NASA Technical Memo 4297

IN 73

P- 54

Fusion Energy for Space Missions in the 21st Century

Executive Summary

Norman R. Schulze

AUGUST 1991

Fusion Energy for Space Missions in the 21st Century

Executive Summary

Norman R. Schulze
NASA Office of Safety and Mission Quality
Washington, D.C.

SUMMARY

As mankind completes the missions and planning within the reach of low energy chemical propulsion missions – less than 10 km/sec-we need to look beyond where Δv's on the order of 100 to 20,000 km/sec are required. This energy level – defined as High Energy Space Missions – is essential for the continued advancement of space missions including science and the safe, economical exploration of space.

Let us become free thinkers on future space missions without arbitrarily self-imposed limits of chemical propulsion systems and be as imaginative as we dare within the known bounds of science. As space missions extend further, the mission costs, complexity, and time will increase. What approach should be taken?

Missions. Totally unexplored is Pluto. It has not been visited close-up by visiting spacecraft. Nor have comets or asteroids been closely examined. Next, beyond our solar system lies the region of the Oort Cloud, commencing at 20,000 AU, home of comets, with planetesimals considered to be a few degrees above absolute zero and having very little reflectivity, making study from Earth's observatories practically impossible.

The next known mass of interest is Earth's closest stellar neighbor, Alpha Centauri. Actually a 3-star system at 4.3 light years, Alpha Centauri is theorized by some to be incapable of maintaining a stable planetary structure sufficiently long to allow the formation of life. Others theorize its mass dynamics is, to the contrary, sufficient to permit the stable formation of a planetary system. Is the formation sufficiently stable for life to exist? If present, does the existence of life bear any resemblance to Earth's primitive life? If life exists, is there an intelligent life form? A little further out is Earth-sized, but older, Barnard's Star. Does it have a planetary system? If so, is Barnard's Star a sun too ancient to contribute the vital heavy elements considered to be necessary for life? These are questions that address the most fundamental understanding of man and his currently perceived unique status in the universe. There is no technical reason to expect our sun to be the unique owner of a habitable planetary system, but we have no data to the contrary. In-situ spacecraft will provide answers.

Closer to home, we find that from basic science and exploration missions we enhance our knowledge not only about the visited planet but about our home planet, Earth, and ourselves as well. Nowhere has the truly delicate nature of the only known form of life in the universe been better revealed than by the Apollo mission as it departed Earth and by Voyager as it departed the Solar System. If there is no other benefit from the space program than to illustrate the delicate gift of nature, thereby preserving the Earth and its environment, then there is no better investment that man could have made.

Understanding the solar system is best accomplished by in-situ instruments where they are able to accurately characterize the physical composition of the planetary environments. The data permits understanding the Solar System as a

dynamic operational system. Using that in-situ/exploration philosophy to project future missions, we are in a position to anticipate spacecraft designs that give man close-up knowledge of the planets and also of the near-by stars. These missions will include sample returns as well as the operation of remote sensing laboratories and unmanned systems operating throughout the solar system. Those missions are among the challenges that await us.

Man's nature is to explore. He hopes for the challenge of visiting the planets, just as he desired to land on the moon, ascend the highest mountains, and penetrate the deepest oceans – all very inhospitable environments. Serious consideration is being given to a permanent presence of man on Mars. For that to become reality, the settlement capability must become safe and affordable. Space logistics must be provided, and we must ultimately develop the means to be independent of logistics by having a self sustaining utilization of local planetary resources. That capability requires high power – multi-megawatts.

Mission requirements. The question is, "Can we meet these objectives and still meet other national commitments?"

> *The answer resides in reducing the requirements for vehicle mass placed into low Earth orbit.*

Reduced mass to orbit results in much greater economic and safety payoffs than increasing orbital accessibility from Earth's surface. The current Earth-to-orbit launch vehicle class will serve adequately for High Energy Space Missions provided the right form of energy is developed for space propulsion and power, although enhancements in chemical propulsion safety and reliability nevertheless remain important. These new propulsion systems must exhibit performance properties well beyond the regime of chemical propulsion.

To provide the space transportation infrastructure solution using the requirements reduction approach, not only must we develop the energy sources into practical high performance flight operational systems, but also the energy conversion systems must be inherently efficient and safe to the environment and man. These high performance flight vehicles must be designed using the **highest form of specific energy** possible, coupled with realistic flight energy conversion systems capable of performing propulsion using minimal mass. That is, the **highest specific power** possible that can deliver the energy at the desired rate and duration within space flight mass constraints is essential.

Let us then postulate the objective of sending a 130 metric ton (MT) manned payload to Mars, returning 60 MT, and accomplishing the flight within 3 months each way. Using that flight time we have already qualified our crew to Mars by virtue of the Skylab Program, and the Mars mission could proceed directly from low Earth orbit, desensitizing extensive supporting space infrastructure. Further, it may be difficult to qualify the crew physiologically and psychologically

for approximately 2 years of space flight time. Launch operational constraints result from the low performance approach and cause multi-year launch delays from missed launch windows. These are operational problems which we wish to avoid. Launch window concerns must be substantially reduced, preferably eliminated. Abort to Earth must be made possible to enhance flight safety. The pursuit of manned exploration of any of the outer planets, including manned asteroid missions, within 4-5 years of round trip flight time is also a goal worth considering. The knowledge gleaned from successful remote space operational experiences in flying the Mars mission and outward would contribute toward qualifying man step-wise for complete solar system exploration.

Similarly, consider sending a 20 MT outbound science mission payload and returning soil to Earth for detailed analysis via a 10 MT inbound payload. This mission would provide a generous sample of soil from a visited planet anywhere in the solar system and return it within ~2 to 7 years of space flight time, thereby establishing an upper limit of ~20% of the career-life of a scientist. Multiple unmanned asteroid missions including sample returns are necessary for the efficient, economical conduct of science on these rewarding, but numerous, planetesimals. Space scientists will no longer be required to spend nearly an entire career waiting for a spacecraft to travel to reach its destination. The payload sensitivity to mass growth – a major science program cost factor – can be virtually eliminated, and greater payloads can be carried per mission to achieve a high science gain efficiency.

For solar system science and exploration missions, reasonable values of initial vehicle mass in low Earth orbit, coupled with acceptable flight times, can be achieved by specific power systems exhibiting 1 kW/kg or better. Low constant acceleration vehicles serve these missions well, even at the 10^{-3} to 10^{-4} g range. For low thrust to be optimal, long firing durations of two-thirds of the flight time are required. Now consider the power level and initial vehicle mass. The unmanned missions will require jet power levels on the order of 20-60 MW and the manned missions to Mars – on the order of 150 MW. For those missions the initial vehicle masses in low Earth orbit are reasonable, 60-320 MT and 600 MT respectively. To accomplish such missions we look to propulsion systems which can deliver variable specific impulse ranging from 10^4 seconds to 4×10^4 seconds, clearly well beyond the limits of 500 seconds for chemical systems and 900 seconds for fission thermal. An order of magnitude increase in system specific power to 10 kW/kg decreases the initial vehicle mass by a factor of 3 for the manned Mars mission. Clearly it is in our interest to develop the highest specific power and performance possible.

Finally, as a new science initiative, let us examine a 10 MT payload that conducts a rendezvous mission with our next-door neighbor star, Alpha Centauri, to determine its expected mission bounds. But to perform the stellar/Oort Cloud missions, the next missions beyond the solar system, the 1 kW/kg system is inadequate. The development of a 10 kW/kg propulsion system as part of the space vehicle infrastructure improves the mission

performance, but it remains a very challenging mission. The first stage power level for a rendezvous mission with the nearest star using a 4-stage vehicle is 3×10^6 MW, and the initial vehicle mass is 1.6×10^6 MT. That vehicle requires 244 years to rendezvous its payload with Alpha Centauri, permitting continued studies for years. The average specific impulse is 270,000 seconds. To decrease the flight time requires a less ambitious mission or, alternatively, the expenditure of greater power. A fly-by mission can be accomplished in 3/5 of the rendezvous mission time, but the mission's science yield is greatly reduced. If we wish to rendezvous within 100 years, then a first stage power level of ~30 terawatts is needed using a 100 kW/kg system. A new source of energy is thus mandated.

To achieve the requisite reliability over such long durations, the system design, in essence, must be what is defined here as a "solid state propulsion system" – one exhibiting no moving or eroding parts.

Energy sources for propulsion. The highest specific energy release resides within the nuclear reactions: matter-antimatter, fusion, and fission. Compared to chemical systems, matter-antimatter is nearly 10 orders of magnitude greater but has many difficulties, such as low flight system technological understanding, the nature of energy release, safety, and economic issues. Fusion produces more than seven orders of magnitude energy, whereas fission is close behind at over six orders of magnitude. A controlled, confined fusion energy conversion system, pursued since 1952, has not been demonstrated. The problem is with providing stable plasma confinement of a hot ($\sim10^8$-10^9K) plasma necessary to obtain the release of fusion energy. Fission thermal propulsion was demonstrated in the 1960's and further research dropped since chemical propulsion was shown to be capable of accomplishing the Mars mission without the safety concerns and operational complexity associated with fission. Fission flight systems will be very difficult and expensive to test on Earth without impacting the environment.

Fusion energy for propulsion. Fusion energy has the greatest potential for meeting High Energy Space Mission system requirements as exhibited by its high specific energy content. Flight systems using fusion energy have the potential capability to fulfill the aforementioned system and operational requirements.

The use of fusion energy, using the prudent selection of fuels, will provide a mission capability that no other energy source can provide, at least in the foreseeable future. The high energy operational level of the reactor's charged particle plasma produces high specific impulse which can be controlled for trajectory optimization to minimize vehicle mass. The use of low neutron yield deuterium-helium-3 as the space fusion fuel will offer NASA many overall safety and operational advantages over other fusion fuels, nuclear fission energy sources, and chemical propellants. Neither fuel is radioactive. Its reaction products can be almost exclusively charged particles, permitting the direct conversion of the plasma energy to propulsion and power. Further, it will not

impact the Earth's environment as will other fusion fuels and nuclear fission energy. Fusion propulsion systems will operate as relatively low thrust systems, particularly considering the mass of these vehicles. But the small velocity changes from low acceleration spacecraft, when integrated over very long flight durations, can have a very high payoff, just as compounded interest. Fusion systems operate optimally at the high power levels discussed and are not known to scale downward.

The physics of fusion as a source of energy is well established. The theoretical performance level from fusion energy is capable of meeting the above High Energy Space Mission requirements. The missing ingredient is research and development (R&D) on suitable space fusion plasma confinement concepts. Studies have indicated that the performance of fusion propulsion systems should be possible in the 1 to 10 kW/kg range, yielding gigawatts of power. But whether or not adequate specific power systems can be developed, the key technology after the demonstration of net power production, cannot be stated at this time. Testing is needed for confinement and systems development.

The terrestrial fusion program's key experiments pursue two plasma confinement approaches, magnetic confinement fusion (MCF) and inertial confinement fusion (ICF). MCF is terrestrial electrical power related. It develops the technology to provide just one electrical power source of many into a multi-energy source electrical power network grid. In other words, it is not currently a "mission enabling" technology for the commercial electrical power industry. ICF is weapons related. There is no space fusion research, although NASA had conducted a very small activity (~$1M/annum) at the NASA Lewis Research Center from 1958-1978 (Sch91).

For magnetic confinement reactors, those designs having a large β are mandatory. A reactor's β is a design parameter which indicates the ratio of plasma pressure to magnetic field pressure. High β is obtained by substituting magnetic fields created from plasma currents in place of those due to external coils. They, therefore, efficiently use magnetic fields. A second, equally crucial characteristic, is a magnetic geometry that allows efficient energy coupling to a flowing propellant. Magnetic fields offer the potential for designing a "solid state propulsion system" – essential for high reliability systems.

The Field Reversed Configuration (FRC) is cited as an example of an approach providing both desired characteristics: high β (90%) and good thrust conversion (linear). Other examples include the tandem mirror, spherical torus, and magnetic dipole. The FRC's funding has been at a very low level, roughly equivalent on a gross national product basis to Dr. Goddard's initial very simple chemical rocket work, but fusion is clearly a task many orders of magnitude greater in technical difficulty. The FRC is a lesser developed concept than the tokamak, the leading reactor design approach, but one inherently too massive for space use, having a β of ~6%. Further, recent study of an advanced tokamak (ARIES III) did not show it to be a design capable of burning D-^{3}He, the fuel of preference for space.

Key to any reactor's use in space is plasma stability and, for propulsion, stability under propellant flow. The FRC's plasma stability characteristics were better than the theoretical predictions although problems were recently encountered in testing at the Los Alamos experiment, FRX, in 1990. Propulsion is an experimental unknown although the FRC's inherent linear properties point to the nature of the important characteristics desired for space.

While net power output from fusion has yet to be demonstrated, the gains that have been made in surmounting greater than anticipated obstacles give a high level of confidence that success will be met. The three key physics parameters for plasma burning – density, time, temperature – have individually met the criteria required for deuterium-tritium breakeven, a condition that defines when the energy produced by the fusion reactions is equivalent to the power placed into the reactor. The gain is now 0.8 for magnetic experiments for burning deuterium-tritium; the ICF gain is classified. The space fuel, D-^{3}He, will be more difficult to demonstrate than deuterium-tritium due to the higher temperatures required, but the engineering solutions are considered to be simpler.

The flight system requirements also present a high degree of technical challenge. Therefore, in addition, advanced fusion-powered flight vehicle system research must be accomplished to integrate the fusion reactor with the energy conversion hardware for propulsion, electrical power, and system controls. The reactor's energy conversion system must be researched for integration into a space vehicle system. Thermal management and control will be a great challenge. Proper research planning for system integration is key and is the proper managerial approach for meeting with program success.

Thus, the terrestrial requirements and those for space diverge sufficiently that a separate space fusion program is essential if the space program is to achieve a space fusion capability. The present commercial fusion program is not faced with weight requirements, so the key terrestrial fusion experiment under development – the tokamak – is unfortunately not suitable for adoption to space propulsion applications. The tokamak lacks applicability to space due to its inherently large mass/power associated with the complex coil system, among other factors. A significant challenge to the terrestrial program is ash removal from the products of fusion combustion. With space propulsion this problem is simplified since the function of propulsion systems is to intentionally bleed a proportion of its plasma mass over board. That constitutes a major technical reason why space fusion energy use may well precede the ground application.

Fusion propulsion development. Critical to the future of space missions, whether for research, exploration, or commercial purposes, is the High Energy Space Mission class. Fusion is considered capable of accomplishing space propulsion and power in a technologically superior manner compared to all known sources of energy when all system aspects are considered. There are great economic and safety benefits to be gained from fusion energy. A return of investment capital sufficient to pay for the developmental costs of fusion can be realized based upon one manned flight to Mars. Using a space-based

propulsion system designed to 1 kW/kg, we can reduce the number of Shuttle-equivalent launches from 37 to 7 for one trip. That mission includes a more generous payload allowance for the fusion powered vehicle, a safer–fast mission for its crew, and a reduced environmental impact resulting from fewer launches and the use of non-radioactive "space" fusion fuels.

A research strategy in non-chemical propulsion systems which can accomplish these missions and provide major benefits is totally absent. The United States fusion research program, under the management of DOE, is directed toward terrestrial electrical utility power applications and weapons technology – the DOE charter. Consequently, the terrestrial program is not expected to directly benefit the space program due to technical requirement differences – and due to agency mission and, therefore, program priority – differences.

To illustrate the significance of program priority differences, the alternate experiments which might have space applicable spin-offs were terminated in 1991 due to a reduced budget.

Instead, research will proceed with the tokamak which will be useful in understanding plasma physics on only one particular confinement approach – one which is not of interest to space propulsion. Space issues, therefore, remain to be addressed. NASA must be in a position to control its future in this critical technological area.

A NASA space fusion program which performs space fusion reactor research and related experiments to achieve plasma burning and propulsion is necessary. The proper time to initiate the program is now. Timeliness is important in view of the lengthy development effort and the importance of fusion energy to space missions. *The preferred strategy is to commit to full scale experiments due to the difficulties with modeling plasma performance – particularly reactor plasma transport – and due to the major cost and safety benefits to the space program if fusion were developed.* Even with this expedited approach, it is difficult to project that the first operational fusion powered flight will occur in less than 30 years from program start. Equally important for the program to address are the key fusion flight system related matters for implementation in a space flight operational environment. While fission propulsion research has been conducted, fusion and fission are physically entirely different natural phenomena requiring different technical approaches and design solutions to address for implementation. There is no reason why the development of the two should be linked such as the development of one preceding the other. The physics is not the same.

Fusion's potential is of strategic value to space. The conclusion is that NASA should proceed with its own space fusion development program to advance toward space priorities and technical requirements, rather than to be deferred in

response to world energy demands. The very limited preliminary analyses of fusion systems for space flight and confinement progress appear sufficiently encouraging to warrant that approach and program initiation.

Timeliness is important; space has an immediate, critical need for a flight fusion system. The development time is a function of program commitment and the cooperation of mother nature. It is important to leverage the terrestrial program, however, so that we can take advantage of the results achieved from alternate experiments and from the expertise gained in the terrestrial program. Progress is determined by the program budget. But fusion funding, by space standards, has always been low, approximately $50 M up through 1974 and subsequently increased to ~$350 M average over the next 15 years. But the level has been steadily decreasing to ~$200 M in 1991 from the maximum of $475 M in 1984. By comparison NASA spends an estimated $500 M on chemical propulsion – a mature technology.

The Space Fusion Program should be funded at a level commensurate with its importance to the space program. The $500 M chemical propulsion level maintains a technical status quo rather than provide the quantum leap in the space propulsion capability which the space program needs. A minimal level of $150 M at the working level (i.e., exclusive of overhead, 1990 dollar reference) would support a key experiment plus a limited investigative development of several promising alternate confinement approaches. An equivalent funding level for fusion as expended for chemical propulsion should provide results in a time frame that will support missions that are just now starting to be considered.

In summary, a space fusion energy capability in the multi-megawatt to multi-gigawatt power level range is considéred mandatory for NASA's High Energy Space Missions. The pursuit of faster space transportation systems is mandated by the Space Act of 1958. The present fusion research program will not serve as a substitute. Research to develop space fusion energy is considered to be high risk, but extremely high payoff – mission performance, safety, economics, and reliability. Otherwise, the future of the United States' space program can be expected to stagnate as advanced space missions soon become energy constrained. If we act now and if mother nature cooperates, then fusion could be made available to support the High Energy Space Missions on a timely basis. Furthermore, if the United States does not act, some other forward reaching country can be anticipated to fill the technology void by undertaking the development of fusion energy for space. Clearly the country that masters fusion energy will dominate space in the 21st century. Further, from a management perspective the space fusion program has the advantage in that it offers an energy option to fission. Otherwise, fission becomes a critical infrastructure single failure point in mission planning. But fusion, also, offers other gains as well.

FUSION ENERGY
FOR SPACE MISSIONS IN THE
TWENTY FIRST CENTURY

With exciting visions to the future, imaginative new space science missions and human exploration programs can be performed, ones requiring power levels of a magnitude not yet considered by the space program. The missions considered herein require large velocity changes, on the order of 90 km/sec up to 30,000 km/sec and large power levels of 10's of gigawatts. These are achievable provided the space program develops energy conversion systems having sufficiently high specific power systems with variable, high specific impulse propulsion systems. For continued advancements in space science and exploration, high specific power energy conversion systems, on the order of 1 kW/kg to 10 kW/kg with a variable specific impulse ranging from 10^3 to 10^6 seconds are important to NASA's space flight fleet in the 21st century.

The next step beyond solar system science and exploration is stellar science and extra-solar system planetary science. There is much that we could learn about Earth and our solar system from the study of extra-solar systems, provided that they are found to be accessible. With an entirely new mission capability, NASA would be in a position to include stellar rendezvous missions such as flying a 10 MT science payload to Alpha Centauri in less than 300 years of flight time. Missions beyond could be considered. Safer Manned Mars Missions would become possible. Flight times there, flying substantial manned vehicles, could be reduced to 3 months or less. We would have available the potential to explore other planets and their moons using manned vehicles.

These missions can be accomplished with substantially reduced costs by decreasing the requirements for mass to be placed into low Earth orbit (LEO) and by increasing the space payload mass fraction. The latter is mandatory if space is ever to become economical. For meeting science goals, any planet within the solar system can yield a significant soil mass for return to Earth via a 20 MT outbound-10 MT inbound spacecraft, an accomplishment of a round trip flight time to the furthermost planet within eight years or less. Multiple asteroids, up to six, can be visited at a distance of one AU apart and samples returned to Earth within 3 to 4 years or less. The region of the Oort Cloud mass can be scrutinized from locally observing spacecraft which can conduct a rendezvous science exploration mission within fifty years of flight time.

Energy, the key element in NASA's mission architecture, will be necessary to fulfill the "U.S. National Space Policy," approved by President Bush on November 2, 1989. The development of advanced, high performance space power systems needed for making these missions possible, safer, and affordable is absolutely crucial. The space power machines necessary to

1

perform the enabling mission technology can be provided only if the proper form of energy is available.

"High performance space power systems" is attained by high specific power systems and variable, high specific impulse propulsion systems. Fusion energy was selected from the results of this study as the source that appears most attractive. Therefore, supportive of the National Space Policy, fusion energy technology, if available, would provide the performance permitting the mission enabling capability for space, one that should become a part of the space transportation infrastructure.

Further, beyond the mission enabling enabling capabilities, there are significant safety and economic advantages to be attained from high specific power machines. Fusion energy, in comparison with other high energy sources, offers significant inherent safety advantages as well as economic benefits. Fusion powered spacecraft will become crucial to NASA's advancement in the space science and exploration missions in the 21$^{\underline{st}}$ century.

The accomplishment of high energy missions of the type considered in this study will not be easy to achieve. Energy requirements for space propulsion will grow. This class of high energy missions requires NASA's commitment *now* for the space program to realize timely benefits. The long lead time in bringing this striking new capability forward alerts us to the importance of commencing the challenging research early. *Now* is a proper time to assume the world leadership role in developing a high energy space fusion capability.

The ultimate future for the continuation of the advancement of space exploration and space science depends upon the development of that high specific energy capability. Man's space exploration capability can be anticipated to be very energy constrained in the not too distant future, perhaps within the next quarter century, by energy limitations that will not have quick solutions. Space travel's economics will become great, possibly to the extent of making high energy missions not affordable. That will prevent our ability to press forward with more ambitious missions. Therefore, the initiation of a relatively modest investment *now* for a well-planned theoretical-experimental demonstration program, one designed to achieve a high energy space mission capability, constitutes a major investment opportunity for the future of the space program.

Fusion energy has the capability to perform the missions. Its potential offers such great dividends that it can not be ignored.

To arrive at the content of this summary a study was conducted as shown in Fig. 1:
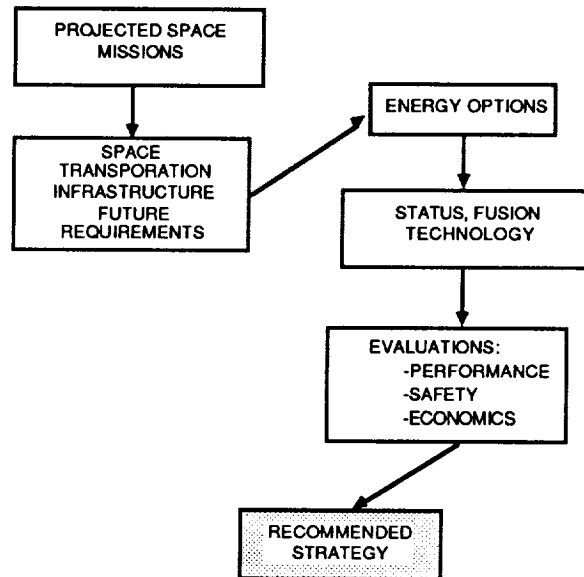
Fig 1. Study content and flow.

Space fusion energy supports the "U.S. National Space Policy"

The conclusion that fusion energy can serve as a key element in the accomplishment of the "U.S. National Space Policy" is based upon an excellent matching of fusion's capabilities with the policy's missions — the thesis of this summary.

The overall goals of the United States space activities are: ... (2) to obtain scientific, technological and economic benefits for the general population and to improve the quality of life on Earth through space-related activities and to expand human presence and activity beyond Earth orbit into the solar system. (Anom89, p1)

The objectives of the United States civil space activities shall be (1) to expand knowledge of the Earth, its environment, the solar system, and the universe; (2) to create new opportunities for use of the space environment through the conduct of appropriate research and experimentation in advanced technology and systems; (3) to develop space technology for civil applications and, wherever appropriate, make such technology available to the commercial sector; (4) to preserve the United States preeminence in critical aspects of space science, applications, technology, and manned space flight; (5) to establish a permanently manned presence in space; and to engage in international cooperative efforts that further United States overall space goals. (Anom89, pp 2,3)

In order to advance or even to continue space science research and to conduct manned exploration much beyond Earth orbit will demand the availability of high energy sources to move large payload masses and to conduct timely missions at greater and greater distances as the lesser energy demanding missions and space goals become fulfilled. The space program will be compelled to incorporate into its space transportation infrastructure more efficient systems that offer quantum leaps in performance rather than minor refinements in lower specific energy systems. That new space transportation infrastructure will be required for logistical support beyond the Earth-moon operational regime to achieve the economy necessary for reasonable support of those missions. Fusion energy has the unique potential for providing that energy source due to its high specific energy release; the quantity of fusion energy released is more than seven orders of magnitude greater per unit mass than for chemical reactions. The caveat is that the technology has to be appropriately developed for space use.

Space fusion and high energy mission class background

At the present time, fusion energy, the energy source of preference derived from this analysis, is researched solely for the terrestrial power application, although a modest NASA space related fusion program was implemented earlier at the Lewis Research Center (1958-78) (Sch91). The high energy class of space science missions of the type considered herein has not been given consideration in advanced science mission thinking and planning. Ion engines powered by electrical energy, generated by fission energy, represent the most advanced, high specific impulse performance propulsion concept that has been actively considered and researched.

The rationale for this lack of consideration for the fusion energy level mission class is attributed in part to the fact that controlled fusion is not today a demonstrated technology. While many consider fusion as an unproven technology not applicable to space missions until a long time far into the future, that opinion is not shared by those key individuals involved in this study. An active space fusion energy development program, if initiated now at the proper level of funding, could be expected to produce mission results by the middle of the 21st century, assuming the cooperation of mother nature. Lacking a dedicated space fusion energy program and using the strategy of relying upon the development of space fusion energy as a spin off from the terrestrial fusion program, NASA has neglected this critical class of missions and cannot anticipate advancing to that next phase of space science and exploration missions.

Flight programs will become energy constrained without a timely solution unless timely action is implemented soon.

Study content

Based upon the anticipation that high energy missions need to become an active part of NASA's planning, this study was initiated. The study scope included:

- high energy space flight missions for the conduct of space exploration and space science,
- other potential applications of fusion in NASA's mission,
- energy sources,
- establishment of key mission performance parameters,
- key, high energy developmental issues,
- a consideration of the terrestrial fusion program and fusion's status to establish its applicability to space and which of the current activities might have space application,
- an evaluation of the advisability of a NASA Space Fusion Program,
- the acceptability of fusion energy for space flight operations including safety, economics, and reliability with and without fusion,
- program options for NASA to consider, and
- a recommended strategy for NASA to pursue.

In comparison with other energy sources which are currently, or which might become available for space use, fusion energy was concluded to be optimal. But fusion technology research is a missing element in the planned space program's energy infrastructure.

The benefits from fusion energy are striking. It enables new missions and enhances others while providing a high value to the United States space exploration program by providing important safety benefits for the space traveler. These benefits extend to the Earth's population during the conduct of those missions as well. It's high performance properties can make the 21st century space missions affordable and appears to offer the only hope that commercial venture can become viable in space.

5

Additional studies are not needed to demonstrate fusion's importance, or, for that matter, the value of any high specific power propulsion space flight systems. Instead, the most significant and appropriate investment of resources are analyses and activities that focus on and result in experimental test demonstrations of the feasibility of fusion to achieve high specific power systems. The most significant question to address is, "Are the advanced mission studies making the proper assumptions in terms of specific power and specific impulse performance as well as for safety?" Experimental and test results are needed to provide answers.

The product required now is proof, from data, that the space fusion "state vector" for plasmas can indeed be accomplished by man. That constitutes the first step that we must take. There are many other equally important technical matters in addition to the demonstration of plasma confinement that must be resolved before space fusion can become a reality, such as the supporting flight system performance characteristics as thermal control and space restarts. In fact, once that space fusion "state vector" has been demonstrated, there is no task of greater significance than that of attaining a space rated reactor design, i.e., one exhibiting space favorable specific power characteristics. Hence, more mission studies and evaluations can only conjecture upon what are the real, "hard" requirements hardware issues and do not settle the important question of the technical solutions for fusion plasma confinement and fusion plasma energy conversion into propulsive power for spacecraft. Fusion makes attainable the goal of maximizing efficiencies via direct "plasma-power" conversion — as opposed to thermal power which is less efficient. The means to that end is research, analysis, and testing.

Because there are significant differences in system designs between the hardware used for fusion energy for space in comparison with that used for commercial terrestrial applications, the NASA and DOE technology approaches and requirements differ fundamentally. DOE must demonstrate an economical utility power production fusion system without regard to the key NASA mission objectives – a high specific power and high specific impulse, variable-thrust, space flight propulsion system. This difference in application could very well lead to different design solutions and approaches although there could very well be significant similarities. In the aeronautics and space applications of power producing devices, we have necessarily incorporated different designs and operational approaches. Power systems even vary from space to aeronautics.

Even on a programmatic priority basis the agencies differ. The space program could make use of fusion energy *now* if it were available; terrestrial fusion, by contrast, must first prove itself to be economically competitive with other commercial power energy sources. But at the current rate of progress, there is no reason to expect fusion energy becoming available for *space* within the next 50 years or longer, as the result of a Pygmalion effect – perceptions becoming reality – if for no other reason. In that case, the space programs will have

forfeited the major mission enabling performance ability, the safety, and the cost advantages that fusion energy reactors have to offer.

In reaching that conclusion, an extensive analysis of the flight system aspects of fusion energy was conducted. The analysis ranged from fusion's practical uses for benefitting NASA's exploration and space science programs – to the fusion system requirements – to a proposed fusion research plan that addresses all known major challenges in arriving at a space fusion capability.

Value of fusion to space

Fusion has an attractive high specific energy yield, which provides a mission enabling capability. We have 5 major sources of energy to accomplish space propulsion and power: chemical, solar, fission, fusion, and matter-antimatter. To accomplish missions with minimal mass is essential, and accordingly we see that fusion offers a 10^7 improvement in specific energy over chemical systems. Fission is close within an order of magnitude of fusion, and matter-anti matter is two orders of magnitude beyond. A good high performance propulsion system must exhibit high specific power properties to use the high specific energy efficiently. Fusion system studies indicate that such vehicles should exhibit 1 to 10 kW/kg whereas fission is an order of magnitude less. Efficient, low thrust missions require a variable thrust and variable specific impulse propulsion system. Specific impulse, to be optimum, should match the space craft's velocity so the higher energy, faster missions which shorten flight times require high specific impulse values from 10^4 to 10^6 seconds. Fission thermal systems, the only advanced system demonstrated, is limited by material properties to 800-900 seconds because specific impulse is proportional to $\sqrt{T/m}$ where T is the gas temperature and "m" is its mass. Fusion occurs only at high temperatures, at least for practical space applications. (Muon catalysis is the exception.) Very high exhaust velocities (approaching the speed of light) can be obtained with fusion whereas the kinetic energy for fission is limited to 3000 F. Fusion systems are anticipated to operate in the multigigawatt regime which is well beyond the other systems except matter-anti matter. That energy source is not well defined as a space system, and the fuel availability is non-existent. Serious safety concerns exist too. Fusion may not supplant the lower performance fission-thermal and ion systems which would also play a role in the development of space.

Very significant mission benefits for science and solar system exploration can be attained by fusion's presence.

First, it is mission enabling. Consider hypothesized high energy missions and the energy requirements to meet those applications. Due to the physics of fusion reactions the practical applications of fusion all relate to large energy consumption missions, namely, those in the multimegawatt category and

7

higher; fusion is not currently foreseen as a competitor to, nor a replacement for, the low energy systems, at least for the near term initial applications.

A few of these high energy missions include:

- faster and therefore safer Manned Mars Missions,
- manned missions beyond Mars,
- in-situ stellar science,
- interstellar plasma science,
- understanding and mapping of the heliosphere,
- interstellar astronomy,
- Oort Cloud exploration and science,
- multiple planetary visit missions using just one spacecraft on a single mission,
- comet/planet rendezvous with sample returns,
- polar solar science,
- faster trip times to the outer planets with more massive and better equipped science payloads,
- science exploration of the inner planets,
- remote planetary materials processing energy, plus others.

Those new and more efficiently conducted advanced missions could be achieved provided that a high performance space fusion reactor, capable of yielding the propulsion and power characteristics as discussed herein, can be developed. Consequently, the importance of and the need for critical test demonstrations are stressed. These missions can be contemplated because of the theoretically high fusion reactor performance, calculated to be up to 10^6 seconds for specific impulse and to be variable for trajectory optimization, with a specific power ranging from 0.5 kW/kg to 10 kW/kg for case studies available to this effort. The ultimate capability is unknown. A 100 kW/kg system, for example, if one were possible, would permit a 10 MT payload rendezvous mission with Alpha Centauri to be conducted within the life span of just two generations.

Second, fusion enhances flight safety. Perhaps the greatest value of fusion in the relatively near term is to the safety it would offer the Manned Mars Mission. Using a moderate initial vehicle mass in LEO (Low Earth Orbit) – approximately 600 MT – NASA could deliver an outbound 133 MT manned payload to Mars in 3 months and return a 61 MT payload to Earth in 3 months. That time could be reduced to a very attractive, short flight time of only one month, provided that a 10 kW/kg propulsion system can be achieved. The mission performance characteristics of a 1 kW/kg system would be very attractive as shown by Fig. 1.

Using chemical propulsion, with its attendant less preferred operational techniques, the trip time is expected to take approximately one-two years. Unlike chemical propellants, an accidental mixing of the fusion propellants will not result in a fire or explosion. The difficulty with igniting controlled nuclear fusion reactions is a fact to which personnel in the terrestrial program will readily attest. Fusion fuels, when considered as potentially reactive chemicals, are thus inherently safe elements. The fuel preference for space consists of non radioactive isotopes of hydrogen and helium. A less preferred option is to burn hydrogen (deuterium) with hydrogen (tritium). From a chemical reactivity viewpoint, either reaction is obviously totally inert.

Third, fusion makes possible a permanent presence of man in space. That is achieved by a very major reduction of requirements for placement of mass into low earth orbit. That reduction not only has enabling performance implications that makes the permanent presence affordable; it makes the program safer by a very significant reduction of launch missions simply to place the propulsion energy mass into low Earth orbit. The performance capability is discussed below. It is obviously safer and cheaper to place the mission mass using 7 shuttle launches, rather than 37!
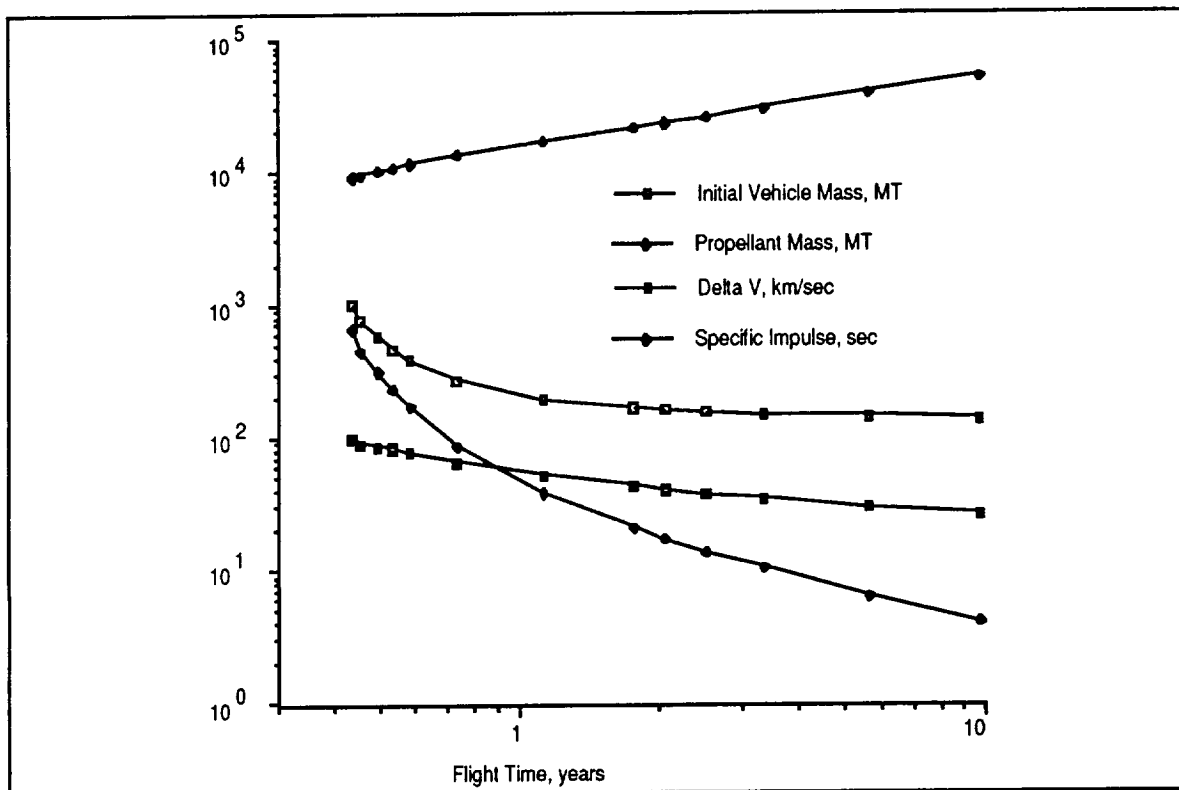

Fig. 2. Mission performance characteristics for a Manned Mars Mission – 133 MT outbound payload, 61 MT return. Round trip flight time for a vehicle having a specific power of 1 kW/kg. (Refer to Section 2, Sch91)

9

Improved crew safety results because of the reduced flight time, thereby reducing the crew's exposure to galactic cosmic rays, solar flare exposure probability, and other safety factors pertaining to reduced flight times. That level of performance permits the use of propulsive, not the currently planned aerodynamic, energy transfer for braking maneuvers. A safer flight operational mode is considered to result from the use of propulsion braking.

Fusion powered missions must accomplish more than slight improvements for the committed expenditures as must fission, mirror matter, or any other advanced sources of energy. Fusion will have to be cost effective for the missions that it serves, not simply provide a slight measure of performance increases. Hence, the advanced mission "system" aspects including fusion energy demonstration, conversion of plasma energy to propulsive power, flight systems, safety, reliability, costs, and mission performance, all became a significant, integral part of this work. These system aspects are key in the implementation of any new technology development. The competitive performance of the chemical systems for the Manned Mars Mission was the basis for the NERVA fission program's demise twenty years ago. Table 1 summarizes the key mission design data showing the requirements for mission performance gains to be attained. A range of values is included which presents the results of calculations for a rapid trip as well as trip times offering economy of propellant and fusion vehicle size while still permitting the accomplishment of the same mission objectives in a reasonable round trip flight time. Note the advantages of high payload mass fractions, the shortened flight time, and the low propellant requirements, particularly for the 10 kW/kg system.

TABLE 1. Manned Mars mission parameters for 133 MT out-bound payload, 61 MT return.

α_p, kW/kg	t, years	M_o, MT	M_p, MT	γ,%	P_j, MW	<Isp>, seconds x 10^3	Δv, km/s
1	0.44	1041	681	12.8	227	9.4	98
1	0.5	613	335	22	145	10.6	90
10	0.18	1034	676	12.9	2255	18.9	196
10	0.5	185	30	72	227	35.8	90

While fusion may offer the greatest value to the space exploration program, and particularly to the safety of manned missions, fusion energy equally permits the conduct of very interesting space science missions. Such missions examined here included soil sample return from the moons of the outer planets with flight times varying from 1.6 years for round trip flight times to Jupiter's moon, Europa, to 7.4 years for Pluto's moon, Charon. Note that those times are for the round

trip flight time, exclusive of stay time for science gathering at the site. A very substantial 20 MT payload would be flown to the planetary destination and a 10 MT payload returned to Earth where its precious cargo of extraterrestrial soil can be analyzed in depth.

The power required to perform such missions is high by today's standards, ranging from 20 MW to 60 MW; and the propulsion system performance is demanding, with the specific impulse ranging between 17,000 seconds and 140,000 seconds. The mission parameters and capabilities for outer planetary missions are summarized in Table 2. While these are very demanding requirements, advanced fusion power systems should be capable of meeting these requirements.

TABLE 2. Summary of typical outer planetary missions performance values for specific powers ranging between 1 kW/kg to 10 kW/kg for sample return missions flying 20 MT outbound payloads and 10 MT return payloads.

2.a. Specific Power = 1 kW/kg

Mission	t, years	M_o, MT	M_p, MT	γ, %	P_j, MW	<Isp>, seconds $\times 10^3$	Δv, km/s
Europa	1.56	320	243	6.3	57	17.7	209
Titan	2.99	74	36	27	18	26.2	196
Miranda	5.34	60	26	33	14	35.7	233
Triton	5.85	108	62	19	25	35.1	314
Charon	7.42	81	41	25	19	40.5	317

2.b. Specific Power = 10 kW/kg

Mission	t, years	M_o, MT	M_p, MT	γ, %	P_j, MW	<Isp>, seconds $\times 10^3$	Δv, km/s
Europa	1.56	32	6.8	63	50	64.1	209
Titan	2.56	29	5.3	68	40	81.2	223
Miranda	5.34	26	3.4	77	27	118	233
Triton	6.85	27	3.8	74	30	130	283
Charon	7.42	27	4.1	73	32	137	317

Visits of a sample returning spacecraft to three separate asteroids at 1 AU distance apart can be quickly performed, i.e., in less than only 2 years, flying a 20 MT outbound payload and 10 MT returned payload. For purposes of these

calculations the sample is assumed to be picked up at the first visit. Fig. 3 graphically presents the mission's initial vehicle mass variations versus flight time for specific power propulsion systems of 0.067 kW/kg, 1.0 kW/kg, and 10 kW/kg during the conduct of an asteroid sample return mission where 3 to 6 asteroids are visited.
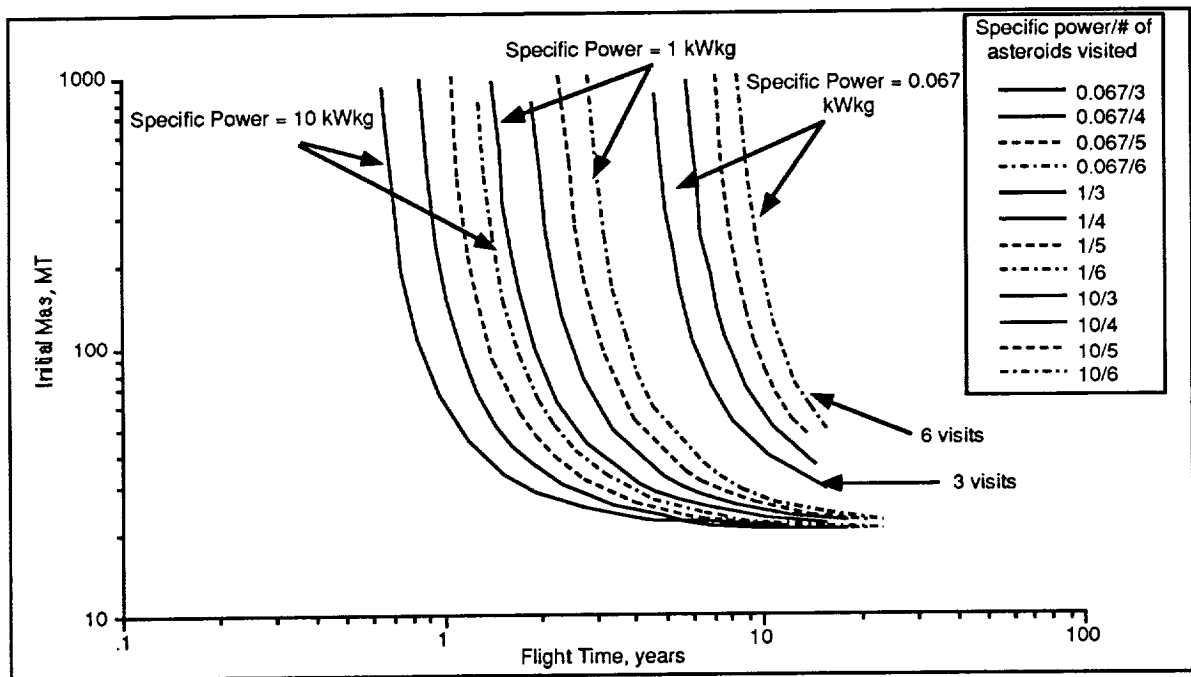

Fig. 3. Asteroid sample return mission, initial vehicle mass variations with flight duration for 3 through 6 visits. (Sch91)

The 0.067 kW/kg value is considered as a reasonable performance target for the specific power for nuclear electric propulsion (NEP) systems. There is a point of diminishing returns where performance gains are best achieved by increases in specific power rather than by initial vehicle mass. These are at the knee of the curves. Specific asteroid belt data of interest are presented in Table 3. This table shows a great mission capability in the asteroid belt, permitting the conduct of fundamental science goals and the accomplishment of space exploration objectives.

TABLE 3. Asteroid sample return typical mission parameters for a 20 MT outbound payload, 10 MT return, 3 and 6 visits to asteroids at one AU separation distance.

3.a. SPECIFIC POWER=1 kW/kg

Visits	t, years	M_o, MT	M_p, MT	γ,%	P_j, MW	<Isp>, seconds x 10^3	Δv, km/s
3	1.72	162	107	12	36	18.6	185
6	3.39	162	105	12	36	26.1	254

3.b. Specific Power=10 kW/kg

Visits	t, years	M_o, MT	M_p, MT	γ,%	P_j, MW	<Isp>, seconds x 10^3	Δv, km/s
3	1.2	44	15	45	96	57.0	257
6	2.57	41	12	49	83	86.7	329

Even more exciting is a visit to the Oort Cloud, offering perhaps the only viable means to obtain a good understanding of those small, dark pristine bodies and an accurate characterization of cis-solar system space. In a fly-by mode, a 10 MT payload can be sent to a 20,000 AU distance into the Oort Cloud region within 100 years using a 1 kW/kg system, a period of time which can be halved if the specific power can be increased to 10 kW/kg. The range in jet power that is required to perform the mission is high, 40 MW up to approximately 275 MW, depending upon the reactor design. Engine specific impulse performance requirements vary from 100,000 seconds to 250,000 seconds. Shorter flight times are possible but at a high cost of added propellant mass.

To perform an Oort Cloud rendezvous mission, a 700 MW power source operating a 1 kW/kg system will accomplish that mission in 120 years, while a 10 kW/kg specific power system completes the trip in 55 years, using a 7 GW reactor power output. The energies here are obviously of a magnitude that a new energy source is mandated. Such energy levels will be very difficult to achieve, particularly anytime soon, but we cannot achieve that for which we fail to strive.

Our nearest stellar neighbor, Alpha Centauri, actually a 3-star system – α, β, and Proxima – at 4.3 light years distance offers a still greater technical challenge. Alpha closely replicates our sun's characteristics, exhibiting nearly the same brightness properties, age, and mass. Does this multiple star system possess planets? A mission there would produce answers to an important

13

question concerning the structure, and perhaps formation, of multiple star systems — the configuration of greater than half of the known stars. If planets cannot survive sufficiently long in the multiple star systems for evolution to take place, then the odds of finding life beyond Earth have been obviously been significantly diminished.

This is not a mission for a specific power of 1 kW/kg reactor system which takes 300-400 years for a fly-by mission (Fig. 4). For a rendezvous mission, even a specific power system operating at 10 kW/kg requires close to 300 years.
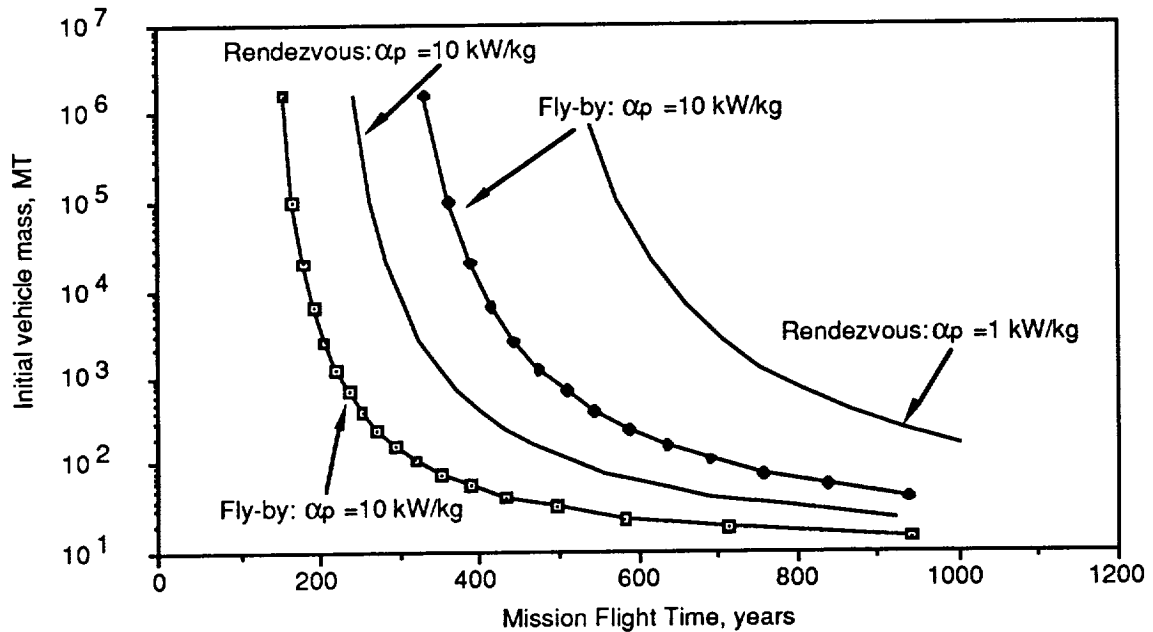

Fig. 4. Flight time to Alpha Centauri for initial vehicle mass variations over a range of specific powers, 4-stage vehicle, 10 MT payload mass. (Sch91)

Vehicles operating in the fly-by mission mode over these long flight durations provide a limited time at the target, particularly at the high speeds involved, to acquire very precious, unique science data. The more worthwhile approach for such an ambitious mission of this nature is the rendezvous mission. This mission can be accomplished in 300 years using a first stage power level of 24 GW, a 4-stage vehicle operating at a specific power of 10 kW/kg and average specific impulse of 382,000 seconds. The initial vehicle mass is 10^4 MT. The propulsion system imparts a mission Δv of 12,940 km/s.

The four stage vehicle has been designed to shorten the reactor's firing duration, thereby to enhance reliability and reduce system design requirements, rather than to achieve greater flight performance. That approach increases the initial vehicle mass which places great demands upon the reactor output,

requires a thrust of 5×10^4 Newtons, and requires an averaged specific impulse of 4×10^5 seconds. After the completion of the powered phase of the mission, a large power source is still required for the conduct of the space science operational phase. It was estimated that a 10 MW to 15 MW power output is required of the spacecraft's transmitter to achieve a minimal acceptable imaging data rate of 100 bits per second from Alpha Centauri. A continuum of science data transmitted once or twice annually provides an attractive extra-heliospheric space science program, producing new science data within several years from departure. That duty cycle would not consume much fuel. A large 3-meter telescope has the resolution power to achieve unique, meaningful astronomy in this application. Table 4 contains a summary of key mission parameters for the Alpha Centauri mission and Table 5 presents the same parameters for the Oort Cloud mission. The calculations were purely inertial and Newtonian, i.e., no interstellar drag nor relativistic effects were taken into consideration.

TABLE 4. Summary of Alpha Centauri (4 stages) fly-by and rendezvous missions for specific power propulsion system designs from 1 kW/kg to 10 kW/kg.

TABLE 4.a. SPECIFIC POWER = 1 KW/KG

Mission	t, years	M_o, MT x 10^3	M_p, MT x 10^3	Stage	P_j, MW $\alpha_{p1} = 1$ kW/kg	P_j, MW $\alpha_{p10} = 10$ kW/kg	$<Isp>$, seconds x 10^3	Δv, km/s
Rende-zvous	573	100	76	1st:	21,620	216,200	148.7	6,715
				2nd:	2,162	21,620		
				3rd:	216	2,162		
				4th:	22	4216		
Fly-by	361	100	76	1st:	21,620	121,620	118	5,330
				2nd:	2,162	2,162		
				3rd:	216	216		
				4th:	22	22		

15

TABLE 4.b. SPECIFIC POWER = 10 kW/kg

Mission	t, years	M_o, MT x 10^3	M_p, MT x 10^3	Stage	P_j, MW $\alpha_{p1} = 1$ kW/kg	$\alpha_{p10} = 10$ kW/kg	$<Isp>$, seconds x 10^3	Δv, km/s
Rende-zvous	266	100	76	1st:	21,620	216,200	320	14,470
				2nd:	2,162	21,620		
				3rd:	216	2,162		
				4th:	22	4216		
Fly-by	168	100	76	1st:	21,620	121,620	254	11,483
				2nd:	2,162	2,162		
				3rd:	216	216		
				4th:	22	22		

TABLE 5. Summary of Oort Cloud (3 stages) fly-by and rendezvous missions for specific power propulsion system designs from 1 kW/kg to 10 kW/kg.

5.a. SPECIFIC POWER = 1 kW/kg

Mission	t, years	M_o, MT x 10^3	M_p, MT x 10^3	Stage	P_j, MW $\alpha_{p1} = 1$ kW/kg	$\alpha_{p10} = 10$ kW/kg	$<Isp>$, seconds x 10^3	Δv, km/s
Rende-zvous	111	2.963	2.129	1st:	703	7,030	85.3	2,380
				2nd:	105	1,055		
				3rd:	16	158		
Fly-by	106	0.156	0.090	1st:	136	242	99.7	1,344
				2nd:	15	109		
				3rd:	6	49		

TABLE 5.b. SPECIFIC POWER = 10 kW/kg

Mission	t, years	M_o, MTx10^3	M_p, MTx10^3	Stage	P_j, MW $\alpha_{p1}=1$ kW/kg	$\alpha_{p10}=10$ kW/kg	<Isp>, seconds x10^3	Δv, km/s
Rende-zvous	55	2.963	2.129	Ist:	703	7,030	183.7	5,127
				2nd:	105	1,055		
				3rd:	16	158		
Fly-by	52.9	0.110	0.060	Ist:	136	242	229	2,690
				2nd:	15	109		
				3rd:	6	49		

The 21st Century Space Energy Program

The capability of NASA to perform these missions resides with meeting several key factors:

1. the ability to develop a specific power system of 1 kW/kg, or 10 kW/kg in the case of the stellar mission;

2. the ability to produce sufficiently high thrust for a vehicle of this size and a variable specific impulse (5×10^3 to 10^6 seconds);

3. reliable performance for as lcng as 50 years of continuous power operation;

4. reactors ranging from 20 MW to 30,000 MW jet power production.

Fusion energy has the inherent properties needed. The management structure for producing the systems enabling high energy space mission is crucial to NASA's future and is examined here. The present United States' fusion research program is focused toward commercial electrical power production. Lacking the space focus as a program goal, it will not produce devices suitable for space. To compare the two applications, if we consider the major system parameters, only the plasma output power level from terrestrial reactors – gigawatts – can be expected to match the space fusion requirements. The preferred applications of fusion energy to space missions would require even a wider operational range. The category of low, several megawatt reactor power levels at a high specific power density, i.e., physically very small fusion reactors, is a substantial technical challenge for fusion reactors, more so than the mid-

power, 100's MW to 10's GW range reactors. That is the nature of the physics of nuclear fusion reactions.

The Oort Cloud and stellar missions are well beyond the capability of nuclear fission powered ion propulsion systems, which are considered ultimately capable of attaining a specific power of 0.067 kW/kg. Fission thermal reactors are relatively performance limited, and possess inherent flight and ground safety hazards which have to be resolved for manned applications and ground testing. One major concern with the fission systems is whether safe multimegawatt reactors can be ground tested and constructed for space use in a cost effective manner that meets safety constraints and operational requirements. Gas core fission reactors possess greater inherent safety hazards which face even greater technical challenges to resolve. The mass annihilation of antimatter/matter (mirror matter) reactions is not presently foreseen as a replacement for the fusion role due to economic factors, its safety problems, tremendous energy consumption needed to produce the fuel, gamma ray production, and the resultant system design penalties that can be expected to make this energy source a practical flight system.

If the specific power assumptions for fusion can be demonstrated, it does indeed possess unique space mission capabilities. Without the high energy performance provided to space vehicles by fusion energy, the engineering solutions, and therefore program costs, become too impractical for some missions to be accomplished by lesser energy intense systems. This applies to current plans for Manned Mars Missions involving settlement.

The mission demands, that is, the mission duration and science payload mass requirements for meeting future science objectives, become too severe for chemical or fission energy systems to accomplish. The desire will be to continue to advance science and exploration. But we will arrive upon a space energy "stone wall," beyond which, from an energy requirement perspective, it will be impractical to push future missions without fusion or another equivalent alternative, but currently unidentified, high specific energy source. A very significant time is required to develop the fusion machinery or other concepts. Therefore, a strong NASA space fusion energy research program should be initiated now if we are to achieve capabilities made possible by fusion engines and fusion power generation within the next 25-50 years.

Since fusion has not been a part of significant space mission considerations, it was difficult to establish cost figures of merit for any vehicle design and operations at this time. Only one low-level funded study was recently conducted for one type of plasma confinement approach, an inertial confinement fusion (ICF) system. Detailed system analyses using magnetic confinement fusion energy for propulsion have not been funded. NASA has not sponsored such studies because its approach has been to await the development of fusion for terrestrial use. But in view of the differences in the DOE program already cited, this approach is not advancing the technology for the space application. The mainline terrestrial experiment is too massive.

18

Consequently, from the current approach NASA lacks the means to control its destiny in this critical technology. Technical feasibility needs to be demonstrated before valid system studies can be performed and program operational costs accurately determined. This has developed into somewhat of a self perpetuating circle, not unlike the proverbial chicken and egg question.

Fusion plasma confinement concepts applicable to meeting NASA's space flight requirements

The major DOE fusion experiments were examined as well as those not funded by DOE. Confinement approaches examined include the following:

- tokamak
- field reversed configuration
- tandem mirror
- spheromak
- spherical torus
- electric field bumpy torus
- electrostatic
- elmo bumpy torus
- stellerator
- inertial confinement
- Migma
- plus others.

For space, where the need to convert plasma energy to thrust is essential and where minimum mass is required, we must examine the potential candidate approaches from the standpoint of yielding maximum β where β is the ratio of plasma pressure to magnetic field pressure. Configuration linearity is important to the production of thrust where the space approach is simplified by the design function to intentionally "leak" plasma from confinement in order to produce thrust. The importance and selection of high β confinement approaches must be made without regard to confinement maturity. The status of maturity then becomes a guide for a development program which researches the many other important parameters that have not been taken into consideration when using β and thrust production as initial screening criteria. While the DOE-developed technology for alternate confinement concepts is too premature to conclude that a particular approach will meet NASA's requirements, some approaches appear to offer the desired characteristics.

19

Based upon the review of the current magnetic reactor concepts the Field Reversed Configuration (FRC) presently appears to have the inherent characteristics necessary for space applications. The characteristic plasma ion flux is illustrated in Fig. 4 by the arrows in the torus which provides strong confinement fields without the complication of many heavy external coils employed by the tokamak and related designs. This arrangement results in a closed toroidal magnetic field configuration surrounded by open field lines.

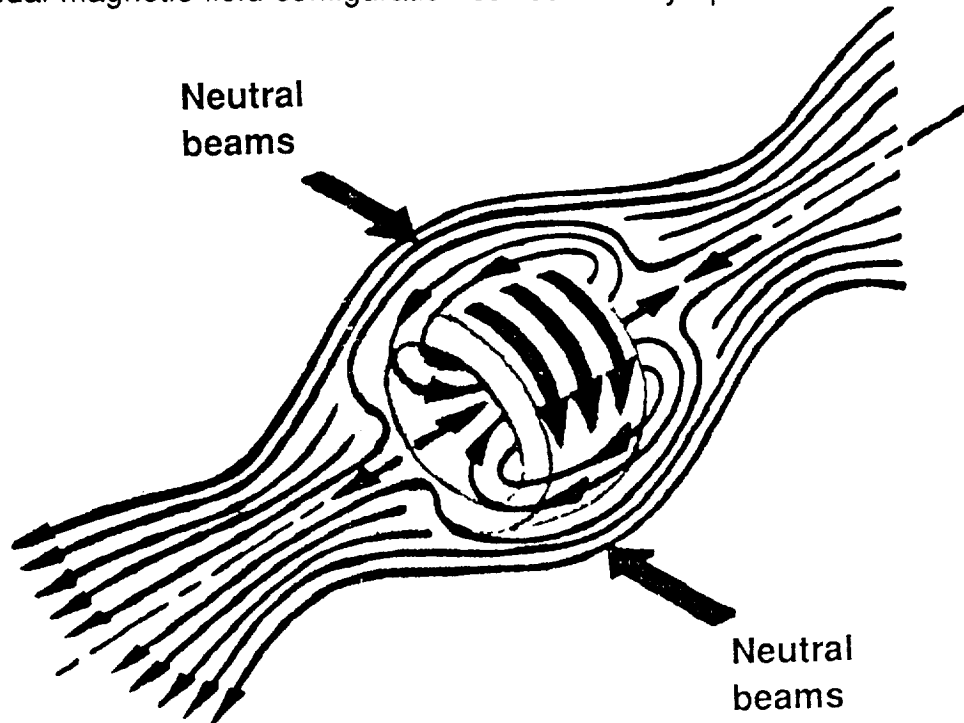**Neutral beams**

Neutral beams

Fig. 5. FRC plasma ion flux description

The FRC combines attractive features of both toroidal and linear systems. The closed inner field surfaces provide good confinement of the plasma. Yet, the linear topological nature of the external field lines allow the efficient production of direct thrust. These features result in a very high β (90%), good plasma confinement, a high power density, potential for steady state operation, and overall compact design.

One possibility for achieving ignition is to heat the fuel to the ignition temperature by quickly compressing the plasma with a rapid ramping of the plasma current and an increased magnetic field strength. Another is to inject a high energy neutral beam. Once the plasma is ignited, fusion products heat the plasma, providing an attractive reactor energy balance. A preferential flow of the fusion products is also predicted, adding to the ion currents shown in Fig. 4 easing the task of achieving a steady-state power output.

In summary, the capability of the FRC to meet the space requirements is considered a good match. Thus, it appears to have very desirable inherent

properties for the space application, but analyses and testing are needed as shown by Fig. 6.

Fig. 6. FRC status for meeting space parameters.

SPACE REACTOR PARAMETER GOAL	FRC PERFORMANCE AND RESEARCH STATUS			
	Potential to meet	Cannot meet	Unknown	Comments on FRC status
Specific Power	✔			Limited study. Requires design.
Thrust:				
(a) low: 1 N to 10 kN	✔			Limited conceptual work.
(b) medium: 10 kN to 50 kN			✔	Has not been addressed.
(c) high: 50 kN to 500 kN		?		Requires a large plasma volume.
Specific Impulse	✔			10^3 to 10^6 seconds.
Fuel Cycle			✔	Can burn D-^{3}He.
Beta	✔			90%.
Ignition	✔			Needs design study. Requires testing.
Throttle capability	✔			Needs design study. Requires tests.
Plasma Stability			✔	Limited analysis. Burn experiments required - major issue.
Power Level	✔			~50 MW to 30+ GW.
Electrical Power Variability	✔			Requires design study.
Dual Mode Operation	✔			Requires design study.
Mass			✔	Requires design study.
Efficiency ($P_{thrust}/P_{fus.}$)	✔			High.
Recirculation Power	✔			Low.
Modes of Operation	✔			Work will follow net power demonstration.
No (low) Neutron production	✔			<2%.
Failure Tolerance	✔			Requires design study.
Space Environment	✔			Requires design study.

Note that several key parameters, such as plasma stability, require further investigation, providing the basis for a developmental plan. The major technical risk resides in the maintenance of plasma stability. Although the concept has performed with greater stability than predicted, an instability mode of concern was experienced with the Los Alamos experiment during 1990 (Tus91). Further work is required to confirm this as a viable concept.

Thus, this evaluation must necessarily be considered as subjective since the FRC experimental data base is small. Only two FRC experiments were in operation at the time of this study, one at Los Alamos and another at Spectra Technology in conjunction with the University of Washington. That is in contrast with several tokamak experiments, four of which are much larger than the FRC's. However, due to a limited budget these programs have been or are in the process of being terminated in 1991 (Fpa90).

Fusion Engine

It is instructive to examine how a fusion energy source can be converted to thrust. The FRC is ideally suited to propulsion by virtue of its external topology. Engine thrust is produced by the controlled release of a portion of the plasma, directed by a magnetic nozzle accomplished by a field imbalance, Fig. 7.
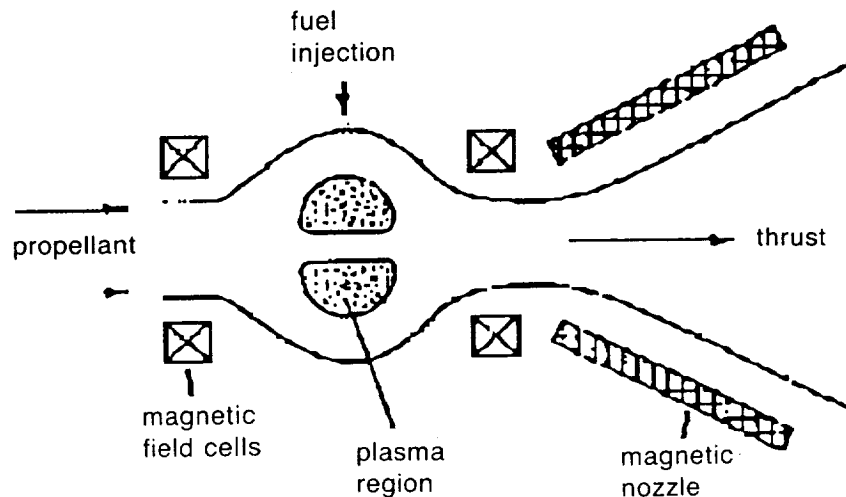

Fig. 7. FRC fusion engine. (Cha89)

The thrust and specific impulse are varied by changes in the propellant flow rate. One advantage of magnetic reactor designs is the absence of moving parts – except possibly for the pellet injection system – and of parts subjected to erosive wear. These have the inherent features that are important to the long life time operational requirements of the space program. The reactor is fueled by pellets which are injected into the plasma. Thrust and specific impulse are simultaneously controlled by the injection of propellant into the scrape-off layer. The propellant is heated by the plasma as they mix, and the unique FRC configuration suggests that a fairly uniformly heated mixture can be obtained as required for efficiency. Plasma thrust can be varied by the control of the fuel and propellant flow rates along with variations in the nozzle's magnetic field strength. A reactor of the power magnitude required for the manned programs would be characterized by the parameters as shown by Table 6 below (Cha89, Ref. Table 2).

22

TABLE 6. FRC High Power Design Parameters.

Total power	0.5 GW
Plasma Volume	80 m^3
Elongation Factor	6
Propellant Addition	0 - 0.8 kg/s
Specific Impulse	10^6 - 10^3 seconds
Thrust	0.4-50 kN

The above discussion of the FRC illustrates major points to be made for a space fusion reactor. We cannot state at this early date that it can be made to perform at net power. Other concepts could include a high β tandem mirror or a compact torroid. Ideas that could yield major breakthroughs are continually being forwarded. Since this study was conducted a new approach, referred to as a magnetic dipole, has been proposed (Tel91). In addition, DARPA has undertaken the development of an electrostatic confinement approach.

The use of the magnetic nozzle and plasma entrapment makes this concept attractive because the plasma remains physically away from the wall. The absence of moving parts in the engine and lack of components not subjected to erosion make this a concept potentially for providing a very long-life, highly reliable engine.

Fusion propulsion's generic specific impulse performance is shown by Fig. 8. Three operational modes are considered: the high impulse–low thrust region employing a pure plasma exhaust, a variable thrust–impulse range attained by mixing various quantities of propellant with the escaping plasma, and a high thrust thermal conversion mode, comparable to the more conventional thermal systems. Thrust is increased as specific impulse decreases due to the added exhaust mass from introducing propellant which simultaneously reduces the plasma velocity as the two thermalize.
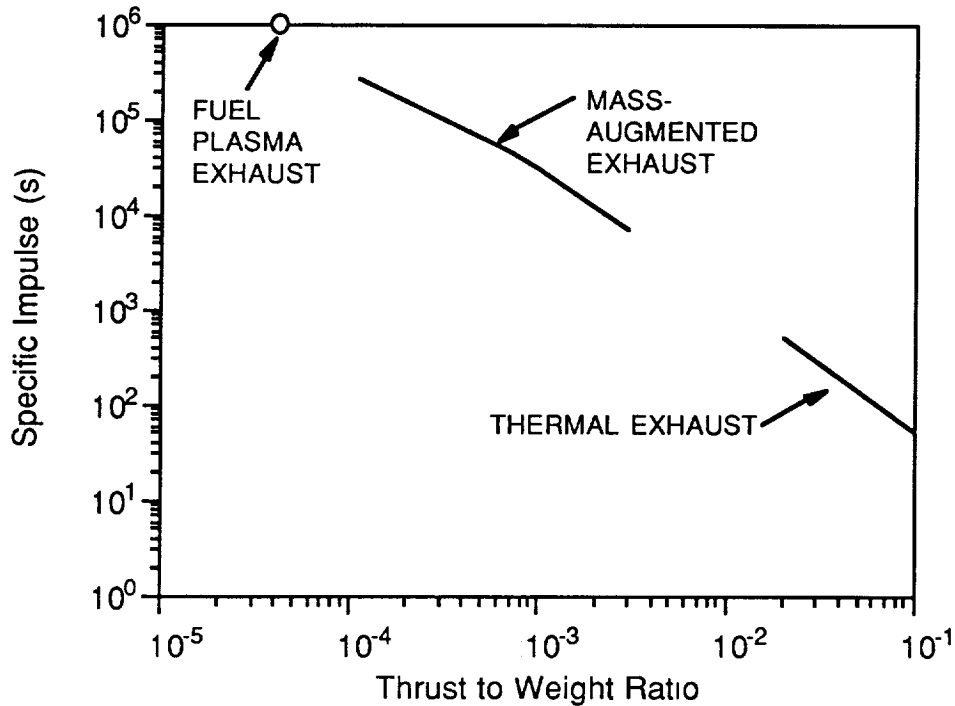
Fig. 8. Specific impulse versus thrust to weight (San88).

The deuterium–helium-3 reaction $(D+^3He \rightarrow \alpha+p)$ is particularly attractive and is preferred over other fusion reactions since it is the easiest "aneutronic" fusion reaction to achieve. Fortuitously, more than 95% of the $D-^3He$ reaction's energy is associated with charged particles, namely, alpha particles and protons. Their energy can be converted directly to propulsion and/or electrical power without the usual inefficiencies encountered in thermal conversion systems. By the proper use of design parameters, the neutron flux can be reduced to approximately 1-2%. Not only is this an essential condition relative to the reduction of radiation shielding, but neutron damage to the reactor materials should be low so that the long life times needed should be possible. With regard to its availability, helium-3 can be mined on airless bodies having no magnetic fields like the lunar surface or from other planets like Jupiter or can be bred using proton acceleration onto lithium-6 or, alternatively, via the production and decay of tritium. There is sufficient helium-3 available now on Earth for initiating a meaningful test program without developing a breeder facility or undertaking lunar mining.

The $D-^3De$ cycle is more difficult to ignite, but in order to make a flight worthy system once ignition has been demonstrated, the engineering solutions will be easier to achieve than with D-T. This is also a safer fuel to use since ^3He is not radioactive, unlike tritium. The difficulty with ignition of $D-^3He$ is illustrated by its higher nuclear cross section of ~5 in comparison with tritium. The cross section peaks at a temperature of ~5 times higher than for D-T. So we state that $D-^3He$

has a reactivity that is 25 below that of D-T for a given magnetic field strength (plasma pressure). Consequently, a larger volume is necessary where plasma reactivity is reduced or a higher magnetic field is used. This higher field strength has to be traded against synchtron radiation losses. On the highly positive side for D-^{3}He is the reduction of neutron production from 80% for D-T to 5% for D-^{3}He, perhaps even to 1% – a space reliability and maintainability advantage. That difference in charged particles power versus neutron power equates to a 5 times improvement in propellent power per unit fusion power using D-^{3}He, perhaps even greater when the engineering solutions have all been considered.

The current outlook for fusion energy

The sun demonstrates that fusion works. It operates in an extraordinarily efficient manner in supplying energy to the Earth at a very dependable rate, using gravitational forces, rather than magnetic or inertial forces. Man's mastery of the reaction's basic physics is evident from the hydrogen bomb. The problem has been in the successful confinement of reasonable sized ("miniature sun") hot plasmas long enough to obtain significant fusion reactions. Steady and very positive progress has been and is being made toward the demonstration of controlled terrestrial fusion. Magnetic confinement fusion energy experiments have already produced 100 kW from the fusion of D-^{3}He fusion in the laboratory. Planned experiments would demonstrate 100 MW of D-T fusion power before the year 2000.

In retrospect, the controlled fusion demonstration task has certainly turned out to be a more difficult one than anyone had originally anticipated, and the results from the terrestrial energy program are being obtained later than those involved in the program would have desired. The fact that these greater than anticipated problems are being resolved leaves little doubt on the ultimate success of the DOE fusion program. The question is, "What about space fusion energy?" The development of fusion reactors suitable for space applications, however, is <u>not being pursued</u>. Funding has been at a zero level since 1978, and even before then, the level was low, approximately $ 1M annually (Appendix A).

The advanced confinement concept having greatest potential for meeting space specific power requirements is the FRC which has been funded by DOE at a very low level of approximately $5M. The follow-up question is, "How viable is the FRC for space application under flight operational regimes?" Can it be designed to burn at the required power levels and maintain a stable plasma? Will it burn steady-state; or, if pulsed, will it meet specific power levels? Those questions can only be answered through a fusion development program. In space energy technology, as with all endeavors, we will extract a benefit from any activity in proportion to that which we place into it. Will DOE's funding for

25

the FRC continue as the fusion program funding decreases or as larger tokamaks are built for producing net power?[1]

The study approach, then, first and foremost, was to use the years of experience of those theoretical and experimental fusion experts having an interest in the space applications of fusion energy. Secondary emphasis was placed upon reviews of reports for guidance, and they were used typically to cite the source of data or to show the results of specific events cited in the text. The references cited comprised by no means the complete set of documents used for the report. To a large degree many conclusions were based upon the best available data and technical advice of those who have spent their professional careers in the theoretical and/or experimental aspects of fusion energy. The conclusions drawn in the report are a matter of the author's judgment and do not necessarily represent any endorsement by organizations and individuals.

The heart of the study then boils down to the key question, "Is fusion a viable source of energy for space; and, if so, what should be done to seize upon it?" To address that question, site visits for extended durations were made to some of the most prominent researchers involved in advanced fusion concepts and thinking, particularly those with a space oriented interest. They included researchers and scientists at the University of Illinois, the University of Wisconsin, and the Lawrence Livermore National Laboratory. A brief visit was later made to the Los Alamos National Laboratory and later, another to Princeton Plasma Physics Laboratory.

The mainline world fusion program has settled to the tokamak confinement concept which utilizes a poloidal and toroidal superconducting magnets to confine the plasma in a torus configuration with twisted fields. Following a progression of smaller tokamak experiments, this effort has now evolved into four large devices worldwide, TFTR in the US, JET in England, JT-60 in Japan, and T-15 in the USSR. Advanced, larger tokamaks are under study which will comprise the focus of fusion research probably for the next quarter century or longer.

Two points about the terrestrial program should be stressed, however. The tokamak has been selected largely because it provides a proven way to get a test bed plasma, not that its engineering features (interlocking coils, etc.) are so attractive. There is little hope that it could evolve into a useful configuration for space applications due to the small mass required in comparison with the large mass inherent with this confinement concept. The progression, however, has clearly demonstrated that progress is only possible in this field by a concerted

---

[1]Unfortunately this concern, made in 1988, appears to be valid: "Faced with a Congressional cut of $50 million from the magnetic fusion budget (see our November newsletter), the Department of Energy, Office of Fusion Energy has decided to terminate essentially all of its experimental programs aimed at developing a more attractive magnetic fusion reactor concept. Instead, it will protect the budget of its "conventional" tokamak program." (Fpa90)

experimental effort. But, as a consequence of the emphasis on tokamaks, other concepts like the FRC of interest here, lag substantially in having sufficient experiments to establish a sound data base.

Most magnetic fusion concepts employ a thermalized, or Maxwellian, plasma. Non-Maxwellian fusion may be useful for burning D-^{3}He with very low neutron yields. However, only two MCF non-Maxwellian designs have received any consideration. One uses magnetically directed colliding ion beams, and the second uses electrostatic fields to focus colliding beams. The first design is referred to as MIGMA, the second as HEPS. MIGMA has received Air Force funding, and HEPS is funded by DARPA. Neither are a part of the DOE fusion energy program. The fact that these agencies provided separate studies of this type coincides with the point that spin-off from the DOE program cannot be considered a sound policy in situations where the space application will be addressed.

Inertial Confinement Fusion, or ICF as it has become known, offers another approach to controlled fusion energy release for propulsion. With ICF, fusion of the fuels is to be achieved by sudden, very directed, highly compressive loads imposed, as for example, by powerful lasers focused upon the fuel. That causes a union of the nucleons and a release of energy. Modeling of the ICF reaction parameters is reported to be making great strides in the matching of predictions with experimental results. Indeed a recent design study indicated that it might perform quite well for missions within the solar system. The study assumed the D-T fuel, however. To burn D-^{3}He, the fuel of choice, it appears difficult to develop laser drivers of the magnitude needed.

Importance of considering fusion for space now

During the post-Apollo era when NASA experienced a substantially reduced space budget, relatively little emphasis was placed upon advanced missions. NASA for the past four years commenced examining new missions and space challenges. In 1990 the study and emphasis have been more intense.

Reflective of this new mission consideration and strategy, Dr. Fletcher, as NASA Administrator, initiated renewed interest in advanced space missions:

> NASA has recently embarked on an effort to define the goals, objectives, and program thrusts to guide the future of the Nation's civil space program. It is our intent that this process produce a blueprint to guide the United States to a position of leadership among the space faring nations of the Earth.
>
> The process will necessarily be lengthy, as it must be both thorough and creative...

> NASA embodies the human spirit's desire to discover, to explore, and to understand. Our overarching goals are to achieve advances in aeronautics, in space science, and in the exploration of the solar system...
>
> In order to achieve our goals, we must develop world-class facilities, advance technologies, and improve our transportation capabilities....(Fle86).

No better statement can be made to encompass the thought and goal of this study. The implementation of an Alpha Centauri stellar mission program and a fusion research program for space reflect that philosophy in the purest sense. Development of space fusion is synonymous with guiding the United States to a position of leadership. Space fusion equates with the capability to explore, to discover, and to understand beyond our current bounds. The ability to conduct more science and exploration of the solar system and beyond will advance space science and our knowledge of the universe from direct in situ exploration of it. For explorations outside of the solar system, an entirely new dimension of understanding will be attained. This is space science creativity at its finest!

President Bush has endorsed a strong U. S. space program. He has requested a program which will return the U. S. to space and allow us to remain there.

> I believe that before Apollo celebrates its 50th anniversary of its landing on the moon -- the American flag should be planted on Mars.[2]

On the occasion of the 20th anniversary of the Apollo lunar landing, July 20, 1989, he stated that,

> In 1961, it took a crisis -- the space race -- to speed things up. Today we do not have a crisis. We have an opportunity. To seize this opportunity, I am not proposing a 10-year plan like Apollo. I am proposing a long-range, continuing commitment. First, for the coming decade -- for the 1990's -- Space Station Freedom – our critical next step in all our space endeavors. And next -- for the new century -- back to the future. And this time to stay. And then -- a journey into tomorrow -- a journey to another planet -- a manned mission to Mars.

---

[2]Text from remarks by the president in the Texas A & I University commencement address, May 11, 1990.

On February 2, 1990 President Bush made the following statement at the University of Tennessee:

> Our goal: To place Americans on Mars -- and to do it within the working lifetimes of scientists and engineers who will be recruited for the effort today. And just as Jefferson sent Lewis and Clark to open our continent, our commitment to the moon/Mars initiative will open the universe. It's the opportunity of a lifetime -- and offers a lifetime of opportunity."

On February 16, 1990 President Bush approved the Space Exploration Initiative (SEI). The initiative includes both lunar and Mars program elements, as well as robotic missions. In the near term the focus will be on technology development, that is, to search for new and innovative approaches and technology, to invest in high leverage innovative technologies with potential to make a major impact on cost, schedule, and/or performance, and to develop the technology in parallel with mission, concept, and system analyses. To advance spacecraft designs for future missions beyond the currently conceived energy/time constrained vehicle systems will require more concentrated energy sources for spacecraft propulsion.

To implement this new propulsion energy capability, NASA should possess internally the technical research ability and should have the facilities that provide the capability to conduct space fusion research. The program should provide an internal fusion research expertise and in particular, the capability to quickly seize upon new concepts that may have a space related benefit. NASA, as in the chemical propulsion development programs in the past, will be assisted by the aerospace industry in the flight vehicle developmental process. The ability to conduct space fusion research, without having that research tied to non-space related objectives, is fundamental to NASA being in a position of controlling its technological future and accomplishing its mission. The "U.S. National Space Policy" was approved by President Bush in 1989. In the "Civil Space Sector Guidelines" section of the "Policy Guidelines and Implementing Actions" the policy states (pp 5,6) that:

> -- Introduction. In conjunction with other agencies: NASA will continue the lead role within the Federal Government for advancing space science, exploration, and appropriate applications through the conduct of activities for research, technology, development and related operations; National Oceanic and Atmospheric Administration will gather data, conduct

research, and make predictions about the Earth's environment; DOT will license and promote commercial launch operations which support civil sector operations.

-- Human Exploration. To implement the long-range goal of expanding human presence and activity beyond Earth orbit into the solar system, NASA will continue the systematic development of technologies necessary to enable and support a range of future manned missions. This technology program (Pathfinder) will be oriented toward a Presidential decision on a focused program of manned exploration of the solar system.

-- Unmanned Exploration. NASA will continue to pursue a program of unmanned exploration where such exploration can most efficiently and effectively satisfy national space objectives by among other things: achieving scientific objectives where human presence is undesirable or unnecessary; exploring realms where the risks or costs of life support are unacceptable; and providing data vital to support future manned missions.

The capability of NASA to conduct fusion research is consistent with the aforementioned policy statements on developing the necessary enabling technologies. The capability includes possessing both the analytical and experimental expertise. That is, of course, no different in having the internal capability for the use of chemical energy conversion for space propulsion and power, or an aeronautical propulsion capability for aircraft. NASA has had the charter since the original National Space Act was approved to research faster and safer propulsion. Management control of its research program ensures a proper focus on space related goals, ones which will be emphasized by a NASA program to be responsive to NASA's program needs. Where NASA manages the program internally, a streamlined, less complex management system results. It, too, is preferred to have the management controls and therefore the assurances that the program is optimized to meet space purposes. However, the implementation of a successful fusion program requires long term strategical planning and budgeting. The bottom line is, it requires NASA's full commitment.

Granting then the authority of NASA to proceed, what is the point of departure for NASA to pursue the technology? Since the DOE terrestrial fusion program's initiation, the DOE has made great strides in advancing toward the demonstration of scientific breakeven, defined as the point where the plasma is producing as much energy as it consumes. Refer to Fig. 9 for some perspective of the progress made.
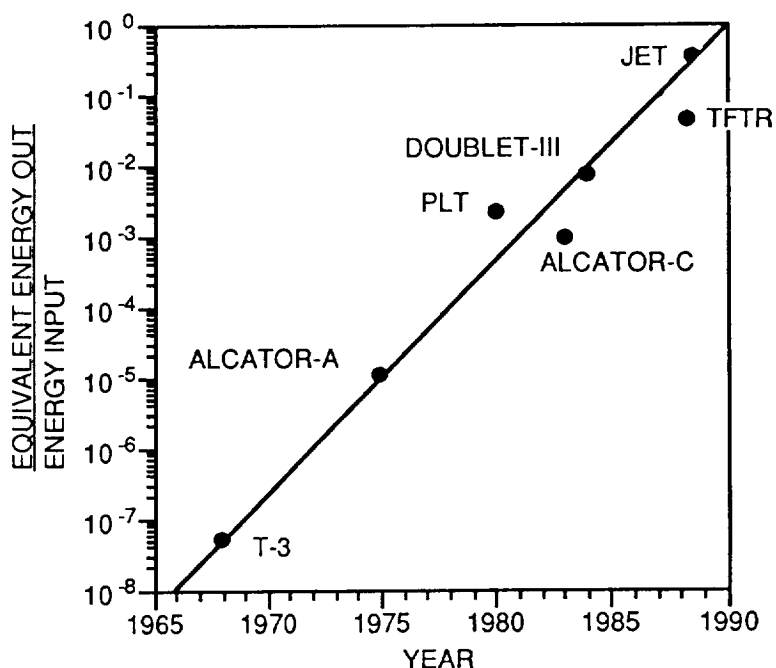
Fig. 9. Progress in magnetic fusion research. (San88, updated)

The demonstration of energy breakeven will be followed by a vigorous technology program aimed at the development of a test tokamak reactor. The progress being made in the terrestrial program, the availability of helium-3 from the moon (Wit86), the economic advantages to the space program, the safety improvements, the length of time required for fusion energy development, the mission durations, and the new NASA space program initiatives all converge beautifully to make this a proper time to implement a program for the use of fusion energy for space. There are tremendous economic advantages for doing this research. If space fusion were available today, NASA – in the launch of just one manned flight to Mars – could save over $8 to 9B, using the current Shuttle launch costs and its performance as a reference. The fusion development program will more than pay for itself.

The text above presents the mission performance advantages and the technical requirement considerations – high specific power, high specific impulse, long durations, etc. But we need to demonstrate capabilities by testing and to better understand fusion powered flight systems by relevant system analyses which are based upon test data. That technological development of space fusion energy reactors and the conversion of fusion energy into space fusion engines and electrical power generators will require a significant investment of resources and time. But the economic yield from the research investment is enormous. This is high risk - very high gain technology.

31

The chance that fusion can be provided for space

What is that chance without the dedicated space fusion research? In an energy research program directed toward the application by utility companies which operate in a profit motive market, one where the lowest cost of energy available dictates the strategy and research program developmental priority, the unique requirements for space science and exploration applications naturally fall neglected. Reflective of that conclusion is the fact that the NASA Lewis Research Center fusion research program, was very much concerned with the importance of reducing mass to a minimum and reducing space flight hazards. Consequently, NASA was a leader in exploring the use of D-^{3}He fusion and employing superconducting magnets for fusion. NASA also selected a unique configuration, the bumpy torus for propulsion applications. Indeed as might be anticipated, when the NASA program stopped, the D-^{3}He approach was not pursued by programs having different goals.

The 1990 level of funding for the terrestrial program is not high by space standards, approximately $325M, and has being decreasing. Funding for the entire fusion program, summed since the beginning, has been much less than just the Apollo Program. The key experiment of interest for space application is funded at an annual level of only $5M. When viewed in relation to the gross national product that is roughly the equivalent of Dr. Goddard's request in 1916 for $0.005M to perform chemical rocket propulsion work. And that low funding level for fusion is to develop a much greater and technically more challenging task. By comparison it is estimated that NASA spends approximately $500M annually on chemical propulsion flight systems and technology. That funding is used almost entirely to maintain chemical energy propulsion systems, i.e., to maintain the technological status quo.

Fortunately, there is an option. Some attractive fusion reactor design concepts can be considered for space: the FRC, other compact toroid magnetic confinement concepts – tandem mirror and spheromak and inertial confinement concepts. Other confinement approaches are the dipole and electrostatic approaches. NASA Lewis developed a magnetoelectric confinement approach (Sch91), but it was funded at a grossly inadequate level. Fusion, in general, has not been funded at a level commensurate with its benefit.

The interest over the FRC reactor stems from the high specific power which it is believed capable of delivering. Other design options are included in Sch91. The key is to use the leverage made available by the DOE program in order to avoid costly and time consuming repeats of past work. We are now able to leverage those resource expenditures by focusing upon fusion energy technology applications to space for incorporation into the space transportation infrastructure. A cadre of individuals in fusion technology, comprised of experts from the universities, national laboratories, and industry, exists. A team needs to be assembled and a program implemented.

The question that needs to be answered is, "Can we make fusion work to suit the needs of a space flight operational vehicle system and have it continue to

perform reliably?" Testing is of paramount importance towards the achievement of the objective of providing space fusion viability. We know that space fusion reactors, exhibiting performance characteristics as defined herein, would provide mission and performance advantages. Studies and evaluations cannot provide the assurance needed. They can only conjecture and conduct "what if" evaluations. **Instead, the strategy must assure that experimental demonstrations and results are attained by testing.** Consequently, it is necessary to stress fusion experiments, testing, and theory with the objective of focusing valuable resources on hardware and less on paper studies. That is the focus of resources which will yield the greatest dividends. Only then can the critical technical questions be properly addressed.

As the first critical step in the development of a strategy we must address the fundamental fusion experimental objective of demonstrating the burning of deuterium and helium-3 in a reactor design having a propulsion system specific power capability commensurate with space requirements (1 kW/kg to 10 kW/kg). In addition, the fusion powered vehicle must also meet all of the system criteria associated with space flight. Reliance upon only the resolution of plasma physics issue is not enough for space applications. Beyond the plasma physics issues, a space fusion system has to be practical from both a space design and operational view point if we are to use it. Therefore, the complete fusion vehicle system aspects must be given significant attention.

The space system level requirements reveal that some very difficult tasks lay ahead, in fact, many whose solutions will not be researched because space tasks are not the charter of the terrestrial fusion program. One of the most difficult requirements is the requirement to provide a space reactor restart capability. The other concern is the capability to deliver the specific power performance that is needed. If there is an unexplored challenge with the use of fusion for space, other than demonstration of the basic confinement and specific power technology, then that is the most significant one. And it has not received attention.

The principal motivating force behind fusion for space, other than the major mission enabling propulsion and power performance, is the mission and system safety advantages that it provides over other advanced energy sources. If, for example, a fission reactor is required to start the fusion reactor, a significant system safety advantage is lost. That points to another example of the importance for NASA to undertake a space fusion program. The original National Aeronautics and Space Act of 1958 states that:

> (c) The aeronautical and space activities of the United States shall be conducted so as to contribute materially to one or more of the following objectives: ... (2) The improvement of the usefulness, performance, speed, safety, and efficiency of aeronautical and space vehicles; ... (Anom58, p. 1)

The "U.S. National Space Policy" revision states that

> ... NASA will continue the systematic development of technologies necessary to enable and support a range of future manned missions. (Anom89, p. 6)

The terrestrial program must focus upon economical and environmentally clean electrical power generation. The space program has an entirely different mission focus. Two other major tasks which the terrestrial program will not likely address include a reactor system design for minimal (or no) maintenance for operations in the space environment and the ultra high reliability required for very extended mission durations. There is no need for the terrestrial program, nor funding available, to examine these key space related topics. As discussed, some fusion reactors could be required to endure a trip lasting for over a century, up to three centuries, and perhaps longer, as required for reactors in a mission to the nearest star, for example. That performance level must occur without external maintenance of the flight system. On the ground, clearly there is an advantage to design the system for maintenance if total system design and operational costs can be lowered.

Recommended NASA strategy

The objective is to enhance NASA's space science, exploration, safety, and mission success posture. The successful implementation of a fusion energy space technology research program appears to offer the best approach for meeting that objective. The NASA fusion energy program will accomplish the objective by providing a new high performance propulsion and power technology that is destined to become part of the space flight system infrastructure in the 21st century. It is important to incorporate that program into the overall planning of NASA's science, exploration, research, and technology programs now under way.

The fundamental strategical premise forwarded is this:

> If the advantages of fusion energy are to be realized for space, it will be incumbent upon NASA to initiate its space fusion program without relying upon the terrestrial program.

There are two fundamental factors to be considered:

1. Mission, policy, program priority differences
2. Technical requirements differences.

The <u>first</u>, **mission differences**, establishes policy and relative priority variances between the two agencies. The current strategy to develop space fusion energy is to rely upon the research pertaining to the terrestrial fusion energy program. Thus, the space fusion research status, unfortunately, under the present strategy has now become hostage to the overall economics of global energy costs and to the global energy supply and demand situation. The program for the terrestrial fusion research as applied to commercial utility use will be funded and pursued in a commercial electrical energy responsive mode, i.e., one designed to meet global economical competitiveness with oil, coal, fission, natural gas, and solar energies. "Low" prices for oil and coal used to produce electrical power eliminate any incentive to fund fusion at a rapid level. Fusion energy's anticipated cost of electricity is uneconomical by comparison with that of its competitors. While that may apply to the United States' commercial electrical power production position, the free market costs of energy may be less of a determining factor for other more energy dependent, or importing, countries, particularly Japan where fusion development and the lunar mining of helium-3 are believed to receive a high priority. With space, the opposite applies – it is anticipated to effect great economies today if space fusion propulsion were available.

As a consequence, the situation currently is that all of the potential space program's economic benefits stemming from fusion propulsion are being determined [and being delayed] by the non-space related global energy economies. This important point is underscored by the DOE decision to conclude alternate experiments during FY 1991 to concentrate on the tokamak while accommodating a reduced budget. These are approaches that NASA could use. The tokamak is too massive and cannot burn the space fuel of preference, D-^{3}He, according to the ARIES III design study completed in 1991.

The space program could benefit immediately because of fusion energy's superior competitiveness. NASA could effect economies by many billions of dollars as a consequence of reduced operational demands, whereas a commercial fusion powered generator plant would sit idle, awaiting a rise in the cost of oil to make it economically compatible.

Consider the Manned Mars Mission. At the time of the initiation of this study, the most thorough hardware systems study conducted for manned Mars exploration is the one made by North American Aviation entitled, the "Mars Excursion Module." In it, a manned Martian lander spacecraft was designed to deliver a smaller payload than the one evaluated in this study. That single mission using chemical propulsion and aerodynamic braking will require the launch energy equivalent of 37 Shuttle launches to place the chemical propulsion powered

35

Martian vehicle's initial mass into low Earth orbit. At the launch rate of 6 per year (current) 7 years are required to place the Martian vehicle mass into low Earth orbit. The trip time is 300-400% greater than for a fusion powered vehicle. A specific power of 1 kW/kg requires only five Shuttle launches to transport a more massive spacecraft to Mars. Greater lift launch vehicles than the Shuttle can, of course, be developed to reduce that number, but that does not alter the physics of the flight energy requirements. Launch costs scale with the physics.

The prudent approach is to reduce the requirements placed on the mass to low Earth orbit and not in the construction of larger launch vehicles as the solution. Furthermore, any launch benefits to be gained from a larger lift capability would also correspondingly benefit the fusion system. But the availability of space fusion vehicles makes the Shuttle transportation launch class adequate, eliminating the need to develop a new, more massive lift logistics space transportation system. Other requirements for heavy lift launch vehicles may exist. The fusion propulsion vehicle is assumed to be designed to have the capability for placement into orbit and to remain there permanently as space-based equipment. Thus, the launch of that mass occurs only once allowing for reuse in space. The Shuttle in this scenario, then, is used only for logistics to transport fusion propellants to LEO.

The advantages of fusion energy are truly far reaching. A specific power of 1 kW/kg propulsion system will consume a quantity of propellant, which is defined as fuels plus diluent, that could be delivered by only five Shuttle launches. That contrasts with the 37 launches required by chemical systems of the Shuttle performance level in order to send a 133 MT payload to Mars and to return 61 MT. Fusion will, therefore, return to Earth a much larger mass of Martian soil samples than the chemical system – a major mission objective of the initial Mars flight. The chemical energy propulsion system takes at least a half year longer in terms of flight time, adding to the flight operational costs and increasing very significantly the crew's exposure to hazardous radiation. Using today's costs, that performance difference between a chemical propulsion system and a 1 kW/kg vehicle is at least a conservative $8.5B savings just in the launch operational costs. And that is to perform a lesser science exploration mission. Just consider the savings for logistics to support a permanent presence of man on Mars where it is not unreasonable to expect 2-4 flights per year.

That savings, an achievement made possible by the high specific power systems, is a very important, strong motivational incentive for NASA to significantly bolster its investment in advanced high specific power performance propulsion, regardless of the nature of the energy source. By reducing the number of chemical propulsion launches, flight operational costs for the Manned Mars Missions are drastically reduced. Mission safety is enhanced by accomplishment of the same mission in less flight time. Exposure of the flight crew to cosmic rays using the slower chemical system is nearly quadruple that of the higher performance vehicle. Launch _operational safety_ is aided by the _reduction_ of the energy equivalent of 32 Shuttle launches _per launch_ of one spacecraft to Mars. It should be clearly stated that in no way does this report

suggest that fusion will be available in time for the first Manned Mars Mission, assuming the mission to take place soon after the turn of the century. But it does state that if it were available today, the economies and overall safety would be overwhelmingly for NASA's benefit. If the mission occurs in 2-3 decades, then perhaps fusion powered engines may become an option, under the right set of circumstances.

Thus, if NASA defers space fusion developments pending terrestrial demonstrations, the opportunities for significant cost savings are eliminated; the conduct of exciting new space missions are forfeited; and the safety advantages, which result from the shorter flight times, are lost. The cost leverage principle of investing some of the future operational costs to launch payloads into space favors the initiation of space fusion research today. The investment offers NASA a reasonable risk versus gain opportunity. At stake ultimately in a space program involving settlement on Mars are 100's of billions of dollars, if not even the very concept. That is such a large number that fusion must be considered to be the primary Mars mission enabling technology.

With regard to the second rationale for NASA initiating a space fusion program, the **space technical requirements differ significantly** from those required to produce competitive commercial electrical power on Earth. For example, to meet NASA's missions requiring propulsion, the laws of physics demand light weight vehicle designs for space operations, and hence, the use of compact reactor designs with minimum neutron output where radiation cooling is mandated. Earth based electrical power generators are not mass critical unlike the the space application.

No neutron flux production from the reaction is optimal. But lacking that, we desire both high fuel reactivity and the minimal, practical neutron flux as the space program fuels of preference. That is deuterium and helium-3. Reactors designed to burn D-^{3}He are more difficult to ignite, but the engineering demonstration of a net energy gain is simplified in comparison with the terrestrial program's preferred fuel cycle, deuterium and tritium. Thermal conversion electrical power systems, where the experience is great, may be the preferred approach for terrestrial applications with good rationale. But NASA programs have a requirement for a flight propulsion system capability and a reactor design that will serve a dual function of electrical power generation and propulsion. NASA's mission power requirements can vary over a wider range encompassing many orders of magnitude from tens of megawatts to gigawatts, depending upon the specific power attained and the initial mass of the vehicle. The space operational environment of vacuum and zero gravity may provide the spacecraft designers with desirable design options not available to the terrestrial power plant designer. The commercial power plant designer in turn has design options which are not available to the spacecraft designer, particularly in maintenance, cooling, and greater operational flexibility, i.e., other power generators on the grid can serve as back-up options if the fusion system is shut down for any reason. In space there is no such back-up option.

When considering a NASA space fusion research program to address the fundamental space issues, one should recognize, too, that this will not be a "first" since fusion energy research for space applications was pursued at the Lewis Research Center (Appendix A). The significance of a NASA space fusion program is illustrated by the fact that in the pursuit of a flight system the NASA researchers were the first to make use of superconducting magnets. The NASA fusion program made other significant contributions to the technology. It was terminated in 1978 as a result of the extensive cost cutting measures occurring in NASA during that era. A program and technical bibliography are presented in *Fusion Technology*, January 1991, which is provided as Appendix A for ready reference. The article, produced as a result of this study review, provides details on the results of the key NASA researchers and their main contributions.

Mission and operational differences between agencies will result in different design approaches to the fundamental reactor configuration and its development path. What is not generally realized outside of the fusion community is that the resolution of the physics in one reactor design will not necessarily resolve the physics problems in another. Each reactor design has a unique set of physics issues to resolve as well as differing system problems and solutions. Lightweight propulsion reactor designs for space flight will differ from those whose purpose is to produce commercial power for a profit. Therefore, the resolution of physics issues for the terrestrial program will probably not be directly applicable to a space reactor, at least not without extensive additional research. The philosophy here is to use the right tool for the right job. So, why not commence the proper research on the proper experiments for space now? As a good example, just consider the mainline fusion's program key experiment, the tokamak. Because of its specific power limitations, we could not use that design for spacecraft flight reactor applications, although it might possibly be considered for powering laser driven spacecraft from a ground base station as discussed in depth in Appendix B.

RECOMMENDATIONS

SPACE FUSION ENERGY FOR PROPULSION AND POWER

The recommendation is to commence a NASA space fusion program.

❏...THE FIRST STEP

> Recognize the importance of fusion and fund it independently of the utility power application. As the first priority, design and build an FRC to produce net power and commence testing recognizing the need to first address stability concerns.

An extended program plan containing fusion energy conversion options should be prepared by NASA following the collective deliberations of a Space Fusion Energy Workshop comprised of space-oriented fusion experts. The task is to pull together a modest experimental test program for *space reactor designs.* The Fusion Workshop could address the key technical and program technology topics in greater depth. Options will be the key to success because it is not clear that the FRC or any other approach will be able to surmount the obstacles.

To proceed with the development of fusion energy for space flight, NASA should fund space fusion experiments at a level commensurate with its payoff potential. An annual research investment of $150M (FY90 dollars-direct research) is considered to be a minimal level which could provide dividends on a reasonable time scale.

A complete, detailed MCF space reactor design study of options is recommended. Current studies have not addressed an MCF design. Such studies may assist in evaluating potential propulsion system specific powers options in greater depth. It is recommended, however, that plasma confinement testing and experiments be given the highest funding priority now as a new start.

The Field Reversed Configuration (FRC) with its inherently high β plasma density is clearly one good potential concept. But it has stability concerns. It lacks priority in its developmental pace in comparison with the tokamak. Work on it is being terminated in FY 91 which confirms the thesis that NASA should pursue a space fission program. That experiment is sufficiently key as a high β device that work on it should proceed at an accelerated schedule without

39

awaiting the Fusion Workshop's results. The FRC should be designed and tested immediately to net power production using D-^{3}He as fuel. The testing should investigate FRC plasma transport and stability phenomenon without delay by proceeding to net power production (>40 keV plasma temperature) using D-^{3}He since that level in the end appears to determine the final answer. The design should include neutral beam injection to raise the plasma temperature and to maintain plasma stability. Another approach is to quickly heat the fuel to the ignition temperature by rapidly compressing the plasma with a rapid ramping of the plasma current and increasing the magnetic field. This experiment does not require advanced technology magnets.

The Fusion Workshop would assist, too, in defining a thorough, focused specific FRC program plan. Because it is too early to decide upon the best overall space reactor approach to the exclusion of all others, the Fusion Workshop should recommend a program for alternate confinement approaches. Use of the existing National Laboratory, university, and fusion research industry personnel would provide an excellent team that could commence research on the new technology – an essential resource to NASA, which it should use directly. Substantial benefit would be derived from the use of their background, expertise, ingenuity, and facilities. That approach reflects the resource leveraging principle alluded to earlier. This team, linked with the aerospace industry for flight system analyses, would provide an excellent energy-flight integration team to complete the work for development of a high energy capability for space missions.

The space fusion program should target for the Manned Mars Mission as a matter of first priority. That application provides the highest value of investment return to NASA in terms of performance, economics, and safety. That mission defines a specific impulse performance requirement from a fusion rocket engine of 5,000 seconds to 13,000 seconds, a jet power of 100-150 MW, specific power of 1 kW/kg, average thrust of 2,500 N, and a total mission steady state firing duration of 0.3 year in two 0.15 year durations. The vehicle size is approximately 610 MT, containing 330 MT of propellants. It imparts a Δv of 90 km/s, and has a payload mass fraction of ~22%. For assisting NASA in the advancement of its space science programs, a manned flight vehicle system can be scaled for the conduct of science missions using the technology developed for the manned mission. As an option-nuclear electric propulsion is not a mission enhancing energy system for the Manned Mars Mission of the flight vehicle mass, velocities, and power levels as envisioned by this study.

High Energy Space Missions

Fusion energy is an enabling technology for high energy missions such as manned Mars, sample return science missions, interstellar physics, and stellar flights. Where energy requirements are less, such as the small velocity

changes typically required for LEO, other propulsion systems typically have the advantage. Exceptions would be for very large LEO plane changes and for high power consumption missions in the tens of megawatt range.

A Scientific and Exploration High Energy Space Mission Workshop is recommended to explore the potential for new space science opportunities which can be made possible by high energy missions. The Mission Workshop participants should represent a wide scientific interests, not exclusively those of space scientists. The workshop will be of value in further defining mission requirements. NASA should initiate, at a modest funding level, a stellar science mission planning activity now and effect the economies of savings by having a well thought-out program of an advanced research nature such as this would entail. International participation should be encouraged. Concepts for scientific outposts, for more science intense payloads, and for greater science gains through mission efficiency via shorter flight times, should be encouraged.

Concluding Remarks and Thoughts

This study has shown fusion's energy capabilities are such that research into space fusion energy conversion should be pursued by NASA, particularly in the nature of an experimental test program. In the aftermath of the tragic Challenger accident which prevented a very large percentage of key spacecraft from being launched, one theorem is particularly reinforced: we should strive with every resource at hand to avoid critical single point failure systems. The current approach considers only fission as an option to chemical propulsion since it has received some testing. Looking beyond chemical propulsion systems, NASA has no advanced energy option to fission – a critical single failure point in NASA's advanced planning. We have observed what has happened to nuclear fission power. After numerous plants were built, public opinion turned, and new plants have not been constructed for many years. Newly built ones are not being turned-on. If the public attitude were to turn more strongly against the use of large fission reactors for either commercial power or for space applications, including the transport to and operations within Earth orbit, an alternative high specific energy source is not available to NASA. If space science and exploration are to continue, a back-up space energy source option is mandatory in order to carry out those large power consuming space missions. Fusion can not only potentially provide that energy option for NASA ... but much, much more.

The development of an internal NASA fusion capability is an essential part of the space infrastructure in the fulfillment of its missions. National space policy requests it. Fusion energy infrastructure for propulsion and power should be the goal just as we have developed an in-house capability for the use of chemical energy for propulsion and power. NASA cannot use the D-T fueled tokamak upon which the terrestrial fusion has totally focused, to the exclusion of

41

all alternate confinement experiments in 1991. It should also be noted that this separate approach offers the advantage that confinement options to the terrestrial fusion research are provided although space fusion reactors are not necessarily anticipated to be the same as terrestrial reactors.

References

Anom58   "National Aeronautics and Space Act of 1958," GPO:19580-29011 (416) Washington, D.C. (July 29, 1958)

Anom89   "US National Space Policy, White House," Washington (November 2, 1989)

Cha89   Chapman, R., Miley, G., Kernbichler, W., and Heinder, M., "Space Fusion Propulsion, with a Field Reversed Configuration," *Fusion Technology*, **15**, 1154-1159 (1989)

Fle86   Fletcher, J.C., "NASA's Visions and Goals Statement," Memorandum to All NASA Employees (December 9, 1986)

Fpa90   "Executive Newsletter," Fusion Power Associates, Gaithersburg, MD (December 1990)

San88   Santarius, J.F., et al, "Critical Issues for SOAR: The Space Orbiting Advanced Fusion Power Reactor," University of Wisconsin, UWFDM-753 (August-1988)

Sch91   Schulze, N. R., "Fusion Energy for Space Missions in the Twenty First Century", TM-4298, NASA Headquarters, Washington (1991)

Sch91   Schulze, N.R. and Roth, J.R., "The NASA-Lewis Program on Fusion Energy for Space Power and Propulsion 1958-1978," *Fusion Technology*, **19**, 1, pp 11-28 (January 1991)

Tel91   Teller, E.; Glass, A.J.; Fowler, T.K.; Hasagawa, A., and Santanus, J.F. "Space Propulsion by Fusion in Magetic Dipole," prepared for First International A.D. Sahharov Conference on Physics, Moscow, USSR; May 27-31, 1991 and "Fusion Technology," April 12, 1991, LLNL report UCRL-JC-106807.

Tus91   Tuszewski, M., Barnes, D.C., Chrien, J.W., Rej, D.J., Siemon, R.E., Taggart, D.P., and Wright, B.L., "Observations of Tilt Stabilities in Field-Reversed Configurations of a Confined Plasma," Physical Review Letters, **66**, 6, 711, (February 11, 1991)

Wit86   Wittenberg, L., Santarius, J., and Kulcinglui, J, *Fusion Technology*, **10**, 167 (1986)

REPORT DOCUMENTATION PAGE

Public reporting burden for this collection of information is estimated to average 1 hour per response, including the time for reviewing instructions, searching existing data sources, gathering and maintaining the data needed, and completing and reviewing the collection of information. Send comments regarding this burden estimate or any other aspect of this collection of information, including suggestions for reducing this burden, to Washington Headquarters Services, Directorate for Information Operations and Reports, 1215 Jefferson Davis Highway, Suite 1204, Arlington, VA 22202-4302, and to the Office of Management and Budget, Paperwork Reduction Project (0704-0188), Washington, DC 20503

1. AGENCY USE ONLY (Leave blank)	2. REPORT DATE August 1991	3. REPORT TYPE AND DATES COVERED Technical Memorandum

4. TITLE AND SUBTITLE
Fusion Energy for Space Missions in the 21st Century —
Executive Summary

5. FUNDING NUMBERS

6. AUTHOR(S)

Norman R. Schulze

7. PERFORMING ORGANIZATION NAME(S) AND ADDRESS(ES)

NASA Office of Safety and Mission Quality
Technical Standards Division
Washington, DC 20546

8. PERFORMING ORGANIZATION REPORT NUMBER

9. SPONSORING / MONITORING AGENCY NAME(S) AND ADDRESS(ES)

NASA
Washington, DC 20546

10. SPONSORING / MONITORING AGENCY REPORT NUMBER

NASA TM-4297

11. SUPPLEMENTARY NOTES

12a. DISTRIBUTION / AVAILABILITY STATEMENT

Unclassified, Unlimited
Subject Category 73

12b. DISTRIBUTION CODE

13. ABSTRACT (Maximum 200 words)
Future space missions were hypothesized and analyzed and the energy source for their accomplishment investigated. The missions included manned Mars, scientific outposts to and robotic sample return missions from the outer planets and asteroids, as well as fly-by and rendezvous missions with the Oort Cloud and the nearest star, Alpha Centauri. Space system parametric requirements and operational features were established. The energy means for accomplishing missions where Δv requirements range from 90 km/sec to 30,000 km/sec (High Energy Space Mission) were investigated. The need to develop a power space of this magnitude is a key issue to address if the U.S. civil space program is to continue to advance as mandated by the National Space Policy. Potential energy options which could provide the propulsion and electrical power system and operational requirements were reviewed and evaluated. Fusion energy was considered to be the preferred option and was analyzed in depth. Candidate fusion fuels were evaluated based upon the energy output and neutron flux. Additionally, fusion energy can offer significant safety, environmental, economic, and operational advantages. Reactors exhibiting a highly efficient use of magnetic fields for space use while at the same time offering efficient coupling to an exhaust propellant or to a direct energy convertor for efficient electrical production were examined. Near term approaches were identified. A strategy that will produce fusion powered vehicles as part of the space transportation infrastructure was developed. Space program resources must be directed toward this issue as a matter of the top policy priority.

14. SUBJECT TERMS
Fusion energy
Propulsion power
Advance power space Exploration

15. NUMBER OF PAGES
56

16. PRICE CODE
A04

17. SECURITY CLASSIFICATION OF REPORT	18. SECURITY CLASSIFICATION OF THIS PAGE	19. SECURITY CLASSIFICATION OF ABSTRACT	20. LIMITATION OF ABSTRACT
Unclassified	Unclassified	Unclassified	UL