



Fusion Space Propulsion—A Shorter Time Frame Than You Think

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December 2005

UWFDM-1287

Presented at the 53rd Joint Army-Navy-NASA-Air Force (JANNAF) Propulsion Meeting,
Monterey, 5-8 December 2005.

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ABSTRACT

This paper reviews the critical physics and engineering issues for fusion space propulsion and argues that the development time frame can be much shorter than the many decades assumed in most present space development planning. Three reasons exist for the sound-bite dismissal of fusion as “perpetually forty years in the future”:

- (1) scientists underestimated the obstacles in the early days of fusion research,
- (2) the perceived lack of urgency for developing new terrestrial electricity sources kept fusion budgets modest and the planning horizon distant, and
- (3) physics considerations dominated engineering issues.

How has the situation changed? First, researchers overcame many daunting hurdles, understood the key critical issues, and developed experimental, diagnostic, computational, and theoretical tools into a powerful predictive capability. Obstacles exist, but they are essentially known. Second, although the terrestrial fusion electricity research program remains bogged down by the perceived lack of energy urgency, the fusion space propulsion R&D pace depends on the priority given to space development. This paper addresses what can be done if the willpower exists to construct the fusion bridge into the Solar System at high priority. Finally, fusion propulsion research can build on the terrestrial lessons, and its development requires intense, parallel, and coordinated efforts in both physics and engineering. The key theses of this paper are that:

- (1) the use of the advanced fusion fuel combination of deuterium and helium-3 will relax engineering constraints significantly, allowing the use of near-term technology and shortening the development time, and
- (2) the fusion concepts most suitable for space propulsion—these are not the mainline terrestrial electricity fusion concepts—primarily require research into physics issues using modest-scale experiments that would have reasonable development times and costs.

INTRODUCTION

The U.S. Government declassified magnetic fusion energy research in 1958.¹ Optimism ruled the early days, and high expectations created visions of fusion in ten years. Reality, in the form of daunting plasma physics and fusion engineering challenges, fractured those hopes.² Fusion researchers have made immense progress since the 1950s, but the sound-bite dismissal of fusion as “perpetually 40 years in the future” now haunts the field. Taken out of context, the Department of Energy’s present 35-year plan to the first deployment of a magnetic-fusion demonstration reactor (Demo) could feed that sound bite. That plan, however, is predicated on limited budget expectations and no public sense of urgent need to develop a new energy source. The essentially non-existent role of fusion in U.S. energy planning also reflects the view of fusion as lying far on the horizon.³

Distribution Statement A: Approved for public release; distribution is unlimited.

* Research funded by the University of Wisconsin and under grant DE-FG02-04ER54751 by the Department of Energy.

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This paper addresses a related but nearly orthogonal question: “What time frame could a dedicated, well-funded, fusion space propulsion development program achieve?” The main thesis of this paper is that the time frame could be shortened by concentrating on fusion configurations, fuels, and modes of operation where primarily physics issues must be solved, because the related engineering issues are already reasonably well in hand. Such concepts fall into two main categories: (1) high-pressure concepts burning advanced fusion fuels and (2) pulsed concepts. This paper will discuss the key issues, but it will not attempt detailed descriptions of the candidate concepts—a task left to the references.

Historically fusion device development follows either a physics-driven or an engineering-driven path.⁴ In terms of physics performance, the tokamak has been most successful in achieving good confinement and, thereby, high plasma temperatures. In terms of engineering issues, however, concepts with high β (plasma pressure / magnetic-field pressure), linear geometry, and the ability to burn deuterium/helium-3 ($D-^3\text{He}$) fuel have advantages in the engineering areas of materials damage, plasma-surface interactions, induced radioactivity, magnets, energy conversion efficiency, and maintenance. The argument for the engineering-driven path stems from recognizing that resolving physics issues generally requires smaller devices and less funding than resolving engineering issues. The neutrons carry 80% of the energy in deuterium/tritium ($D-T$) reactions, damage materials, and their energy can only be thermally converted. Charged particles dominate $D-^3\text{He}$ fusion and, as they slow down, give their energy to the background plasma, whose charged-particle losses can be converted directly to thrust or electricity. The magnetic fields also protect wall materials from charged particles. $D-T$ fusion carries the liabilities of high radiation damage to containment vessels, large induced radioactivity and afterheat, a large radioactive waste volume, complex tritium-breeding blankets, and extensive radioactive tritium handling.

Fusion reaction rates averaged over a Maxwellian distribution vs. ion temperature for the most important fusion fuel cycles appear in Figure 1, and Figure 2 shows the resulting neutron production. The reactions and the energies of their fusion products are listed in Table 1. This paper concentrates on $D-^3\text{He}$ to illustrate the benefits of advanced fuels, because this reaction balances physics requirements with a low neutron production. The $p-^{11}\text{B}$ fuel cycle produces very few neutrons, and $^3\text{He}-^3\text{He}$ produces essentially no neutrons, but these cycles require high ion temperatures and a means of collisional decoupling between ions and electrons to allow ignition against bremsstrahlung radiation.

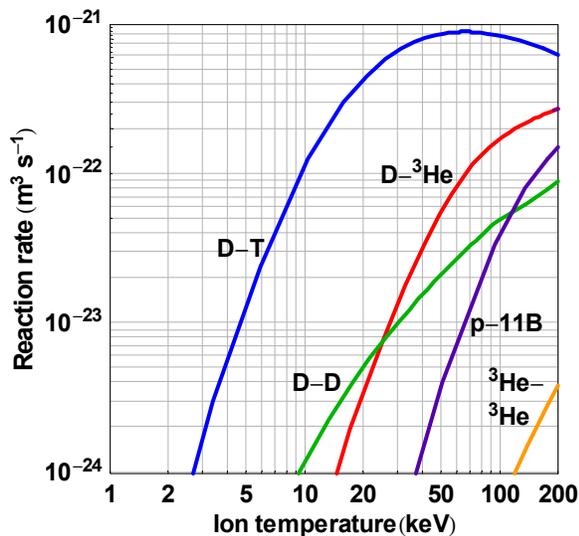


Figure 1. Fusion reaction rates averaged over a Maxwellian distribution vs. ion temperature for key fusion fuel cycles.

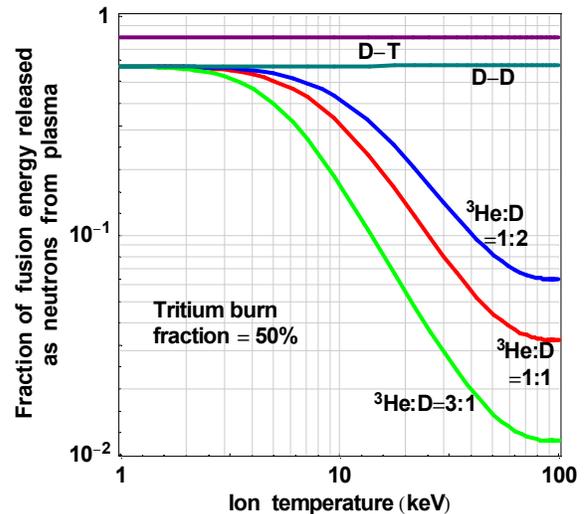
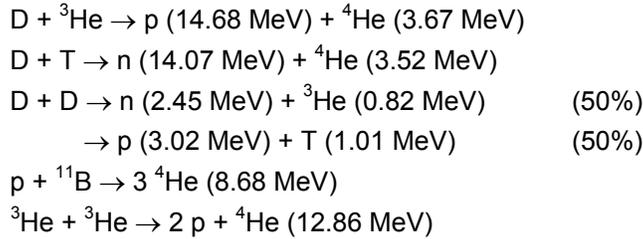


Figure 2. Ratio of neutron power to fusion power for the $D-T$, $D-^3\text{He}$, and $D-D$ fusion fuel cycles, assuming that 50% of the T produced in side reactions is also burned. Several values of the $^3\text{He}:D$ density ratio are shown.

Table 1. Key Fusion Reaction Rates vs. Ion Temperature.



Motivated by the high specific energy of fusion fuels, engineers and scientists at NASA and elsewhere created conceptual designs for magnetic fusion space propulsion systems shortly after fusion energy's declassification in 1958^{5,6,7,8,9} and slightly later for inertial-confinement fusion.^{10,11} Modern fusion propulsion systems have changed many of the details, but the predicted high specific powers (~1-10 kW/kg) still hold true in more recent conceptual designs.^{12,13,14,15,16,17,18,19,20} Generic arguments, based on analyzing the components expected to have the largest mass, support the detailed studies.²¹ In conjunction with the high exhaust velocities ($v_{ex} \gg 10 \text{ km/s} \Rightarrow I_{sp} \gg 1000 \text{ s}$) that plasmas provide, the high fusion specific powers would open the Solar System to human exploration and development.^{22,23,24} Fusion propulsion systems must carry the extra mass of their power source, but the high thrust per unit mass that arises from the intrinsically high exhaust velocities of plasmas reduces the propellant mass, and this overcomes the power-source mass penalty for high-energy missions. Figure 3²⁵ shows the capabilities of fusion propulsion enabled by these high specific powers and exhaust velocities. Figure 4¹⁷ shows fusion propulsion's performance for fast transport of humans between circular solar orbits for Earth-Mars and Earth-Jupiter one-way rendezvous missions. Alternatively, fusion rockets could trade thrust for exhaust velocity, increasing travel time while increasing payload fraction. It has long been known that specific power values of ~1 kW/kg would enable great improvements over chemical rockets by traveling in such modes, which Stuhlinger called "sports-car" and "truck" modes.²²

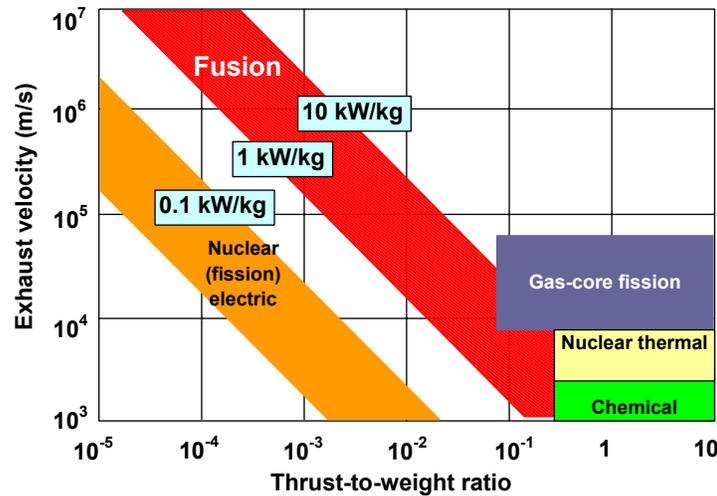


Figure 3. Comparison of propulsion system capabilities. The thrust-to-weight ratio refers to the total power and propulsion system referenced to Earth's surface gravity.²⁵

Worldwide, fusion research has made exceptional progress in plasma physics, plasma heating and current drive, understanding plasma-surface interactions, neutron effects, and magnet design. Nearly from its inception, however, the fusion program concentrated upon concepts, such as the high-magnetic-field, low-power-density tokamak, that provided the best energy confinement and, therefore, highest plasma temperature in small experiments. This confinement-motivated approach paid scant attention to concepts that perform better with regard to important considerations for space applications, such as high power density and reactor engineering. Fortunately, the tools now available—experimental

techniques, diagnostics, numerical codes, and sophisticated theory—will help shorten the physics development time of any emerging concept. The remainder of this paper begins by discussing fusion fuel resources. Subsequent sections will explore the physics and engineering of fusion space propulsion systems based on the high- β , D- ^3He and pulsed approaches. Finally, a potential development plan and time frame will be discussed.

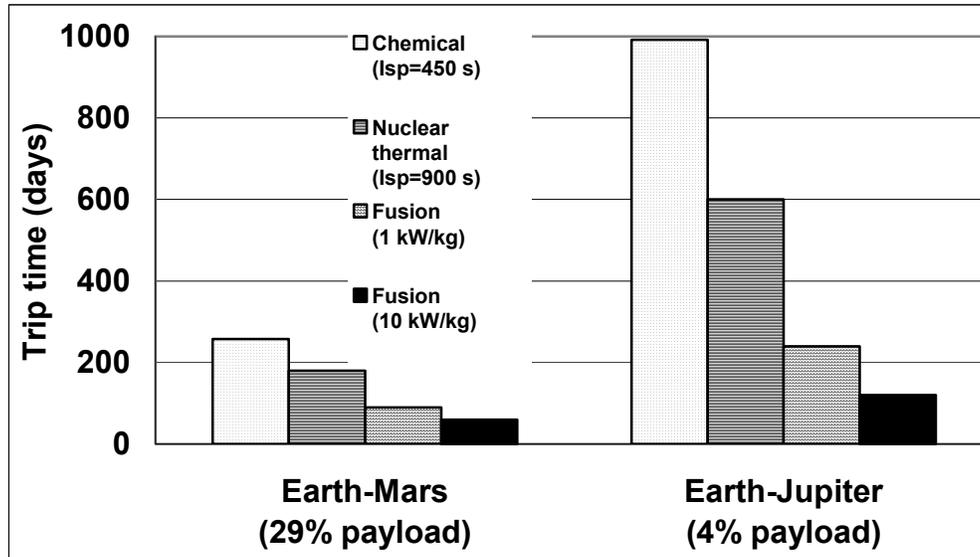


Figure 4. One-way trip time for the same payload fraction comparison of fusion, nuclear-thermal, and chemical propulsion.¹⁷

FUSION FUEL RESOURCES

Economically accessible ^3He exists on Earth in sufficient quantities (a few hundred kg, equivalent to a few thousand MW-years of fusion power) for an engineering development program through the flight-test stage, but not for routine Solar-System travel.^{26,27} Either ^3He must be bred using other nuclear reactions, which presently appears unlikely to succeed economically, or the million-tonne ^3He resources of the Moon must be mined.^{26,27}

The resources of lunar ^3He and the process of mining it have received considerable attention since the connection between this resource and fusion power was pointed out two decades ago.^{26,27} The mining process requires using a bucket-wheel excavator to dig ~3 m into the lunar surface, conveying it through the mining vehicle, heating the fine lunar regolith to ~700 °C, collecting the outgassed volatiles, and processing them.^{28,29,30,31,32} The environmental consequences to the Moon would be minimal and invisible from Earth.³³ The required technologies have essentially been demonstrated, primarily on Earth but some at small scale in the Apollo program. Furthermore, the process of acquiring each kilogram of ^3He generates 1000s of kilograms of volatiles useful for life-support and other purposes, such as H_2 , ^4He , CO_2 , CO , CH_4 , N_2 , and H_2O . The water comes from H reduction of the mineral ilmenite, where much of the ^3He resides. From the space-development perspective, accessing the lunar ^3He resource appears consistent with the time frame for achieving D- ^3He fusion space propulsion.³⁴

Concepts that use D-T fuel must supply tritium through external means or, as in terrestrial electric designs, on-board breeding. It also requires on-board storage of 100s to 1000s of kilograms of radioactive tritium per mission. Breeding tritium significantly complicates fusion core design and requires carrying a tritium-processing system. In geometries without nearly 4π coverage to absorb neutrons, which is typical of pulsed designs, tritium breeding cannot be accomplished unless the fuel is changed from D-T to D-D with a D-T “spark-plug” core. Purchasing tritium from an external source raises supply questions similar to those arising from the closing of the Savannah River fission reactor that produced tritium for nuclear weapons.

HIGH- β , D-³He FUSION PROPULSION SYSTEMS

For High- β , D-³He systems, the critical issues lie in the physics and ³He acquisition, but their engineering appears to be reasonably well in hand. The physics and engineering issues will be treated in separate subsections here; fuel questions were treated in the previous section, *Fusion Fuel Cycles*.

PHYSICS ISSUES

The key physics questions for D-³He fuel are:

- (1) whether sufficient plasma density, energy confinement time, and temperature can be achieved to burn it, and
- (2) whether sufficient power density can be achieved.

The first issue usually is characterized by the product of three plasma variables, $n\tau T$ (density x energy confinement time x temperature), and the substantial tokamak progress in this figure of merit is shown in Figure 5. The challenge for D-³He fusion will be to develop devices that can achieve this level of performance at high β (for good power density). Tokamaks have already achieved fusion core temperatures sufficient to burn D-³He fuel, but about a factor of ten improvement in tokamak $n\tau$ product would be required to ignite a D-³He plasma.⁴ Other configurations trail the tokamak by a large margin in terms of the $n\tau T$ product.

The plasma β value characterizes the power density, and the need for high β in D-³He space propulsion devices derives from the scaling of the fusion power density in the plasma approximately as $\beta^2 B^4$, where B is the magnetic field in the plasma. For tokamaks, where $\beta < 0.1$ (transient tokamak values have gone slightly higher), reactor designs lead to magnetic fields of 14-20 T, and magnet technology constraints leave little room to increase power density using the B^4 part of the dependence. In contrast, for high- β concepts such as the field-reversed configuration (FRC), which routinely achieves $\beta=0.9$ in experiments, conceptual reactor designs typically optimize at 2-3 T, allowing a large improvement through this term if magnetic fields are pushed toward their limits (~20-24 T, if magnet structural constraints are included). The potential gain in power density through these routes appears in Figure 6. For a given β value, D-³He fuel optimizes at about 80 times lower power density than does D-T fuel, which can thus be compensated for relatively easily.

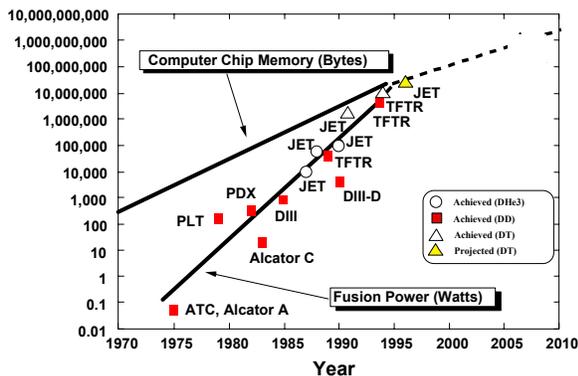


Figure 5. Tokamak fusion power produced compared to computer memory progress.

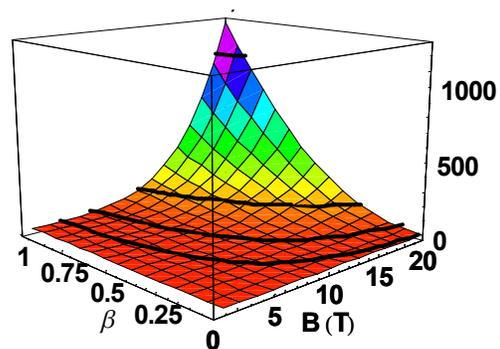


Figure 6. Power density relative to a D-T tokamak ($\beta=0.05$, $B=15$ T) as a function of β and magnetic field (B). Based on today's magnet technology, the approximate limits of magnet design are 9 T for NbTi and 20-24 T for Nb₃Sn, depending on details of the configuration.

ENGINEERING ISSUES

The crucial engineering issues are surface heat fluxes, energy conversion, waste heat rejection, neutron effects, magnet design, and maintenance. As was shown in Figure 2, D-³He greatly reduces the neutron production, but the consequent increase in charged-particle power must now be handled by material surfaces. In a toroidal chamber, such as that of a tokamak, the heat diffuses across magnetic field lines and either concentrates onto a special region called a divertor or gets spread over the reactor

first wall. The first D-³He fusion propulsion conceptual design selected linear magnetic-field geometry,⁵ and similar geometries have usually been favored in subsequent D-³He rocket designs. This is primarily because the linear geometry of the external magnetic field—the internal field may be toroidal—allows the exiting charged particles to be guided along magnetic fields to convert their energy directly to thrust or electricity at high efficiency (60-90%).^{35,36} Highly efficient direct conversion benefits the system both by increasing the thrust generated and by reducing the waste heat and concomitant radiator mass. Doubling the energy conversion efficiency from thermal cycle values of ~33% to direct conversion's ~66% raises the ratio of useful energy to waste heat by a factor of four. Bremsstrahlung radiation produces almost all of the heat fluxes on D-³He reactor material surfaces, and the resulting surface heat fluxes of ~1-4 MW/m² are manageable without undue difficulty, eased further by the lack of a tritium-breeding blanket, which allows design flexibility and more robust first walls.⁴

Neutrons in D-³He systems, although greatly reduced in number and fusion power fraction, still typically lead to radiation shields for the magnets of ~0.5 m in radial extent, compared to ~1 m for D-T systems. Besides the reduced shield mass, the lower neutron flux benefits D-³He systems by reducing materials activation (induced radioactivity), which facilitates remote maintenance, and increasing materials lifetimes against radiation damage to ~30 full-power years. Materials in D-³He systems would survive the full lifetime of almost any mission, whereas D-T first walls and nearby structures would require changeout after 3-5 years using remote-maintenance technologies. Materials for D-³He reactors are essentially in hand, whereas D-T reactor materials require a substantial experimental research and development program that presently does not exist.⁴

Superconducting magnet designs for high-β systems with linear geometry generally require only solenoidal, relatively low-field coils. Low-β systems need very high fields, 14-20 T, and often use D-shaped coils or other, more complicated geometries. Today's technology can create such magnets, but their mass will be considerable. Magnets with the fields required for a D-³He FRC fusion reactor, ~6-10 T, have already been tested at large scale,³⁷ and high-temperature superconductors are developing rapidly for this magnetic-field range.³⁸ If high-temperature superconductors suffice for D-³He rockets, the refrigerator mass required to chill the coolant will be greatly reduced.²¹ Both NbTi and Nb₃Sn need 4 K, MgB₂ needs ~30 K, and some perovskite superconductors have critical temperatures over 100 K.³⁸

Thus, the critical engineering systems for D-³He fusion rockets are essentially in hand, and some recent technology developments appear likely to make them even more attractive. The FRC concept¹⁶ fits especially well the criteria given above, but other concepts also present strong possibilities. These include the dipole,^{39,18} spheromak,¹³ spherical torus,^{13,20} tandem mirror,^{17,14} and colliding-beam fusion.¹⁹ One unresolved issue related to the magnets is detachment of the plasma from the magnetic nozzle (expanding magnetic flux tube). This long-standing issue has received some attention, but remains an active research area.^{40,41,42,43,44}

PULSED-POWER FUSION PROPULSION SYSTEMS

Two main types of pulsed-power fusion systems exist: (1) inertial-confinement fusion (ICF), where laser or ion beams compress a pellet, and (2) magneto-inertial fusion (MIF), where an initially solid, liquid, or plasma-jet wall compresses a magnetized plasma target. The key issues lie in the physics of the plasma compression and the engineering issues related to pulsing and driver technology.

PHYSICS ISSUES

The most important physics issues for pulsed-power systems lie in the areas of driver and compression physics. The intrinsic efficiency of the driver and of the coupling of the driver beams with the plasma each translate nearly linearly into driver power and mass. For ICF, driver options include lasers, light-ion beams, and heavy-ion beams. The ICF pellets may be compressed directly or by x-rays generated by the beams hitting the inside of a hollow metallic container known as a hohlraum. The pellet implosion must generally be uniform to less than ~1%, and directly driven systems typically use ~100 beams. Indirectly driven systems, typical of the heavy-ion beam approach, usually generate the necessary x-rays using two target plates slightly inside of the hohlraum ends. Past ICF fusion rocket designs, which began in the 1970s,⁴⁵ have usually invoked D-T fuel (D-³He is hard to burn in ICF) and been very large.⁴⁶ The fast-ignitor approach⁴⁷ would reduce the required input power substantially, and

this option might perhaps be used in future ICF propulsion design studies. A conceptual design of a D-³He fusion rocket aimed at interstellar missions has also been performed.⁴⁸ Both “classical” ICF and fast-ignitor theory and experiments are presently very active areas of research worldwide, so significant progress in understanding may be expected over the next decade.

For MIF, either a high current through a solid or liquid creates a plasma pinch,^{49,50,51} plasma jets merge to create a moving, conducting wall;^{52,53} or the magnetized plasmoid gets translated into a converging flux conserver.⁵⁴ The plasma target generally is an FRC or a spheromak.⁵⁵ The main physics issues arise during the implosion phase: maintaining plasma stability, reducing thermal conductivity using the magnetic fields, and minimizing mixing of boundary layers. Small experimental research programs exist that are addressing some of the related issues.⁵⁶

ENGINEERING ISSUES

For many of the ICF and MIF concepts, the fusion burn gets initiated in space, rather than in an internal rocket chamber, and the expansion of the plasma against a magnetic nozzle provides momentum transfer to the rocket.^{45,46,52} Theory and experiments have begun addressing the issues.^{57,58,59} Key engineering issues include the effects of repetition rates and duty cycles on materials properties, plasma flow into the rocket end wall through the magnetic nozzle’s cusp, material stresses during the pulses, heat fluxes, and heat rejection. These are mass optimization issues, not feasibility ones. If D-T fuel is used, the neutron heating of structures can be substantial, although neutron damage is generally minimal due to the limited solid angle of the magnetic nozzle and the modest operating times for most missions.

For ICF, issues related to mass and lifetime of laser or ion-beam optics will be important, as will be tritium inventory (if D-T), plus target injection and tracking. The fast-ignitor concept would considerably reduce the required input energy for a given fusion yield, easing the engineering requirements for the driver and the recirculating power systems. For PHD, which is at an early stage of development, the velocities required of the injected FRC have essentially been demonstrated in other experiments, and the crucial issues relate to the stresses and heat fluxes on the wall materials as the FRC gets compressed in passing through the flux conserver’s narrowest region. For magnetized-target fusion (MTF) space propulsion, plasma jets appear to be the necessary approach for compressing the target plasma,^{52,53} and the critical engineering issues is plasma injector design. The MTF space propulsion concept has recently been the subject of a modest conceptual design effort.⁶⁰ In general, for all of the pulsed-power concepts, the engineering issues can be tested at small scale.

A POTENTIAL DEVELOPMENT PATH FOR FUSION SPACE PROPULSION

One possible fusion space propulsion development plan appears in Figure 7. This twenty-year plan focuses on the type of fusion configurations discussed in this paper, but it does not attempt an explicit plan for the specific approaches. Such fusion systems require significant physics research, hence the attention to proof-of-principle experiments and integrated test experiments. The relatively short time frame for the burning plasma experiments and the demonstration system (Demo) reflect the modest engineering development times predicted to follow from either the D-³He or pulsed-power routes to fusion. This plan attempts to steer a path between the nearly lethargic approach of the terrestrial electricity fusion program and the race-to-the-finish approach of the Apollo program, and it tries to include lessons learned

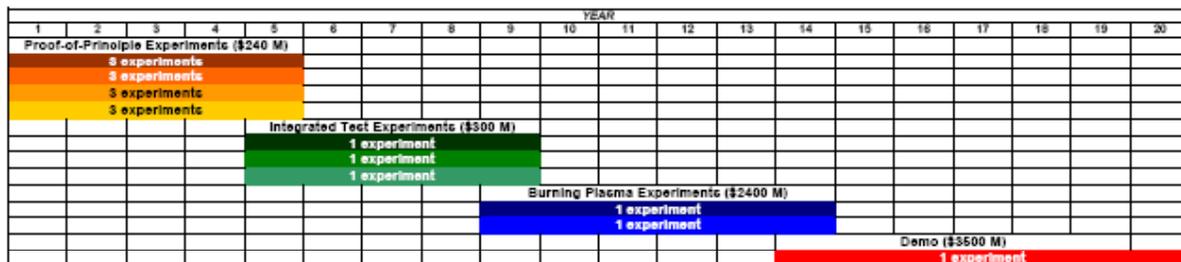


Figure 7. Potential fusion development plan.

from the latter program.⁶¹ The budgets listed are intended to allow sufficient input power and diagnostics, two areas that provide research flexibility but are usually underfunded in the terrestrial “innovative confinement concepts” small-experiment research program. The budgets translate roughly into 12 proof-of-principle devices (3 each addressing different issues for 4 configurations) at \$4M/year each, 3 integrated test experiments at \$20M/year each, 2 burning plasma experiments at \$200M/year, and 1 demo at \$500M/year. Other concepts already at one stage would feed into the selection process for the following stages. The large number of configurations and devices included in the early stages of the plan stems from the substantial physics development needs of most of these concepts. The total program cost of ~\$6,400M reflects the weighting of the relative development required in physics and engineering areas for D-³He and pulsed-power fusion.

SUMMARY AND CONCLUSIONS

Many fusion space propulsion options exist, but they typically lie outside the mainstream concepts being developed in the research program for terrestrial fusion electricity. These concepts possess high power density, and their critical issues tend to be in physics areas rather than engineering ones, lessening development risk. Concepts that can achieve high pressure plasmas burning D-³He fuel or utilizing pulsed-power approaches appear to be particularly attractive. A twenty-year development plan for these concepts, based on appropriate budget levels, should be feasible.

ACKNOWLEDGMENTS

Research funded by the University of Wisconsin.

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