD BE ENVIRONMENTALLY AND SOCIALLY ACCEPTABLE

SINCE:

- THERE ARE NO COMBUSTION PRODUCT WASTES
- RADIOACTIVITY INDUCED BY NEUTRONS IN STRUCTURAL MATERIALS IS IN A MORE BENIGN FORM THAN IN FISSION REACTORS
- THERE IS NO POSSIBILITY OF RUNAWAY NUCLEAR REACTIONS
- THE HAZARDS ASSOCIATED WITH POSTULATED ACCIDENTS ARE THOUSANDS OF TIMES SMALLER THAN FROM FISSION REACTORS
- THE MINING, MILLING AND TRANSPORTATION OF MATERIALS REQUIREMENTS ARE SMALLER THAN FOR OTHER ENERGY SOURCES
- THERE ARE NO REQUIREMENTS FOR TRANSPORTATION, REPROCESSING OR LONG-TERM OFF-SITE STORAGE OF RADIOACTIVE SPENT FUEL

SLIDE 2

Also keep in mind that we are developing fusion for environmental reasons and social reasons also. There are a variety of features of fusion that make it different qualitatively from other energy sources and that will make the world a better place to live in if fusion is developed. I might mention the fact that fusion is such an intense energy source in terms of its utilization of the fuel that, for example, a 1,000 megawatt electric powerplant can run for a whole year on only one small pickup truck load of fuel and contrast that to a similar-sized coal plant that requires 191 trains of 110 cars each to deliver the coal and a like number to remove the ash.

I would like to comment on this question of the way of looking at the program as a research program or as a development program.

The fusion program consists of a variety of scientific approaches all of which require proof of principle experiments at one level or another and it is supported by a variety of research and a variety of engineering development.

In the past several years we have initiated the construction of two machines of very large size: the Tokamak Fusion Test Reactor [TFTR], and the Mirror Fusion Test Facility [MFTF]. Those two machines together cost about a half a billion dollars. They are $200 to $300 million each. In the process of building those machines, we have already moved into the engineering phase of fusion development. We are developing engineering in order to build these machines.

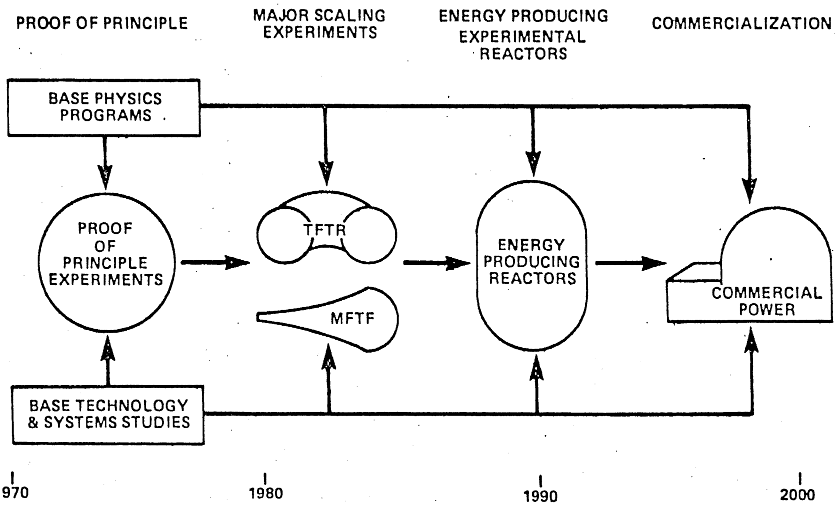
In addition to those machines, we are developing engineering on items like the Large Coil Project at Oak Ridge and the Tritium Systems Test Assembly at Los Alamos. So the fusion program has already got a heavy component of engineering in it. What we are in is a transition from a program which was dominated by scientific research to a program which has to move, in the 1990's, to actually build reactors that make a lot of fusion power.

This is the question: How fast are we going to develop the engineering and make the commitment to actually be building prototype fusion power reactors that make copious amounts of fusion power? In that context, I would like to put in a very strong plug to this committee to keep the MFTF project on schedule. That machine is costing you $200 million. If you delay that program by 2 or 3 years, you are going to pay about $100 million of increased costs before you get it finished.

I would like to comment that although the TFTR is a very important project and it is the lead program in the tokamak program, it does not have superconducting magnets; it is not a steady-state device such as eventual fusion reactors very likely will have to be. The MFTF, on the other hand, has superconducting magnets, it is a steady-state type of experiment and that kind of technology is very important to the tokamak program and the overall flow of fusion development. It is needed on a reasonable time scale in order that decisions can be made to build the right type of energy producing reactors in the nineties.

[Slide 3 shown.]

STAGES IN FUSION ENERGY DEVELOPMENT



SLIDE 3

This slide was made a few years ago. Since that time the Elmo Bumpy Torus has come on the scene and I remind you that whereas it is called a "proof of principle" experiment, its price tag is $100 million. It is a steady-state machine and it also has superconducting magnets. It is an engineering machine as well as a physics machine and its impact on the fusion program is only second in importance to the MFTF and the TFTR. The EBT-P project should also be constructed expeditiously. We have already made commitments: We have picked a site; we have picked a contractor and we ought to get on with the job.

I would like to comment about the Department of Energy and how it manages this program. Looking around at the pictures on this wall, as Mike McCormack mentioned, I am a little bit moved to note the contrast in the way that NASA managed that program as to the way the Department of Energy conceives of managing this program.

It is not true that the space program did not have scientific uncertainties in it; there were a lot of scientific uncertainties about whether man could survive in space and there were a lot of scientific experiments conducted in satellites and elsewhere and are still being conducted about how we move into space, as well as a big engineering development program.

The Department of Energy seems to look at the fusion program as research—a program that should be managed in a kind of "research mode." It looks at proposals from the scientific community, it reviews them in the peer review process, it does the best it can to meet its milestones and so on; the DOE does not seem to have a concept of how to run this program as a development program with a systems management approach to it.

I think two things happened during this last year, that you are well aware of with regard to the Fusion Act, that point out the problem of management at the Department with respect to this program.

The first is that the act required that they prepare a plan for you on how to establish a Center for Fusion Engineering and they were supposed to tell you that last summer. As you know, they told you that they were not going to do it. They were also told by the act that they should give you a comprehensive program management plan a couple of months ago and they told you that they could not do that either. Now, neither of those actions' required by the act, would have required hardly any money at all for them to do and yet in neither case did they choose to comply and, as far as I know, on the Center for Fusion Engineering they still do not plan to establish it. There is a line item in the fiscal year 1982 budget of $9.5 million, in this year's budget, that is labeled, "Center for Fusion Engineering." None of that money is being spent on a Center for Fusion Engineering; it is all being spent on various engineering projects of various sorts and conceptual designs, but it is not being used to establish a Center for Fusion Engineering.

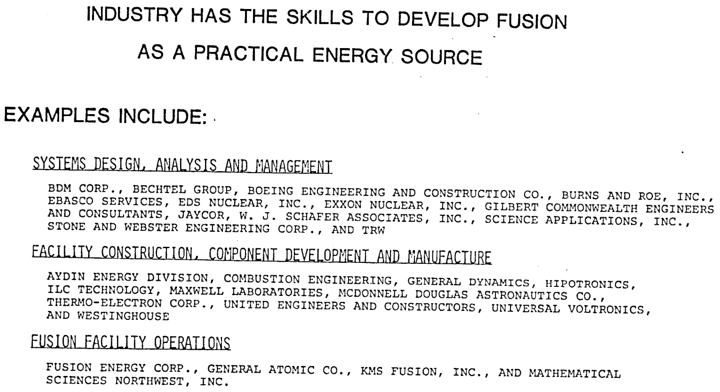
With respect to the comprehensive management plan that you asked for, I understand from testimony yesterday that they will now start to prepare it for you. As they start to do this I would urge you to keep your eye on whether or not they are providing you a real management plan because their tendency will be to provide you with program descriptions and program analyses and program milestones, and not deal with management issues.

One has to really force this issue, it seems to me, about how this program is going to be managed. It requires a systems management approach. One of the reasons they are not going to establish a Center for Fusion Engineering is because they conceive it as another national laboratory and we have got so many national laboratories now, why do we need another one? If you conceive of the Center for Fusion Engineering as another national laboratory, I agree that we do not need another one; we have got plenty and we could just convert one. The Center for Fusion Engineering needs to be a dynamic systems management organization; a lean organization that does the things that Ken Matson described to you in his testimony earlier. That is the kind of organization it should be. It does not cost a lot of money but it does bring discipline and an engineering approach to developing a program that will lead to a practical product and that, I think, is the big distinction between what we are saying to you today about engineering and management and development as opposed to scientific research.

I am not opposed to scientific research; I do not think any of us are opposed to scientific research. The best minds in this program are the scientists and the program totally depends on their inventions in order to be successful. Nobody is talking about cutting them off at the knees or cutting off the new concepts that are coming up. But this program has got to start thinking like a successful development program, the way the Apollo program thought of itself. They were going to do it in a specific time, they were going to bring in the contractors to do the job and get on with it in a certain manner. That is what has to come to bear on this program that it does not now have.

Although the Magnetic Fusion Engineering Act and this committee deal only with magnetic fusion, I would like to divert slightly at the moment to comment that the Department of Energy also sponsors a program in inertial confinement fusion that is managed by the military. I would urge this committee to influence your colleagues in the Armed Services Committee to spend some fraction of whatever budget they come up with on the civilian applications of that technology. It would be a tragedy if that technology gets developed only for military purposes and by some oversight of who is responsible for it they stop looking at what the civilian reactors might look like or what the civilian applications could be. It is a relatively small amount of additional money that is required for that and I would urge you to take an interest in it.

[Slide 4 shown.]



INDUSTRY HAS THE SKILLS TO DEVELOP FUSION
AS A PRACTICAL ENERGY SOURCE

EXAMPLES INCLUDE:

SYSTEMS DESIGN, ANALYSIS AND MANAGEMENT

BDM CORP., BECHTEL GROUP, BOEING ENGINEERING AND CONSTRUCTION CO., BURNS AND ROE, INC., EBASCO SERVICES, EDS NUCLEAR, INC., EXXON NUCLEAR, INC., GILBERT COMMONWEALTH ENGINEERS AND CONSULTANTS, JAYCOR, W. J. SCHAFER ASSOCIATES, INC., SCIENCE APPLICATIONS, INC., STONE AND WEBSTER ENGINEERING CORP., AND TRW

FACILITY CONSTRUCTION, COMPONENT DEVELOPMENT AND MANUFACTURE

AYDIN ENERGY DIVISION, COMBUSTION ENGINEERING, GENERAL DYNAMICS, HIPOTRONICS, ILC TECHNOLOGY, MAXWELL LABORATORIES, MCDONNELL DOUGLAS ASTRONAUTICS CO., THERMO-ELECTRON CORP., UNITED ENGINEERS AND CONSTRUCTORS, UNIVERSAL VOLTRONICS, AND WESTINGHOUSE

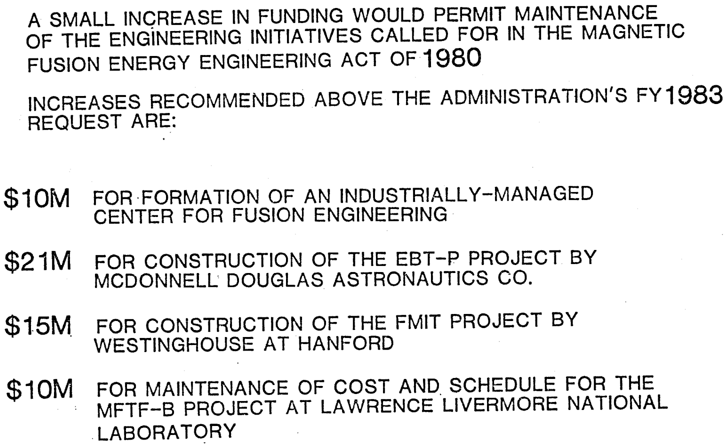
FUSION FACILITY OPERATIONS

FUSION ENERGY CORP., GENERAL ATOMIC CO., KMS FUSION, INC., AND MATHEMATICAL SCIENCES NORTHWEST, INC.

SLIDE 4

Just to give you some examples which I have taken from my own membership, the industry is out there and ready to do this job. You notice the kind of skills I think they can bring to bear on this problem: systems design analysis and management; facility construction; component development and manufacture; fusion facility operations. These are not research topics. We are not trying to get the industry in here to usurp the scientific roles of the laboratories and the universities. These are the kinds of skills that, in fact, the industry excels in and the laboratories and universities do not excel in. This is a complementary function that needs to be brought into this program.

[Slide 5 shown.]



A SMALL INCREASE IN FUNDING WOULD PERMIT MAINTENANCE OF THE ENGINEERING INITIATIVES CALLED FOR IN THE MAGNETIC FUSION ENERGY ENGINEERING ACT OF 1980

INCREASES RECOMMENDED ABOVE THE ADMINISTRATION'S FY 1983 REQUEST ARE:

$10M	FOR FORMATION OF AN INDUSTRIALLY-MANAGED CENTER FOR FUSION ENGINEERING
$21M	FOR CONSTRUCTION OF THE EBT-P PROJECT BY MCDONNELL DOUGLAS ASTRONAUTICS CO.
$15M	FOR CONSTRUCTION OF THE FMIT PROJECT BY WESTINGHOUSE AT HANFORD
$10M	FOR MAINTENANCE OF COST AND SCHEDULE FOR THE MFTF-B PROJECT AT LAWRENCE LIVERMORE NATIONAL LABORATORY

SLIDE 5

Finally, I would like to make recommendations on additions to the budget. They are the identical recommendations that Ed Kintner and Mike McCormack made: That is not an accident. Mike and Ed and I put our heads together several weeks ago to ask: What could we really come up with that would be helpful to this committee? If this committee, in fact, were able to come up with these dollars, we could at least attempt to keep this program on track even though not on the schedule envisaged in the Fusion Act.

There are these four items:

Ten million dollars for the formation of an industrially-managed Center for Fusion Engineering. There is no advocate out there for it. There is no laboratory going to come in here and lobby for it. The industry is not sure who should lobby for it because there is no competitive process going on and no contractor has been selected. But that is a very important recommendation in this budget.

I mentioned earlier the EBT project: It has been started. It should be finished.

The FMIT project, which Mike McCormack mentioned and which Dominic Repici indicated to you is important even to the private sector programs should be completed, and the MFTF project, I think probably the most important of all, you have got to keep that machine on schedule or you are going to pay a very big price tag financially on it and you are also going to hurt the chances that we will be able to make a clear decision on our first energy-producing reactors between tokamaks and mirrors.

Thank you.

Mr. YOUNG. Thank you very much, Dr. Dean for your fine testimony.

[The prepared testimony of Dr. Dean follows:]

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STATEMENT OF
DR. STEPHEN O. DEAN
PRESIDENT
FUSION POWER ASSOCIATES
TO THE
SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION
OF THE
HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY
MARCH 24, 1982

Fusion Benefits to Society

The astounding changes in living standards of the twentieth century are directly traceable to advances in technology. There is no evidence that the rate of technological innovation has slowed; in fact technological change is impacting our lives at an ever increasing rate. Looking to the future, history cautions us not to be too presumptuous of our ability to predict it. Who, for example, would have predicted 50 years ago the widespread use of microprocessors and a growing market in personal computers?

A common denominator which underlies modern society is the availability and affordability of energy. Here, many would have us believe that the world is soon to run out of it. Others believe energy is abundantly available and the whole problem is one of economics. Still others believe the energy problem is primarily one of who owns the most easily used resources. The truth is, of course, that all these views are partly correct but not complete and that the true energy problem is a complex one which defies a unique "solution". Predictions of future demand and use patterns, let alone economics, is a fiction of the professional energy analyst. Systematic analysis is worthwhile; but we should not claim any great precision for the resulting forecasts.

Many sources of energy are in use today: fossil (coal, oil, gas, wood), nuclear fission solar, etc. Each has advantages which result in its use for many applications. Each has its list of difficulties. Coal, for example, is difficult to mine, transport and burn in a safe and environmentally acceptable manner. Many nations of the world have no coal. Oil is now largely controlled by a cartel which has the power to set prices and availability in a way which could destroy economies and be the source of world war. Nuclear fission deployment has spawned unprecedented social resistance and has been regulated to the point where the costs of construction are primarily tied to licensing delays. And so on. Still, each of these sources of energy is playing an important role in today's world. Technological advance does not stand still, however, and we can already begin to see that a new energy source, fusion will emerge early in the twenty-first century.

"TO TAKE FROM THE SEA AND TO IMITATE THE SUN"

Fusion is the energy source of the Sun and stars It is the combination of hydrogen at high temperature to form helium. Fusion is the opposite of fission, the process in which heavy atoms (uranium, plutonium) are split, releasing energy. For a given weight of fuel, the energy released by fusion exceeds the energy released by fission and far exceeds (by millions of times) the energy released in any chemical reaction (e.g., the burning of coal or oil). Practical amounts of fusion energy will be produced in a fusion power plant.

Fusion takes its primary fuel, deuterium, from the sea and the reaction is so energy efficient that a 1000 MWe power plant can run for a whole year on only one pickup truck load of fuel. Contrast this with a coal plant of the same size which would require 191 trains of 110 cars each to deliver the coal and a similar number to haul away the ash.

Energy from fusion may be used in many ways to benefit society. For example, fusion may be used to:

- Make electricity.
  - Studies show that the cost of electricity should be roughly comparable to that from other energy sources in the next century. Abundant electricity generated from non-polluting sources would have a major impact on future society, e.g., through the widespread use of electric vehicles.

- Produce hydrogen.
  - Hydrogen may be burned with oxygen (to form water vapor) and used, for example, as a fuel for transportation. It is also an essential ingredient in most synthetic fuel processes.

- Make fissile material.
  - The neutrons from fusion reactions are efficient producers of fissile material for use in nuclear fission applications. For example, a fusion reactor could produce enough nuclear fuel to supply up to 20 fission reactors of equal capacity.

Other uses of fusion energy include most industrial processes and potentially new chemical and metallurgical processes. In the long-term, fusion may be used as a source of energy for propulsion of space vehicles. It has by far the highest specific impulse of any propellant and is the only known potentially practical way to reach speeds which are required for exploration beyond the solar system.

Environmental Characteristics

As world population and energy consumption inevitably increase, environmental damage (some potentially irreversible) becomes of growing concern. Most of the current resistance to the widespread use of coal and nuclear fission stems from fears of permanent damage to our environment, health or safety. Less widely known is the massive mining of materials which would be required should solar energy systems, as now conceived, be deployed as a baseload energy source for society. Fusion energy should be environmentally and socially acceptable because:

- There are no combustion wastes.
  - The product of the fusion reaction is helium--a non-toxic, non-radioactive, valuable gas.

- Radioactivity induced by neutrons in structural materials is in a more benign form than in fission reactors.
  - Calculations indicate that the biological hazard potential from postulated accidents is hundreds to hundreds of thousands of times less than similar postulated accidents in fission reactors.

- There is no possibility of a runaway nuclear reaction.
  - The fusion process is difficult to initiate and sustain. Any malfunction immediately quenches the reaction.

The mining, milling and transportation requirements for materials are smaller for fusion plants than for other energy sources and there is no requirement for transportation, reprocessing or long-term off-site storage of spent fuel.

Readiness for Engineering Development

The Magnetic Fusion Energy Engineering Act of 1980 sets forth clear goals and timetables for fusion development. Several prestigious review panels have certified the readiness of the fusion program to move into engineering development and their recommendations were incorporated into the fusion Act.

Rapid progress in fusion energy development has occurred in recent years. Success, from a scientific viewpoint, is now assured. Although there remain many difficult engineering problems to be overcome, all have potential technical solutions.

Industry has played an increasing role in fusion development. Today there are many industries with proven skills to contribute, indeed to provide leadership, to the engineering development phase of fusion. Key areas of industrial expertise include (1) systems design, analysis and management (2) facility construction, component development and manufacture and (3) fusion facility operations. Examples of companies which have experience in these areas are shown below.

Systems Design, Analysis and Management

BDM Corp., Bechtel Group, Boeing Engineering and Construction Co., Burns and Roe, Inc., Ebasco Services, EDS Nuclear, Inc., Exxon Nuclear, Inc., Gilbert Commonwealth Engineers and Consultants, JAYCOR, W. J. Schafer Associates, Inc., Science Applications, Inc., Stone and Webster Engineering Corp., and TRW

Facility Construction, Component Development and Manufacture

Aydin Energy Division, Combustion Engineering, General Dynamics, Hipotronics, ILC Technology, Maxwell Laboratories, McDonnell Douglas Astronautics Co., Thermo-Electron Corp., United Engineers and Constructors, Universal Voltronics, and Westinghouse

Fusion Facility Operations

Fusion Energy Corp., General Atomic Co., KMS Fusion, Inc., and Mathematical Sciences Northwest, Inc.

DOE and the Fusion Act

Two specific events of the past year suggest that DOE has been lax in complying with the fusion Act. First, in a July 7, 1981, letter from Secretary Edwards to Congressman Fuqua, Edwards states "We have determined that it is premature to establish fully the national Magnetic Fusion Engineering Center (CFE) at this time." In response (July 30, 1981) Mr. Fuqua said, "I would ask that you reconsider your present intention...and instead move forward within your FY 1982 budget, with formally soliciting proposals from all interested parties on establishing a CFE management scheme and operating team." The DOE has not responded further, and there is no evidence that they intend to take any action on the CFE. A second example is a January 29, 1982, letter of Secretary Edwards to Congressman Fuqua explaining why DOE has found it "impossible to prepare a Comprehensive Program Management Plan (CPMP) for Fusion by January 1, 1982" (as required by the Law). Edwards says this "impossibility" was due to "the uncertainty which has accompanied development of the budgets for Fiscal Years 1982-1983 and the fact that these budgets do not support the pace of the program development envisioned in the Act." Mr. Fuqua responded (February 23, 1982) saying, "The language of the Act and the intent of Congress with regard to the CPMP are clear. It does not seem to me that the fact that the Fiscal Year 1982 budget request for the magnetic fusion energy program was below the level envisioned in the Act and the subsequent uncertainty in the final appropriation process can serve as a basis for ignoring the requirements for preparation and transmittal of a Comprehensive Program Management Plan."

Recommendations

I recommend that the Congress reiterate to the DOE its expectation that DOE implement the engineering initiatives contained in the fusion Act, including the shifting of appropriate responsibility to industry.

A small increase in funding would permit maintenance of the engineering initiatives called for in the fusion Act. The increases which I recommend be added, above the Administration's FY 1983 request, are:

- $10M for formation of an industrially-managed Center for Fusion Engineering
- $21M for construction of the EBT-P Project by McDonnell Douglas Astronautics Co.
- $15M for construction of the FMIT Project by Westinghouse at Hanford
- $10M for maintenance of cost and schedule for the MFTF-B Project at Lawrence Livermore National Laboratory

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BIOGRAPHY

STEPHEN O. DEAN

PRESIDENT AND CHIEF EXECUTIVE OFFICER

FUSION POWER ASSOCIATES

Stephen O. Dean is president and chief executive officer of Fusion Power Associates, a non-profit association formed to pursue the development and utilization of fusion as an environmentally attractive energy option.

Prior to forming Fusion Power Associates in 1979, Dr. Dean was, from 1972 to 1979, Director of the Magnetic Confinement Systems Division of the Office of Fusion Energy at the U.S. Department of Energy and its predecessor agencies, the Energy Research and Development Administration and the U.S. Atomic Energy Commission.

From 1968 to 1972, Dr. Dean worked as a research physicist at the U.S. Naval Research Laboratory, where he performed experiments which were among the earliest in the field of laser fusion.

Born in Niagara Falls, New York on May 12, 1936, Dr. Dean earned a B.S. degree in Physics from Boston College, a Master of Science degree in nuclear engineering from the Massachusetts Institute of Technology and a Ph.D. in physics from the University of Maryland.

Mr. Young. Mike and Ed, can you tell us your understanding of where the FMIT project stands today? How many more dollars do we need to complete the effort and when can we expect to start getting data out of the device?

Mr. McCormack. Mr. Chairman, I will submit for the record the total number of dollars required to complete the FMIT. We have recommended $14 million this year. The status of the program is that excavation is completed, and that, at Los Alamos work on the accelerator is essentially complete. Facility design has been completed, and a construction bid package could be offered any time. In short, if the Department of Energy would do now what the Congress said last year it should do now, a bid package for construction could be offered at once; and the plant could be on the line by 1985 or 1986.

Mr. Chairman, it should be emphasized that the FMIT is absolutely essential to every fusion project in the free world. The European Economic Community and the Japanese Government have, in reliance on the actions of this Congress and previous administrations, refrained from building such a machine themselves. It is utterly unrealistic to contemplate building of the fusion engineering devices being contemplated in our Federal program or overseas without first studying the characteristics of candidate metals for the first wall, and so forth. This can only be accomplished using the FMIT. The entire free world is dependent upon and waiting for the FMIT.

Mr. Young. Mr. Kintner, do you want to address that? How many more dollars do we need if you have that information?

Mr. Kintner. The total estimated costs for the project was $105 million on its previous schedule. What it would be now, I am not prepared to say.

I would like to add that I think if you took one individual project—lay aside the question of time and priority—but one individual project that is vital to fusion worldwide it is FMIT.

Mr. Young. Dr. Dean, you talked about a system management organization and systems management approach. Could you elaborate a bit more on how this might be done?

Dr. Dean. Yes, the situation is very analogous to the way, for example, that NASA managed the Apollo project or the Defense Department manages a major weapon systems development.

A fusion device and reactor is a very complicated object, it has many different subsystems: magnet subsystems, vacuum subsystems, power conversion equipment subsystems, materials, and so on. The skills to develop these various subsystems reside in different places. Somebody has to pull all of that act together in a way that minimizes the cost and schedule of the overall program. You can put contracts out with the various people to do development in these various areas, but if you do not have somebody who sees what is needed and when, so that the whole program fits together in some way that minimizes cost and schedule, then you will take a very long time and you will waste a lot of money. The systems approach is to look at the total problem, break it down into its subelement pieces and then find the people to do those things that are needed. Then, when a budget cut comes, you look at the schedule and you say, "OK, if this one slips a little bit, it is not so critical, but if that one does, it is critical." Then you adjust all of your program budgets and distribution in terms of an impact on the total flow of the total program as a system. This kind of approach is what is needed to be laid out in the comprehensive program management plan you have asked the DOE for. But it has not only to be laid out, the program has to be managed in a way to implement the decisions that way on a regular basis.

Mr. YOUNG. Thank you, Dr. Dean.

Mr. Kintner, on page 9 of your testimony, you say that:

> Fusion has no precedent as a research and development program if you say that not only because it is so long term, but also because it is different in kind from its large predecessors, such as fission energy or space exploration.

How is fusion different in kind from fission energy or space exploration?

Mr. KINTNER. Well, first, the important difference has to do with the fact that these other major development programs were, fundamentally, military—had military objectives, and that makes it much more difficult to obtain the persistent support needed to carry through a program like fusion.

I believe also that, taken as a whole, the variety of technological advances that must be made for fusion to be successful is broader than it was in space or than it was in fission, at least if one includes the original development of fission as simply for weapons.

It will take several decades, and that requires a kind of persistence and determination over a longer period of time than either of these other two programs have required. That is what I meant when I said it is "different in kind."

Mr. YOUNG. Very good.

Dr. Dean, do you agree that the first commercial application of fusion energy may well be the fusion-fission hybrid? Do you think the Department of Energy's program is addressing this technology in sufficient detail?

Dr. DEAN. I think it could well turn out to be. As, I think, Dr. Matson pointed out, it depends on the marketplace for fission reactors. If, in the year 2000 or even earlier, utilities are buying a lot of light-water reactors, they will want an assured fuel supply and the hybrid could provide them the confidence to order light-water reactors.

I think producing fuel would be an easier market for fusion to compete in, if there is a market, than making electricity, because the electricity market is very difficult to crack. The people who buy electric powerplants want to know that they are reliable, that they will not go off the grid very often, and when you come up with your first generation of fusion reactors there will not be much operating experience so they will be nervous about buying them. So, if you can have an application that generates fissile material or generates hydrogen or something like that, that is off line, you can stockpile it and you do not have this problem that, if the machine shut down every once in a while for unscheduled maintenance, that that really affects your overall economics deliteriously.

In terms of whether the Department's program addresses that, I think the physics and technology in general does support any of these applications. But I do think they need to spend more money on systems design and analysis in order to identify what the unique features are and then, perhaps, start some unique development programs that might be needed for that and I think they have been somewhat remiss in doing that. A couple of years ago fission was so unpopular with the administration that to even talk about this application of fusion was an anathema. I think that attitude has gone away.

Mr. McCORMACK. Mr. Chairman, may I comment on that?

Mr. YOUNG. Mr. McCormack?

Mr. McCORMACK. Steve Dean and I have not discussed this concept previously, and I would like to take a slightly different position with respect to it than he has.

I think it would be a bad idea to develop a hybrid, and I recommend strongly against doing so. I think there are overwhelming reasons for not doing so.

In the first place, the Members of Congress and the public conceive of fusion as providing a clean, unlimited resource that is free from the problems of fission products or meltdowns. I think we should keep faith with that concept.

I think that if we need experience in operating fusion plants before we start producing electricity, there are a number of ways to do this other than making plutonium. For instance, fusion machines can be used as a heat source for making alcohol, or making steam for extracting heavy oil from underground. The hybrid was advocated primarily by individuals who were opposed to the breeder, but who wanted to go ahead with their own program for making plutonium. I think it would be a mistake to follow them.

I think that one of the reasons a hybrid looks attractive is because the disadvantages that go with a conventional nuclear towerplant are not taken into account. Today the laws regulating fission plants require that plants be located a long distance from cities; and we are all aware of regulatory delays associated with getting fission plants on the line.

On the other hand, I can easily foresee small pure-fusion machines being placed underground within our cities. There is no reason that this could not be done. There is no significant residual heat or problem of meltdown and the amount of radioactivity associated with a pure-fusion machine, is quite small as compared to a fission plant; and what radioactivity that does exist is in a much more benign form.

Thus it may be possible to eliminate long transmission lines, which are very expensive, environmentally offensive and vulnerable to sabotage. Small fusion plants, located at various underground sites within large cities, could be connected directly to the electric distribution system within those cities. If one takes into account the economics of transmission lines, which I do not think has ever been done when evaluating a hybrid as compared to a pure-fusion plant, the economics would look a good deal more attractive for pure fusion.

Mr. YOUNG. Good point. Thank you, Mike.

Mrs. BOUQUARD. Thank you very much, Mr. Chairman.

I certainly appreciate the testimony of you very knowledgeable gentlemen today.

As I was listening to you it seems the only common thread that I see in any of our energy programs as we have discussed our budget issues this year, is that everything we do is piecemeal. There is no continuity in any of our energy programs; we approach everything on a feast or famine basis. In reality we say that we would like to have international cooperation, that we want industrial cooperation, but the very process and the way we go about any of our energy research and development programs, we in effect go against the thrust that we say we want to push forward. I do hope that this administration—all the administrations realize it. We must have a policy on energy that will go beyond a 4-year span.

I was very concerned about the budget, of course, for our fusion program. We have read a lot about the interference of OMB and OSTP and the formulation of this budget. It seems to me—Mr. Kintner, you may be interested in replying to this—Mr. Keyworth's philosophy on fusion is that it has been oversold, has not made much progress in the last 30 years, and that we should redirect our activities to science only. Did you see this sort of philosophy reflected in the budget?

Mr. KINTNER. Well, that is what concerned me most with the circumstances as I saw them and what, eventually, forced me to resign. I had no choice in the matter because I could not lend myself to that kind of a judgment or that basic approach to expenditures of sums of this magnitude.

It does seem to me that the fundamental issue with regard to fusion is whether we are going to work hopefully and deliberately toward practical goals, or we are going to turn the program back to what it was 8 or 10 years ago, a scientific investigation into plasma physics.

Mrs. BOUQUARD. Well, you say that doing basic plasma physics that really $440 million is too much to spend.

Do you think that the program is really being set up for cuts by its enemies within the administration?

Mr. KINTNER. I do not know how to answer that question——

Mrs. BOUQUARD. What sort of cuts might occur?

Mr. KINTNER. I would only observe that if, in fact, one wished, downstream, to make further reductions, putting the program in this particular position makes it far easier to do so, yes. I observed that. I observed it before. I do not know whether that is the objective or not but it clearly does, in fact, put the program in that vulnerable position.

Mrs. BOUQUARD. Suppose we had cuts of $100 million. Would not this pretty well take us back to the drawing board?

Mr. KINTNER. No. We will always have useful experiments going on. We have a broad program. We have a number of very effective organizations at work. There are useful things being done in the rest of the world; we would not go back to the drawing board so far as scientific investigations are concerned. But the science is absolutely useless unless the practical engineering to put it to use is also available not the time that the scientific knowledge is available. That was the basic thrust of the program and the act and that is what I fear is being lost.

Mrs. BOUQUARD. Mike, I could not help but notice the fact that you said that the Magnetic Fusion Energy Engineering Act of 1980 passed with only seven dissenting votes—what a difference from this Congress. I cannot recall any bill with a dollar sign on it passing this Congress with only seven dissenting votes. It has not happened.

Mr. McCORMACK. Have faith.

Mrs. BOUQUARD. I think that all of you stated that $56 million would apparently take care of the underfunded programs to permit the maintenance of the initiatives that were called for in this act, such as $10 million for the Center for Fusion Engineering; $15 million for FMIT; $21 million for EBT–P; and, $10 million for MFTF-B; for a total of $56 million.

Well, let us try a different scenario. Suppose we do not get the additional $56 million. What redirection of funds would you recommend within our constraints of $444 million? Where can we shift? Who wants to answer that?

Mr. McCORMACK. Nobody wants to answer that. [Laughter.]

Madame Chairman, that is something like asking a theoretical question: "in case of a fire, which one of your children would you save?"

I do not think we can answer that question, but each of us may wish to respond.

The success of the fusion program from its very inception has been based upon: (1) the concept of an unlimited source of energy for mankind; and (2) teamwork within and between the laoboratories, thus avoiding cut-throat competition, with someone trying to sacrifice someone else's program. .

No one of us can say, "Well, I will sacrifice this child to save that child." I recognize that this creates a potential hardship for you. I can only say that if you have no choice, if you cannot authorize more than $440 million, then certainly we would be willing to try to help you. However, I do not want at this time to say, and I hope I never have to, what program I would sacrifice in order to keep the program going.

Mr. KINTNER. Can I speak to that just a minute?

Mrs. BOUQUARD. Please, Ed.

Mr. KINTNER. I think that Mr. McCormack's analogy of which child do you wish to get rid of is a good one. Each one of these projects should be recognized for what it is. It is not something that jumped up whole cloth this year. We worked 5 years on selection of a site, on selection of a concept for the Fusion Materials Irradiation Test, on the development of the accelerators, on the architect-engineering work; 5 years and $60 million have been spent as of now on that project. It is like a child, you have now got him through first grade and you do not want to give up on your investment. On the Elmo Bumpy Torus proof of principle we spent a full year trying to decide which one of the various concepts was the most important to our program as a whole, would make a major contribution to the physics understanding and would have the greatest potential from a practical point of view. Then we spent another year—a year and a half—in the competitive process which eventually resulted in the selection of McDonnell Douglas as a lead contractor. That program is 4 years old now; it is not something that is just a creature of this year in this budget.

Similarly, with regard to the Fusion Engineering Device and Center for Fusion Engineering, we studied that question for 6 years and went through a number of cycles until it came down to the present one which was, in fact, accepted by the Buchsbaum Committee and by the Congress as the proper approach, the proper strategy and the proper thrust for future development. So these are not just questions of individual projects.

I think the program must be thought of as a focused, organized, intellectually sound, technically honest program and not as a collection of projects. That, also, is one of the reasons that I felt most important to make an issue out of the changes which were made in the budget this year.

Mrs. BOUQUARD. In listening to your testimony, you mentioned the RIGGATRON concept. Mike, what is your opinion of the merits of the RIGGATRON concept?

Mr. MCCORMACK. I think they are excellent, Madam Chairman. I am quite optimistic that the RIGGATRON will succeed in demonstrating a successful fusion reaction in the 1984–86 time frame. I certainly hope so.

I agree with Dr. Don Repici that there are ways that the program can be helped by DOE studies: materials testing for first wall and blanket materials, for instance. Also the DOE can provide computer codes which, as he said, have been paid for by the taxpayers. There may be other similar ways in which the DOE can assist private industry in magnetic fusion RD&D, either with information already in hand or in studies to be done. This should be a DOE policy.

I think of the RIGGATRON as a parallel program with the Federal fusion efforts. I am closer to the RIGGATRON, so I cannot speak as well about the OHTE program, but I think of RIGGATRON and the main line Federal program as insurance against each other's possible failure. We cannot be absolutely certain that either one of them is going to succeed; the odds are that both of them will sooner or later. The question will be: Which one will provide the greater potential for commercial application, and when? Of course, if the RIGGATRON comes on the line first, it will have a profound effect upon all the other energy programs, in every technology anywhere in the world. I am optimistic that the RIGGATRON will succeed, and I wish the INESCO team all the luck in the world.

Mrs. BOUQUARD. Dr. Dean, I received a letter from you recently on behalf of the board of directors of the Fusion Power Associates expressing your concern and their concern as well that our Government is failing to give proper recognition to the potential civilian applications of inertial confinement fusion. What specific energy applications not now being funded would you recommend funding and at what level?

Dr. DEAN. Inertial confinement fusion, potentially can serve all the energy applications that magnetic confinement can serve in terms of civilian applications. What is needed is systems studies and analyses of these applications for inertial confinement system schemes. I do not know exactly how much money is needed for those kinds of studies. My guess is that it is on the order of $10 million per year. I think they have that amount of money that they could make available for it in their budgets, and there are people working in the field that want to spend that kind of money on those things. The thing that worried us and worried me was that there was some pressure coming from the military committees and the military management saying, "do not work on these civilian applications because this is military money and we do not want you to spend it on these civilian studies." I think that all concerned have to take a little broader view than that. If it is $10 million out of a $200 million program that protects your civilian applications in the long run, one ought not to be so hardnosed as to what color money it is, it seems to me.

Mrs. BOUQUARD. Do you think that this research should be classified such as magnetic fusion research was in the late 1950's?

Dr. DEAN. Absolutely not. I think this classification situation is a disgrace and it ought to be forthwith abolished.

Mrs. BOUQUARD. Thank you very much. Thank you gentlemen. Thank you, Mr. Chairman.

Mr. YOUNG. Mr. Volkmer?

Mr. VOLKMER. Thank you, Mr. Chairman.

I just have a few comments to make and I will ask one question. I appreciate the panel's comments, especially on the hybrid and where Mike fears that might lead us. But that leads me to another thought in looking over the total budget that we receive from the Energy Department and this administration on research and development on all our energy. I am just wondering, do you see any possible competition between those who are promoting the use of fission and breeders and fusion?

Mr. MCCORMACK. No, Harold, I do not.

I know that there are some individuals in this country—not very many, but a few—who fear that funding for Clinch River will harm funding for fusion, or funding for fusion will cause cutbacks in funding for Clinch River. I think that is a false premise, and I think that you and this committee should insure that it remains a false premise. I think that each one of these projects must be evaluated on its own merit and funded accordingly.

Mr. VOLKMER. Let me interject a moment.

Mr. MCCORMACK. Certainly.

Mr. VOLKMER. We are only going to have so much need for so much electrical energy in the year 2000, 2020, and 2050. If we are able to obtain that energy by use of fusion, then why do we need fission? If we are going to do it through fission, why do we need fusion?

Mr. MCCORMACK. I made a presentation on this subject to the January meeting of the American Association for the Advancement of Science, and I submitted that testimony for the record today. I will also provide you with a copy.

To summarize, I will start with President Carter's assumptions for energy requirements for the year 2000.

Mr. VOLKMER. They are probably a little high.

Mr. MCCORMACK. Perhaps. Even so, they assume energy consumption equivalent to 2½ times our present coal production and, in addition, 400 nuclear plants on the line, along with the most optimistic scenario for every other energy technology, including oil, gas, solar and geothermal energy, waste conversion and alcohol production. This would still require 400 gigawatts of nuclear power and 2½ times our coal production to make President Carter's conservation target of only 56 million barrels of oil per day equivalent in the year 2000.

Let us assume that magnetic fusion is demonstrated in the year 2000, and that the first practical fusion plant goes on the line in 2005. Assume also that there will be only 1-percent growth per year in total energy consumption from the year 2000 to the year 2070, then total energy consumption will be equivalent to 112 million barrels of oil per day in the year 2070. If we assume that the equivalent of 75 million barrels of oil of that is for electricity, and that only half of that from fusion, that would still require 1,250 gigawatts from fusion. Thus would mean that, starting in 2001, we would be required to have a new 1000 MWe fusion plant on the line every 20 days for 70 years, assuming nothing for replacement of worn-out plants. Now, that is another way of saying we need absolutely every bit of energy we can get, from every realistic technology, from now until sometime in the middle of the 21st century, just to keep up with minimum growth. Most of it will be electricity.

Mr. VOLKMER. Why, necessarily, would you want to replace the fission system?

Mr. MCCORMACK. My personal priorities are as follows:

When fusion becomes available and competitive for generation of electricity, I would immediately terminate the use of coal. I would already have terminated the use of oil and gas except for peaking purposes. I would immediately terminate the use of coal because of the obvious pollution and environmental hazards associated with coal. Then, as each individual fission plant completes its normal lifetime and is ready for decommissioning, I would replace it with a fusion plant, so that by sometime in the second half of the 21st century, we would be fundamentally on fusion. It is obviously a much cleaner source of energy than any other significant option.

Mr. VOLKMER. In other words, you are saying there really is no competition at this time. The competition—it is just a timeframe in which you are trying to utilize different forms to generate electricity?

Mr. MCCORMACK. That is correct.

Mr. VOLKMER. You would still be using fission up to 2050 or 2060?

Mr. MCCORMACK. Yes, and we will be using coal until 2010 or 2020. We need absolutely every bit of energy we can get, from every source we can get, especially for electric energy generation from now on, from this day forward. This is the second most important element of the energy crisis, the first, of course, to reduce our dependence on imported oil and the vulnerability associated with this dependence. However, the need to produce adequate electricity for this country is going to be more and more critical with every passing day.

Mr. VOLKMER. Let me ask you the next question then. Suppose we go back and we follow the administration's proposed budget, and let us assume that 1984 is a lot like 1983. Suppose also that you go along with the assumption that proof of concept is really a demonstration and not basic research and the Government is not to be involved in that. If we continue with this logic for 4 or 8 years, where does that put us with respect to these projects that we need to continue such as the FMIT and the Elmo Bumpy Torus? If we do not do those things, who will? Private industry is not going to do those.

Mr. McCormack. Well, unless a private fusion machine succeeds, this country will be in absolutely desperate economic trouble. It will make the present economic problems seem like a Presbyterian Sunday school picnic.

Mr. Volkmer. After the year 2020 or 2030?

Mr. McCormack. We will be in trouble after 1990 with respect to electric generating capacity, and it will keep getting worse and worse and worse.

Mr. Volkmer. Well, fusion will not be there in 1990.

Mr. McCormack. I hope it will be, but we can't count on it. That is why we need to be pushing the fission program and synthetic fuels now.

Mr. Volkmer. Thank you, Mr. Chairman.

Let me just conclude. In other words, what you are saying is that this extra $56 million on top of the $444 is an investment in the future?

Mr. McCormack. Without qualification; and it is probably the most important investment in the future that this Congress could make.

Mr. Young. Just one statement and all three of you can answer or just one.

Is the present level of the private sector's financial and manpower commitment in fusion energy development appropriate at this time, do you think? Any one of the three of you may answer.

Mr. Kintner. Well, Steve may have a different view than I do on this matter but I think it is not.

By that I mean that it seems to me fusion offers sufficient opportunities for technology development and is sufficiently important to the future that there should be more commitment on the part of industry as of now to its development. They would benefit from it far more than they realize should they become involved. But, it is perfectly obvious also that industry's interest depends upon the interest of the Government; if the Government is interested in fusion from a practical, technologically advanced point of view, then industry will become interested. If it is not, then industry will fall away.

Mr. McCormack. May I also answer your question, Mr. Chairman.

I agree with what Ed said. Industry is not going to become involved unless they see that there is a continuing program in which it can participate. We forecast a cost of $20 billion over 20 years. Industry, cannot fund a program of that magnitude. It can—and I think it will—participate more and more heavily if there is a realistic program that has continuity. Of course, INESCO is operating exclusively with private funding, and may succeed before the Federal program.

Mr. Young. Would you like to summarize?

Mr. McCormack. Thank you. On behalf of Ed Kintner and Steve Dean, I thank you for inviting us to testify today.

Each of us, in one way or another, has made a personal commitment to the fusion program. In each case, it is without any significant pecuniary reward or promise.

We are here to encourage you to consider several basic facts. The first is that the Congress has always directed the fusion program. It is a strange fact of history that the Congress has always done this in some detail. Starting in 1973, we line-itemed seven or eight items and said, "do this for the fusion program." We have been doing essentially the same thing ever since, including the passage of the Fusion Act of 1980. It included significant detail.

Last year, this committee and other committees of Congress did so again when you rejected deferrals and rescisions requested for certain projects such as the FMIT and the Elmo Bumpy Torus. Nobody has ever run the fusion program as well as the Congress and the Director of Fusion Energy. I do not think it is appropriate to leave it suddenly to someone else whom you don't even know, especially in violation of the law.

Second, the other industrial nations of the world are already moving ahead of the United States in fusion engineering development. This has been going on for the last 12 months. They are increasing the pace of their programs as ours begins to decline.

Third, we emphasize that we are not criticizing the Department of Energy, or anyone in the Department of Energy. You on the committee served with me during the 4 years of 1976 to 1980 when, on several occasions, I commented that the witnesses coming here before us were obligated to say what they and we knew was not really what they believed, but what they were directed to say.

Yesterday you heard testimony from DOE spokesmen who said that they are following the law. That was a misstatement of fact. They are not following the law. They are attempting to cancel the CFE, the FMIT, and the EBT-P; and that is a violation of the law. Nothing they say changes that. Only what you say can change that.

In addition, we are certainly not criticizing anyone in the Congress. We appreciate the difficulties that you face, but, there is a fundamental difference between the philosophies of the Congress and the administration with respect to the fusion program. Remember that the Congress has always supported the fusion program.

When we proposed the Fusion Act in 1980, there were those who said, "You will never get it out of committee, you will never get it through the Rules Committee, you will never get the votes for it, you will never get it out of the Senate, and so forth", but the Congress supported the act because you believed in fusion as the ultimate source of energy.

There is a difference between that philosophy of moving forward with faith in the future—the philosophy of Congress with respect to fusion—and the new philosophy of defeat and disarray that has been written into this budget, imposed upon DOE by OMB and OSTP.

This has created the challenge you face now: whether or not this country is going to go ahead with a program to develop fusion electricity at the earliest possible time; whether we are going to go ahead with a fusion engineering and materials development program. On this point the Congress must be firm.

Thank you very much.

Mr. YOUNG. Thank you, Mr. McCormack.

Mrs. BOUQUARD. I want, again, to thank our excellent panelists for being with us today and for your input into our program. We do want to emerge with an effective energy program.

I would also like to thank Congressman Young for effectively chairing our hearing today. Thank you very much.

Mr. YOUNG. Thank you Madam Chairman.

I want to thank both panels for their candid comments today and I believe your testimony will help us to get back on the right track and make the strong commitment that this country has to make as we try to move into the 1990's and the year 2000.

I think it was very aptly brought out today, so on behalf of the committee, I do want to thank all of you for your fine participation.

The subcommittee will meet again tomorrow, March 25, at 9 a.m. in room 2325 to hear from the Department and non-Government witnesses on electric energy systems, energy storage systems, and small-scale hydroelectric programs.

The hearings for this morning are adjourned.

Thank you very much.

[Whereupon the subcommittee was adjourned at 11:50 a.m.]

[Appendix III, additional statements submitted for the record follow:]

APPENDIX III

ADDITIONAL MATERIALS SUBMITTED FOR THE RECORD

ADDITIONAL REMARKS BY HON. MIKE MCCORMACK

Madam Chairman, I was invited to present a paper on the "Implications of Fusion Energy Development" before the January meeting of the American Association for the Advancement of Science. A slightly modified version of that address is attached for insertion in the record of this Subcommittee's proceeding.

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Although there are many important areas of research and development -- not only in fusion but in many other disciplines as well -- it is no exaggeration, I think, to state that magnetic fusion research and development, especially engineering and materials testing, is now at least as important to the people of this nation, and of the entire world, as any other human activity. I make such a statement with full recognition of the importance of many other areas of research in medicine, biology, genetics, electronics, materials and other disciplines. The fact is, however, that the successful demonstration of a practical, economically competitive fusion electric technology will have a truly profound and almost instantaneous impact upon the entire world.

I am confident that scientific feasibility will be demonstrated in the TFTR and the MFTF-B, as well as such devices as JT 60, JET and a Russian device within the next 3 to 5 years. It was this confidence, and the recognition of the extraordinary potential value to the human race of the development of fusion power that lead the prestigious Fusion Advisory Panel chaired by Dr. Robert Hirsch and the ERAB Panel chaired by Dr. Sol Buchsbaum to idependently conclude in 1980 that the time had come for the United States to move into an aggressive program of magnetic fusion engineering development and materials testing, with the announced goal of building and successfully operating a magnetic fusion electric power demonstration plant by the year 2000. It was the confidence of these two independent panels, each composed of some of the world's outstanding plasma physicists, engineers, research scientists and corporate executives, that this country could succeed in an Apollo-like program to have a magnetic fusion electric power demonstration plant on the line by the year 2000, that generated in us on the House Committee on Science and Technology the conviction that this country should go ahead: that we should compose, introduce and work for the Magnetic Fusion Energy Engineering Act of 1980. It was that conviction that our nation could accomplish the goals set forth in the act that inspired almost unanimous support in the Congress in passing the legislation, in the public in supporting it, and in the Administration in signing it into law.

Setting this nation forth on an intense 20 year, $20 billion program is not a frivilous act. It was undertaken by the leaders of both political parties in the House and Senate, and the Administration after serious consideration of the critical problems this nation faces with respect to providing adequate energy for our people and, at the same time, a recognition of the extraordinary potential that nuclear fusion offers in solving this nation's -- and the world's -- energy problems. It was an act of faith, but it was a serious and deliberate act by responsible leaders of all political, philisophical and economic persuasions. It had almost unanimous support in the Congress of dedicated men and women who concluded that they were, at last, doing something truly worthwhile for this country and for the future. This was an act of statesmanship, deliberation and commitment. It represents the kind of dedication that has made the U.S. a great nation -- and a great people.

The rational for enacting this legislation may be divided into two parts: (1) the critical problems facing this country -- then and today -- with respect to the energy crisis, including the need for a dramatically increased domestic energy production capacity from environmentally acceptable and economically realistic sources; and (2) the limitless source of clean, practical, universally available energy that nuclear fusion offers.

To put the importance of making fusion power available as soon as possible into context, it's necessary to look at the big picture with respect to energy, and then explore the role that fusion will play. This takes us back to the realities of various aspects of the energy crisis. Underlying our concern is the fact that our paramount obligation really is to our country; to its stability, strength and prosperity, and to the freedom, health and continually increasing living standards of our people. Our next concern must be for international stability; for peace, freedom, and higher standards of living for all men and all women everywhere.

This is fundamental, but an adequate supply of energy for the United States and for the other nations of the world is essential to achieving those goals. It is essential for our nation and for our people. An adequate supply of clean cheap energy for all the people of the earth is almost certainly one prerequisite for a chance at world peace.

Our requirements for energy in this country, and for the world, will continue to increase in spite of any conservation program, however spartan and however successful. We will need all the energy we can get, from every realistic domestic source, for as far as we can see into the future. Furthermore, that energy must be as clean, safe and environmentally acceptable; as much as is practical now, and progressively more so with each passing year.

In 1977 President Carter suggested reducing this nation's energy consumption growth rate to about 2% per year between 1977 and the year 2000, thus reducing our total energy consumption from all sources to the equivalent of about 56 million barrels of oil a day in the year 2000 (down from 84 MBOE). President Carter's conservative goal assumed an extraordinarily spartan and successful conservation program, and it assumed a dramatic reduction in our imports of petroleum.

However, the people of this country, and their leaders, seem to have forgotten the energy crisis and the very real hazards that we face because of it. Our dependence on imported oil, and the economic, political and military vulnerability that results from that dependence is the <u>first element of the energy crisis</u>.

In 1981 we imported about 6 million barrels of oil a day, at a cost of about $70 billion and one of the largest trade deficits in history. After all of the political rhetoric about federal spending programs, deficits, inflation rates and tax relief, one fact which is being studiously ignored is that our trade deficits, caused by the importation of oil, are a significant factor in weakening our economy. We continue to be vulnerable to increases in the price of oil -- over which we have no control at all -- and each price increase has resulted in substantial inflationary pressures in this country for months thereafter.

The severe problems associated with the very real political, economic and military vulnerability caused by our continuous dependence on imported oil must be solved in this decade, and it is obvious that fusion energy can make no contribution to solving them. It should be observed, however, that there are programs which, if initiated, could make a significant difference in a relatively short time. In addition to continuing our successes in reducing our consumption of gasoline, we could dramatically reduce the 3 million barrels of oil we burn each day for space heating primarily in residences. This could be accomplished by installing instead heat pumps. They -- and the electricity they use -- would be cheaper over the long run for the homeowner than the oil they burn. Also, we can reduce the amount of oil still being used to produce electricity -- more than a million barrels a day, much of it for base load electricity. This is a scandal, and as much as anything betrays this country's unwillingness to face up to the first problem of the energy crisis.

In the longer run, with batteries perhaps twice as good as the present "top of the line" lead acid batteries, practical electric vehicles could be used as service vans and for commuting. They would be cheaper, far cleaner, quieter and completely practical for this use. By the end of the century, if 25% of our automobile fleet were electric vehicles, we would be saving about one and a half million barrels of oil a day, and U.S. manufacturers might regain their share of the auto market.

<u>The second element of the energy crisis</u> involves the need for adequate energy production capacity from realistic domestic sources -- to allow economic stability and adequate standards of living and national defense during the next 25 years. The 1980 Venice conference of Western leaders concluded that about 90% of the new energy generated between now and the end of the century must come from coal and nuclear fission. This is true for this country as well as the world in general. Other energy sources, including hydroelectricity, solar and geothermal energy and energy obtained from the burning of wood or the conversion of organic materials to useful fuels and energy would, in sum, produce no more than about 10% of our energy requirements during the next two decades.

If, in the U.S., we are to reach the production level set by President Carter's conservation goals, it will still be necessary to produce between 30-35 million barrels of oil equivalent from coal and nuclear fission in the year 2000. This is in addition to optimistic projections for energy production from oil, gas, hydroelectricity, solar and geothermal energy, energy from organic materials, etc. Thus, if we more than double our 1980 coal production capacity, it will still be necessary to have between 300-400 GWe of nuclear power on the line by the year 2000. It is obvious that such goals cannot be reached without the implementation of energy policies and programs involving major commitments which are not now contemplated.

This is a national tragedy. Even with spartan and successful conservation programs in all aspects of private life and in the industrial community, a significant decline in the standard of living of the American people, and a reduction in our industrial production capacity is almost certain to accompany our failure to meet our electric energy production capacity requirements. There is an extraordinarily close relationship between electric energy production, GNP and civilian employment in this country, especially since 1974. Although one cannot easily establish a mathematical proof of a causal relationship between Gross National Product and electric energy consumption, a little common sense helps. Most of our major wealth producing, job-producing industries are energy intensive. Many of them are electric energy intensive. It would be irresponsible at best to ignore these extraordinarily close relationships.

If one assumes that demonstration of fusion electricity will occur in the year 2000, then fusion power can have no impact in helping solve this problem between now and the year 2005. If, however, practical fusion power is demonstrated by 1990 by a small privately developed machine such as an OHTE or a Riggatron, then a beneficial impact -- although small in the total energy picture, could occur by 2000. I am optimistic that the Riggatron will succeed in demonstrating a fusion electric power capacity by 1990. Such a small fusion device would be extremely attractive, if one can be made that is technologically and economically feasible.

There are several apparent advantages that make any fusion electric facility potentially extremely attractive. We are all aware of the unlimited supply of cheap fuel, the freedom from fission-product build-up, and the freedom from the generation of large amounts of heat after shutdown. There are two other advantages, however, that merit mention at this time. One is flexibility. Even if small fusion machines (200 MWe or less) prove to be impractical (for any reason), large machines may still be located near, or even within, large cities, thus sharply reducing transmission line requirements. Transmission costs constitute a significant portion of the total cost of electric energy generation and distribution. They are a constant cause of litigation and controversy, and they are vulnerable to sabotage. Elimination of long transmission lines can be a significant advantage for fusion.

With small machines (fusion only -- not hybrids) we can contemplate locating individual generating facilities at various underground sites near load centers or trunk lines within cities, and connecting to the city's distribution system with super-conducting cable. It may also be practical to combine such facilities with super-conducting storage rings for peaking power requirements. Such an advantage for small fusion machines should make them extremely attractive.

There remains the third element of the energy crisis. It relates to environmental protection and to resource conservation. This nation, and the world, must -- as soon as possible -- sharply reduce the burning of fossil fuels to produce energy. This is obviously not possible now, and the rate of reduction will depend on many differing factors in each country. However, it must be our stated goal, if we are to protect our atmosphere from unacceptable levels of pollution, and our world from the Greenhouse effect; and if we are to protect our petrochemical resources from thoughtless depletion. They should be saved for future generations, and we should encourage other nations to accept this philosophy.

In this country, we should plan to limit the use of petroleum products and natural gas for the generation of electricity to peaking power only, and then only when such fuels are the most practical and economical. The use of oil and natural gas for residential heating and hot water should be discouraged, except when it is clearly cheaper and more practical than electricity, electric heat pumps or solar assisted heat pumps and hot water systems, or some combination thereof.

We have no choice but to depend upon coal for the next three or four decades for electric energy production, but this use should be phased out as soon as practical. Until that time, a maximum effort must be mounted to produce clean fuels from coal, and off-gas cleaning systems must be continually improved to reduce air pollution. Of course, it is impossible to know what the actual impact of the Greenhouse effect will be, or when it will appear. Hanging over our heads, however, is the possibility that we will "go over the cliff" without knowing it until it is too late, overload the world's atmosphere with carbon dioxide, and inflict terrible environmental damage on our world, from which it may take centuries to recover.

Our conventional nuclear power industry, backed by a future breeder program is, of course, our mainline protection today against environmental damage from the burning of fossil fuels, but just as doubling our present coal production by the year 2000 will still require having about 400 GWe from nuclear fission on the line by that date, so too will having 400 GWe of nuclear power on line still require us to about double our coal consumption -- or suffer crippling damage to our economy and our standard of living from loss of electric production capacity.

So we must depend upon both coal and nuclear fission until fusion power can begin to make its contribution. When this occurs, and if we can install electric production capacity sufficient for our needs, then we can start phasing out coal plants. Of course, no new coal plants should be started, once fusion power is available.

The implications of a successful program to develop fusion energy cannot, of course, be discussed in a vacuum. Every new energy source or technology must compete in a complex marketplace with all of the others. In addition, significant penetration of the energy market, even by a superior competitor, seems agonizingly slow, even under the best of conditions. Let us assume, for instance, that magnetic fusion electric generation is demonstrated in the year 2000, and that it is, for one reason or another, superior to every other energy source or technology. Let us then assume that, starting in the year 2001, a policy is established that all new energy generating facilities shall be fusion plants. Let us assume also that energy consumption in the United States will be equivalent to 56 million barrels of oil a day from all sources in the year 2000 -- equivalent to about 1850 GWe capacity from plants operating at 33% thermal efficiency and 67% load factor. If we assume only a 1%/yr. growth rate for total energy consumption starting in the year 2000, then our total consumption would be 112 MBOE in the year 2070. If we assume that 75 MBOE of our energy consumption is for electricity in the year 2070, and half of that from fusion, this would require 1250 GWe from fusion, or one new 1 GWe plant on the line each 20 days for 70 years. This schedule does not consider replacement of early fusion plants or the number of small plants required to total 1250 GWe.

Whether or not such a scenario is even remotely realistic may be debated at length, but I suggest that it may prove to be conservative. In addition, it emphasizes why it is so important to move toward a demonstration of practical, competitive fusion power as soon as possible.

There are other aspects of early demonstration that are important to consider. Many of our utilities are almost paralyzed today, failing to plan for the future because of regulatory and economic problems. They find it extremely difficult to plan to build new plants. No new nuclear plants, and only a handful of coal plants, and some of these quite small, have been ordered in three years. Still the demand for electric production capacity continues to grow, and the percent of reserve production capacity dwindles toward brownouts and blackouts. The Electric Power Research Institute predicts that if GNP grows at the rate of 2.5% to 2.9% per year from 1980 to 2000, then electric energy consumption will grow at 3.3% to 4.2% per year during that time. A growth rate of 3% per year would mean doubling in 23 years.

As I have observed, fusion power can make no contribution during this century unless a small machine such as OHTE or Riggatron is demonstrated by the year 1990, and only a small percent by the year 2000, even in that case. However, the demand will be there as soon as fusion power becomes available, and it will be tremendous.

In addition, I believe that demand will grow in foreign nations, including some of the poorer ones, and that the anticipation that fusion power will be available will have a salutory effect on our relations with such nations.

It is interesting to observe that within only a few weeks after our legislation was signed into law in October, 1980, the Japanese reviewed their magnetic fusion program and promptly committed their country to an accelerated schedule for engineering development and materials testing, with goals quite similar to those expressed in our law. The European Economic Community appointed a blue-ribbon review committee even before the end of 1980. By June of 1981, the committee had recommended an accelerated program in engineering development and materials testing, and increased funding to support it.

Unfortunately, both the Japanese and the E.E.C. are dependent on the partially completed Fusion Materials Irradiation Test Facility at Hanford. This is one of the projects that some individuals in the OMB are attempting to kill -- in spite of the fact that it is absolutely critical to our federal program, and is urgently needed by the Japanese and European Community. It is the only such machine in the free world, and the Congress specifically directed in the recently enacted appropriation law for fiscal year 1982 that construction continue.

Now the Administration again proposes to kill the FMIT and the EBT-P. It also is ignoring the directive in the law to move forward with creation of a Center for Fusion Engineering to guide the research and development/leading to construction of a fusion engineering test bed (the Fusion Engineering Device) by about 1990, and then a Magnetic Fusion Demonstration Plant by the year 2000.

The refusal of the OMB to comply with the law is, from my perspective, a major tragedy for the world. Not only does it inflict great damage on the federal program, it creates confusion among our foreign friends in Europe and Japan who first followed our lead and relied on this country for cooperative use of partially completed facilities, and who now don't know how to deal with an unreliable ally -- or how to proceed without critical facilities.

Just as important, failure by the Administration to follow the law is disillusioning for scientists and executives of interested industries, and for the young citizens of our nation who looked to the fusion law as a beacon of hope for their future.

I think it is important to recognize that if we bestir ourselves, magnetic fusion power is within the grasp of the human race, and that its potential for benefiting humanity staggers the imagination. I have stated that when we step across that line with the first demonstration of fusion electricity it will constitute the second most important event with respect to energy in human history -- second only to the discovery of fire. The advent of controlled fusion will stand out as a giant step in the march of mankind toward true civilization.

Above all else, fusion is a technology that we can share with every human being in the world. It crosses all political boundaries. Clearly, every single person in the entire world benefits when everyone has the energy he needs, and from that energy, the water and materials that he needs.

Just as fusion is the ultimate source of energy in the universe, so it will be here on earth. When and how this happens is up to us, and the future of mankind, during the lifetimes of our children, will be profoundly affected by what we do now.

Thank you.

MIKE McCORMACK

BIOGRAPHICAL SKETCH

Residence: 508 A Street, S.E.
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(202) 544-0254

Spring, 1982

Born: 14 December 1921, Basil, Ohio
Military Service: Parachute Infantry Rifle Platoon Officer - WW II.
Education: B.S. and M.S. (1949) in Chemistry, Washington State University.
Primary Professional Activity: Research Scientist, Hanford (AEC) Facility 1950-1970.
Family: Married (1947) to Margaret Higgins of Toledo, Ohio; three adult sons.

Mike McCormack served in the Washington State House of Representatives (1956-60) and Senate (1960-1970). He authored legislation creating a new state community college system, a modern authority for industrial development by port districts, and a thermal power plant siting law (which is a model for other states).

McCormack was elected to Congress in 1970 and served through 1980. He was a member of the Committee on Public Works and Transportation and the Committee on Science and Technology; and was Chairman of the Subcommittee on Energy Research and Production.

McCormack is widely recognized for his leadership role in energy-related matters during his service in the Congress. His expertise in all fields of energy is respected today among members of both political parties, by the White House and the Department of Energy, and by industrial and labor leaders.

The following federal laws were conceived, written and sponsored by Mike McCormack:

Public Law 93-409, the Solar Heating and Cooling Demonstration Act of 1974.

Public Law 93-473, the Solar Energy Research, Development and Demonstration Act of 1974.

Public Law 93-410, the Geothermal Energy Research, Development and Demonstration Act of 1974.

Public Law 94-413, the Electric and Hybrid Vehicle Research, Development and Demonstration Act of 1976.

Public Law 95-39, the National Energy Extension Service Act (with Congressman Ray Thornton of Arkansas).

Public Law 95-238, the Automotive Propulsion Research and Development Act of 1977 (with Congressman George Brown of California).

Public Law 95-590, the Solar Photovoltaic Research, Development and Demonstration Act of 1978.

Public Law 96-386, the Magnetic Fusion Energy Engineering Act of 1980.

Public Law 96-567, the Nuclear Safety Research, Development and Demonstration Act of 1980.

In Addition:

McCormack has been the leader in the House of Representatives during the period 1977-80 in the campaign to preserve the nuclear breeder program and to move forward with nuclear energy development within the U.S.

In 1979 and 1980 McCormack sponsored realistic legislation for the handling of low-level nuclear waste and a series of demonstrations for the geologic storage of glassified high-level nuclear waste. These bills were delayed in other committees, and did not become law.

Honors:

Awarded an honorary Doctorate in Engineering by the Stevens Institute of Technology (1976).

Elected a Fellow by the American Association for the Advancement of Science (1980).

Elected a Fellow of the American Nuclear Society (1980).

Elected a Fellow of the American Institute of Chemists (1980).

Received the "Centennial Award" of the American Society of Mechanical Engineers (1980).

Received the Distinguished Public Service Award from the Institute of Electrical and Electronics Engineers (IEEE) (1980).

Received the Legislative Recognition Award from the National Society of Professional Engineers (1980).

Designated "Solar Energy Man of the Year" by the Solar Energy Industries Association (1975).

Received the 1976 Distinguished Service Energy Award from the National Energy Resources Organization (NERO).

Received the "Triple E" Award from the National Environmental Development Association for "significant contribution" for balance in energy, environmental and economic issues (1976).

As a private citizen in 1981, McCormack:

- Served on Secretary of Energy Edward's Task Force on the New Administration's Energy Policy (NEP-III).
- Accepted Appointment to the:
  - National Research Council's Space Advisory Board.
  - General Accounting Office's Committee on Program Evaluation
  - The Fusion Advisory Panel serving the U.S. House of Representative's Committee on Science and Technology.
- Was elected to Membership on the Board of Director's of the Universal Voltronics Corporation of Mt. Kisco, New York.
- Was named by Technology magazine as one of the top 100 innovators in the world in the year 1981, "... without whom some important technical advance during the past year would not have occurred."

In 1982, McCormack founded a new corporation: McCormack Associates, Incorporated, offering consulting services in science, energy, government and public information.

McCormack has written hundreds of papers and public addresses on energy, primarily designed to help the public understand the basic facts concerning energy, with special emphasis on nuclear energy and nuclear safety issues.

McCormack is frequently requested as a spokesman by electric utilities, the Edison Electric Institute and the Committee for Energy Awareness. Recent addresses by McCormack include:

- "America's Energy Future" (Professional Advisory Council on Energy, Philadelphia, 16 Feb 82).
- "Implications of Fusion Energy Development" (American Association for the Advancement of Science, Washington, D.C., 7 Jan 82).
- "Helping the Average Citizen Understand and Accept Nuclear Power" (American Nuclear Society, San Francisco, 1 Dec 81).
- "We, Who Are The True Environmentalists" (American Nuclear Society, San Francisco, 2 Dec 81).
- "Getting Priorities Straight on Energy" (American Power Conference, Chicago, 28 Apr 81).
- "The Urgent Need for Realistic Planning Now" (International Scientific Forum on Geopolitics of Energy, Fort Lauderdale, 10 Nov 80).
- "Facing The Facts About Energy" (Soroptimist International of the Americas, Denver, 24 Jul 80).

STATEMENT OF DR. JOHN B. YASINSKY
WESTINGHOUSE ELECTRIC CORPORATION
BEFORE THE
U.S. HOUSE OF REPRESENTATIVES
COMMITTEE ON SCIENCE AND TECHNOLOGY
SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION
MARCH 24, 1982

MADAME CHAIRMAN,

I am John Yasinsky, General Manager of the Advanced Power Systems Divisions of the Westinghouse Electric Corporation. Our company is involved in the development of a broad spectrum of advanced energy technologies, which include both the liquid metal cooled fast breeder reactor and nuclear fusion. Through our Advanced Reactors Division, we play a lead role in the Clinch River Breeder Reactor Project, and Large LMFBR Plant studies. Through the Westinghouse Hanford Company, we operate the Hanford Engineering Development Laboratory for D.O.E., including responsibility for the Fast Flux Test Facility and the Fusion Materials Irradiation Test Facility (FMIT).

Our position in support of the fast breeder is well known; at this time I would like to acquaint you with our equally strong support of, and interest in, the timely development of fusion technology. Westinghouse has been active in the development of fusion technology since the early 1950's. We are currently involved in all aspects of the national fusion program, ranging from the manufacture of components for the Tokamak Fusion Test Reactor to the conceptual design of fusion power plants involving a variety of alternative confinement approaches. These activities are indicative of our strong support for the fusion program and particularly our endorsement of the aims of the Magnetic Fusion Engineering Act of 1980.

We recognize the limited government funds available for the development of advanced energy technologies must be used prudently. Nevertheless, short range economic evaluations cannot be the sole basis for near-term decisions affecting our future energy supplies because there are inherently large uncertainties associated with the availability and competitive position of primary fuels and advanced energy technologies over a period of 2-3 decades. A national energy policy that depends heavily on economic projections, major discoveries of domestic energy resources and/or technological breakthroughs entails substantial risk that our country will not have an adequate and reliable supply of energy in the future. Accordingly, the U.S. msut pursue a supply strategy that provides for a mix of energy sources which will permit our various consumption sectors to accommodate a broad range of political, economic and technological developments.

As a consequence we see a strong incentive for the simultaneous pursuit of both the LMFBR and fusion technology. In view of the large difference in technological maturity and potential deployment time frame for these two energy generation technologies, we see no explicit conflict in promoting the development of both technologies as part of a prudent and broad based national energy policy.

On the basis that LMFBR technology is ready for commercial demonstration and that nuclear fusion is still in the early stages of engineering feasibility, we believe that the emphasis in the Administration's FY-83 budget request on the LMFBR program is technically and strategically sound. However, this does not imply that the fusion effort should be de-emphasized in any way. In fact, we would recommend that careful consideration be given to augmenting Administration's FY-83 budget request in the magnetic fusion area to continue several important technical initiatives which are vital to expediting the transition from a physics-based program to an end-product focused engineering development effort. Key program elements in this regard are the Fusion Materials Irradiation Test Facility and the definition of an appropriate Fusion Engineering Device to provide a strong focal point for near-term engineering development activities. Many of the major technological questions facing fusion today, such as reactor size, complexity, and cost can only be addressed in the context of such hardware development, test, and demonstration programs.

The impressive progress in the magnetic fusion energy experimental program to date has generated great confidence that the United States will ultimately achieve the conditions necessary to generate electrical power from fusion. However, an essential engineering step which must be taken is the development of materials to withstand the intense neutron environment of fusion power devices. The fusion environment is extremely harsh on existing alloys. Without improved materials, fusion plants would operate well below desired levels and would not be economically viable. In order to develop improved materials the fusion community has devised a strategy to take alloy data from fission reactors and translate these to fusion reactor conditions using the FMIT. Without FMIT the application of the fission data is not possible and alloy development and engineering characterization for fusion could not be completed. In short, without FMIT, fusion commercialization is jeopardized.

The first fusion reactor plants, such as FED or Demo, cannot substitute for FMIT in the development of a high energy neutron radiation effects data base. While these plants are appropriate for many engineering problems they will require 10-20 years to do what FMIT can do in 2-3 years. The alloy development process requires a series of scientific investigations followed by testing in service-like conditions, and final optimization. Such a process would take as long as 50 to 70 years in a fusion plant, far too long for practical success.

If fusion commercialization is to be achieved as early as possible then, we must proceed rapidly through the engineering development phase. Operation of FMIT by 1987 is absolutely required to complete this effort.

The FMIT Project, once again, continued to make outstanding progress this past year. A major accomplishment was the recent completion of the design of the building and utilities portion of the facility by the R. M. Parsons Company in California. This engineering effort successfully incorporated the individual designs of the accelerator, target, handling and control systems into an integrated facility design. This means that the facility is ready for construction as soon as funds are available.

The Accelerator Division of the Los Alamos National Laboratory (LANL) joined the Hanford Engineering Development Laboratory (HEDL) in an outstanding cooperative effort to design and develop the FMIT. International acclaim in accelerator technology has been achieved with development of the FMIT advanced low-velocity accelerator, known as the radio-frequency quadrupole (1981 IR Award). At HEDL, the World's largest liquid lithium test loop has operated with extreme reliability for more than two years demonstrating the performance of lithium system components and the unique lithium "target". Consequently the outstanding technical risks are small.

Fabrication of accelerator and lithium system equipment in underway in California, Ohio, Texas, Maine, Minnesota and Washington. Approximately 55% of the accelerator equipment is under contract. Delivery to HEDL has already started and all vendors are proceeding on schedule and within budget.

Fifteen of the 77 accelerator drift tubes will be completed at HEDL this summer. A major component, the prototype radio-frequency power supply, is now under final test at Hanford. One critical lithium component, the electromagnetic pump, has already been completed and another key component, the lithium-to-organic heat exchanger, is in fabrication at Southwest Engineering Corporation in California.

The FMIT has achieved a high degree of momentum with the completion of facility design and a substantial amount of procurement either complete or in process. Site preparation has been completed and the project stands ready to initiate construction activities.

The FMIT Project has all the ingredients necessary to assure success:

- The technical feasibility has been demonstrated through the research and development programs.

- A proven management team is in place.

- Highly qualified technical personnel at HEDL and LANL.

- Project management systems necessary to control budgets and schedules are in place.

- The project scope and requirements are well defined.

By the end of Fiscal Year 1982, approximately $80M or about 40% of the total program funds will have been allocated. I strongly recommend continued funding in Fiscal Year 1983 to maintain project momentum to capitalize on the investment already made. Two options are viable. The first would be to proceed with construction and procurement, this would require a funding level in 1983 of $25-30M. If this budget is too difficult then a second option would be to proceed to complete equipment design in preparation for a 1984 construction start.

If the United States makes the commitment to start construction and complete the project on a timely basis major funding from foreign participation seems assured. In this event the remaining U.S. commitment would be less than the amounts committed to date.

It is absolutely mandatory to have an FMIT facility in order to achieve a practical fusion machine. In view of these factors, disbanding of the management and technical team with a plant ot reestablish the team at a later date in fraught with great risk. Therefore, from both technical and management viewpoints the continuation of the FMIT project is strongly recommended by the Westinghouse Electric Corporation. This is the most cost effective approach in the use of limited government resources.

DR. JOHN B. YASINSKY
WESTINGHOUSE ELECTRIC CORPORATION

Dr. John B. Yasinsky is General Manager of the Advanced Power Systems Divisions of the Westinghouse Electric Corporation. Joining the corporation in 1963, Dr. Yasinsky was assigned to the Bettis Atomic Power Laboratory, working in reactor theory, design, dynamics and control in support of the naval nuclear program. In 1971 he was named manager of electric systems and plant analysis at Bettis.

The following year John was named to the White House Fellows Program and served as Special Assistant to the U.S. Secretary of Commerce.

Returning to Westinghouse, Dr. Yasinsky was named Manager, Business Development for the Breeder Reactor Divisions, moving through a variety of marketing management jobs. In 1978 he was appointed Director of the Advanced Coal Conversion Department which was responsible for the firm's activities in low and medium BTU coal gasification research and development.

Appointed President of Westinghouse Hanford Company in January 1979 he was responsible for the operation of the Hanford Engineering Development Laboratory for the U.S. Department of Energy. A major responsibility there is also the operation of the Fast Flux Test Facility for testing breeder fuels and materials. Work is also underway on developing and testing materials for fusion reactors.

He was appointed to his present position in August of 1980. In this post he is responsible for the Advanced Reactors Division, the Westinghouse Hanford Company and the Advanced Energy Systems Division.

Dr. Yasinsky received his B.S. in physics from Wheeling College. He earned a master of science degree in physics from the University of Pittsburgh and his Ph.D. in nuclear science from Carnegie Institute of Technology (now Carnegie-Mellon University). He has authored numerous technical papers and reports and taught in the graduate programs at Pittsburgh and Carnegie Mellon. Dr. Yasinsky is a member of the American Nuclear Society, the National Security Industrial Association and has served on the Advisory Council of the National Energy Resrouces Organization. He is a member of the White House Fellows Association.

UNIVERSITY OF WASHINGTON
SEATTLE, WASHINGTON 98195

College of Engineering
Department of Nuclear Engineering

March 17, 1982

Honorable Maralyn L. Bouquard, Chair
Subcommittee on Energy Research
and Production
Committee on Science and Technology
U. S. House of Representatives
Suite 2321, Rayburn House Office Building
Washington, D. C. 20515

Dear Congresswoman Bouquard:

We are writing on behalf of the University Fusion Association. Our organization represents 48 universities with 170 members engaged in fusion research, mainly through DOE funding.

We have been unable to arrange to testify at your subcommittee hearing next week. However, we wish to provide the following statement for the record.

In present more limited fusion funding budgets are projected to be "flat" over the next few years. University programs, representing $41M, or 12 percent, are a very cost effective element of the magnetic fusion effort.

A unique contribution of the universities is training of fusion scientists and engineers. Under 66 separate contracts 70-80 new Ph. D.'s per year are trained and placed for the most part in the fusion power industry, national laboratories and universities.

The university programs have an impressive record of research results, critical technical review and new ideas for the main line fusion efforts and new approaches. This comes both from small, single-investigator projects and larger university programs.

Broadly speaking, the administration's FY 1983 budget preserves the base fusion program in Confinement Systems, Development and Technology (D&T) and Applied Plasma Physics (APP) while putting some large advanced projects, FMIT, EBT-P, CFE and part of MFTF-B, on "hold" for more favorable future funding.

The university programs are very vulnerable to funding shifts between base programs and projects. The APP and D&T university programs are down to basic, salary-intensive funding in most cases. We hope

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that additional funding can be provided for important large projects. However, relatively small funding shifts in the presently proposed FY 1983 fusion budget could wipe out many university programs. Once students are turned away, considerable time would be required for them to return to fusion and an even longer time to complete the training necessary to maintain the flow of trained personnel. University programs cannot be turned on and off like a water faucet.

For these important reasons we urge support for the university programs in the fusion budget your committee is considering.

Sincerely yours,

THE UNIVERSITY FUSION ASSOCIATION

Fred L. Ribe
Professor of Nuclear Engineering
The University of Washington
Chairman

J. Leon Shohet
Professor of Electrical and
Computer Engineering
The University of Wisconsin
Vice Chairman

George H. Miley
Professor of Nuclear Engineering
The University of Illinois
Secretary-Treasurer

FLR/JLS/FHM:mt

cc: Executive Committee

STATEMENT ON FY83 AUTHORIZATION FOR MAGNETIC FUSION

BEFORE THE

SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION

OF THE

HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY

MARCH 23, 1982

BY

THE FUSION ENERGY FOUNDATION

The Fusion Energy Foundation, a nonprofit, educational organization with more than 20,000 members in the United States committed to the application of advanced technologies to the achievement of economic growth, submits this statement on the fiscal year 1983 nuclear fusion budget in the belief that the members of this committee face a task of awesome significance for the future of the United States. We believe that the fusion budget line, although it has never been quantitatively even 1 percent of the federal budget, makes a qualitative contribution to the future well-being of the nation that no other item in the federal budget does. Our testimony outlines the importance of this item not only in scientific and technological terms, but in the much larger economic and national security terms that have concerned foundation members over the past year.

The federal budget for FY1983 is unique in being entirely dependent on the current economic situation for its realizability. If interest rates continue at their present heights, the federal budget deficit arising from interest payments alone willwreck the positive programs in the budget. For the first time in decades, the President did not actually make the budget; the budget was determined by the policies of the Federal Reserve Board and Federal Reserve head Paul Volcker's hidden agenda for "controlled economic disintegration."

This budget and its debate by Congress thus labors in an atmosphere of a certain unreality--the real policy determinants of the budget process are being debated in the halls of the Federal Reserve Board, not those of Congress. The Fed has assured the country that a recession--called by its right name a depression--is necessary for the country's economic health, that a "shakeout" of American industry would be beneficial, that 8 percent unemployment is unavoidable, and that a falling living standard is actually good for us all.

Unless reversed, the Federal Reserve's policies are certain to cause a collapse of federal revenues by mass bankruptcies and unemployment before Oct. 1, 1982, and a budget deficit of $250 billion or more for fiscal 1983. Such a deficit will destroy the current budget, whatever its particular inadequacies. But should Congress and the President reverse Federal Reserve policy, the debate on the budget would become healthy. A budget far superior in its impact on national well-being and national progress could then be shaped.

Our testimony before this committee is based on the results of a series of detailed studies conducted by the Foundation over the past three years. These studies have shown the following:

(1) The most significant single cause of the economic and social decline of the United States in the past decade is the lack of a "science driver" for the economy. With the demise of the Apollo program and NASA's fulfillment of that role in the late 1960s, the country has been without a commonly perceived, well-funded advanced scientific-technological program to guide education, to power technological innovation in industry, and to inspire the youth of our country.

(2) This lack of a "science driver" has combined with increasing obsolescence in industry, monstrously high interest rates, a plague of drug addiction, and falling birth rates to produce the current depression.

(3) The consequence of this situation is a serious and continuing decline--as measured by any but the most self-consoling analysts--in national security. We have today a military capable of, and prepared for, fighting only the most localized conventional wars, with the objective of controlling natural resources. Today, the traditional American military dedicated to the mission of "nation building" does not exist.

(4) To remedy this increasingly grave predicament

requires a combination of policy initiatives. We have concentrated in this testimony on the critical role that science policy, specifically policy concerning advanced energy research, can play in changing the direction of this country. The studies we have conducted document our conclusion that a program for fusion energy development, like the one mandated in the <u>Magnetic Fusion Energy Engineering Act of 1980</u>, would provide a large measure of the "science driver" required to renew the American economy.

We have provided a detailed budget proposal for advanced energy research and specifically a budget for fusion research, in several attachments. An adequate advanced nuclear research budget requires the expenditure of $500 million more than what is proposed by the FY1983 administration budget. For magnetic fusion, we believe that a budget of $660 million (rather than $450 million) should be invested, to fulfill the mandate of the Magnetic Fusion Energy Engineering Act of 1980. This investment would be the first step toward the engineering realization of nuclear fusion--a clean, safe, limitless source of the most concentrated energy man has ever mastered.

THE ECONOMIC BACKGROUND

All "American System" economists, from Alexander Hamilton through Henry Carey and E. Peshine Smith, have agreed that the most palpable measure of an economy's health is its ability to utilize "artificial labor" or the "gratuitous power of nature." In the early 19th century, these economists established the very modern-sounding thesis that energy and its concentration by means of new technologies was the embodiment of economic health and progress. They also established that the most accurate measure of this health or progress potential was provided by quantifying the population potential of such an economy. Lyndon H. LaRouche, Jr., noted economist and a member of the board of directors of the Foundation, has termed this measure "relative potential population density."[1] That is, the most important indicator of the success of an economy is the number of people that can be (potentially) supported in a manner that not merely continues the present living standards for them and their descendants, but in a manner that makes possible continued progress. Equilibrium or zero-growth (a "sustainable future" is the misnamed term of the modern-day Malthusians) is not a possible future. The only adequate measure of economic policy is whether it fosters continued progress.

The Fusion Energy Foundation has conducted a number of econometric studies on the relation between energy and potential population density, and the conclusions of these early American economists have been borne out in the most precise detail. Perhaps the most graphic indication of the close relation between energy and population is provided by a recently completed study of the role of advanced energy use globally in the past 15 years. Using the LaRouche-Riemann econometric model, a large, computer-based model that has an engineering rather than a financial structure,[2] the Foundation conducted a set of comparative scenarios for the global economy by varying the mix of energy sources used during this period. Taking the actual economic history of the past decade and a half as a base scenario, we measured the economic and demographic impact of different energy production technologies.

The results of the high-technology scenario merit special attention. Taking as a possible high-technology scenario the projections for the use of nuclear energy developed by the Atoms for Peace planners in the late 1950s and early 1960s,[3] the LaRouche-Riemann model and its energy submodel were used to analyze the economic impact of the accelerated adoption of nuclear energy. This analysis showed that economic growth during the past 15 years could have been increased on the average 3 percent per year globally over the rate actually achieved had maximum use been made of conventional nuclear energy and associated technologies. There are two interconnected reasons for this substantial improvement in economic growth, both characteristic of advanced technologies:

First, more advanced technologies are cheaper. In the case of nuclear energy this was true even before the oil crisis of 1973; after 1973, nuclear energy became even more economically advantageous. The refusal to use nuclear energy on the part of some advanced countries, and the denial of nuclear energy to many underdeveloped countries was, in economic terms, a subsidy to the more expensive forms of energy like oil and coal, and thus a tax on the world economy. This tax on the world economy resulted in a small, almost constant depression of possible growth rates, as desperately needed capital during the 1970s was used to pay unnecessarily high energy costs. The impact was especially severe in the developing sector.

Second, advanced technologies spur general productivity increases. Although this is less important in the first 5 to 10 years of this period, in the long term new technologies like nuclear energy had a significant impact on productivity. More than 2 percent of lost economic growth in our analysis resulted from lost increases in productivity on a world scale, because the world used the more labor-intensive technologies associated with coal and oil, or the even more labor-intensive and inefficient technologies associated with conservation and solar energy. For the advanced sector specifically, our studies estimate that for every 1,000 megawatts produced with an advanced technology, there is a 0.01 percent increase in the average productivity that will not occur if that same energy (even at the same price) is produced by coal or oil. This effect is well known from the Apollo program,[4] and is the most significant economic impact of any advanced technological project, including military ones, in the advanced sector.

The LaRouche-Riemann model was then used to measure the demographic impact of this depressed economic growth in the developing sector. It is well known that economic well-being is closely connected with life expectancy, fertility, and supportable population levels. Using the population submodel of the LaRouche-Riemann model, we arrived at an estimate of the demographic consequences of substituting oil, coal, or conservation for a more advanced energy source such as nuclear: Approximately 115 million people worldwide died unnecessarily. In other words, 115 million people were alive in 1965 who would still be alive today had the world economy benefited from an aggressive program of nuclear energy use. These are people--about 35 percent of whom would be economically active today--who died before the age of 25. The other 65 percent represent perhaps an even greater tragedy, for they died as children.

These 115 million people did not, of course, die from lack of electricity. They died from malnutrition, disease, poor sanitation, inadequate housing--the typical conditions of underdevelopment that depress life expectancies and raise infant mortality. Advanced technologies are the cure for these economic diseases. To avoid the use of these technologies, for whatever reason, is to condemn literally tens of millions of people to death every year.

This analysis estimates that a continuation of the present antinuclear policies of the United States will condemn another 135 million people in the next decade. The enormity of such a policy is difficult to comprehend.

The impact of advanced energy technologies in the advanced sector is, of course, different from that in the developing sector. The demographic impact in the advanced sector is not as grisly or as easily measured, but the economic impact is even greater. The Foundation has conducted a number of studies of different aspects of the connection between advanced technologies and economic health; the most relevant of these for the present testimony concerns the role of infrastructure development, advanced technologies, and electricity production. This recently completed study shows that there is a complex, synergistic interaction among these three aspects of an economy, in which a decline in any of the three accelerates causal factors that depress indicators of the other two, leading to calamitous declines in the ability of an economy to reproduce the material prerequisites for continued existence.

However, we concentrate here on the policy implications of the study in which we examined the consequences of implementing a concerted program of infrastructure development, advanced scientific and technology research programs, and accelerated electrical energy production. A sequence of historical analyses using the LaRouche-Riemann model data base showed a striking correlation between gross expenditures on infrastructure (roads, canals, water systems, and so on) and the "gross reproductive capacity" of an economy. This measure of an economy's general thermodynamic efficiency is unique to the LaRouche-Riemann model and is calculated as a ratio of the gross tangible profit in an economy to the total of that economy's tangible output that is devoted to the equilibrium reproduction requirements. This generalized measure of productivity shows that infrastructure development has a much more profound impact on economic growth than the improvement of communication or transportation usually associated with it. Infrastructure does not produce tangible output; it produces productivity.

Especially critical in this is the role of electricity. Electricity as an energy carrier is a specific part of infrastructure; it does not produce energy, but it carries energy in a way that encourages productivity growth. Several recent studies by other energy analysts have shown a similar critical role played by electricity.[5] One of these studies concludes that electricity growth paces economic growth (as measured by gross national output), and that electricity growth is always 2 percentage points greater than overall economic growth. This analyst summarizes his findings as follows: "Electricity growth is, practically by itself, the 'locomotive' of GNP growth whereas non-electric energy, while basic to the economy, has very little relation to growth. At this time, we shall note that the growth in electricity use must either outpace economic growth by two percentage points or progress at a 50 percent faster rate, as the case may be."

In policy terms, then, the question is one of how to increase electricity production. The results of our studies are unequivocal: The cheapest and most advanced technology must be used. This is nuclear energy. We summarize below some specifics of our program for nuclear energy production, but in a more general context, the production of nuclear energy is only the first step in an economic policy constellation designed to reinvigorate the U.S. economy.

As Alexander Hamilton especially stressed, it is not enough to produce more; an economy must also produce the conditions for new technologies, and it must produce the prerequisites for further growth. The genius of this "American System" approach to economic development is that the process of research and development on these new innovations that ensure continued progress itself improves present production techniques, education, and general productivity. This role of new technologies was dramatically shown in the course of the Apollo program. In outline, this dual role is the importance of advanced research projects like the fusion research program.

Fusion is certainly the natural successor to conventional nuclear technologies; its significance as the next century's energy source of choice is the basis of the Magnetic Fusion Energy Engineering Act of 1980. But, fusion research itself, in a sense separate from its realization as an energy producer, provides a long list of potential benefits for the American economy. Among the most exciting areas are new materials, new methods for handling high temperatures, new vacuum technologies, new superconducting processes, new magnet technologies, automation advances, high-speed control and electronics, insight into high pressure and high density phenomena, methods for controlling high neutron fluxes, and new machining techniques. These technologies can already be applied in the areas of advanced transportation using magnetic levitation, space travel using nuclear propulsion, advances in medical diagnostic tools, astrophysical research, and the development of human interfaces to high speed computing equipment.

But in many ways the most profound "spinoff" of this fusion research effort has been, and will continue to be, the national defense impact of this research in high energy density physics. Although there is no fusion machine that can function as a weapon, the physical laws governing the bizarre states of matter characteristic of the fusion process are the same that must be mastered for the weapons and military technologies of the 1980s. We mention the following areas as especially prominent:

(1) The development of nuclear explosives. Little noted is the tremendous potential for the peaceful use of small, nearly fallout-free nuclear explosives for civilian purposes. Such devices have been demonstrated with yields as small as 500 tons of TNT and negligible uncontrolled radiation.

(2) Advanced propulsion systems. The need for advanced propulsion systems of the nuclear or fusion type has long been recognized and the magnetic technologies required are the basis of fusion research.

(3) Directed energy beam weapons. The development of these defensive weapons for the mission of anti-ballistic missile defense is regarded by many analysts as the most important task of military science in this decade. Such research is integrally intertwined with fusion research. The development of appropriate pulsed power sources, materials for beam production, understanding beam propagation, laser driver technologies, and

tailoring of energy on target are all critical questions that are attacked as part of a broad-based fusion program. It is instructive to note that the Soviet progress in beam weapons has been entirely dependent on their fusion program, and their present advanced state in weapons development relative to the United States is a result of their very broad-based fusion effort.

AN ADVANCED TECHNOLOGY ALTERNATIVE

The Fusion Energy Foundation believes that it is impossible to discuss the fusion budget adequately without situating these expenditures in the entire energy and research budget of the nation. Therefore, we have proposed a four-part energy and science policy to address the larger questions of energy, technological development, world economic recovery, and scientific progress.

(1) A near-term, aggressive program for nuclear power construction. Specifically, we propose the construction of 150 new nuclear plants during this decade. This represents a doubling of present construction plans and would, according to our econometric studies, save more in the cost of energy and economic multiplier effects than the plant construction cost involved. This program would require the federal funding of several crucial new energy technologies, specifically the high-temperature gas reactor and advanced fossil fuel technologies like magnetohydrodynamics.

(2) A broad-based, advanced nuclear energy research program, based on the breeder reactor and a broadened basic nuclear science program.

(3) A fusion program based on the funding levels set forth in the Magnetic Fusion Energy Engineering Act of 1980. This funding would continue a large scientific research effort, but initiate a full-scale engineering track of the fusion research effort, leading to an engineering prototype by 1990 and commercial fusion by the year 2000. This admittedly aggressive schedule is <u>slower</u> than the Japanese program for fusion energy development, and the proposed U.S. fusion budget presented by the Office of Management and Budget is less than the Japanese budget for fusion development!

We further propose that the laser fusion program, traditionally included under the Department of Energy's Office of Military Applications, be included in an Office of Fusion Energy and be rapidly expanded.

(4) The initiation of a national program for the development of directed energy beam weapons for ballistic missile defense. The funding of such a research and development program, under the auspices of either the Department of Defense or the Department of Energy, would

produce what Dr. Edward Teller called "the most important development in strategic warfare since the ICBM." Its military importance, however, is overshadowed, we believe, by its impact on the civilian economy, fusion research, space exploration, and almost every area of advanced scientific research.

NOTES

1. Lyndon H. LaRouche, Jr., "Economics and Population," The Campaigner (New York), October 1980.
2. Steven Bardwell, "Overturning Equilibrium Economics: The physical laws of economic development," Fusion (New York), June 1981.
3. Glenn T. Seaborg and William R. Corliss, Man and Atom: Shaping a new world through nuclear technology (New York: Dutton, 1971).
4. The Economic Impact of NASA R & D Spending, Michael K. Evans, Chase Econometrics Associates, Inc. NASA Report CR-144352-3 (1973).
5. Fremont Felix, "Our Top Priority: Expanded electrification will substantially reduce oil use, while propelling economic recovery," Gibbs and Hill report delivered at the Economy, Energy, and Electricity Conference, Toronto, Canada, 1980.

Appendix A

PROGRAM FOR ACCELERATED NUCLEAR POWER CONSTRUCTION

The Fusion Energy Foundation has called for construction of 150 gigawatts of nuclear power as the "driver" of a depression recovery program for our country in the 1980s. This program is based on an economic impact analysis developed by the Foundation. A conservative estimate shows that this recovery program will save the nation approximately $150 billion dollars over and above the construction costs of the nuclear plants required.

The program for 150 gigawatts of new nuclear generating capacity by 1990--100 GWe by 1987--roughly involves doubling the utilities' construction plans as of mid-1981 to meet the requirements of both recovery from the depression and sustained economic growth. The utilities then planned to build approximately 90 GW nuclear by 1987. Since then--because of cancellations brought on by useless regulation and the usurious high-interest rate policy of the Federal Reserve System, this figure has dropped to 83 GW in only six months time.

Utilities have been starved of the credit needed to build new capacity by the Fed's policies. In 1980, the year of the most recent complete figures, they raised on $37 billion of the $45 billion required by their own, highly conservative calculations. The nuclear industry and our nation's utilities need low-interest credit to become a driver for economic growth. We cannot let the Federal Reserve subject them to the same interest rates that are applicable to gambling casinos.

For $225 billion over the 1980s, with a rollback of nuclear regulatory practices inspired by opponents of economic growth, our nation can build 150 gigawatts nuclear and base our economic growth on productive investment again.

The construction program alone will create some 1,000,000 skilled jobs for the course of the decade, lowering the national unemployment rate by at least 1 percent. As President Reagan has pointed out, every such 1 percent reduction in unemployment saves our national treasury $25 billion per year. This alone will save the nation construction costs for the needed 150 gigawatts nuclear between now and the end of 1990.

In addition, bringing these plants on line throughout the 1980s--replacing far more expensive oil or coal-fired power with nuclear--will save power consumers (industry, consumers, and government) at least $100 billion by 1987, the first phase of the program, for a total of $150 billion by the end of 1990.

A vigorous nuclear power construction program--costing $225 billion--will save $375 billion in direct benefits of reduced unemployment compensation, increased tax revenues, and reduced costs for higher quality electricity. This estimate does not include savings derived from millions of new jobs created as these plants come on line.

Appendix B

PROPOSED NUCLEAR FISSION BUDGET

The budget proposal below, which calls for federal spending of $21 billion on advanced fission technology development, will solve existing problems facing the nuclear energy source our nation desperately needs before these problems become acute. With the proper, rapid allocation of funds, we can close the nuclear fuel cycle by the end of the decade. Budget items 3, 4, 5, and 6 are devoted to this effort.

However, the industrial process heat applications of the high temperature gas reactor (HTGR) show that advanced fission systems demand funding appropriate to the enormous economic benefits they will yield.

NUCLEAR FISSION BUDGET (in millions of 1982 dollars)

Category	Reagan 1982 Budget	1982	1983	1990	Total
1. Conventional Reactors	69.5	70	75	10	420
2. TMI Cleanup	--	75	100	--	500
3. Commercial Waste Management	240.3	260	300	75	1,810
4. Spent Fuel Storage	7.3	7.3	10	5	152
5. Breeder Reactors	736.5	750	800	500	6,325
6. Fuel Reprocessing	--	250	250	50	1,350
7. High Temperature Gas Reactor	--	250	300	800	5,250
8. Other Advanced Fission Systems	38	38	45	40	408
9. Uranium Supply and Enrichment Activities	194.8	210	225	250	2,460
10. Basic Nuclear Science	160.6	175	190	325	2,265
TOTAL	1,447	2,085	2,295	2,055	20,940

Appendix C

PROPOSED NUCLEAR FUSION BUDGET

The following are projected budgets for fusion research and development that would lead to the realization of commercial fusion by the year 2000, the goal set by the Magnetic Fusion Energy Engineering Act of 1980.

The Magnetic Fusion Program. In order to balance the overall magnetic fusion effort, given the large Engineering Test Facility program element based on the tokamak, a vigorous mirror and alternate concepts program are both brought to the point where commercial demonstration plant construction would begin in 1991. These would include the Mirror Fusion Test Facility upgraded to the full reactor-grade plasma tandem configuration, the MFTF-B; a Mirror Reactor Test Facility, MRTF; an Alternate Concepts Physics Test Facility, together with several Alternative Concepts Proof-of-Principle experiments, ACPOP; and a reversed field pinch proof-of-principle experiment, which would probably duplicate the MFTF-B in attaining reactor grade plasmas.

The primary focus of the accelerated magnetic fusion program would necessarily be technology development. However, this technology would be universally applicable to all magnetic confinement approaches and many aspects of inertial confinement, so that the first commercial plants could be based on alternative concepts to the tokamak.

Inertial Fusion Program. In the inertial fusion program, existing facilities at the major national labs would be completed as rapidly as possible. By the mid-1980s, the Livermore Nova glass laser system and the Sandia Particle Beam Fusion Accelerators (light ion beams) will have demonstrated scientific breakeven for ICF. But in order to demonstrate high gain pellet targets needed for commercial ICF reactors, a megajoule Super Nova glass laser system will be needed by the late 1980s. While this type of laser is probably not capable of meeting the requirements of a commercial reactor, it offers the nearest term and most flexible technology for demonstrating high gain targets.

Simultaneously, the Los Alamos carbon dioxide gas laser system and a krypton flouride(KrF) laser system would each be completed on a scale sufficient for breakeven studies. These more reactor relevant drivers could prove capable of going all the way. But the chief base for an Engineering Test Facility will be that of the well known technology of heavy ion beam accelerators.

BUDGET FOR ACCELERATED MAGNETIC FUSION PROGRAM (in millions of 1982 $)

	1983	1984	1990	Total
1. Confinement Systems Division	42	54	--	198
2. Basic Operations Budget	180	180	180	1,440
3. Planning and Projects Division	145	130	80	995
4. Engineering Test Facility PACE	--	240	--	1,320
5. Engineering Prototype Reactor PACE	15	15	240	580
6. Other	278	529	800	4,725
TOTAL	660	1,150	1,300	9,218

BUDGET FOR ACCELERATED CONFINEMENT FUSION PROGRAM (millions of 1982 $)

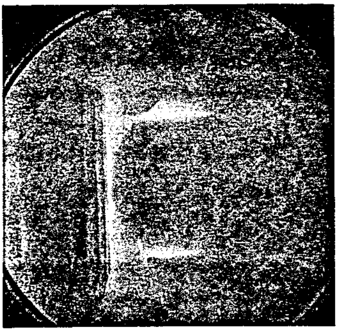
	1983	1984	1990	Total
1. Operations	150	200	300	1,750
2. Nova Laser	70	50	--	120
3. Super Nova Laser	40	80	--	380
4. Carbon Dioxide Laser	40	80	--	340
5. Other	160	300	800	4,240
TOTAL	460	710	1,100	6,830

The promise of fusion: What and when?

The tantalizing attraction of fusion is unlimited availability of energy resources. Experts claim the physical principles have been established and the time has come to move from research into engineering

By **Stephen O Dean,** Fusion Power Associates

In 1960, when putting a man on the moon was made a high political priority by then-President Kennedy, the nation marshalled the necessary financial and human resources to take the project out of the laboratory and make it a reality. The result is history.



The glow of fusion as seen through a viewing port in a Princeton University tokamak

Today, the problem of dwindling energy resources certainly merits the same order of concern. Experts closest to the fusion concept say it is time to express the same national resolve. They seem to agree that this is the time to bring utilities, architect/engineers, and manufacturers into the picture, so that the engineering concepts can be used to develop machinery they can live with.

What makes the time propitious? Both the need and the state of the art. At current production levels, our known oil reserves are being depleted at the alarming rate of 10% per year. Zero growth may be forced on our economy before very long if political considerations continue to dictate sharply reduced reliance on imports and the problems with expanding other energy resources continue.

As for the state of the art, the key fusion parameters—plasma temperature, density, and confinement time—have all been achieved separately in laboratory tokamak devices. Researchers are convinced that everything will come together in Princeton University's Tokamak Fusion Test Reactor (TFTR) to produce the first fusion reaction in which more energy is released than is consumed. TFTR is scheduled for completion later this year. Experiments have shown that the concept can be scaled up with confidence, and that several problems that had been anticipated will be resolved.

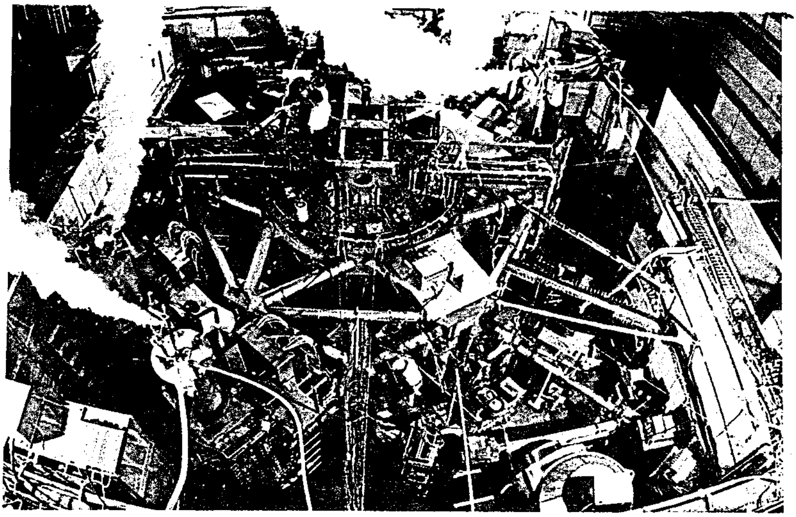
As a virtually inexhaustible energy source, fusion offers a long-range economic advantage. Being insensitive to the shortage-induced price rises experienced by petroleum, gas, coal, and uranium, it will become increasingly competitive with other energy sources.

Recent studies conclude that fusion reactors of 500 to 1000 MW will be feasible. This size range is optimum in terms of both capital requirements and system vulnerability to outages. Once considered difficult to attain with the fusion concept, this range has been a conscious design goal even in the basic research phase of the program.

Fusion is expected to reduce the environmental and siting problems faced by utilities. Radioactive waste will consist primarily of structural members, in which neutron bombardment has produced isotopes with short half-lives, and tritium, a part of the fuel-cycle product with low radiotoxicity and short half-life. The radioactive structural members will require storage for much shorter periods of time than the waste products of fission plants—decades compared to hundreds of years.

Criticality accidents are not possible in fusion reactors, and the absence of afterheat in the fusion core makes coolant-flow disruptions a much less serious problem than in a nuclear-fission plant. Since the deuterium/tritium-fusion fuel cycle will be almost entirely self-contained in the reactor, the usual fuel services and their potential environmental impact will be eliminated. Unlike fossil-fuel plants, there will be no problem with chemical or particulate emissions or with ash disposal. If fusion technology advances to nontritium fuel cycles, there will be almost no radioactive byproducts, and the production of energy will truly become environmentally benign.

In terms of human history, the international political significance of totally eliminating our dependence on limited resources for energy production may be even greater than environmental issues. Clearly, if fusion can replace oil, natural gas, coal, and uranium soon enough,



some of the potentially most explosive international tensions could be defused.

Fusion-technology options

Despite having similar names, fusion and fission are alike only in that both are based on subatomic physical reactions, just as both a steam engine and a rocket engine rely on Newtonian physics and the laws of combustion.

Where fission splits nuclei of atoms into smaller components, fusion releases energy by fusing two nuclei together. The fusion reaction occurs when nuclei of deuterium (the hydrogen isotope having one neutron and one proton in the nucleus) and tritium (the isotope with one proton and two neutrons) combine to produce a helium nucleus. Since the helium nucleus has two protons and two neutrons, one neutron is excess. It is released in the reaction, and much of the energy released is associated with it.

Fusion is the reaction of the sun. The sun converts 657-million tons of hydrogen every second into 652.5-million tons of helium. The heat and light of the entire solar system, extending 5-billion miles into space, is produced by the conversion of that 4.5-million tons every second of solar mass into energy.

The sun can accomplish this at a comparatively low temperature. The tremendous gravitational forces of the great mass of the sun compress and heat its substance to about 15-million C at a high enough pressure for the reaction to take place. It is not possible to create on earth a pressure equivalent to the gravitational force of the sun. The reaction temperature, therefore, must be increased to compensate for the lower pressures involved. Thus, fuel temperature must be raised to about 100-million C—six times the temperature of the interior of the sun—to effect fusion!

The two isotopes, deuterium and tritium, are readily obtained. Deuterium is abundant in sea water and can be recovered fairly easily. Although tritium does not occur naturally, it can be obtained from lithium, which is abundant. In practice, tritium will be made in the reactor itself. A deuterium/tritium fuel cycle will most likely be chosen for the first-generation fusion reactors, because this reaction has the lowest temperature threshold and releases the most energy.

In a fusion reactor, once the fuel is at a high enough temperature, the basic problem is to confine it at that temperature long enough to release more fusion energy than was spent getting it hot and keeping it confined. This is termed the breakeven point, and represents the first milestone. The next one is the ignition point, where the reaction becomes self-sustaining and the external energy source can be removed.

Magnetic confinement leads

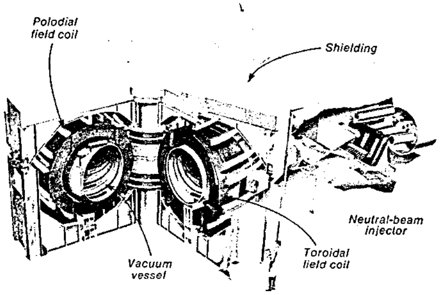
Fusion devices are classified primarily according to the method used for containing the plasma: magnetic or inertial. Major research successes in magnetic confinement have been achieved in recent years. The lead concept is the tokamak, a Russian acronym for "toroidal chamber with magnetic coils." It describes a doughnut-shaped device in which magnetic-field lines, twisting around the torus (doughnut), close on themselves to confine the plasma (Fig 1). Because the particles of the plasma are charged, they resist crossing the magnetic lines of force.

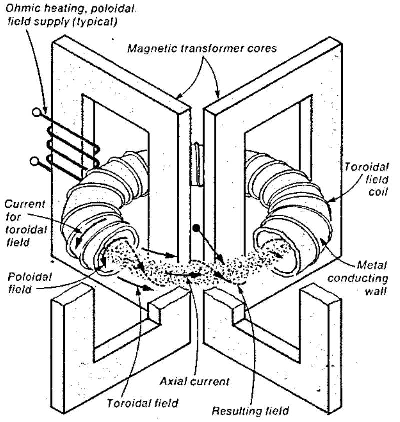
First fusion breakeven is expected within the next two or three years in the TFTR. It cannot be predicted with certainty that the tokamak will ultimately prove to be the best machine for economic production of electricity, but for a variety of reasons, it is further along than other concepts and ready for engineering development.

Tokamak projects so far have concentrated on one parameter, either plasma density, temperature, or confinement time. They have been designed to push the parameter to its critical value, but usually at the expense of the other parameters. Doublet III at General Atomic Co—the world's largest operating tokamak—was designed to develop high values for all three parameters simultaneously, using a hydrogen plasma (Fig 4). TFTR's goal is to bring all the parameters to the breakeven point with a deuterium/tritium plasma. Note that a plasma is a unique form of a gas: At the extreme temperatures involved, the electrons normally surrounding the atomic nuclei are torn away, forming a fourth state of matter.

Containment of the extremely hot plasma long enough to enable sufficient fusion energy to be released is the basic problem characterizing the concept. A popular misconception is that the magnetic field is used to contain the plasma,

1. Magnetic confinement of plasma is the most advanced fusion concept to date. Sketches represent hardware (below) and operational concept (right) embodied in the favored geometry—tokamak, a Russian acronym for toroidal plasma chamber with magnetic coils. Photo at left is an overview of Princeton Large Torus (PLT), an experimental device that has achieved 60-80 million C temperatures. Tokamak Fusion Test Reactor (TFTR), below, scheduled for completion this year, is expected to reach energy breakeven at 100-million C by the middle of this decade





since any ordinary container would obviously vaporize at a temperature six times hotter than the sun's interior. Actually, the plasma's density is so low (1/100,000th the density of air at sea level) that melting of the vessel would not be a problem. So sparse a population of particles would not heat up the metal containment beyond its capabilities, even at that temperature.

More correctly, the fundamental confinement problem is to avoid the loss of heat. Contacts with the first wall—the structure immediately surrounding the magnetically confined plasma—would cause an unacceptable loss of heat, making it impossible to get the plasma's temperature up to the level required.

One of the most dramatic scientific/research stories of the past decade has been the inching up of plasma temperatures toward the critical value (Fig 2). The goal for a reactor is a minimum of 45-million C for breakeven, and 80-100-million C for an economical reactor. The Ormak device, developed at Oak Ridge National Laboratory (ORNL), achieved 20-million C in 1975. The Princeton Large Torus (PLT) recorded 60-million C in 1978, and 80-million in 1980. The latter value was also achieved by another Princeton device—the Poloidal Divertor Experiment (PDX)—in 1981. The TFTR is expected to reach temperatures up to 100-million C by the mid-1980s.

The earliest temperature milestones were achieved in the 1960s solely by ohmic heating. In this method, plasma offers resistance to the current induced in it by the magnetic field. However, as in familiar ohmic-heating devices such as light bulbs and electric toasters, the hotter the plasma gets, the less resistance it offers. This makes it difficult to heat the plasma beyond a few tens of millions of degrees by this means alone.

In the 1970s, a new auxiliary heat source became practical. It enabled injection of intense beams of energetic hydrogen atoms into a plasma. Researchers at ORNL and at the Lawrence Berkeley Laboratory (LBL) developed high-current ion sources capable of generating neutral beams in the 100-kW range at ion energies of tens of kilovolts. Addition of these to the PLT and other tokamaks has made possible the temperature increase noted above.

Confinement of sufficient fuel for the minimum specified time is the other fusion requirement. This is measured by the Lawson number, the product of energy confinement time in seconds and plasma density in particles per cubic centimeter. The higher the density of the plasma, the shorter the time that it must be sustained to yield the desired energy release; the fusion reaction rate is faster at higher densities.

The minimum Lawson number for breakeven is 10^{13} sec cm^{-3}. It applies to what is known as the two-component mode, where the deuterium and the tritium are at different temperatures—a condition yielding the lowest threshold. This value of the Lawson number was first achieved at MIT in 1975, in the Alcator A experiment; it went on to achieve 3×10^{13} sec cm^{-3} in the Frascati tokamak in Italy, in 1981. A new MIT tokamak, Alcator C, is now operating with a goal of exceeding the thermal-mode (with the two components at the same temperature) breakeven threshold of 6×10^{13} sec cm^{-3}.

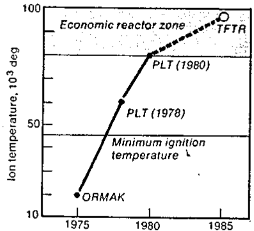
Thus, Lawson numbers now attainable are within the breakeven range. The TFTR should achieve the simultaneous conditions of temperature and Lawson number required for a fusion reaction and, for the first time, more energy will be produced than was invested (Fig 3).

Early in the development of the tokomak concept, many researchers had feared that temperatures and confinement times successfully attained in comparatively small experiments could not be maintained predictably when the plasma was scaled up to actual reactor conditions. In 1978, however, in a crucial test at the PLT—a scaled-up tokamak—the scaling laws that had been developed were verified at temperatures up to 70-million C. No apparent deterioration of confinement or instability was evident.

Magnetic mirror advances

Another magnetic-confinement concept is the linear magnetic mirror. In its simplest form, it is an open-ended tube of magnetic-force lines (Fig 6). Stronger magnetic fields exist at the ends, to reflect the plasma particles back into the interior where the magnetic field is weakest.

For commercial applications, the mirror concept has two attractive features: easier access for maintenance, and operation in a steady state rather than in



2. How plasma temperatures have inched up toward economical operating range

pulses (like the tokamak). Scaling up, moreover, would present fewer problems. The primary hurdle here is to minimize losses of charged particles at the ends of the device. The physics involved has not been developed sufficiently to define reliable reactor concepts.

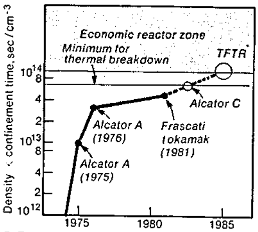
Higher values of temperature and density numbers have been achieved in magnetic mirrors than in tokamaks. But the highest Lawson number achieved is only about one one-hundredth of that for a tokamak. The 2X-IIB at Lawrence Livermore National Laboratory (LLNL) achieved an ion temperature of 130-million C (in 1975), compared with 70-million C for the PLT tokamak.

Lawrence Livermore has been assigned responsibility for developing the magnetic-mirror approach. The MFTF-B, now being built there, will operate in 1984. It is a large scaling experiment to test the mirror physics beyond the plasma parameters achievable in existing facilities.

Variations. There are several other toroidal configurations for magnetic confinement. A different form of magnetic-mirror device is ORNL's Elmo Bumpy Torus. It features a toroidal geometry as a method for feeding end losses back into the reactor. Another toroidal device—not based on the magnetic mirror—is General Atomic's Ohmic Heating Toroidal Experiment (Fig 5), primarily a joint venture with Phillips Petroleum Co. It uses a helical winding around the torus to achieve more efficient energy utilization, hence a more compact reactor.

Inertial confinement

Two steps are involved in the inertial-confinement concept: (1) Fusion reactions are initiated in the center of a small, super-dense fuel pellet, and (2) these reactions propagate outward for as long as the pellet remains intact. Just how long this lasts is determined by the size and temperature of the pellet. At the end, the pellet blows apart and the energy output ceases (Fig 8).



3. Product of density and confinement time has also approached breakeven

Small pellets—typically about a millimeter in diameter—are used because larger pellets would require a greater input energy, and because the terminating microexplosion must be contained. Typically, these small pellets have inertial times of the order of 10^{-10} seconds before disintegration.

The amount of energy released during this time depends on the pellet's compressed density, and varies with other specifics of a given pellet. Calculations show that a pellet must be compressed to at least 700 times the density of water (as many as 3000 times in some cases) for a significant energy gain to be realized from an input of 1 MJ.

Achieving high compression density with low fuel temperature in the core is the primary measure of progress in inertial-confinement fusion. An incident laser or energetic-particle beam introduces the input energy at the surface of the pellet, imploding it (Fig 8). During this compression process, it is important that the temperature of the fuel at the center (core) remain sufficiently low until the last possible moment. Otherwise, the core will exert a backpressure, inhibiting the compression and, therefore, increasing the energy input required.

Scientists at LLNL have succeeded in compressing fusion fuel pellets to more than 100 times the density of liquid hydrogen. Although this still falls about 90% short of the requirements, it is a compression level unheard of only a few years ago.

Intensive investigation is being conducted into the wavelength dependence of energy absorption, and its relation to preheating of the pellet core. Experiments now under way at a number of laboratories in the US and Europe indicate that, at shorter wavelengths, the pellet absorbs laser energy more efficiently, and the number and energy of hot electrons are smaller. (Note: Hot electrons are the cause of preheat.) Thus, energy-input amounts soon to be available should be adequate to achieve the compression levels required for ignition.

Suitable lasers, however, are not yet available. They must have the optimum combination of efficiency (6-15%), energy (1-5 MJ), and multiple-pulse capability (1-10 Hz). At short wavelengths, candidates under active investigation include krypton fluoride, the free-electron laser, and new solid-state lasers (other than glass) capable of frequency multiplication.

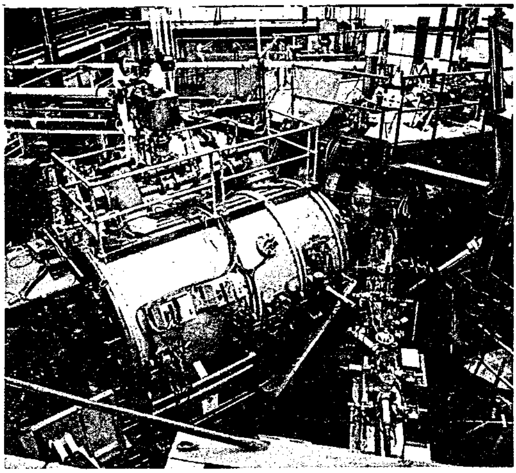
For inertial confinement, ignition is expected to occur on an energy absorption of about 1 MJ. The Particle Beam Fusion Accelerator (PBFA-I) recently completed at Sandia National Laboratories, rated at 1 MJ, has already operated at up to 840 kJ on its first test runs, without energy focus on the target (Fig 7). By the mid-1980s, researchers there hope to focus a few hundred kilojoules on target from PBFA-I, and more than 1 MJ from an upgraded version, PBFA-II. An upgraded laser at LLNL also aims at achieving ignition by the mid-1980s. (For completeness, note that heavy-ion beams are also considered a potential fusion driver. The R&D program in this area, however, has not progressed very far.)

The new lasers (krypton fluoride and free-electron lasers) have not been developed sufficiently for use in critical tests of inertial-confinement physics. The only ones suitable for tests are the existing lasers—neodymium-doped glass (1-micron wavelength) and carbon dioxide (10 microns).

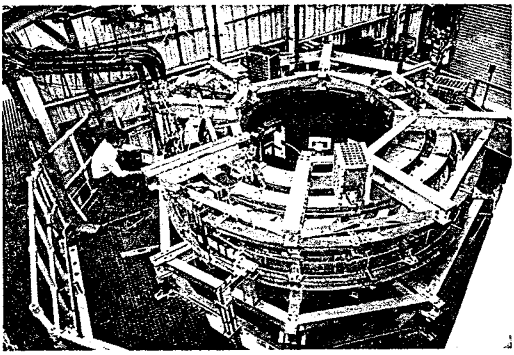
The former have a low energy-conversion efficiency, and cannot be fired rapidly—only once in 5-10 minutes, as opposed to 1-10/sec. The carbon dioxide design converts electrical power into laser light at fairly high efficiency, and could be developed to pulse at the high rate that is needed for a commercial reactor. But, because the emission wavelength is long, it does not couple energy to the pellet very efficiently.

At first glance, the laser would appear to be the best choice of driver, because its energy can be focused accurately on the target from distances long enough (more than 100 meters) to remove the high-technology components from the chamber where the reaction takes place. However, rough cost estimates indicate that the kind of lasers needed will have to be less expensive than today's lasers if they are to provide economical commercial power.

Light-ion beams are relatively low-cost and efficient, and appear likely to achieve the desired power and repetition rate. But accurate, long-distance focusing has not yet been demonstrated. Heavy-ion beams, the least-developed driver, appear to have good absorption and focusability, and high efficiency. On



4. Doublet III (right rear), world's largest operating tokamak, allows for D-shape plasma cross section, rather than circular. Neutral-beam injector is in foreground



5. Ohmic Heating Toroidal Experiment (OHTE), joint venture of General Atomic Co and Phillips Petroleum Co, aims for better energy utilization, more compact reactor

the other hand, they may necessitate the use of large machines involving intolerable capital investment.

The road to commercialization

The next step for the magnetic-fusion concept on the way to economical commercial power may be the Fusion Engineering Device (FED) in this country, or the International Tokamak Reactor (INTOR) internationally. FED is the prototype to be built by 1990 at the Center for Fusion Engineering, as mandated by the Magnetic Fusion Energy Engineering Act of 1980 (PL 96-386). It will not only bring together the technology developed to date and scale it up toward a commercial size, but it will include fueling, maintenance, and other practical engineering aspects. Most important, it will feature the direct involvement of industry—the utilities, manufacturers, and engineering companies that will actually build and use future commercial fusion-power stations.

Among the engineering questions to be faced in the construction of this device are the materials for the first wall (the inside wall of the vessel); materials for the blanket; design of blankets for heat transfer and tritium breeding; technology for superconducting coils, refueling and ash-removal systems, and auxiliary heating systems; tritium processing and handling techniques; remote-maintenance methods, and the reactor-control system.

John Gilleland of General Atomic Co is head of the FED-management team. As he sees it, a major goal of the FED will be to demonstrate the tritium fuel cycle in a practical setting. This includes finding answers to such questions as how to extract the tritium without losing any and without suffering mishaps, how to perform maintenance and repairs by remote control, and how to accomplish periodic replacement of the radioactive walls of the reactor.

The configuration assumed for a working commercial fusion reactor is a plasma surrounded by a blanket of lithium. The lithium nuclei will absorb the fusion neutrons and convert to tritium in the process, closing the fuel cycle within the reactor itself. Concern about the first-wall materials has two aspects: impurities entering the plasma from sputtering of the wall by energetic charged particles, and the maintenance and radioactive-waste problems created by activation of the wall material.

Impurities present in the plasma cause radioactive heat loss from the center of the plasma, making it difficult to achieve the temperature and confinement parameters necessary for ignition. Sputtering occurs where the outer magnetic-field lines encounter solid structural materials. One solution to the problem is use of "limiters." These are made of a heat-resistant material, such as tungsten or molybdenum, in the form of heavy bars; they are strategically located to limit the amount of sputtering.

Researchers have also experimented with the use of graphite as the limiter material. It sputters more readily than tungsten, but the resulting impurities—carbon ions—are far less damaging to the energy balance of the plasma, because carbon atoms become fully ionized in the plasma's interior. The heat is thus radiated mainly from the cold outer edge—a less-critical region, since most of the fusion reaction occurs at the center.

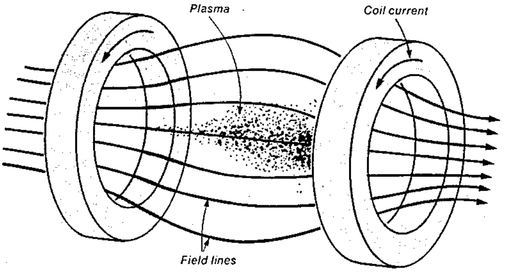
Although radioactive wastes from a fusion reactor are less than those generated in conventional nuclear plants, the radioactivity has two forms: tritium gas and activated structural members.

Tritium is a low-radiotoxicity, short-half-life isotope. Its maximum allowable concentration is among the highest for any radioactive material. It is excreted rapidly from the body, and cannot be biologically concentrated in the environment, in food chains, or in man. Since it decays with a half-life of 12.3 years to harmless helium, its health hazard is not very long-term, compared to some products of nuclear fission. However, there will be substantial quantities of it (an estimated 1-10 kg) and, being a gas, it is hard to contain. It readily permeates most common structural materials.

The other radioactivity problem arises from the fact that the deuterium/tritium reaction releases most of its energy in the form of high-energy neutrons. These pass unimpeded across the magnetic-confinement fields; some penetrate the walls of the vacuum chamber, and others are absorbed by the chamber walls. They induce radioactivity in the structure and can cause sufficient damage to require periodic replacement of the first wall. Because the structure becomes radioactive, any required replacement must be done with remote-handling techniques.

Despite the high temperatures within the plasma (around 100-million C), temperatures at the first wall can be kept at acceptable levels—300-600C—with various cooling techniques. But damage through swelling and embrittlement, and surface erosion through sputtering, will make it necessary to replace the first wall regularly, perhaps as often as every five years. Development of materials that are more radiation-resistant, a part of the assignment for the FED, could extend the periods between replacements. Selection of operating temperatures, design configurations, and protective features will also be factors in the longevity of first walls.

The ultimate reactor size will be partly determined by engineering work to be done on making the first wall radiation-resistant. In reference designs, the neutron energy-flux limits for the first wall have been set at about 2 MW/sq m of wall. Aggressive development work on materials should permit an increase in this limit. As an example, it has been found that adding titanium to stainless steel greatly improves the ductility of irradiated material at high neutron exposures and high temperatures.



6. Magnetic-mirror concept is based on open-ended tubular array of magnetic force lines. Stronger fields are applied at ends to reflect plasma back into tube's interior

Development of better first-wall materials will also enhance plant availability, a key element in utility acceptance of the concept. Frequent replacement of the first wall, combined with limited personnel access, could require time-consuming remote maintenance. Emphasis in current design studies has therefore been placed on decreasing component size, increasing accessibility, and improving manned access through shielding. Engineers working with these designs are confident that the technical problems can be solved, and that plant-availability factors no worse than those of today's nuclear-fission reactors, and consistent with utility economics, can be achieved by the time fusion is ready for commercial use.

Although problems with first-wall life and structure irradiation are common to all fusion-reactor concepts, component accessibility varies. With its doughnut-shaped reaction chamber, the tokamak has more access problems than other concepts of both inertial confinement and magnetic mirrors.

The radioactive-waste problem encountered in fusion-energy generation is created by activated structural members that have deteriorated to the point where they must be replaced. It differs from that encountered in fission energy in several important ways. Waste products of fusion are generally nonvolatile. Bound up in the structural materials, they are extremely difficult to release to the environment, even in accidents. Moreover, the types and amounts generated can be minimized by design, engineering, and materials selection. This is not true of fission radwastes to the same degree.

Another engineering question to be addressed is construction of superconducting magnetic coils. Although the technology for building superconducting magnets is essentially in place, those required for fusion-power reactors will be larger than any built to date. R&D in this direction is in progress at Lawrence Livermore and ORNL, as well as Westinghouse Electric Co, General Electric Co, and General Dynamics Corp.

Refueling methods for commercial plants will be developed and tested during the FED project. At the present laboratory level, where fusion reactors are designed to operate for very short times, refueling has not been necessary. Ultimately, it may require nothing more than surrounding the plasma with a neutral-gas blanket that feeds gaseous deuterium/tritium at the plasma edge. However, if the fuel must be injected into the center of the plasma, the problem will be more difficult. One technique proposed has been the very-high-speed injection of frozen deuterium/tritium pellets the size of sand grains into the plasma.

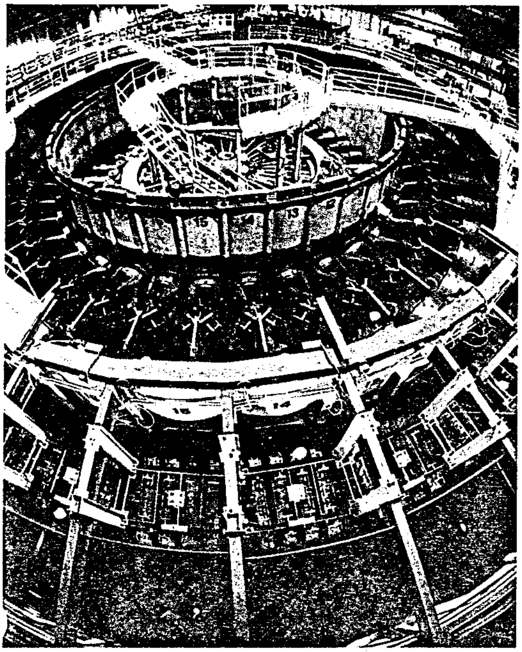
Operation of tokamak is cyclic. The current is induced in the plasma by a transformer positioned inside the torus. Limitations of the transformer flux swing make the discharge pulse cyclic. While the period when energy is not being produced is a matter of milliseconds in today's laboratory machines, it could be much longer in large commercial devices—perhaps several hours.

Energy-storage systems may be required to rectify these swings to produce a steady electric output. Complexity of the storage systems will depend on the length of the burn period, the length of the recovery period between burns, and the design of the reactor. Other fusion concepts, such as the magnetic mirror and the Elmo Bumpy Torus, deliver power steadily, rather than in pulses. Recognizing these disadvantages, fusion researchers have been devoting some effort to driving the current in a tokamak continuously.

Conceptual designs

Taking existing scientific knowledge and combining it with what is known about the kind of commercial fusion reactor that utilities will need, a design team produced a conceptual reactor design. The design is hypothetical and will never be built as such, but it is a key step in focusing future engineering efforts.

Called Starfire, the design is the work of a team at the Argonne National Laboratory, with major input provided by McDonnell Douglas Astronautics Co, General Atomic Co, and the Ralph M Parsons Co. A similar design, called MARS (Mirror Advanced Reactor System), is under development at LLNP,



7. Particle Beam Fusion Accelerator (PBFA) embodies inertial plasma-confinement concept. Plasma-ignition capability appears to be within reach

with TRW, General Dynamics Corp, Ebasco Services, and Science Applications Inc participating.

Primary criteria for making Starfire commercially attractive were economics, safety, and environmental impact. The designers also worked on reactor maintainability and plant availability, selecting design features that could reduce the frequency of failures and shorten replacement times. Systems were incorporated to reduce the tritium inventory and to keep the tritium contained within a series of barriers. Other design steps were taken to minimize the size of any potential tritium leaks. Remote maintenance was incorporated to reduce personnel exposure to radiation, and reactor materials were selected to allow recycling of all materials outside the blanket within 30 years.

To make it comparable to present generating units using other fuels, both in output and availability, the conceptual Starfire was designed to produce about 1200 MW in steady-state operation. Technologically, the steady-state mode (achieved by driving the reactor with radio-frequency power) constitutes the only radical departure from current experimental tokamak designs. Otherwise, Starfire represents a combination of the best options from previous designs, in terms of materials, blanket design, magnets, refrigeration, heat transfer, etc.

With the Starfire concept, downtime is minimized and reliability is improved by using "replace and remove" maintenance, in which failed components are replaced with spare parts and repaired. Redundant equipment is provided in areas where maintenance is more difficult. The shielding and the toroidal field-coil components comprising about 90% of the reactor's bulk are designed to last the plant's lifetime.

Particular attention in designing Starfire was given to maximizing utility compatibility, with regard not only to current practice but also to anticipated trends for future utility operation. Operating parameters for the design comprise base-load operation, a four-week scheduled maintenance shutdown each year, and a four-month outage every 10 years for turbine/generator overhaul and annealing of the toroidal coils. Combining these design features with the industry-average outages for balance-of-plant, the study projects a 75% availability for Starfire.

In extrapolating from what is now known about plasma physics and, in some cases, about actually designing equipment, the Starfire team has made valuable information available to future designers of fusion reactors.

Economic projections

The commercial-reactor design based on the Starfire concept costs an estimated $2000/kW (1980 dollars) and has a busbar-energy cost of 35 mills/kWh. There are obvious uncertainties in predicting the cost of energy for future fusion reactors. However, Charles C Baker, program director for Starfire at Argonne National Laboratory, claims that there appears to be no fundamental reason for fusion not to be economically competitive.

Ebasco has made projections comparing the economics of fusion with other electric-power sources. Such projections indicate that the levelized-busbar-power cost for the first 10 years of plant operation (1992-2002) would be about 134 mills/kWh for a fossil-fired plant burning eastern coal and 121 mills/kWh with western coal; the cost would have to be 112 mills/kWh to be competitive.

This translates into a capital investment no greater than $3374/kW escalated to initial operation at the end of 1991, or about one-third more than a nuclear plant capitalized at $2557/kW. Assumptions include tritium breeding in the fusion reactor, and fixed charges of 18.1% for coal-fired plants and 18.5% for light-water reactors (LWR) and fusion plants.

The fusion fuel cost is extremely low: 0.06 mills/kWh compared with 25 mills/kWh for LWRs and 63-78 mills/kWh for coal-fired plants. The 0.06 mills/kWh figure seems likely to be achieved within an order of magnitude, according to Ebasco. But even at 0.6 mills/kWh, the fuel cost would be low enough to provide the leeway needed for breakeven capital costs. And, as noted earlier, the fusion fuel cost would not be subject to the escalations applicable to exhaustible fuels.

The capital costs of fusion plants are expected to be slightly higher than those for nuclear-fission plants. The quantities of materials should be about the same, but a few special problems peculiar to fusion are anticipated. On the other hand, fewer licensing delays are anticipated during the construction stage.

Inputs from utilities are needed at an early stage if fusion plants are to be competitive. Among the factors considered essential by utility executives are the following:

- Safe and licensable technology.
- Cost and lead time comparable with or better than other generation technologies.
- Environmental impact no worse than that of coal or nuclear fission.
- Design simplicity to minimize probability of an accident and negative impacts on reliability.
- High plant availability.
- Instrumentation and controls matched to operator limitations.

Industry cooperation vital

There should be significant industry involvement in the Fusion Engineering Device. The Congressional Fusion Advisory Panel (Energy Research & Production Subcommittee, House Science & Technology Committee) recommends strongly that DOE immediately perform an in-depth study of organizational options for managing the Engineering Test Facility (ETF) project, and that it include major input from the existing fusion community—particularly from qualified industrial organizations.

The panel has urged that the management approach for the project ensure that "responsibility for the design and construction phases be placed in the hands of an organization or consortium that is experienced in the management and operational aspects associated with the construction of facilities of similar magnitude and complexity."

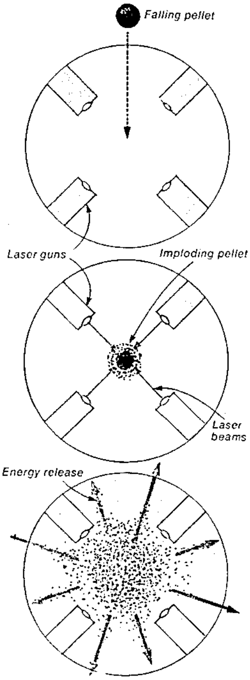
Although the ambitious ETF has been replaced by the somewhat more modest FED, there is no less need for intense industry involvement. The FED will be managed by the Center for Fusion Engineering (CFE). This is yet to be set up, but all indications are that it will include heavy participation of industry. Although it is too early for any industry action to finance fusion, DOE spokesmen have indicated that the time is ripe for corporations to begin determining how to acquire the capital necessary to build and operate fusion reactors.

Industry appears eager to join in. A typical attitude is expressed by Harold K Forsen, manager of Engineering & Materials for The Bechtel Group. Pointing out that CFE, as well as FED, will be responsible for managing development of technology, and of reactor systems and components, he urges government to develop a mechanism to include industry in the design criteria, and to select one organization, possibly an industrial contractor, to head CFE. His main concern is that the engineering and manufacture of fusion systems "may take longer and cost more than developing the science."

The prime mover behind passage of the Magnetic Fusion Energy Engineering Act was former Rep Mike McCormack (D-Wash). Pointing out that fusion "will provide adequate supplies of energy to allow upgrading of lifestyles and to promote world peace," he has cautioned that peace could be in jeopardy as nations increasingly grab for depleting stocks of fossil fuels.

McCormack contends that US energy requirements will increase dramatically by the year 2000, and calls for construction of 400 more nuclear-fission plants and a tripling of coal production by that time.

According to DOE's Energy Information Administration, at the production levels recorded in 1980, the US has 10 years of proven oil reserves. Another nine years are promised by "indicated" reserves (known to be producible with secondary-recovery techniques). The equivalent of a new Prudhoe Bay, therefore, would be needed every decade to continue present production capabilities into the next century.



8. In inertial confinement, fusion is initiated at center of tiny glass pellet. Laser or particle beams implode pellet, raising density sufficiently to support fusion reaction (center). Reaction continues until pellet blows apart (bottom)

For years, natural-gas production has outstripped discoveries of new reserves, although this trend may have been temporarily reversed by the vast discoveries that were made following price deregulation. Although the US has more than a quarter of the 786-billion tons of the world's known coal reserves (the Soviet Union and China each have about as much as we do), much of it is inaccessible with present technology or at present prices.

Both coal and nuclear power continue to face great political problems hampering their full use. Concern for the environment, health and safety—and, in the case of coal, labor stability—all will tend to keep us more dependent on oil than we would like to be, until a less-controversial energy source, such as fusion, is available.

Since the fuel for fusion plants is in virtually inexhaustible supply, fuel costs should be stable and low. The primary cost for a fusion central power station will be the capital investment. Until fusion becomes a widely used energy source, the costs of building the fusion machinery and plants, as with all goods, will rise with the cost of energy. Thus, although the use of fusion-produced power should ultimately stabilize energy costs, even in the face of dwindling mineral-fuel supplies, the level at which they are stabilized will depend on how soon fusion becomes commercial.

Fusion timetable. DOE has projected a timetable leading through construction of the ETF, starting in 1984 and operational by 1992; an experimental power reactor, to be begun in 1997 and operational by 2004; a demonstration plant, begun in 2005 and completed in 2015; and commercial plants actually constructed in the last part of that decade. The timetable is based on sequencing of R&D activities and the level of funding thought likely or desirable.

Our own organization—an association of industrial companies already involved in fusion work—considers the pace of further development efforts to be limited by the funds available rather than by science and technology. In the same vein, the Fusion Advisory Panel says, "The primary problems facing expeditious development of fusion power are not at present technological, they are institutional." Both agree that the timetable can and should be moved up, so that fusion power becomes commercial by the end of the century. This was the sense of the legislation passed by both houses of Congress in 1980. All that is needed now is the funding and the institutional organization to move ahead.

Edited by Sheldon D Strauss

Casting Fusion Adrift

by Edwin E. Kintner

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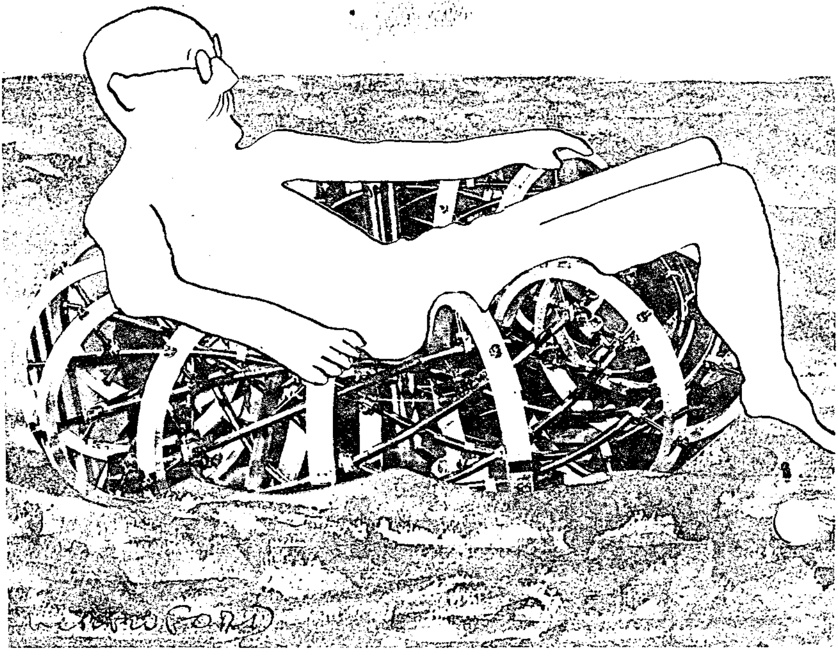
Casting Fusion Adrift

by Edwin E. Kintner

The nation's pace-setting program for exploiting the ultimate energy resource is in danger of going under.

THE development of fusion energy has reached a critical point. There is now a consensus: We can safely assume our ability to generate and contain plasmas under conditions we believe will make possible commercial energy production. We should now proceed in earnest to develop the engineering technology required to refine this scientific achievement for practical use.

But ironically, that judgment has been reached in the U.S. just when federal discretionary funding, especially for energy research, is being sharply restricted. As a result, the technological development

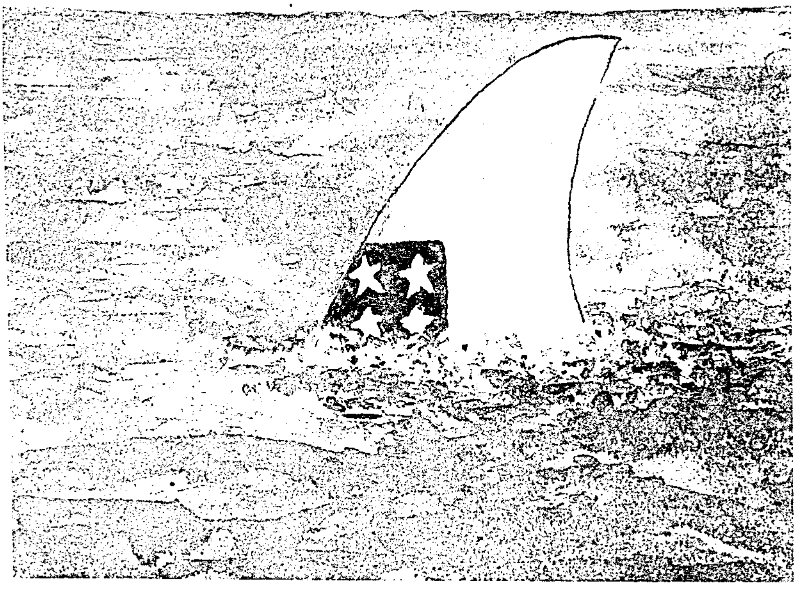


of fusion, although already planned by the Department of Energy and mandated by Congress, will be delayed indefinitely.

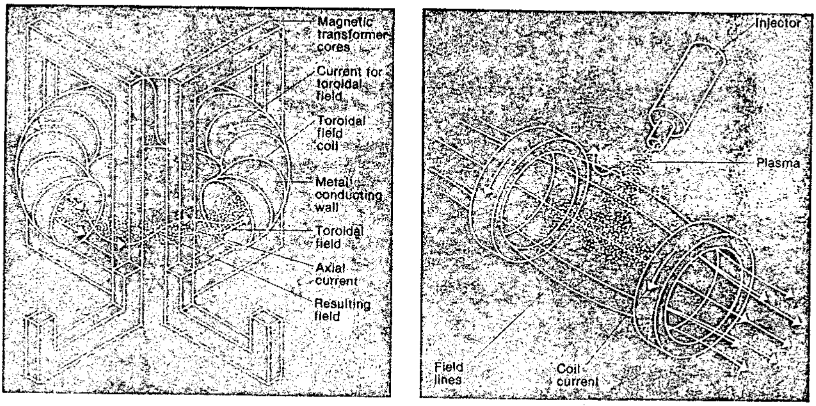
To understand the implications of this decision, it must be put in the context of our overall energy circumstances and the potential contribution of fusion.

Energy is a crucial variable in the future of modern civilization, the sine qua non of industrial society. Energy provides light in darkness, heats our homes, and cooks our food. It is necessary for the production of goods, rapid transportation and communication, and modern agriculture and food processing. Its rising cost in the past decade has already slowed the economies of all industrial nations, and its geographical distribution increasingly influences diplomatic and military decisions. The most important potential trigger for thermonuclear disaster is competition for energy, and its cost and availability have become the dominant factor in Third World development.

Industrial societies have now made use of all known energy resources except fusion, and the availabilities, cost, and effects of these conventional resources are relatively well known. Fossil fuels are finite in quantity and produce negative environmental effects;



Even if the research
goes reasonably well, that achievement would
require the expenditure of
about $15 billion.



How magnets confine plasmas in the two most promising fusion systems. At fusion conditions all materials are plasmas, composed of electrons and dissociated ions, both of which tend to rotate around magnetic field lines. The goal of both systems is to arrange magnetic fields so that they efficiently confine and insulate the plasmas. In the tokomak device, current in a toroidal field coil creates a strong magnetic field that defines a "doughnut" through which the plasma flows. In the mirror device, circular magnets create a bundle of field lines compressed at the ends.

solar energy is useful but limited. Even if nuclear fission, including the breeder reactor, is fully utilized, the United States faces an energy shortfall within the next two decades. There is a vital need for an additional large-scale energy resource.

Controlled thermonuclear fusion meets that need. It offers greater flexibility in end products and brings with it only modest environmental threats, and it uses a benign natural resource: water. If fully developed and successfully deployed, fusion could greatly ease world energy problems indefinitely.

Under these circumstances, it is unthinkable not to deliberately but aggressively continue to develop fusion to practicality. The price of failing to do so will not be paid for many years—perhaps decades—but it will be very high. This is true even if the effort proves unsuccessful, because we will more clearly understand our limits and can better plan the use of our remaining energy resources.

How Matter Becomes Energy in the Fourth State

The goal of the fusion process is to bring together the nuclei of two or more elements under conditions where they fuse together, releasing significant quantities of energy. Since vast energies must be imparted to the nuclei, the lighter elements—especially the isotopes of hydrogen known as deuterium and tritium—are preferred. This fusion process does not release particulates or gaseous effluents. The total radioactivity generated is expected to be one-hundredth to one-thousandth that of a fission reactor producing the same power. The radioactivity is also more benign than that in fission reactors; there are no fission products such as strontium and iodine that pose special health hazards.

The most significant element of radiological concern is the tritium fuel. Tritium has a relatively short radiological half-life (14 years) and a much shorter biological half-life (approximately 12 days), and therefore poses a very different threat from that of the waste products of a fission reactor. A fusion reactor would lack sufficient fuel for a runaway nuclear explosion and contain insufficient residual heat for a core meltdown.

To fuse, the nuclei of light elements must be heated to very high temperatures—around 100 million degrees, or five times the temperature of the sun's

If fully developed and successfully deployed, fusion could greatly ease world energy problems indefinitely.

core—and confined at high pressures. Under these conditions, all materials are plasmas—the so-called "fourth state" of matter in which electrons are dissociated from their nuclei. Such a mixture of charged particles can be confined in one of three ways: by gravitational fields such as those of the sun and stars, by inertial reactions as used in thermonuclear bombs, or by magnetic fields. The first method is impractical on Earth because it requires masses as large as the planet Jupiter. The second is being investigated as a part of U.S. defense research. The third—the most practical—is the subject of the Department of Energy's magnetic fusion program.

Present systems for magnetic confinement of plasmas are based on the fact that charged particles such as ions and electrons tend to rotate around magnetic field lines. Thus, to confine plasmas, scientists seek to construct containments of such field lines—either in doughnut-shaped forms (toruses) or bundles of lines squeezed together at their ends (mirrors). But the confinement is temporary; even with very strong magnetic fields, the plasma particles tend eventually to drift across the field lines and escape. The problem is to find arrangements of magnetic fields in which plasmas can be confined and heated enough to reach the temperatures and pressures adequate for fusion with a net energy gain.

Developing and commercializing this process is a difficult technical task. Fusion research has been underway for 30 years, and another 20 years may be required before its practical usefulness is actually first demonstrated. Even if the research goes reasonably well, that achievement would require the expenditure of about $15 billion. These requirements of time and money are not easily met by a political system that tends to emphasize fast payback, low risk, and flexibility. This difficulty is probably fusion's greatest problem.

Though fusion energy is many years away, our investment in the strong, aggressive program begun during the crisis of the oil embargo of 1973 has brought significant returns. The United States has assumed prestigious world leadership in this advanced scientific field, a leadership held until 1973 by the Soviet Union. This effort has resulted in new developments in many areas in addition to high-temperature plasmas, including superconducting magnets; high-temperature, irradiation-resistant materials; high-voltage, high-power electrical equipment; and mathematics and computational techniques. This advanced work has already yielded valuable industrial spinoffs that have helped maintain U.S. technological leadership and economic strength, and fusion research and development will continue to have this kind of payoff in the future. In fact, no other energy or defense program contributes as strongly to broad, long-term research needs—the kind of support provided in past years by nuclear energy, radar, and space research. This continued leadership has substantial practical value for the United States, especially in a period when the country's technological position seems to be eroding.

A Decade of Ever-Increasing Optimism

The fusion program began in 1951, when a number of scientists, notably Lyman Spitzer at Princeton, noticed that magnetic fields diverted and constrained plasmas in solar flares and the aurora borealis. It was a time of great confidence in nuclear science spawned by the successful development of nuclear weapons, and the problem of confining plasmas at high temperatures and densities for the fusion reaction was tackled with optimism.

But by 1959 there was growing pessimism. Instead of acting as a mixture of discrete particles as early researchers had assumed, plasma exhibited collective electrostatic and magnetic instabilities that allowed it to escape from the confines of magnetic field lines. By 1965 these problems seemed so difficult that a review committee of the Atomic Energy Commission recommended that modest research be continued only in a few carefully delineated directions.

But four years later, the Soviet physicist L. Artzimovich brought to M.I.T. information about plasma containment in a new device called the "tokamak." The Soviets used the same toroidal magnetic-field configuration studied in the U.S. for many years, but they induced a very high current—hundreds of thousands of amperes—in the conducting plasma. The resulting magnetic field around the torus had a helical shape that greatly improved its ability to contain the plasma at higher pressures and temperatures.

U.S. physicists immediately turned to tokamak research and confidence soon grew. Indeed, by 1973, as part of Nixon's Project Independence, the U.S. embarked on a strengthened fusion program with the tokamak confinement concept as its central thrust.

The U.S. program has since progressed rapidly, with almost continuous exponential improvement in all the main aspects of fusion confinement. Scientists have now obtained temperatures in the area of con-

Controlled thermonuclear fusion brings only modest environmental threats and uses a benign natural resource.

finement of 80 million degrees. (The minimum ignition temperature is 45 million degrees, and 100 million degrees is necessary for a practical reactor). The degree of confinement of the plasma by magnetic fields is now within a factor of 5 to 10 of that desired for a practical reactor, and the laws of plasma confinement are now sufficiently understood so that remaining improvement can be assumed. In fact, three experimental tokamak devices expected to generate and contain plasmas at the conditions required for a power reactor—the Tokamak Fusion Test Reactor at Princeton, the Joint European Torus at Culham Laboratory in England, and the JT-60 in Japan—are now under constuction.

The effort to develop a "mirror" of magnetic-field lines to confine the plasma has also made remarkable progress in the last two years, and a comparison between tokamaks and mirrors should be possible by the mid-1980s. There are also efforts to develop several smaller confinement systems. The most important is the Elmo Bumpy Torus in which a number of mirror cells are arranged in a toroid so that the losses from one cell are picked up in its adjoining cells.

Obviously, the intent of the U.S. plasma physics program has not been to press forward at maximum speed by concentrating only on tokamaks as the lead concept. Rather, the goal has been to generate a broad spectrum of knowledge of plasma confinement by magnetic fields so that the we can select the best, most useful arrangement and then efficiently perfect it.

The plasma confinement problem can be thought of as a giant jigsaw puzzle. As scientists gain additional understanding, they are able to gather its pieces together in clumps, some of which—such as the tokamak clump—are larger than others. Just as progress accelerates as the jigsaw puzzle nears completion because remaining pieces fall into the puzzle more rapidly, so progress is accelerating in plasma containment. The several lines of plasma-physics development have converged, and now much of what has been learned about mirrors or tokamaks or Elmo Bumpy Toruses is directly useful in the other confinement concepts. The experimental devices now in place or under construction, and the relatively inexpensive modifications possible over the next decade, should give us the insights necessary for fusion to become a practical success. We will need continued research for the rest of the century to reach highest efficiencies, but a successful end can be predicted with great confidence.

An Unprecedented Technological Challenge

With this assurance we can begin to work in earnest on the new engineering technologies involved in fusion reactors. There are two main objectives: to provide the higher-powered magnets, plasma heaters, and instrumentation for continued advances in plasma-physics research; and to solve the major technological problems in building practical, power-producing reactors using the fusion principle. These engineering tasks will probably require greater effort than the purely scientific development now coming to fruition.

The U.S. program in magnetic confinement, which has already provided the basis for the recent success in plasma temperature and pressure, is the strongest in the world. Meanwhile, our technology program has made a good start by identifying many engineering requirements, and some preliminary programs and facilities to fulfill them are now in place or planned.

The foremost engineering challenge involves structural materials. Almost without exception, new technologies succeed or fail because of materials. Jet aircraft would have been impossible without lightweight, strong aluminum alloys and high-temperature materials for engine blades, fission energy required the development of zirconium and its alloys as fuel cladding materials, space exploration was based on the development of a variety of new materials for rocket engines and ablation shields, computers and modern electronics were made possible by the development of semiconductors. Most of these materials will be essential in fusion technology.

There will also be new and difficult materials challenges: the environment surrounding a burning fusion plasma will contain large concentrations of high-energy neutrons, and no materials now exist that adequately resist deterioriation by high-energy neutron fluxes. In addition, materials are needed for blankets and shields, between the plasma wall and the magnet structure of the fusion reactor, that can absorb and transmit the heat to the power-producing machines while protecting the magnets and breeding tritium fuel. Studies of the engineering requirements for these components are only now beginning.

Obtaining economical fusion energy will also depend upon constructing superconducting magnets of larger size and power than have yet been built. A good start has been made in the development of magnets for the T-7 tokamak in the Soviet Union and for other fusion machines in the United States, and it now

Our investment in a strong fusion program began during the Arab oil embargo of 1973 and has already yielded significant industrial spinoffs.

appears that such magnets can be built. However, a great deal more development and operating experience will be needed.

An operating fusion power reactor will become radioactive—not as strongly as the core of a fission reactor, but enough so that maintenance and repair will have to be done remotely or from behind radiation shields. This problem will be encountered for the first time with the Tokamak Fusion Test Reactor now under construction at Princeton, and considerable development will be required before fusion power reactors can be properly maintained.

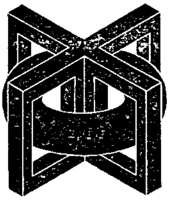
A Congressional Mandate

The recent advances in plasma confinement and the resulting increased interest in fusion have led to a number of independent reviews of the fusion program. The first was initiated by the Department of Energy in 1978 and chaired by John Foster, former head of research and development in the Department of Defense who is now vice-president of TRW. Among the conclusions of this group: "The first objective of the program must be to determine the highest potential of fusion as a practical source of energy. There is an urgent need to answer the questions concerning feasibility, and . . . there must be a major increase in emphasis on engineering problems to provide the basis for proceeding to the choice of a prototype reactor."

The Department of Energy responded to these recommendations with a Policy Statement on Fusion, the first of its kind on any energy technology: "There is no lead time to spare. If fusion energy is to be available when it is needed, the research and development program, rigorously directed toward the goal of commercial utility, must be undertaken in earnest now."

Following these recommendations, the magnetic mirror program was strengthened by a decision to build a major test facility, and an experimental Elmo Bumpy Torus facility was also launched.

In early 1980, Congressman Mike McCormack, chairman of the House Subcommittee on Energy Research and Production, organized a Fusion Advisory Panel of distinguished scientists to hold special hearings on the status of the U.S. fusion program. The panel concluded that "very significant recent technological progress in fusion research" warranted "an engineering thrust centering on an engineering test facility. . . . Our panel believes that fusion can be



Six Policy Imperatives for Fusion Success

GIVEN present constraints, what should be done to assure an effective program to assess and develop the future contribution of fusion to world energy needs?

The following six steps are essential:

☐ A wider public understanding of our present and likely future energy situation, and the significance of fusion within that context, must be encouraged. Such public understanding can be developed only with the active participation of all media and by unremitting efforts of knowledgeable citizens in all parts of society. It is surprising and troubling that fusion has no vocal proponents at influential levels of American life.

☐ A new strategic timetable for fusion development, dictated wholly by technical objectives and not short-term political considerations, should be established as soon as possible. The plan should emphasize pursuit of all aspects of fusion engineering research, including a Fusion Engineering Device.

☐ The United States should take the lead in making fusion an international project by sharing its knowledge with other nations and delegating responsibility for some research areas. Fusion research could provide a unique precedent for international technical efforts in many fields of common concern, such as acid rain and the buildup of atmospheric CO_2.

☐ Industrial participation in fusion research should be encouraged and expanded. If fusion is to contribute to industrial technology and become a practical energy resource, industry must be involved, especially if it is to make large investments of capital and personnel.

☐ The fusion program should develop a strong organizational structure independent of short-term energy programs in sponsorship and funding. Fusion has not done well in competition with other energy technologies and science programs because it does not promise immediate—or even certain—benefits.

☐ The safety and regulatory programs governing fusion must be independent of those controlling fission reactors and not simply an addition to existing programs of the U.S. Nuclear Regulatory Commission. The safety concerns of fusion are unique, and experience and practices with present reactors would not be appropriate precedents.—*E.E.K.* ☐

made commercial before 2000 if a national commitment is made soon."

The Department of Energy chartered another thorough scientific review of the magnetic fusion program later in 1980, this time by a panel headed by Solomon J. Buchsbaum, vice-president of Bell Telephone Laboratories who was then chairman of the DOE's Energy Research Advisory Board. A broadly experienced group of scientists and engineers, the Buchsbaum panel has been called "the most powerful group of its kind ever assembled for a scientific program review." Its major recommendation was that "a broad program of engineering experimentation and analysis under the aegis of a Center for Fusion Engineering" be established to achieve economically feasible magnetic fusion.

A key element of the program would be construction of a Fusion Engineering Device to "provide a focus for developing and testing reactor-relevant technologies and components" and to help "explore and firmly delineate problems of operator and public safety." Such a device should be in operation within ten years, the Buchsbaum panel said, and the cost was estimated at "not more than" about $1 billion (in 1980 dollars). Achieving these goals, said the Buchsbaum panel, would require "a doubling in the size of the present fusion program in five to seven years."

Following this report, the U.S. Congress enacted the Magnetic Fusion Energy Engineering Act of 1980, which declared that U.S. policy is "to accelerate the national effort in research, development, and demonstration activities related to magnetic fusion energy systems.... To ensure the timely commercialization of (such) magnetic systems, the United States must demonstrate at an early date (their) engineering feasibility." The act recommended 25 percent increases in magnetic fusion budgets in each of the first two years of the new program.

With the coming of the tokomak in 1969, fusion had been given mission orientation; the goal was—naively, I think—to compete with other energy forms in the commercial market at the earliest possible time. But this simplistic approach failed to reflect the technical difficulties involved in obtaining energy from fusion. The studies of 1978-80 were far more sophisticated. They focused on the vital issue of determining as soon as possible whether fusion can be counted on as a long-range energy resource, and they set as the major near-term goal the confirmation of both the scientific and technical feasibility of fusion by the early 1990s.

Technology Lost in the Budget Battle

Recent implicit and explicit decisions have dimmed the prospects that the strategy and timetable mandated by the Energy Engineering Act can be carried out.

The initial budget request by the Magnetic Fusion Program Office for 1981-82 was $525 million. This included strong support for two new projects in the scientific program—proof of the Elmo Bumpy Torus principle and construction of an Advanced Toroidal Facility to consolidate what has been learned about tokamaks and other toroidal systems worldwide. It also included funds for construction of a Fusion Materials Irradiation Test (FMIT) Facility, the only project anywhere in the world capable of irradiating materials at the high neutron energies and fluxes of fusion reactors, and $33 million to establish a Center for Fusion Engineering and begin in earnest the design of the Fusion Engineering Device.

The Office of Management and Budget reduced this request by $18 million before it went to Congress as part of President Carter's 1981 budget, and it was later further reduced by the Office of Management and Budget to meet the Reagan administration's guidelines. Thereafter, a series of step-by-step reductions in fusion budget plans for 1981-82 and 1982-83 made it clear that the strategy and timetable for fusion set forth by the 1978-80 studies could not be maintained. The amount available in the 1981-82 budget for the Center for Fusion Engineering was cut to $9.1 million, allowing only for organizational expenses and conceptual design activities. The new proposals effectively canceled the Fusion Materials Irradiation Test Facility, despite the fact that materials development has been recognized for years as the most difficult, longest-term technological problem in fusion. The Elmo Bumpy Torus program received sufficient funding to move ahead at half pace, but the new initiative in toroidal confinement was canceled. Congress reinstated $14 million for the FMIT, and the 1982 fusion budget was settled at $456 million.

The initial request by the fusion program office for 1982-83 was for $596 million. At this level, all the steps required by the Magnetic Fusion Energy Engineering Act could have been modestly reinitiated, including establishing the Center for Fusion Engineering, beginning the long-term research to obtain competitive designs for a Fusion Engineering Device, and continuing a strong physics program.

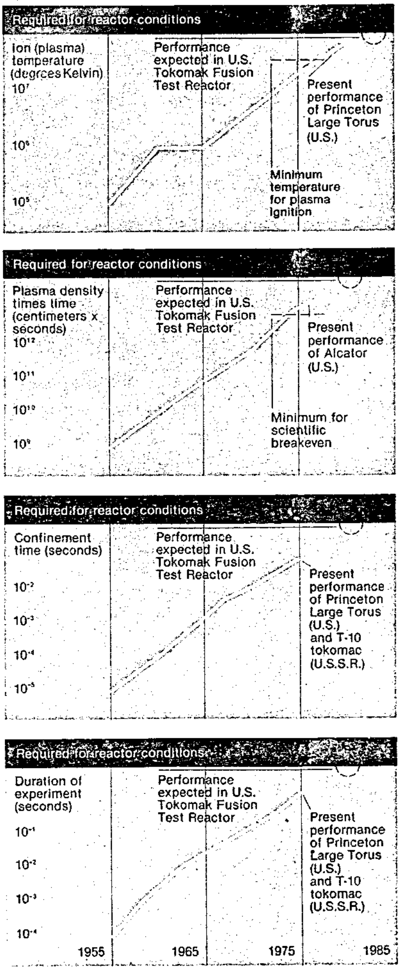
The Department of Energy's review of these proposals in June 1981 continued to embrace the goal of determining "the engineering feasibility of fusion energy during the next decade." But it was understood that while DOE would do engineering work, it was "not prepared to commit to the construction of a Fusion Engineering Device at this time." Accordingly, an 1982-83 budget of $557 million, about 10 percent above the 1982 budget, was proposed—the minimum required to hold to the basic strategy of the Magnetic Fusion Energy Engineering Act, although at a much reduced pace. The large increases in expenses visualized by the act were delayed beyond FY 1985. As the program director, I believed that this proposed 1983 program balanced the need to reduce expenditures with the strong program recommendations made by the review panels and enacted by Congress.

Now, however, with no formal hearings between the Department of Energy and the Office of Management and Budget, the 1983 budget has been presented to Congress with a total of $444 million for the fusion program. This is 25 percent less than the 1977 budget in real terms.

That amount allows for continuation of present laboratory activities, including the tokomak at Princeton. Indeed, most existing experimental efforts and many activities in the plasma physics program may be overfunded. But the $444 million does not provide for the broadening of activities recommended by the Foster committee nor the engineering initiatives recommended by the Buchsbaum panel and authorized by Congress. The 1983 budget provides only sufficient funds to keep a small design team working on the Elmo Bumpy Torus project; it provides no funds for the Advanced Tokamak Facility. It puts the Fusion Materials Irradiaton Facility "on the shelf," although over $60 million has already been spent for its design and development, and there are no funds to actively plan for the establishment of a Center for Fusion Engineering or a Fusion Engineering Device.

Finally and most important, the 1983 budget removes $25 million from the funding profile for the major mirror machine. If similar amounts are removed over the next two years as well, as implied by OMB actions, the mirror machine will be delayed by almost three years.

Two fundamental program decisions are implied in these changes. The first is to forego, for an undetermined but extended period, the strategy and schedule recommended by the Buchsbaum panel and incorporated in the Magnetic Fusion Energy Engineering



Thirty years of progress in magnetic fusion energy research. All four of the conditions believed necessary for the self-sustained fusion reaction required for useful energy production are now being approached under laboratory conditions, and successful achievement of all four is predicted with great confidence. There remains the task of developing the technology to harness these extraordinary reactions in machines of commercial scale.

Recent budgetary decisions leave the fusion program without a strategic backbone.

Act of 1980. (Senior officals in the Department of Energy characterized this act as "permissive" and "a silly piece of paper.") The second is to delay the major mirror facility by up to three years and, therefore, to postpone by at least that long the comparison between toroidal and mirror systems.

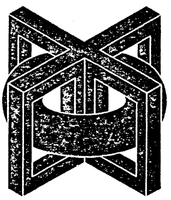
Those decisions leave the fusion program without a strategic backbone—it is a collection of individual projects and activities without a defined mission or timetable. The plan to increase industry involvement in fusion development is postponed indefinitely, and the industrial and economic benefits of high-technology spinoffs, surely an increasingly important by-product of an accelerated fusion technology program, will be lost. These spinoffs could have been expected in areas such as vacuum tubes handling higher power and frequency than any previously developed, new superconducting materials, metals of higher irradiation resistance, robotics, and new computational techniques.

In sum, these budgetary actions redirect fusion research enitrely away from the practical engineering developments that could prove critical when fossil-fuel reserves are exhausted.

Why this sudden reversal in national policy? The obvious answer is that proceeding as planned requires a mortgage on the future at a time when the administration's overriding objective is to reduce such commitments. But there are gross inconsistencies between this thrust and the administration's stated energy policies. The National Energy Plan as forwarded to Congress by President Reagan says: "There is an appropriate federal role in certain long-term research. . . . [to bring] promising technological innovations to the point where private enterprise can reasonably assess their risks. . . The federal government recognizes a direct responsibility to demonstrate the scientific and engineering feasibility of fusion."

The administration's recommendations for other research programs in the Department of Energy (high-energy physics, basic energy sciences, and nuclear physics) generally were consistent with this policy statement. Only the technology programs—solar, synthetic fuels, fusion, and others—have been grossly weakened.

These budget reductions would be more readily understandable if the magnetic fusion program were failing, but the last several years have seen rich experimental and theoretical advance, with more promise for the achievement of practical fusion energy than ever before. These advances represent the start of an invaluable return on the investment the United States



Synthetic Fuels Through Fusion

OF all the fossil fuels, petroleum and natural gas are most in demand and most limited in supply. Substitutes for them will have to be found if we are to avoid future worldwide institutional and social disruptions early in the next century, perhaps sooner. Since many years are required to develop and implement major new technologies, serious studies of all potential alternatives for producing substitute forms of liquid and gaseous fuels must be undertaken now.

As I point out in the accompanying article, fusion is perhaps unique as a potentially inexhaustible major energy source. Its fuel is deuterium and lithium—the former available in seawater, the latter in widespread granite and brine deposits. Two reaction paths are followed in a fusion reactor. The deuterium-tritium reaction produces a helium atom and a highly energetic neutron. The deuterium-deuterium reaction produces either a helium nucleus and a moderate-energy neutron or a tritium atom and a proton. Some or all of the tritium then reacts with deuterium to produce a substantial number of the high-energy neutrons. These high-energy neutrons, carrying 80 percent of the energy from the fusion reaction, are highly penetrating, and will be captured by an absorbing blanket surrounding the reacting plasma chamber at some distance from the plasma.

This means that fusion reactors will have a unique characteristic—they will deliver their principal reaction energy at a distance from the reacting plasma, and this will make it possible to decouple the energy production and conversion systems. The blanket can be designed with operating characteristics essentially independent of the plasma and the requirements for its confinement and reaction. Thus, a blanket design could be developed that focuses almost exclusively on the special requirements of synthetic fuel production.

The key to the production of synthetic fuels is hydrogen; it is the essential ingredient that must be added to coal in converting it to liquid or gaseous synthetic fuel. Two interrelated processes would use the high-temperature heat

The day when fusion can make a significant contribution to world energy resources will be delayed indefinitely.

has made since 1973. A doubling of the present fusion budget from $400 million to about $800 million annually over a five-to-seven year period seems a modest investment considering past progress and the historic implications of fusion.

Delaying the Fusion Era

What are the consequences of this decision to turn away from a comprehensive program for the development of fusion as a practical energy resource?

☐ Strong U.S. leadership in the world's fusion program will be seriously weakened. The U.S. program, although only one-third of the world effort, exerts significant leverage on the remaining two-thirds. The effectiveness and confidence with which the United States has carried out its program since 1973 has stimulated fusion research everywhere, especially in Japan. With the possible exception of the Japanese, world programs will slow down in response.

☐ Divisive forces will surface within the U.S. program: one confinement concept versus another, one laboratory versus another, science versus engineering, industry versus national laboratories, national versus international objectives. In a time of consensus, competition is a source of strength. But in a time of restricted funding, it may become a point of controversy.

☐ With their strategic plan abandoned and additional budget cuts, researchers will be hard pressed to maintain morale and progress even toward reduced goals. The diminished short-term industrial payoffs from fusion development will likely lead to additional attempts to restrict the fusion program's objectives to science-oriented activities.

☐ Most important, the day when fusion can make a significant contribution to world energy resources will be delayed indefinitely.

Must the United States abdicate its leadership role? Must we fail to aggressively pursue the development of the world's last energy resource because our social and political systems lack the will and vitality to accept such a challenge? If we do not now proceed deliberately and vigorously with the development of the engineering side of fusion, the future will *look* different within 10 years and will *be* different within 25.

Edwin E. Kintner resigned early this year after five years as director of the Department of Energy's Office of Fusion Energy in protest over the programatic changes described herein. He has been associated with navy and civilian nuclear power programs since receiving master's degrees in naval engineering (1946) and nuclear physics (1950) from M.I.T.

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from a fusion reactor to produce hydrogen by dissociating the hydrogen and oxygen atoms in water. These processes include thermochemical cycles, in which the energy required to break the chemical bond in the water molecule is supplied directly as heat from the reactor; and high-temperature electrolysis, in which electrical energy is added to the thermal energy needed to dissociate the hydrogen and oxygen.

A study at Brookhaven National Laboratory of a high-temperature electrolysis plant, driven by a tokamak fusion power reactor with a blanket at temperatures above 1000°C, suggests that production of approximately 1,000 metric tons of hydrogen per day is possible. That amount is equivalent to 20,000 barrels of oil if burned directly as fuel, and 40,000 barrels if used to convert coal into synthetic oil. At the same time the reactor would breed tritium in a lithium blanket to sustain operation of the fusion reaction itself.

The overall efficiency of the process, in terms of the energy gained from the fusion reaction compared with the chemical energy of the hydrogen product, is estimated at 50 percent. Conventional electrolysis using fusion-generated electricity could readily be used to decompose water, but this system would be far less efficient because of the limitations inherent in the production of electricity by low-temperature electrolysis.

In addition to providing an efficient source of hydrogen, fusion could also provide the substantial thermal energy required with the hydrogen for converting coal to synthetic liquid or gaseous fuels. With fusion supplying this conversion energy at higher temperatures than can be attained from other sources, the amount of synfuel produced from a given quantity of coal could be increased by a factor of two to three. At the same time, the environmental impacts (including release of carbon dioxide) of coal combustion would be reduced by a similar factor.

As I point out, research on fusion technology for practical use is well underway. We confidently expect the demonstration of energy break-even conditions in a fusion reaction between 1985 and 1990.

If fusion development is now pursued aggressively, a demonstration of technical feasibility, including substantial fusion power production in a high-temperature blanket, could occur in the early 1990s. Following further development and scale-up, the commercial feasibility of using high-temperature blankets for the production of synthetic fuels from fusion could be demonstrated early in the twenty-first century, just when supplies of liquid and gaseous fossil fuels are being seriously depleted.—*E.E.K.* ☐