GENERAL DYNAMICS

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5 February 1992

Dr. Rulon Linford
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Dear Dr. Linford:

Enclosed is our final mark-up of the draft FEAC Subpanel 2 report that was requested by Dick Siemon for submittal by 7 February (we attach only the pages for which we suggest modifications). We have focused our attention **mainly** on sections II D, the "Parallel Path Scenario," and VI, "Industrial Participation."

In general, we find that the report presents an accurate picture of the Subpanel deliberations, yet it seems to have certain weaknesses that I would like to illuminate in this letter.

First, while the section on Industrial Participation is very direct, I fear that if past history is any evidence then industry will not be extended a significant role in the ITER program unless the DOE makes a deliberate and visible policy decision to do so. If this forecast should come to pass, then I fear that the U.S. fusion budget will not be treated well in FY 93 and in subsequent years. Appended to this letter is a hierarchic chart that reveals this logic. I have submitted this chart to Alex Glass and Tom James for their consideration. I sincerely believe that congress has been sending a message with their funding profile for Fusion Energy. It is vital that we respond credibly to reverse the funding trend.

Second, I have learned that the estimated cost of the parallel path scenario is very likely not to be as high as has been discussed in Subpanel deliberations. It is my understanding that MIT has prepared two separate estimates of the cost of a small driven fusion reactor, and each of these is well under $ 1 Billion. In addition, Locke Bogart of my staff performed a fusion reactor design study that was completed in 1987 which also estimated the cost to be substantially less than $ 1 Billion. These independent estimates cannot be ignored by the panel as they were done professionally and with the best available information. Therefore, it would be appropriate to estimate the cost of the driven machine as in the "billion dollar" range. All statements in the text that discuss costs would be more accurate if they reflected this estimate.

Last, we have unsuccessfully tried in previous drafts to include the "global" effects of a shortened overall fusion development program. Review of the current draft reveals that the ITER "program" could be shortened by as many as fifteen years if the parallel approach were to be pursued versus the EC approach. The potential cost saving is **partially** addressed in the current draft. It is stated "if this reduction were taken as 5-6 years (one-half the currently estimated 10-12-yr technology phase) at an annual budget of $350-400 M/y, one obtains a target of $2 billion for the construction costs of the smaller machine." However, this estimate ignores the annual operating costs of the overall global fusion programs which could amount to two or more billion dollars per year in the 2005 to 2015 timeframe. Should the parallel path approach be adopted, then the shorter fusion development time implies worldwide savings that would range between $20 and 30 billion. Once again, we have inserted this idea in our mark-up in anticipation that it be retained in the final report.

In closing, it is my hope that you regard these observations as a sincere effort by U.S. industry to help the federal government define the least expensive, lowest risk and shortest path to the realization of commercial fusion power. The importance of the fusion development effort deserves no less effort and encouragement from industry. Finally, I'd like to thank you for the opportunity to assist in your very important work.

Sincerely yours,

Richard P. Hora,
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ATTACHMENT

FUSION FUNDING HIERARCHY

I. Government fusion funding will grow only when Congress recognizes that it is an "Energy Program".

  A. Fusion will be recognized as an "Energy Program" when there is customer (utility) interest.

    1. There will be customer interest when a credible reactor concept emerges from the Applied R & D process.

      a. A credible reactor will emerge when industry has produced a "roll-back" plan leading to this reactor concept.

        1) Serious industrial involvement will occur when:

          a) Industry is assured a stable contract base, and,
          b) Industry perceives real longer-term business potential (iterate to I.A.1.a.).

            (1) The achievement of a) and b) will occur only when industry has substantial involvement in program direction including:

              (a) setting objectives,
              (b) program planning,
              (c) program evaluation, and
              (d) resource allocation.

II. Congress will recognize this reorientation during formal and informal interactions with industry and utilities (feedback to I).

operation to explore ignition physics and start nuclear testing. In parallel, nuclear testing would be carried out on a lower power high-fluence (≥1 MW-yr/m^2) nuclear testing machine to provide initial qualification of blanket modules and materials. A tokamak that would serve this purpose as a volumetric neutron source would be much smaller than ITER, non-ignited, and beam-driven. In a briefer second phase of ITER, qualified blanket designs, developed and validated in the smaller machine, would be incorporated for integrated testing, with a need for only low fluence (<0.1 MW-yr/m^2). This scenario lowers the risks by providing an alternate path for technology development and fault correction. The initial capital cost is somewhat higher, but the total cost to project completion is likely to be less than the other scenarios because of reduced operating time in the second phase of the larger facility. This scenario also could shorten the time for commercial fusion power development by ten to fifteen years, thus reducing the worldwide costs by $20-30 billion. None of the scenarios address adequately the issue of materials development necessary to achieve the maximum environmental benefit of fusion energy.

The use of copper in an ignited ITER-style device would not reduce cost significantly, nor would it fit within the international ITER consensus.

Data Gap to DEMO (Sec. III)

Physics experimental facilities, using hydrogen/deuterium plasmas, continue to be required in the world mix of facilities to ensure the evolution of an adequate physics basis for a DEMO and for attractive commercial fusion power reactors.

In the absence of a burning plasma experiment, the necessity of using ITER for the first detailed study of high-Q burning plasmas will prolong the physics study phase of ITER and delay the time at which ITER could begin a high-fluence nuclear technology testing phase.

Plasma technologies, such as magnets, heating, high-heat-flux materials, and divertors, are required that are highly reliable and require only infrequent maintenance and replacement. The development of such technologies for DEMO requires specialized facilities and programs.

The construction of a DEMO requires an engineering database on the behavior of materials and components in a fusion nuclear environment over a broad range of operating conditions. ITER is not designed, in any of the scenarios considered, to achieve the high fluence necessary for materials properties measurements at lifetime dpa levels that are needed for the DEMO database for either the low-activation materials or more conventional materials. A 14-MeV neutron source for materials testing remains a necessary, though regularly neglected, element in the world program aiming at DEMO and commercial reactors.

The level of systems analysis currently devoted to fusion commercial requirements is inadequate for a program that is spending roughly a billion dollars a year worldwide and promises to deliver a commercial product on a timetable.

A second, much smaller and less expensive, driven (not ignited), steady-state machine producing neutrons at ~1 MW/m^2 would complement the larger facility in important ways as suggested above. It would be used to preselect blanket and other nuclear technologies, and it would need to operate for sufficient duration to fulfill the ITER fluence requirements, i.e. 1-3 MW-yr/m^2. By starting operation well in advance of the larger machine's second phase, the smaller machine could complete the high fluence much earlier than could a testing program using the larger machine, thereby better matching the planned schedule for the DEMO. A comparison of the time lines for the three scenarios is shown in Fig. II.1.

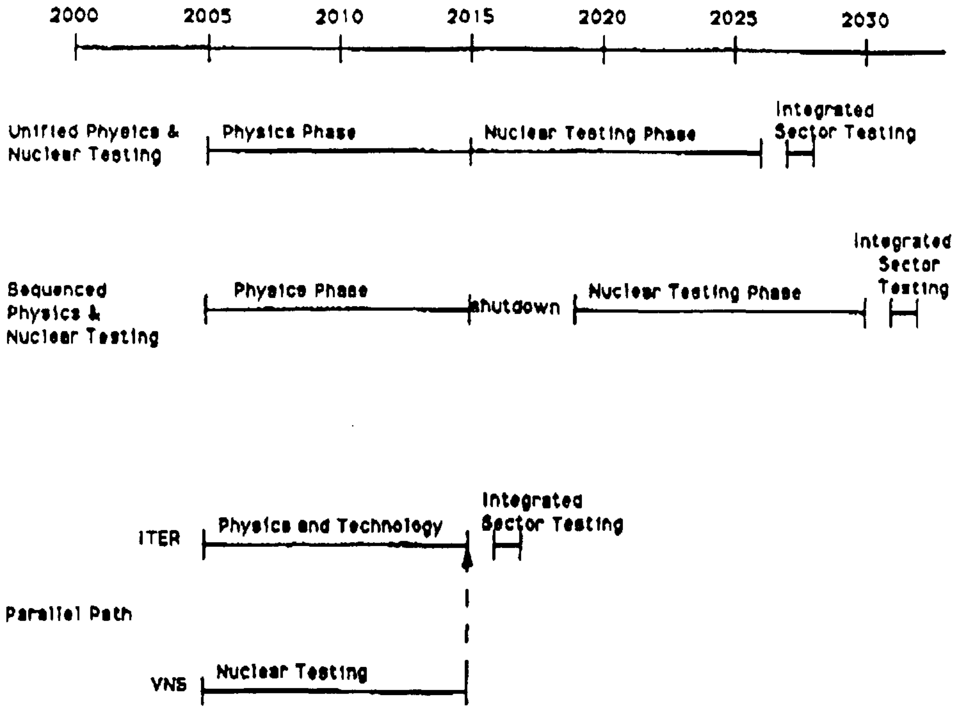

Fig. II.1. Time lines for development scenarios.

In order for the two-machine approach to be economically competitive in terms of overall costs, the capital cost of the smaller machine must be of the order of the savings in costs realized by the reduction in operation of the larger machine. It could be more, as shown in Fig. II.1, but if this reduction were taken as 5-6 years (one-half the currently estimated 10-12-yr technology phase) at an annual budget of $350-400 M/yr, one obtains a target of up to $2 billion for the construction costs of the smaller machine. Designing a technically achievable machine to meet this mission at this budget would be a challenge owing to the costs associated with achieving high fluence. Preliminary estimates suggest that this should be possible, but this cost question needs careful examination.

There is ~~a second~~ an additional way by which this two-machine strategy could be cost effective, although it is a manner that is hard to quantify. Use of the large machine to obtain high-fluence data in the planned 10-yr technology phase has been widely recognized to require a technically very demanding level of availability, 10-30% <u>averaged</u> over a 10-yr period. A similar reliability would, of course, be required in use of the smaller machine for this purpose. However, there, it is expected that necessary high availability could be developed in a less costly manner.

@ In addition to reducing the costs of the ITER program, the overall global costs for fusion power development could be reduced simply because the development time could be shortened by many years. For example, if this time was shortened by ten to fifteen years, then $20-30 billion could be saved, assuming global base program costs of ~ 2 B/yr in the 2005-2015 timefra

The somewhat higher

For the smaller machine to complement the larger in the way described, the two machines would need to be constructed as nearly as possible at the same time. ~~Unacceptably large~~ annual budgets during the construction time could be avoided by omitting the cost of the driver blanket, delaying the introduction of the current drive power, and (possibly) stretching out somewhat the construction of the large machine--emphasizing again that completion of the entire ITER mission would thereby be accelerated in comparison with scenarios one and two,

In the foregoing, it has been implied that the smaller machine would be a driven tokamak. Although the tokamak might indeed prove the most cost effective and useful device, other technologies might ~~should~~ also be considered. If, in addition, the universally agreed-upon need for an intense 14-MeV neutron source is considered, then this scenario has the advantage that it would be possible to site ITER, the nuclear technology test facility, and the 14-MeV neutron source in different countries. This might facilitate the site-selection process for ITER.

In view of the potential advantages that this variant of the ITER program might provide, the Panel believes that it warrants further consideration but recognizes that many important questions remain to be examined.

ITER Development Options Findings

The Panel endorses the ITER EDA, including commitment to construction, as a pivotal activity in the U.S. fusion program. This activity must be coupled with a strong national program that addresses other DEMO-related tasks in addition to ITER tasks. We emphasize that the U.S. program goals, as stated in the National Energy Strategy, would not be achieved if complementary activities to ITER were not carried out.

To accomplish the programmatic objectives of ITER, we find that there are basically three scenarios of interest. The first we call the "unified scenario of physics and nuclear testing;" the second we call the "sequenced scenario of physics and nuclear testing." The third we call the "parallel-machine scenario." The Panel finds that while each scenario has particular advantages and elements of risk, all the scenarios provide an acceptable means of meeting the programmatic objectives.

A **unified scenario of physics and nuclear testing** is accomplished with either the CDA design or its variant known as the high-aspect-ratio (HARD) design. The CDA design is viewed as not entirely satisfactory by the E.C., Japan, and the U.S. Specifically, the CDA design lacks a self-consistent steady-state operating scenario in which the divertor constraints are satisfied.

The HARD design, as typical of a moderately aggressive design to accomplish unified nuclear testing, makes moderately aggressive physics assumptions with respect to aspect-ratio scaling of confinement times, provides some relief in regard to the still severe divertor design and impurity problems, and improves the prospects for the achievement of most ITER physics and technology objectives, including blanket studies, nuclear testing, and steady-state operation.

In the unified scenario of physics and nuclear testing, a strong R&D program will be needed in parallel with ITER design to validate the moderately aggressive technical assumptions and to provide the component reliability needed for a successful and timely nuclear testing program. Otherwise, component failures during ITER operation will lead to increased operating costs because of delayed or extended ITER operations.

A **sequenced scenario of physics and nuclear testing** is represented by the E.C. approach. Based on conservative physics assumptions, the E.C. approach consists of a first stage directed toward the achievement of long-pulse ignition, very limited nuclear testing, and no tritium breeding. The second stage would be devoted to blanket operation, nuclear testing, current drive, and steady-state operation. The fluence in the second stage is moderate, ≤ 1 MW-yr/m^2. The sequenced scenario is likely to provide less nuclear experience and entail larger operating costs than the unified scenario. To the extent that conservative confinement scalings are used, the E.C. device will be larger and more expensive in capital cost than the CDA or HARD designs and, therefore, unattractive from the point of view of cost.

A third **parallel-machine scenario** proposes an ITER-class device with moderate (0.1-1.0 MW-yr/m^2) fluence. This superconducting device would carry out an initial phase of operation to explore ignition physics and start nuclear testing. In parallel, nuclear testing would be carried out on a lower power high-fluence (≥1 MW-yr/m^2) nuclear testing machine to provide initial qualification of blanket modules and materials. A tokamak that would serve this purpose as a volumetric neutron source would be much smaller than ITER, non-ignited, and beam-driven. In a briefer second phase of ITER, qualified blanket designs, developed and validated in the smaller machine, would be incorporated for integrated testing, with a need for only low fluence (<0.1 MW-yr/m^2). This scenario lowers the risks by providing an alternate path for technology development and fault correction. The initial capital cost is somewhat higher, but the total cost to project completion is likely to be less than the other scenarios because of reduced operating time in the second phase of the larger facility.

None of the scenarios address adequately the issue of materials development necessary to achieve the maximum environmental benefit of fusion energy.

The use of copper in an ignited ITER-style device would not reduce cost significantly, nor would it fit within the international ITER consensus.

This scenario also could shorten the time frame for commercial fusion power development by ten to fifteen years, thus reducing the global costs by $20-30 billion.

The ITER-class long-pulse ignition machine could be built initially as in the E.C. two-stage scenario with less current drive, reduced fluence requirements, and no driver blanket. The up-front savings of about $0.9 billion could be used for the nuclear technology machine instead of increased confinement margin, while still preserving the ultimate capability of the ITER-class machine for eventual integrated testing.

The technology testing machine would not operate in an ignited mode, so the size and cost of the machine could be reduced significantly compared with ITER. Assuming the machine were a tokamak, the major radius might be R = 2.5 m, which corresponds to a plasma volume of about 7% of that in the large machine. Among the ramifications of small size are the safety advantages that follow from having an order of magnitude lower radioactivity inventory. The small machine would operate as a low-Q steady-state or very-long-pulse driven device, with fusion power of perhaps 50 MW and flux of about 1.0 MW/m^2. Both copper and superconducting options are possible, although our Panel discussion has tended to favor the copper approach because of lower cost and higher access to the core of the machine.

The total cost of the various ITER scenarios is tabulated in Table IV.2. The possible up-front savings is not a factor because the money is presumed to be spent at a later time. Also not included is the lower cost of R&D and operations expected for the parallel-path scenario in the achievement of high-availability. Apart from this parallel-path advantage, the conclusion of this comparison is that the scenarios do not differ enough in cost to distinguish them given the uncertainties in the projections.

TABLE IV.2.
Total Capital and Operating Costs of ITER Scenarios

Scenario		Capital $B	Operating $B/yr	Yrs	Integrated Cost $B
Unified	ITER	6	0.4	23	15.2
Sequenced	ITER	6	0.4	27	16.8
Parallel-Path	ITER	6	0.4	12	10.8
	VNS	2	0.2	10	4.0

The main advantages of the parallel-path scenario are the reduced technical risk and sched for achieving the nuclear testing mission needed for a DEMO and the earlier time at which such data would be available. This scenario is seen by advocates as placing a more equal emphasis on the importance of fusion technology and plasma physics than do the other scenarios. It avoids the risk that fusion technology, delayed until later phases of ITER, may never actually be done. The smaller machine provides an independent path for technology development and a less expensive means for learning and correcting mistakes. The cost for capital equipment is initially slightly larger, although the rate of spending during construction could be adjusted for the two devices to prevent any increase in the annual budgets.

Finally, the parallel machine scenario could significantly reduce the overall global fusion programmatic costs to and through DEMO simply because the fusion development enterprise would be shorter by ten or more years. At a global fusion cost of, say, $2 B/yr (2015), this savings could amount to $20 to $30 billion.

The first conclusion from Fig. IV.1 is that the CDA design point is indeed a reasonable choice. The projected ITER C value is about 0.95, and the expected value for C is between 0.9 and 1.0 in a reactor. The value of C must exceed about 0.5 in order to have the physics of heating dominated by alpha particles. Figure IV.1 also shows that a finite range of choices is available, and if a "design-to-cost" approach were adopted, one might choose to save perhaps $1 or $2 billion by accepting increased risk with respect to physics performance. A case for doing so might be strengthened by noting that the performance indicated on the graphs has assumed 10% helium concentration (CDA "rules") because of ash accumulation in the plasma. For the first 10 to 20 seconds the ignition performance will be considerably better before the helium ash accumulates, which allows study of short-pulse full ignition physics. If helium ash buildup were to quench the discharge, the ITER program could be directed towards development of improved ash removal techniques.

Schedule. The Panel understands and supports the desire expressed in the FEAC charge to accelerate the EDA schedule if at all possible. The U.S. ITER home team presented their views of the schedule constraints, and the subject was discussed with P. Rebut and M. Yoshikawa during their interactions with the Panel. The schedule has two important constraints: the magnet R&D needed before the ITER design is finished, and the process of selecting a site for construction. By starting immediately on the site selection work and placing high priority on the magnet R&D in the EDA, it appears possible to begin construction as early as 1997, which unfortunately only recaptures the approximately 1-year delay since the CDA ended.

ITER Cost, Risk, and Schedule Findings

Given the ITER terms of reference requirement of "demonstrating controlled ignition and extended burn of deuterium-tritium plasmas," the Panel has been unable to identify a design or scenario that offers the potential for savings of more than 15% in the initial capital cost relative to the CDA design. The reason is that the size of a superconducting ignition device is set largely by tokamak physics and magnet shielding requirements, independent of fluence goals.

The increase in capital cost associated with providing greater machine capability for a unified program of nuclear testing, as for example in the high-aspect-ratio variant, would be about 9% relative to the CDA. The increased R&D and operating costs associated with providing higher reliability/availability are not included in this estimate.

In the view of this Panel, significant non-capital costs specifically for assuring the high-availability, high-fluence nuclear testing phase of ITER operation have not been adequately included in the CDA cost estimates. These costs, which are difficult to quantify, would be incurred because of the increased R&D needed to ensure a very high level of component reliability, and will arise also from the increased operating costs associated with a lengthy program of technology testing in the ITER combined plasma and nuclear radiation environment. These additional costs would be reduced for the parallel machine scenario, offsetting the slightly increased capital cost for this case, because much of the exploratory testing could be done on the smaller machine where operation would be less expensive.

Finally, the parallel path scenario could significantly reduce the overall worldwide fusion program costs to and through DEMO simply because the fusion development enterprise would be shorter by ten or more years. At a global fusion cost of, say, $ 2 billion/year (2005-2015), this savings could amount to $ 20 - 30 billion.

has been the subject of numerous studies and reviews, most recently by the Fusion Policy Advisory Committee (FPAC) in 1990, whose recommendations were incorporated into the Department of Energy's National Energy Strategy (1991). The FPAC recommendations pointed out that attaining the ultimate objective of the program, the commercialization of a new source of electrical energy, "would be expedited by substantial involvement of U.S. industry, not only in the hardware phases of the program, but also in the planning, R&D, and analytical phases." The recommendation proposed specific "steps to bring industry into the planning and R&D activities already under way," which include teaming laboratory, industry, and university resources, establishing a formal industrial participation program, and encouraging personnel exchanges.

The benefits derived from an industrial participation program are broad. The R&D process gains from the proven ability of industry in the manufacturing sector to develop, design, and manufacture equipment with high operational reliability in an economical manner. However, in order to fill this role, industry must be involved from a project's initial planning stages, through R&D and preliminary design, into final design, manufacture, and device operation. These activities extend clearly beyond the usual function as a supplier of materials, equipment, and services. Participation in the operating phases of devices is critical in order to obtain feedback on the performance of components and systems and to incorporate future improvements. In addition, there must be a steady funding base and level of activity, which can be provided by a core industrial program that augments specific projects.

A strong candidate for a continuing core activity is the area of reactor designs for devices parallel to and beyond ITER, including fusion engineering reactors, possible demonstration reactors, and commercial power plants. Benefits would include an increased industrial awareness of the issues concerning fusion and the provision of a useful mechanism for the flow of ideas and concepts from industry into the fusion program.

An industrial participation program will allow the U.S. to expand its industrial fusion infrastructure and to develop a broad constituency for fusion power. To prepare for the eventual demonstration and commercialization of fusion, industries who will ultimately design, build, and service fusion reactors, must participate in ITER and in other program elements in a significant way. Their first-hand experience with factors such as capital costs, licensability, unit availabilities, plant safety, and financial liabilities, as well as the projected cost of power production, will be important in determining the acceptability of fusion power plants to utilities.

~~[illegible]~~

Industry will best fill its role in ITER and in the domestic fusion program through teaming among industries, universities, and laboratories in all portions of the fusion program. The advantage of teaming lies in the synergistic strengths of the participants. To work effectively, such arrangements must be long term and based on realistic assessments of mutual capabilities and commitment. The national laboratories can build on their competence in applied science, ~~and as their experience with engineering and project management.~~ The strength of industry lies in its engineering, design, and fabrication skills, ~~as well as~~ its thorough understanding of the demands of commerce and the market. The strength of universities lies in their focus on basic research and their mission to provide trained individuals to industry. Where there is overlap or similarity in capabilities, emphasis needs to be placed on the differentiating strengths of a given institution. Each partner must give up elements represented more strongly by others in return for effectiveness and competitiveness in the total fusion R&D and commercialization process. To that end, a long-term, broadly-defined teaming relationship best serves the interests of the U.S. and the development of fusion power.

and the ultimate objective of strengthening the competitiveness of U.S. industry.

program management, and