# A FUSION DEVELOPMENT FACILITY BASED ON THE SPHERICAL TORUS

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GENERAL ATOMICS

# A Fusion Development Facility Based on the Spherical Torus

## The Opportunity

The Fusion community could soon have available an opportunity to significantly advance fusion development, an opportunity enabled by the potential of the spherical torus as a plasma confinement system. That opportunity is, in a single device at an affordable price and at a suitable site, to move sequentially through the major fusion objectives of advanced confinement physics, burning plasma and DT physics, blanket and other fusion nuclear technology development, tritium self-sufficiency, and, perhaps at the end with an upside performance outcome, a chance at net electric break even. Meeting this set of objectives would dramatically advance the development of fusion. We would have demonstrated effective plasma self-heating from the fusion reactions in a steady-state device. We would have produced copious power (several hundred megawatts) from the fusion process. We would have extracted heat from the fusion neutrons in blankets. We would have begun the use of low activation materials. We would have learned how to be self-sufficient in tritium supply. And perhaps we might be able to answer affirmatively the simplest question the public asks about fusion, "Does it make more power than it consumes?"

The key to this opportunity is the possible plasma performance we may realize in the spherical torus. The spherical torus (ST) minimizes the size of a next-step fusion device by eliminating nearly all inboard components: no inboard blanket (perhaps a minimal neutron shield), no inboard poloidal field coils, and no Ohmic heating solenoid. The only inboard component left is a single turn copper rod to make the toroidal field. This rod will be shielded to the extent necessary (10–20 cm of shielding) to achieve adequate lifetime against neutron damage for the purposes of the experiment. The reduction in the radial build of the device from the elimination of the inboard components is the main reason why the size of the device needed for the Fusion Development Facility is only about the size of the present DIII–D tokamak, a moderate sized tokamak on the world scene.

To appreciate the strategic opportunity afforded by the spherical torus, we must look at the development path we have been on. Our past strategy has been to build a sequence of devices, machine generations n, n+1, and n+2, etc. Each succeeding generation was considered to be about 2–3 times larger in some dimension (radius and/or magnetic field and/or plasma current) than the preceding generation. This view would seem to be that the basic plasma confinement physics should be developed in a machine of generation n, the burning plasma and steady-state developments steps would be in separate machines of generation n+1, and finally the fusion nuclear technology developments would occur in an integrated machine of generation n+2. The difficulties with this path are obvious. At some cost level, the cost of the next steps become too large to bear. The fact that each development step requires a new and generally more expensive machine puts multiple hurdles on the development path in terms of initiating and funding multiple new projects. In contrast, the ST approach affords the opportunity to carry fusion development all the way through the nuclear technology stage in one device at one site with an affordable total cost with reasonable per year expenditures over time as the FDF Project moves through its sequential stages of accomplishment. This ability to integrate the key fusion development steps in a single affordable project is the main attraction of the spherical torus approach.

## The Content of the FDF Project

The simplicity of this FDF approach is made clear in the conceptual diagram in Fig. 1 of a magnetic fusion energy development path based on the spherical torus. The shaded area indicates those objectives of basic plasma confinement physics that would be done with hydrogen or deuterium plasmas, the burning plasma physics involving DT operation and self-heating of the plasma by alpha particles, the blanket development and material testing stages, and the final push to tritium self-sufficiency and breakeven that could be integrated in a single machine assembly. The device would have a series of upgrades over about a 20 yr period. The basic confinement physics could be done with an initial investment comparable to today's larger experiments with operation in hydrogen and hands-on maintenance. With deuterium operation, the threshold of remote maintenance would be crossed, but simple shields will suffice. If sufficient plasma performance to make the DT phase sensible was not obtained, the project could be stopped at the first decision point. The science gained in regard to advanced plasma performance would be worth the accumulated investment made. The cost of the first phase is based on published costing of similar machines. The cost of succeeding phases are based only on the general size of the device and should be considered as targets or gates. The DT phase would require upgrade investment in the neutron shields and in a tritium plant. If the DT phase was not able to result in sufficient long pulse fusion power output for the blanket development phase, then the project could be stopped at the second decision point. The exciting burning plasma science gained would still be worth the accumulated investment made to that point. But assuming success in the burning plasma phase, the FDF Project would then be in possession of a long pulse, steady-state neutron source and would progress into the blanket development and material testing stages. The device would be upgraded to be able to run truly steady-state and blanket systems would be added. Perhaps three generations of blankets could be foreseen. The first generation should use convenient materials that maximize the chance of tritium production. The last stage of blankets should seek to incorporate low activation materials. The blanket development mission is fairly robust to shortfalls in either fusion power output or the ability to run truly steady-state; long pulses with reasonable duty factor will suffice. At the end of the blanket development phase, if the machine is truly able to run steady-state and if the blankets that can be deployed can result in an overall tritium breeding ratio greater than one, then the machine could move into a steady-state phase with tritium self-sufficiency. Owing to the small accumulated fluence prior to the steady-state phase, the issue of replacing the center-post owing to neutron damage will arise only in the final steady-state phase (18 years into the project). If the overall fusion power performance was on the high side and taking into account customary thermal and electrical conversion efficiencies, the achievement of net electric breakeven might be asserted, even if equipment to convert heat into electricity was not added to the machine. If such equipment was added, an actual net electric demonstration could be attempted. The machine could be run indefinitely as finally configured to produce tritium for the other future experiments in fusion energy development.

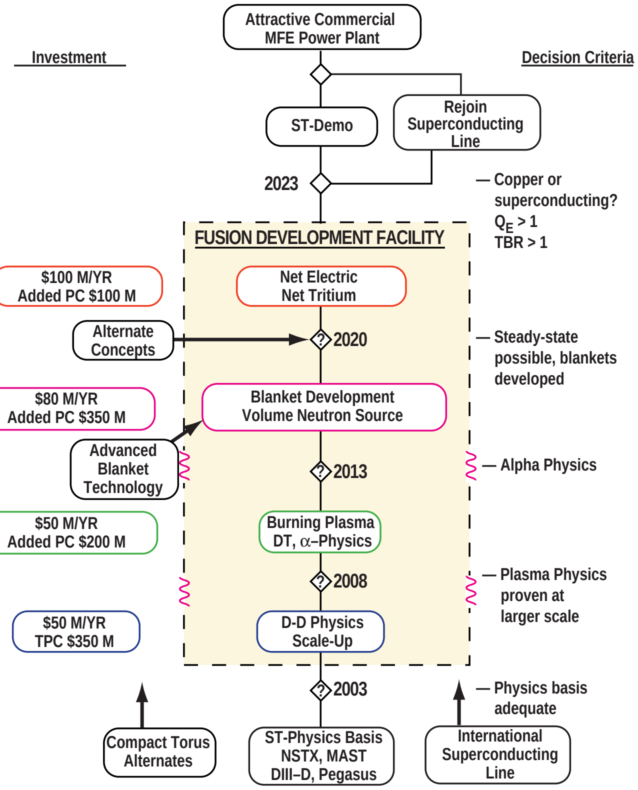


*Fig. 1. Role of a Fusion Development Facility based on the ST in an overall MFE development plan.*

## Position in Overall Fusion Development

It is also envisioned in Fig. 1 that parallel lines of development will continue for the super-conducting coil magnetic confinement line and for the compact torus alternate concept line generally. The decision point which is most at risk in the FDF Project is the final one to actually achieve steady-state operation and possibly with high overall system energy gain. At that time, it would be advantageous for the fusion community to have another compact torus approach qualified from a plasma physics point of view to take over carrying forward the FDF mission if the ST falls short. The superconducting device line is likely to be carried out internationally with devices such as the tokamaks KSTAR in Korea, HT-7U in China, JT–60SU in Japan, and ITER and the stellarators LHD in Japan and W7-X in Germany. The Fusion Development Facility is envisioned to be in the United States as our national component in an internationally coordinated fusion program. At the end of the 20 year FDF Program, the FDF would have advanced blanket development through three generations at high first wall neutron fluxes and into the use of low activation materials. The basis would exist at that time for carrying forward to build a demo based on the ST approach. The ongoing ARIES-ST study and other studies indicate such devices could have overall recirculating power as low as 25%–35% and so such devices with their ease of maintenance with a demountable toroidal coil might be possible as power plants. The superconducting device line by that time should have shown successful plasma physics and steady-state physics and possibly burning plasma physics and some blanket development in a machine of the ITER class. Hence the FDF would add greatly to the technical basis to carry forward the superconducting device line in the more conventional aspect ratio range. It would supply the VNS mission long identified as an essential element in most fusion development strategies. Regardless of what path forward to power plants is chosen, the FDF would have transformed the public attitude toward fusion. Copious steady-state fusion power would have been made. Heat would have been extracted from the fusion process. Fuel self-sufficiency would have been demonstrated. A path to low activation systems would have been opened. The debate will be shifted from the feasibility of fusion to the optimal path of implementation of fusion.

### Alternative Paths and Branch Points

While we have argued for the logic of retaining the several missions of the FDF in a single device and facility as shown in Fig. 1, there are arguments to split up the missions and various branch points the Project might take. First, even after successful results from the current generation 1 MA spherical tori NSTX and MAST, there will be a large leap from these 1 MA devices to the 15 MA needed in the FDF. It can be argued that an intermediate device in the 5–10 MA range should be built to first demonstrate the basic plasma confinement physics, taking the mission of the first stage of the FDF Project and perhaps venturing some distance into the DT phase. This intermediate device would be built with a conventional multi-turn centerpost, postponing the immediate technology step to the single turn centerpost and associated unusual power supplies in the FDF. Such an intermediate device could be powered at existing sites in the fusion program. The interposition of such an intermediate device ahead of the FDF Project would of course add much time to the overall development path. The leap directly to the FDF might be argued as acceptable from two points of view. The knowledge base of the entire world's tokamak program, especially as embodied in predictive codes, can be added to the specifically low aspect ratio database to provide additional confidence in moving directly to the FDF. Secondly, because the size of the tokamak needed in the FDF is modest even on the scale of today's largest experiments, the financial commitment needed to get through the basic plasma confinement physics stage will be only as large as investments we have already made in experiments and about as large as the investment that would have to be made in the intermediate device. Hence we could afford the financial risk of the first phase of the FDF Project.

After the successful conclusion of the burning plasma phase, the FDF device will be a several hundred megawatt, high duty factor neutron source. To operate such a source and carry the project through the blanket development phase will require increasingly significant tritium consumption. A conservative choice to add mainly cold blankets optimized for tritium production will appear attractive to assure the tritium supply for future operation of the FDF. Only portions of the blanket area would be made available for hot blankets aimed at eventual high grade heat extraction. At another extreme, it might be argued at that time that the device should continue to operate mainly as a burning plasma physics experiment, investigating unfinished physics and deferring the blanket development phase. Either of these pressures, taken with the consideration that the basic ST device is rather modest in cost to duplicate, might motivate that a second such ST device be built at the same site early in the blanket development phase. The second device might incorporate improvements learned from operation of the first device. The first device might be devoted to providing the tritium supply for the second device, which might be able to reach higher fusion power output and be a more optimized vehicle for the final push to true steady-state and net electric break-even.

## FDF Project Layout and Schedule

The four phases of the FDF Project are laid out in more detail in Fig. 2. In the D-D Physics Phase, essentially no fusion power would be produced. The device would consume about 200 MW of electric power.  Because the device would have no OH transformer, full non-inductive operation must be done. It appears in such a device a 60 second pulse length must be viewed as minimal to allow enough time to run up the plasma current by entirely non-inductive means. Two thousand such pulses in a year would result in a 0.4% duty factor.  With such a duty factor, even with deuterium operation, remote maintenance would quickly be needed. An initial long period of hydrogen operation to avoid activation seems wise.

Assuming the device passed its various physics milestones in terms of stability, confinement, current drive, and power and particle exhaust sufficiently to project an interesting level of fusion performance (Q > 5), then the burning plasma phase would be started. Initially, small quantities of DT would be used for only the 60 second pulses. The emphasis would be on raising the fusion power output in pulsed mode in order to study the physics of self-heated plasmas. The fusion power would rise from 10 MW initially to 500 MW by the end of the burning plasma phase. Assuming success in these studies, the pulse length would be lengthened to perhaps 5 minutes to study steady-state physics issues in a burning plasma. With such pulse lengths, the device may consume 0.1 kg of tritium per year.

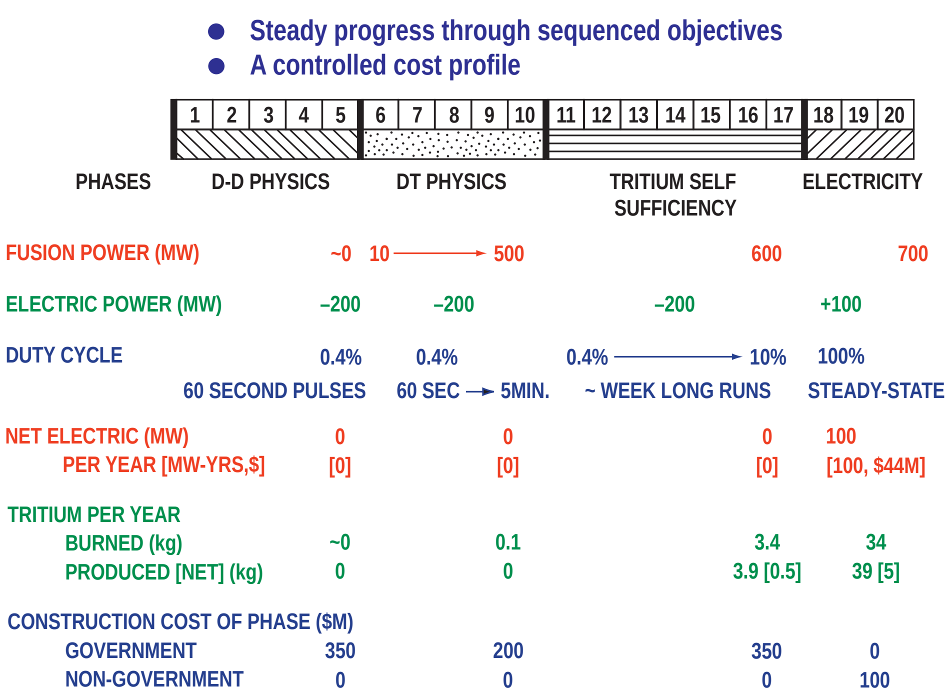


*Fig. 2.  Fusion Development Facility Project Layout and Schedule*

Assuming success in the burning plasma phase, at the end of that phase the device would be capable of producing 500 MW fusion power with in-principle steady-state physics in the plasma. Probably the power and particle exhaust systems will have to be upgraded to enable extension of the pulse length toward steady-state. Blankets will be added to the machine for testing. Blanket developers will want to see pulses of at least many hours duration, if not a day to reach steady-state equilibrium. Materials testing will require fluence and will motivate longer pulses and higher duty factor. The duty factor should rise to 10% by the end of the blanket development phase. Some demonstration week long continuous runs should be made. With such increases in duty factor and pulse length and some increase in fusion power to 600 MW, the device could burn as much as 3.4 kgs of tritium per year at the end of the blanket development phase. While this amount might still be obtained from other sources, there will be strong motivation to devote some portion of the blanket space to blanket modules that make some of the tritium. If the overall machine had a TBR of 1.15, then 0.5 kgs net tritium might be made. At the start of the blanket development period, the first generation blankets would be made of conventional materials like stainless steel optimized for tritium production. By the end of the blanket development period, blankets built with low activation materials and advanced coolant methodologies should be under test. This blanket development phase might be rather elastic in its total time duration.

Assuming that at the end of the blanket development phase a blanket design was available such that if fully implemented a TBR greater than one could be achieved, then the technical basis would exist to configure the machine to actually run steady-state. Besides the tritium issue there could also be other impediments to steady-state operation, such as equipment reliability, material buildups in unwanted locations inside the machine from surface erosion, etc. Presumably the disruption issue would have been largely laid to rest by the week long demonstration runs; but there will be a residual disruption frequency determined by flakes from the walls, hardware faults, and the degree to which the machine operators want to operate close to the stability limits to maximize fusion power output. If the machine could be run steady-state, then it could be operated as a tritium resource into the future. The electric bill could be reduced by adding electric power conversion equipment to the blanket system. If the overall machine performance were high enough, some net electric might be produced. Figure 2 contains a suggestion how to engage utility interest in fusion by asking commercial interests to pay the cost of adding the electric power conversion equipment and to be able to quickly recoup their investment from the proceeds of sale of the net electricity. The basic plant operation would continue to be supported by the government. If such an eventual outcome could attract commercial interest, then perhaps some involvement of utility personnel in the design of the machine could be garnered in year 1 and that involvement could continue at a modest level through the operational phases of the machine.

## Key Technical Issues

The success of this project will largely depend on accomplishing the blanket development mission. That mission is beneficially elastic in that it can probably be accomplished with as little as 250 MW fusion power output. The reach for net electric breakeven would set an absolute performance goal. The FDF mission must have a sufficiently valuable mission short of that goal since such absolute performance goals always result in escalation of the device and project size during design in order to provide more assurance of meeting the absolute performance goal. Net electric breakeven should be striven for but not mandatory to the mission of the FDF.

Within such soft constraints on plasma performance, there still remain significant physics and technical challenges to be met. In the area of plasma stability, the required high beta and normalized beta values with high bootstrap currents are projected based on proven codes. Wall stabilization is assumed and that is the main issue that needs resolution in the stability area. The current issue of neoclassical tearing modes may be of less importance in the ST owing to the high values of the minimum safety factor (~4) and the strength of the stabilizing Glasser term. Because the field is low and the alpha pressure high, careful consideration of the various Alfvén eigenmodes should be made. High values of plasma elongation are required and careful consideration of how to access these states during dynamic and non-inductive startup need to be made.

Owing to the high normalized beta calculated, nearly full bootstrap current can be projected. The current profile is very unusual in tokamak experience, peaking near the outboard midplane. Some additional current drive should be added to this current peak. Recently in the ARIES-ST study it has been realized that positive ion neutral beam technology can supply this current drive near the edge and also supply toroidal momentum for wall stabilization. RF heating and current drive schemes need to be developed. High Harmonic Fast Wave is being pursued on NSTX. ECH and ECCD might be applied if the resonance on the inboard side can be accessed. Because it cannot have an OH transformer, the current must be initiated and run up by non-inductive means entirely. A technical basis for non-inductive startup and rampup using bootstrap overdrive and non-inductive current drive must be developed.

The ST is expected to favor transport barrier formation owing to its large diamagnetic term in the radial electric field promoting suppression of turbulence by sheared E×B flow. This physics is the approach to obtaining the required high confinement performance.

The FDF device will have high power density and exhausting that power in a small device will be challenging. Estimates indicate the divertor area is inadequate so the radiative mantle solution will have to be used to some extent. The compatibility of this radiative mantle with the good confinement and the current peak near the edge needs to be examined.

Finally on the technical side, the single turn toroidal coil requires unusual but commercially available low voltage (8 V) high current power supplies (1 MA) to individually power the return legs of the TF coil. Current regulation to 0.1% accuracy on the ac side of the system is required. The rectifiers must be placed close (5 m) to the machine in order that the Ohmic losses in the large transmission lines not be excessive. This consideration dominates the overall layout of the facility.

Careful consideration must be given to fitting in the neutron shielding between the power supplies and the device and with large penetrations for the transmission lines and neutral beams. The overall resulting remote maintenance capability needs to be considered. The fully demountable toroidal coil is a maintenance advantage. Indeed the design must provide for changeout of the centerpost owing to radiation damage, although the first need for such changeout will occur only very late in the project after true steady-state operation and fluences are produced.

### What Could the FDF Accomplish?

The FDF Project would aim to achieve the following milestones in fusion development.

1. Critical scientific issues for the spherical tokamak in particular and for magnetic confinement in general will have been answered.
2. The research community will have been able to investigate the physics of a self-heated burning plasma.
3. Several hundred megawatts of fusion power would have been produced, making credible the production of power from fusion.
4. The fusion power would have been produced from a high duty factor, if not steady-state device, showing the potential of fusion for high time averaged power.
5. A great deal of materials data would have been obtained on materials irradiated at high fluxes and at high fluences.
6. Power would have been extracted from the fusion process in fusion blankets.
7. Low activation materials would have begun to be used in fusion nuclear systems.
8. Tritium self-sufficiency for a fusion system would have been demonstrated. A tritium supply for future devices would be available.
9. The public would have been shown that fusion devices can make more power than they consume.