NASA-TM-109227

IN-75-TM
179803

FUSION ENERGY FOR SPACE: FEASIBILITY DEMONSTRATION

– A PROPOSAL TO NASA –

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OCTOBER 1992

(NASA-TM-109227) FUSION ENERGY FOR SPACE: FEASIBILITY DEMONSTRATION. A PROPOSAL TO NASA (NASA) 50 p
N93-31309
Unclas
G3/75 0179803

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REFERENCES FOR PROPOSAL ENDORSEMENT:

A list of individuals who have reviewed this document is provided at the end of this proposal. They have agreed to the contents, at least within their expertise and area of responsibility.

PROPOSAL CONTENT

This proposal has been prepared to assist in the implementation of the NASA space mission. The contents are considered to be accurate as discussed in the text.

Prepared by:

Norman R. Schulze

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October 6, 1992

Date

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FUSION ENERGY FOR SPACE: FEASIBILITY DEMONSTRATION

PROLOGUE

The ultimate goals of the United States space program are to understand the universe and to colonize space. Whether or not those goals are met depends to a great extent upon NASA possessing the requisite propulsion systems that will provide a safe, high energy mission capability. The attainment of that capability resides with fusion propulsion. Development of fusion propulsion is, thus, mandatory for the advancement of space science and exploration beyond its current bounds of low performance propulsion systems. Time is of the essence since the need is immediate, whereas the development and flight qualification time for a fusion powered propulsion system is considered to be on the order of 30 years to 40 years, and then only provided that a major space fusion propulsion system program is undertaken. This technology is of such importance that space fusion propulsion system research should be assigned a priority no less than among that of the top programs. This is the top research technological issue facing NASA because the future, or fate, of the United States Space Program will ultimately hinge upon its availability. Fusion energy will determine whether the United States remains a power in space exploration or whether it relinquishes the space exploration role to another nation as the technologically advanced Chinese civilization abandoned their seafaring exploration role to the Europeans in the 15th century.

1.0 INTRODUCTION

Understanding, exploration, and exploitation of space is currently being given serious reevaluation by the NASA staff at the direction of the Administrator under the Space Council's guidance for a new vision - a new order - in space. This new space vision mandates a "gain" for space, that is, we wish to advance beyond our current bounds. How can this new vision be accomplished?

To accomplish "gain" in this, as in any endeavor, a new way of thinking must be instituted. The thought process must be changed, for we cannot advance to meet the challenges of new visions if we operate at the old level of thought. Change involves risk. But we sometimes forget, not to change also involves risk, the consequences of which may be worse than the perceived fear of the unknown from change.

Critical to the implementation of NASA's space vision for the 21st century - and beyond - is the possession of the energy permitting this new vision mission capability. Space ships which have a high energy performance capability, ones capable of velocity changes from 100 km/sec to over 20,000 km/sec, will enable efficient human and robotic flights to all orbiting masses within the solar system – also, robotic missions to the stars. In contrast with today's vision, this new vision demands enormous increases in energy for space travel. Now we are starting to face power levels in the 10's of megawatts to gigawatts range – and beyond. Clearly a new vision for space

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mandates a new strategic propulsion approach to prepare for the 21^{st} century space program.

The energy sources considered <u>currently</u> available to provide power for space missions are very limited: chemical and fission. Conversion of fission energy into thrust is accomplished by either thermal conversion systems or by electrical conversion systems employing ion propulsion. Fission thermal propulsion has been ground-demonstrated as of several decades ago on the NERVA Program. The electrical conversion technology being developed today is in the 100 Kwatt range from a space fission reactor. We have demonstrated the feasibility of an ion engine in space. Concurrently underway, also, is a reexamination of heavy lift launch vehicles needed to provide the transport of very large masses into low Earth orbit (LEO) to accommodate the launch of space chemical propulsion systems and their requisite large propellant mass.

This current approach permits a very limited vision for a new space order. With a high energy capability, as discussed later, we broaden our sights to:

- enable new missions well beyond our current vision
- provide new levels of safety
- permit economical space flight
- commercialize space.

Let us envision a space program with the capability to send man to all of the planets, to deposit science outposts on all planets within a flight time of several years, to conduct sample return missions that permit the safe exploration of all solar system bodies, to send robotic missions to the Oort Cloud region and out to the nearest stars, and to provide for human settlement beyond Earth.

The new thinking which is being proposed herein to accomplish this new vision is simple:

Reduce the requirements for mission mass to be placed into low Earth orbit.

Refrain from trying to make the physics of current energy systems do work which it technically cannot perform with the economic, mission enabling, and safety benefits that high energy propulsion systems can provide. Instead, develop the technology that will place the Mars human exploration mission energy requirements into low Earth orbit by using, for example, just one Shuttle launch, not a large number such as ~40, and accomplish the mission in a fourth of the time while launching a more massive payload and providing a safer operational mission capability.

The approach, as presented herein, shows the path by which NASA can determine the viability of a high energy space flight capability for the achievement of this vision and how to obtain this exciting new operational capability. The necessary technical-managerial steps which reduce the requirements for mission propulsion energy mass placed into low Earth orbit (LEO) are critical to understand and to implement. Enclosed herein, then, in keeping with the spirit of change for space advancement, is the means for developing and implementing a new propulsion strategy consistent with achieving the desired new vision for space.

Highly efficient, high power space energy systems are essential to the future of space missions operating beyond the Earth. This is a subject at the heart of whether or not the United States will continue to play a major role in space. To make the vision become a reality this proposal is offered.

2.0 PROPOSAL

Space Fusion Feasibility Demonstration

Provided herein is a proposal for NASA to undertake a research program having the objective of providing a capability that is critical to the safe and successful application of space for the benefit of mankind.

This proposed program is to initiate a space flight research and development program to develop fusion energy for the space applications of direct space propulsion and direct space power, that is, a Space Fusion Energy (SFE) program.

"Direct propulsion" refers to the use of plasma energy directly for thrust without requiring other energy conversion systems. Further, to provide space missions with large electrical power, "direct space power" is proposed whereby the direct conversion of charged particles into electricity is used, thereby avoiding thermal conversion system losses. The energy release from nuclear fusion reactions makes these highly efficient, high power space systems possible.

Feasibility: The program as presented in this proposal conducts in an orderly, hierarchical manner the necessary planning, analyses and testing to demonstrate the practical use of fusion energy for space. There is nothing discussed that is known to be theoretically impossible. Validation of the engineering principles is sought in this program which uses a cost-benefit approach.

Benefits: Upon successful program completion, space will become more accessible and space missions more safely conducted. The country will have taken a giant step toward the commercialization of space. The mission enabling capability provided by fusion energy is well beyond mission planners' current dreams.

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The concept proposed is in full accord with the Administrator's charge to NASA employees to perform new thinking to produce innovative methods, including new technologies, that will enhance safety and reduce the nation's costs in performing space missions. By performing the work as described in this proposal, both charges issued by the Administrator are addressed. In the long term, this program will determine whether space operations beyond LEO can, in reality, be conducted better, faster, and cheaper to any significant degree.

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3.0 THE SOLUTION

High specific power and variable, high specific impulse space propulsion systems are two key parameters which make space more accessible and space missions safer.

The use of advanced high specific power (α_p) propulsion systems contrasts with the currently planned brute force approach of launching large payload mass to LEO. Space flight vehicle performance leverage is essential for the human exploration of Mars in order to reduce mission costs and to provide for safety. "Leverage" here refers to the gain achieved by the use of high performance space systems over the brute force approach of sending greater mass to LEO to accomplish the same or lesser missions. To develop space, NASA must undertake the development of technology which decreases the LEO mass requirements in lieu of delivering more mass to LEO.

The attributes of high energy propulsions become more lucid as we consider the penalties and the risks associated with low energy systems. We address those attributes in the discussion below.

First, the performance penalty resulting from the use of low performance propulsion systems is made quite clear by considering the chemical propulsion system launch vehicle's mass fraction – the payload mass to gross vehicle mass. For the Shuttle, which flies a LEO mission, the payload mass fraction is only 5.7%. But the Shuttle has energy requirements which are substantially less stringent than that needed for the longer distance lunar missions and beyond. For the Apollo-Saturn V vehicle the Command Module, i.e., the returned payload, is only a very inconspicuous mass, less than 2% of the total vehicle, located on top of a massive vehicle stack. Our returned lunar mass payload – the objective of the missions – was only several pounds, obviously a very small percentage of the total 6,000,000+ lb launch vehicle mass.

We cannot advance the space program in such an inefficient manner. The "economics-safety-time-mass" dimensions scale exponentially for those missions where we wish, or need, to increase mission energy requirements – the direction where we are headed in any new vision for aggressive space operations.

Next, from the safety perspective, based upon today's knowledge, the flight time of approximately one year to Mars carries serious risk to humans from the aspects of physiological deterioration, exposure to space radiation hazards, and psychological stress. A reduction of that time to three months will significantly abate those risks. A reduction in the number of launches decreases risks to the public, environmental issues, and cost concerns. For example, a propulsion system having an α_p of 10 kW/kg permits NASA to launch on just one Shuttle flight the total energy requirements – that is, the propellants – for one human exploration mission to Mars! For chemical propulsion, the payload mass launched to LEO is large, requiring ~40, or greater, "Shuttle launches" for a lesser, more hazardous mission.

Further, the economic impact of using low performance propulsion systems to conduct missions requiring high energies is enormous to NASA and, hence, to the taxpaying citizen. The accomplishment of each new advanced exploration mission on the horizon will require a tremendous number of launches as discussed or, alternatively, the development of new heavy lift launch vehicles. Note that by the use of large launch vehicles we change nothing fundamentally to improve upon the physics of space flight operations. We merely reconfigure the energy problem. To apply the brute force approach to meet energy requirements is analogous to the technical community solving demanding computational problems 30 years ago by adding more vacuum tubes rather than developing solid state electronic systems.

For tomorrow's propulsion systems to serve in a cost effective manner, we must think less mass to accomplish even greater mission yields. We must do more while using less resources! Airlines are able to operate profitably by the use of flight systems permitting high payload mass fractions, approximately 60%, coupled with the safe, quick delivery of payloads. The space program is even more mass-time mission critical. To be effective, space transportation system designs need to be approached in terms of that payload delivery performance level.

One consideration having great importance to NASA's research and space science future is the fact that the space program will, in the not too distant future, run up against an energy "stone wall." That is, space science missions beyond the near-Earth vicinity will require substantially more energy because the low energy missions will all have been completed. The options are to extend flight time or to abandon new, advanced space science exploration missions. Mission reliability is placed in jeopardy with longer flight times. Qualification becomes more expensive and less certain. Long flight time is a deleterious incentive to our future space scientists to pursue new space science objectives and investigate new space phenomena. Even with today's Galileo mission, a scientist could have spent nearly his or her entire career on just one mission. How many scientists will desire to participate where they will not witness the seeds of their endeavors bear fruit?

Going out further or placing more massive payloads on near-by planets will increase propulsion system demands enormously. To place a science outpost on Pluto, to return planetary samples to Earth for comprehensive analysis, to travel to the Oort Cloud region, or to visit the near-by stars will require high energy levels of the type envisioned by this proposal. There are no options.

High performance space propulsion systems are, thus, critical. By high performance propulsion systems we refer to those which are capable of delivering variable specific impulse ranging from 10^3 seconds to 10^6 seconds and which can be designed to an α_p of 1 kW/kg to 10 kW/kg.

Long duration, constant, low acceleration (10^{-3}g to 10^{-4} g) propulsion systems effectively accomplish these new vision missions. Jet power levels from 10 megawatts to 100 gigawatts, produced by 1 kW/kg to 10 kW/kg α_p propulsion systems, are necessary. Propulsion systems must deliver variable specific impulse, ~$5\text{x}10^3$ seconds to 10^6 seconds, with long firing durations of approximately two months to Mars – and ultimately 10's of years to stars – with thrust ranging from 1 N to ~1000 kN to conduct that very wide range of missions.

Viable space-based vehicles demand ultrahigh propulsion system reliability by today's standards. Today, solid rocket motors fire for several minutes. Liquid rocket engines fire approximately 10^+ minutes in the case of the Shuttle main engine, for example. Ultra-reliability is essential to provide maintenance free, long life, and for many missions – reusable – operational space based flight systems. Just contemplate the demands placed on space vehicles which are expected to rapidly traverse distances on the order of 10's of AU's and beyond.

Nuclear energy produces greater than 6 to 10 orders of magnitude increases in specific energy over chemical energy and is the only energy source capable of meeting those high space power requirements. Consider the nuclear energy systems – fission, fusion, matter-antimatter – and their status. Fission thermal propulsion, while demonstrated, cannot provide the required energy. It is temperature (materials) limited. Fusion and matter-antimatter, while not demonstrated, theoretically can. Fusion is magnetic field (plasma) limited.

By developing fusion energy conversion space reactor systems for direct propulsion systems, we leverage the power which is available from high performance propulsion systems.

The total spectrum of alternative, potential energy sources is: chemical, nuclear fission (thermal and gas core), matter-antimatter, and perhaps a hypothesized strange matter stable regime. There are other energy derivatives such as metallic hydrogen and solar sails. But, only fusion has the inherent desirable properties of feasibility, performance, safety, and cost features to make it attractive as the sole purveyor of large energy levels for space travel, i.e., energy in the gigawatt, and perhaps even to the terawatt range.

The importance of high α_p to three typical high performance missions which bracket NASA's areas of interest is clearly illustrated by Fig. 1a, 1b, and 1c: (1) a human exploration Mars mission, (2) an asteroid sample return mission with 6 separate asteroids targeted on a single mission, and (3) a science sample return mission for Pluto's Charon. For a normalized human exploration Mars (Ref. 1), we can shorten the total flight time – including the time to Mars and the return trip to Earth – to a reasonable 0.2 year for a 10 kW/kg system. A more achievable α_p is 1 kW/kg which will permit mission accomplishment in approximately 0.4 year. A reasonable performance target for NEP is 0.067 kW/kg (ref. 1). Based upon that performance assumption, the minimum total flight time is 2 years (Fig. 1a) using NEP.

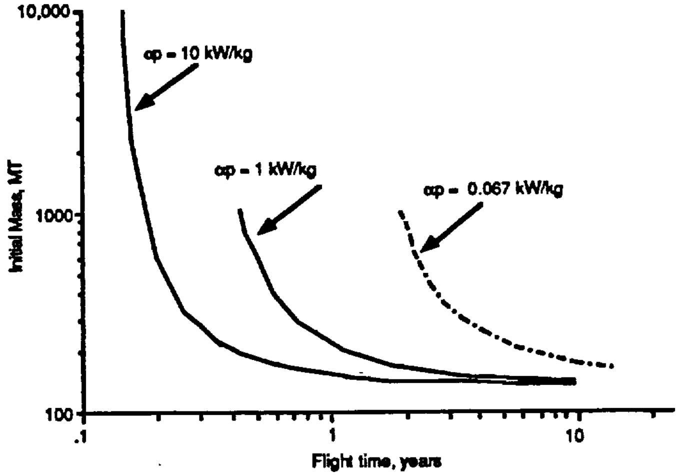

Fig. 1a. Comparison of α_p performance advantage for a human exploration Mars mission (133 MT outbound, 61 MT inbound payload using solely propulsion maneuvers at Mars and at Earth).

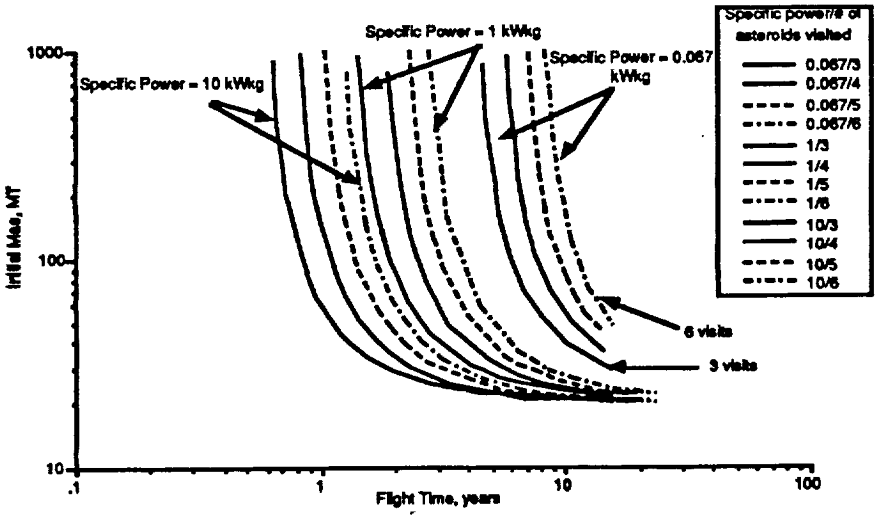

Fig. 1b. Asteroid sample return mission for 3 through 6 asteroids visited at 1.5 AU separation. (20 MT outbound, 10 MT inbound payload).

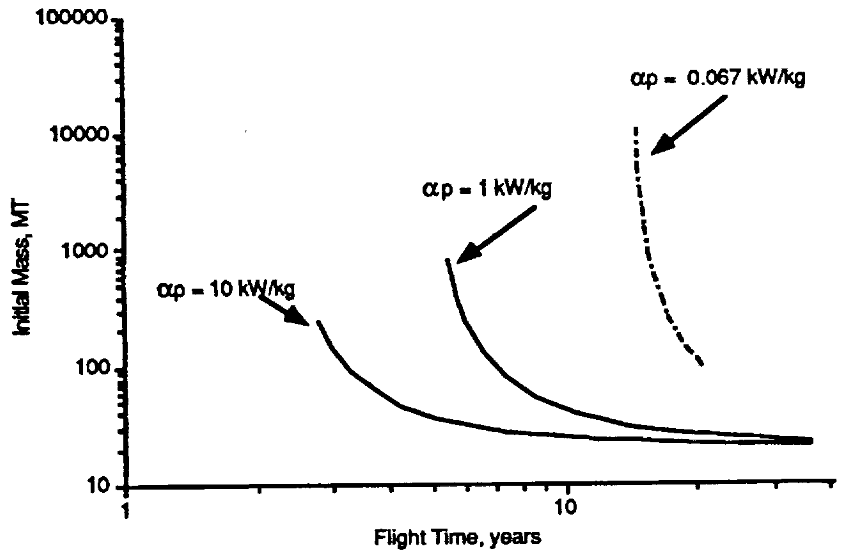

Fig. 1c. Comparison of α_p performance advantage for a Pluto Charon sample return mission (20 MT outbound, 10 MT inbound payload).

To meet future mission propulsion requirements, space vehicles must be powered by extremely reliable "solid state" propulsion systems where moving and erosive mechanisms are not mandated. Furthermore, we need 10^+ megawatts to 100^+ megawatts, ultimately gigawatts, of electrical power:

(1) to accommodate the permanent settlement of Mars, or the moon, where local planetary resources are essential for safety and economic considerations,

(2) to permit long duration human spacecraft missions to the outer reaches of the solar system, and

(3) to permit stellar distance communications.

Fusion inherently has significant advantages over matter-antimatter and fission for safe, economical, high performance space propulsion. The non-radioactive fuels, D-D or D-^{3}He, eliminate global launch radiation hazards in the event of an accidental release and thus would avoid adversely impacting Earth's environment. D-^{3}He's charged particle energy converts directly to thrust for efficient propulsion and produces low – 1% to 5% – neutron energy yields for minimal system mass but is a more difficult fuel to burn at ~40 keV over D-T at ~10 keV or D-D at ~20 keV. Engineering solutions to first-wall exposure become acceptable from D-^{3}He's reduced neutron flux.

Progress with magnetic confinement fusion (MCF) is being made on the tokamak, a large and heavy confinement scheme, but not on a space design concept. Magnetic fields in MCF inherently offer the means for reliably meeting long firing durations, and while attractive from that perspective, mass is a concern.

In space, some aspects of space fusion systems are simplified over the terrestrial applications. The expulsion of plasma particles for thrusting assists with resolving the ash removal problems faced by terrestrial fusion reactors. Space's clean vacuum resolves terrestrial reactor vacuum issues.

Plasma confinement reactor designs exhibiting high reactor β (ratio of plasma pressure to magnetic field pressure) are essential. The reasons are twofold. The mass of the magnets is reduced by a lower stress (magnetic energy is inversely proportional to β). Low β causes the electrons to radiate at a lower level.

Confinement options exist. For MCF, there are the Field Reversed Configuration (FRC) and dipole concepts, for example, in which $\beta \approx 90\%$, illustrative of the required reactor characteristics where less reliance for plasma confinement is placed upon externally driven magnetic fields. The FRC inherently has the desired linear field properties for propulsion. Before we can be more definitive regarding its viability, plasma stability and heating mechanisms require research.

Inertial confinement (ICF) reportedly has shown positive gains in the drive to produce net energy release, but the concept's strict security classification seriously hampers an open evaluation. Driver mass is one concern. Laser size is another. If proven to be achievable, the capability to burn fuels producing low neutron products would be a great asset. Additional research could provide gains in all areas of concern. These and other options are discussed later.

Because none of the specific confinement approaches above can be extrapolated today with certainty for meeting the space requirements, we must accomplish the essential space focused research in NASA. Testing is mandatory! But no space fusion research test program exists nationally or world-wide.

The time required for the space application of fusion is now, but the development of hardware for flight will not be quick. Thus, now is the proper time to initiate a space fusion research program to develop an applicable confinement design. The expedient and cost effective program approach is to demonstrate first principles, then to proceed directly to full scale, net power reactors. An aptly funded program, one at ~$250 M per annum for testing full scale confinement approaches, may make this capability available in a time frame that is on the order of ~30 years. That will be a total investment of $7.5B. Let us consider a Shuttle system to launch the Mars mission using chemical propellants in 40 flights. A specific power system of 1 kW/kg could place the propellants into LEO using only 6 launches, a reduction of 32 Shuttle flights, or a savings of $9.6B, assuming $300M per flight.

But this program will do more than provide a net sum gain to the space program. Significant benefits can be anticipated beyond those to which is directly ascribed in this research program proposal, i.e., significant spin-offs can be anticipated. While we do not present the need for a program based upon spin-off benefits, that is, nevertheless, an important topic to a research organization like NASA, as witnessed by the annual NASA *Spin-Offs* publication. For example, spin-offs from the space program's requirement for low power, light weight, highly reliable electronics has greatly assisted in fostering the current electronics industry. In the early developmental phase of solid state devices, the aerospace industry basically comprised the market. Today, the obverse is true. From being the prime consumer, we are an insignificant portion of the market. But the market spin-off is one consequence which we can obviously be proud to have stimulated.

Space transportation has a similar potential. This program can be expected to have similar benefits in the commercial sector and thus to assist the United States' in economical growth.

There is expected to be a synergistic benefit to the terrestrial fusion program for the development of commercial electrical power, although the equipment for space is expected to differ just as current ground and flight power generation units differ.

The program's applicability, thus, extends well beyond space.

4.0 PROGRAM RATIONALE

Today NASA is in the position of being able to use high energy technology but is without having a plan to pursue its development. This program develops that plan.

Further, our mission planning activities could indeed make use of this high energy capability. The objective of this proposal is to substantiate the program and to produce that plan. The rationale for inaction appear to be both of a technical and a managerial nature. NASA will address those topics by carrying out this program. The initiation of any technical program of this nature requires an examination of both rationale.

The two key technical questions to address for space fusion are:

1.) Is plasma confinement possible for a space-based fusion reactor which is capable of producing a controllable flight propulsion system?

2.) Is a fusion powered propulsion system capable of providing a 1 kW/kg to 10 kW/kg specific power system for space flight vehicles?

Answer these questions and the rest of the work and the future of space falls into place.

This proposal addresses both technical questions through the initiation of a program to demonstrate good confinement – the First Step. Upon successful demonstration of the First Step, we proceed to system demonstrations, the second step which is to prove the feasibility of question 2. Technical aspects were discussed earlier in "The Solution" section and are not discussed further except to show specific performance capabilities desired. A program and options to implement the solution is provided in a later section.

Let us deal in this section with the two key managerial questions, assuming that the answers to the technical questions are affirmative:

1.) Can NASA afford to develop a space-based fusion flight propulsion system?

2.) Is not the development of a space-based fusion flight propulsion system a task for "others" to do?

First, to address the "affordable" question, let us look at:

(a.) the propulsion approach for NASA's future missions,

(b.) the current approach for prioritizing research funding, and

(c.) the long term impact of performing fusion propulsion development.

Then, secondly, let us examine question #2, the leaving it to "others" rationale, and the results obtained by following that approach.

The current propulsion approach, item (a.) above, as planned for future missions is expensive. The cost of using low performance propulsion systems for space flight applications is large due to inherent physics limitations. That low space propulsion performance vehicle approach requires the delivery of massive payloads to LEO. The liftoff mass of the Earth-launched vehicle increases exponentially with orbital space vehicle mass. The successful development of high performance space propulsion systems are expected to reduce the mission requirements for mass placed into LEO by 1 to 2 orders of magnitude. Furthermore, the safety advantages offered to future NASA missions by fusion will make possible that which may be politically highly questioned. We should not forget the Shuttle Centaur lesson, a program which was terminated because of safety concerns after the vehicle was very near flight status. It can happen.

With regard to the efforts that we face, let us be very explicit. To be successful, the development of space fusion energy will require a significant investment. Because the investment offers such a tremendous potential return, we cannot, however, afford to refrain from undertaking this program responsibility. There is, on the other hand, a significant risk in not undertaking this research.

The investment capital required to conduct this program is available. The source is a matter of resource priority. Consider the manner by which our programs are currently defined and implemented, item (b.) above. Numerous small projects are undertaken to perform various technical capabilities, such as, to explore Mars. For example, we develop, or plan to develop, terrain transportation vehicles, backpacks, instruments, data gathering-transmission systems, and all the various other equipment that will be necessary for performing space exploration. Those research endeavors are important; and, further, they are readily achievable. There is little attendant technical risk. These projects are attractive by the fact that they can be accomplished with the expenditure of relatively small funding levels. But what is their ultimate value if we can not arrive at Mars in a reasonably affordable, safe manner? What if the mission cost is so enormous that we can only afford a single or very limited number of exploration missions to Mars? In that light, the result is that our research may then be considered a questionable investment for the long term.

Obviously, we have to arrive at Mars first before that technology can be used. A hierarchical examination of space research priorities, then, shows space transportation at the apex. After all, mankind was only able to dream of going into space until the advent of the rocket engine. The development of propulsion systems has always been the "long pole" in all of our space programs from the cost-time-safety perspective. If we cannot travel to Mars economically, settlement will not occur. Missions will cease just as lunar exploration following the end of the Apollo Program. The vision of space settlement will not become reality. Modern civilization thrives, and survives, based upon cheap transportation. So will space.

Consider (c.) the long term impact of performing fusion propulsion development. The performance leveraging may be sufficiently great that the US could recover its investment costs from this space fusion research program by the flight of one human exploration mission to Mars. The rationale is based upon the reduction in the number of launches which are required to LEO to place the mass to provide the velocity changes using fusion propulsion in comparison with the number of launches using low performance propulsion (chemical or nuclear thermal). Details may be found in reference 1. One can always debate costs at this point since neither the costs for the Mars mission nor the developmental costs of a space flight fusion propulsion system are known. Clearly the colonization of Mars and any space mission which is conducted beyond Mars will require advanced propulsion exhibiting the type as discussed herein. Also, the unmanned space science missions will require this capability if civilization is to press forward with missions beyond the bounds of chemical propulsion.

Inaction, thus, is the unaffordable approach. The performance data for a wide variety of solar system missions are presented in Table 1a-1d to show the advantages, including mission flexibility, of fusion energy performance levels. (Ref. 1) The table also underscores the importance of high α_p.

Table 1a. Performance summary for typical human exploration Mars, outer planetary sample return, and asteroid sample return missions. Performance data are referenced to a 131 MT outbound/61 MT return payload for human missions and a robotic payload mass of 20 MT outbound and 10 MT return. The times shown are the total flight times exclusive of stay durations at the target.

SPECIFIC POWER =1 KW/KG

Mission	t, years	Mo, MT	Mp, MT	γ, %	Pj, MW	<Isp>, seconds	Δv, km/s
Manned Mars	0.50	613	335	22	145	10,610	90
Europa	1.56	320	243	6.3	57	16,690	209
Titan	2.99	74	36	27	18	26,200	196
Miranda	5.34	60	26	33	14	35,680	233
Triton	5.85	108	62	19	25	35,130	314
Charon	7.42	81	41	25	19	40,530	317
Asteroids: 3 visits	1.72	163	107	12	36	18,550	185
Asteroids: 6 visits	3.39	162	105	12	36	26,120	254

Where: γ = Payload mass fraction
M_o = Initial vehicle mass, MT
M_p = Propellant mass, MT
MT = Metric ton
MW = Megawatts
P_j = Jet power, MW
t = Round trip flight time, years

Table 1b. Performance summary for typical human exploration Mars, outer planetary sample return, and asteroid sample return missions. Performance data are referenced to a 131 MT outbound/61 MT return payload for human missions and a robotic payload mass of 20 MT outbound and 10 MT return. The times shown are the total flight times exclusive of stay durations at the target.

SPECIFIC POWER =10 KW/KG

Mission	t, years	Mo, MT	Mp, MT	γ, %	Pj, MW	<Isp>, seconds	Δv, km/s
Manned Mars	0.50	185	30	72	227	35,770	90
Europa	1.56	32	6.8	63	50	64,070	209
Titan	2.99	29	5.3	68	40	81,180	223
Miranda	5.34	26	3.4	77	27	117,509	233
Triton	5.85	27	3.8	74	30	129,620	283
Charon	7.42	27	4.1	73	32	137,069	317
Asteroids: 3 visits	1.72	44	15	45	96	57,020	257
Asteroids: 6 visits	3.39	141	12	49	83	86,735	329

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Table 1c. Performance summary for a fast human exploration Mars, outer planetary sample return, and asteroid sample return missions. Performance data are referenced to a 131 MT outbound/61 MT return payload for human missions and a robotic payload mass of 20 MT outbound and 10 MT return. The times shown are the total flight times exclusive of stay durations at the target.

SPECIFIC POWER =1 KW/KG

Mission	t, years	Mo, MT	Mp, MT	γ, %	Pj, MW	<Isp>, seconds	Δv, km/s
Manned Mars	0.44	1,041	681	12.8	227	9,440	98
Europa	1.43	976	843	2.0	113	14,720	223
Titan	2.11	858	733	2.3	105	17,750	264
Miranda	3.48	809	687	2.5	101	22,860	337
Triton	4.59	1,031	895	1.9	117	25,780	393
Charon	5.49	797	677	2.5	100	28,570	420
Asteroids: 3 visits	1.44	955	819	2.1	116	14,600	217
Asteroids: 6 visits	2.82	992	852	2.0	120	20,300	301

Table 1d. Performance summary for a fast human exploration Mars, outer planetary sample return, and asteroid sample return missions. Performance data are referenced to a 131 MT outbound/61 MT return payload for human missions and a robotic payload mass of 20 MT outbound and 10 MT return. The times shown are the total flight times exclusive of stay durations at the target.

SPECIFIC POWER =10 KW/KG

Mission	t, years	Mo, MT	Mp, MT	γ, %	Pj, MW	<Isp>, seconds	Δv, km/s
Manned Mars	0.18	1,034	676	12.9	2,225	18,870	196
Europa	0.81	92	50	22	219	42,210	352
Titan	1.20	99	56	20	234	50,650	437
Miranda	1.93	112	66	18	261	63,300	576
Triton	2.87	77	39	26	185	79,820	609
Charon	2.76	237	171	8.4	464	70,134	816
Asteroids: 3 visits	0.65	898	766	2.2	1,119	30,920	455
Asteroids: 6 visits	1.30	796	670	2.5	1,053	43,920	631

Notice in Table 1 that payload mass becomes increasingly diminished in importance to missions as α_p increases. That point is shown by the large values of the payload mass fraction, γ, which are possible with high α_p. The importance to NASA is that we can detune the high sensitivity of vehicles to the inevitable payload mass changes which have cost the Agency many billions of dollars over the years just to implement weight reduction programs. We also observe the significant decrease in propellant mass required – a cost benefit and a safety benefit as well. We trade propellants for inert, reusable propulsion systems mass, a highly desirable objective. The relative increase in propulsion system mass is apparent. The propulsion system role becomes increasingly great. Propulsion, thus, requires greater attention in these missions than ever previously given. To shorten fight times requires high vehicle energy for which high power levels are essential. The high velocities also require variable, high specific impulse capabilities. The importance of the role of a plasma as the working fluid becomes apparent.

Now consider the leaving it to "others" rationale, the second inaction rationalization. The bottom line is that the space program, by leaving it to others, does not possess the high performance space propulsion capability which it needs. But should we become pursuers of new space propulsion technology? Examine history. NASA developed the technology that enabled humans to travel to the moon and to more routinely go into space using the reusable Shuttle. Even with all of the criticism about the Shuttle and its costs, one must ask the question, "Where would the US space program be today without it?" Surely, there would not be anywhere near the diversity and the large number of individuals who have now flown into space. Perhaps, unrecognized at this time, that wide variety of space flight personnel is tacitly setting the stage for a future significant market and industry, provided NASA can develop the technology that will reduce space transportation costs substantially. Further, we have conducted many in-space experiments - very small ones included - that otherwise would not have been done. We have broadened tremendously the base for space program participants. We have, furthermore, performed payload recovery missions.

These achievements have all been made possible by NASA taking the lead to provide the propulsion capability: NASA developed the transportation systems in both cases to accomplish those feats because NASA has the charter for space technologies, including power and propulsion.

Where would we be today if we had waited for "others" to do the work for us? Not very far along. The program objectives of "others" would receive a different priority. Where is the new, advanced space equipment that the "others" are working so diligently to resolve our space transportation needs? The new equipment does not exist. We cannot point the finger of responsibility at DoE who has the charter to produce price-competitive power for commercial applications of electrical energy for use on Earth without regard to specific power or propulsion. The terrestrial application has a different technology requirement. In fact, the key alternate confinement experiments that might have had some benefit to space have all been canceled! The DoE is focusing on the tokamak which has a specific power too low to support space propulsion. In today's terrestrial energy environment with a nearer term focus, no overwhelming requirements exist for fusion-produced electrical power. Fusion is being decreased in importance to the DoE – there are sufficient electrical power options available in the United States without it: coal, fission, crude oil, wind, hydraulic, solar, and geothermal. Consequently, other programs receive greater priority.

In space, there are no such options available. We must rely on nuclear energy. Fusion is enabling technology for space.

One might point the "responsibility" finger in another direction, arguing that the development must be left to the military as a matter of strategic importance. But is that likely to happen? The space flight energy requirements that are of interest to NASA are considered well beyond any that the DoD could be expected to require, at least in the foreseeable future. Fusion provides very high energy levels for missions far beyond the Earth where no military threat exists. Fusion, as we now understand the technology, is not a low energy system for LEO missions, the military's space operational regime. In space, it is not even currently envisioned as a lunar mission propulsion system. The energy requirement is too low. A small-mass, high-power density system that could power spacecraft, aircraft, ships, or submarines, however, would change that scenario; but there would have to be new developments and thinking within the technology to do so.

The need for high energy performance propulsion is, thus, quite clear. The applications and feasibility have been studied and are reported in reference 1. Peer review by the space science community has been supportive. Their question is, "Can it be done?" The results of the reference 1 study suggested revolutionary results may be possible, and the merits warrant the pursuit of research. By the conduct of the work performed, as described in this proposal, NASA will address that critical question as the first step.

Technical feasibility is the objective sought by this proposal. This NASA fusion R&D program will not set a precedence since a fusion research program once was conducted at the Lewis Research Center (ref. 2).

NASA needs to control its destiny on matters of such grave importance; and further, NASA needs the in-house fusion propulsion expertise for support of future space missions.

What is the reaction to the development of this capability from the NASA technical community, particularly the various program staff? There is a tendency, in general, for NASA program managers to avoid taking strong positions which demand new technology and thereby run the real risk of not being able to obtain program approval. Program managers are not going to take high risks relative to requiring new technology, as historically shown. And that is a reasonable "program-manager" conclusion. What program manager would step forward with a mission proposal: "Here is an excellent program from the conduct of which I will explain all the key science issues concerning our solar system. Oh, by the way, I require gigawatts of fusion generated power in space to perform this mission."

Even with our new exploration initiatives under consideration, it would be prudent to show a program based upon a plan to fly vehicles which can be accomplished using current operational (chemical), or currently demonstrated (nuclear thermal and ion), technology without making the case for a fusion propulsion capability or any other high risk technology. They will wish to do it "quick and cheap" with short term dividends readily apparent. That is a trait of human nature. But we must reach beyond the short term to provide for the future.

This "stay-the-technology-course" approach deters new technology as we have unfortunately historically experienced. And programs eventually suffer when we fail to plan beyond the immediate future. Let us refrain from abdication of responsibility, let us eliminate inhibitors to space technology advancement. Let us leave the legacy to future generations who can look back and state that we made a wise decision.

There is no doubt that the accomplishment of this proposal's main objective will permit the radical advancement of space science and the practical use of space. The capability, <u>once demonstrated</u>, will be welcomed by programs.

Having defined the vision, the solution, and the rationale for action, we now address the recommended program approach to conduct this research program.

5.0 PROGRAM APPROACH

There are four major steps to the realization of this great potential. In this proposal, we concentrate on taking that most important first step.

STEP I

–OBJECTIVE: DEMONSTRATE SPACE FUSION FEASIBILITY–

Phase I: Program Definition Planning
Phase II: Space Fusion Feasibility Demonstration

As the **top priority item**, we need to show that fusion works for space flight. We accomplish that goal by analysis, design, and testing. Ideally, one would like to build a full scale reactor at the onset of the program. Before that can be done, additional empirical/experimental data is necessary, the effort of which needs to be defined in greater depth - the first program step. A reactor by itself, which cannot be converted into an efficient propulsion/power system, is meaningless. Hence, the proposed program also includes analyses which evaluate the fusion system's capability to meet flight system parameters. The space program has the advantage of being in a position to leverage analytical tools developed for the terrestrial program.

Determination of the program specific content will be the first program objective – the Phase I planning work. The preferred program approach to accomplish Step I is a fast paced one, that is, one in which the program has been structured to:

Expedite analyses and designs for space fusion propulsion/power systems and conduct the necessary testing to demonstrate the confinement of fusion plasma using a minimum number of experiment design upgrades.

A fast, or expedited, reactor feasibility demonstration, Phase II, is defined as one which focuses on the most rapid program path to achieve full power demonstration without necessarily fully understanding all reactor physics phenomena. Since fusion physics has proven difficult or misleading with respect to scaling, the most cost effective approach to its development is to use the quickest approach, namely, to proceed using a fast paced technology test program, one which advances quickly to full scale experiments. The feasibility of that approach is an important feature of this proposal. Historically, that approach is not significantly different from other technologies, including chemical propulsion. If full understanding were a prerequisite before proceeding with operations of a new technology, many advancements would not be available today.

That fast paced program approach is justified, based upon the tremendous impact that fusion will have upon the space program, coupled with the length of time required to develop space fusion. The risk is minimized by the conduct of proof-of-principle experiments. In the event, however, that a slower approach test program is directed, Phase II can be accommodated at a reduced funding level or, alternatively, using the budgeted funds to examine more options.

Basic mission goals, studies, and analyses have been performed as shown in reference 1. The space program is being redefined frequently, and no approved national space model exists. Hence, it is concluded that the development of further mission and system studies would not be of sufficient merit at this time to warrant additional resources. Studies have been completed to show that if controlled space fusion/power were available, then its advantages would be pursued vigorously. Upon demonstration of principles, additional mission and system analyses should be conducted to refine the flight fusion system requirements. For now we simply need to focus on the key question, "Can it be done?" To obtain the answer, we analyze the options, perform proof-of-principle testing, and then we bore-in on the most attractive approach.

The major program projects are summarized in Fig. 2. Table 2 summarizes the program's funding and products. Program funding has been set at a low level for the first two years to establish a firm space fusion program planning foundation, the missing element in NASA's future. No significant reprogramming of resources is required. A management option, if desired, does exist to proceed with a faster paced approach which will entail reprogramming.

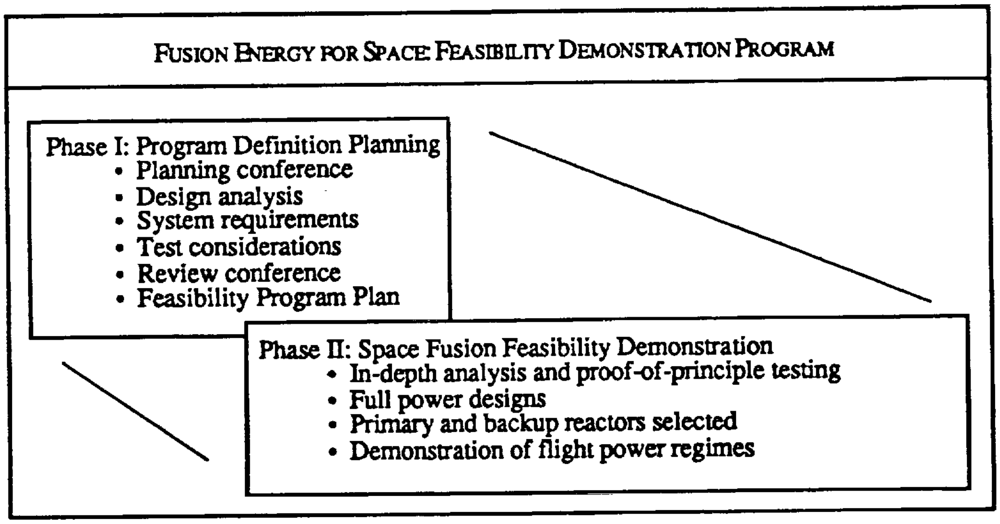

Fig. 2. Summary of program projects.

Let us now direct our attention to further technical information on how the program would be implemented and first consider the selection of fusion fuels. The fuel reaction of choice is D-^{3}He:

$$D + {}^3He \rightarrow P\ (14.68\ MeV) + {}^4He\ (3.67\ MeV).$$

There are no radioactive fuels involved in this reaction. The products of combustion are non-radioactive charged particles, a proton and alpha particle, which can be usefully placed into work as thrust or electrical power. There is a D – D side reaction which does produce 1% to 5% neutron energy. The recovery of helium-3 from the moon has been studied and is considered achievable for flight usage. (Ref. 3) Deuterium is available from the ocean. Helium-3 is available in sufficient quantities from non-lunar sources to permit the initiation of a test program. (Ref. 4) If proven to be technically feasible, P-^{11}B would be an ideal fuel due to the presence of released energy solely as charged particles:

$$P + {}^{11}B \rightarrow 3\ {}^4He\ (8.664\ MeV).$$

The terrestrial program uses the D – T reaction which for space has the disadvantage of producing 80% of the released energy in neutrons and which are unavailable for thrust:

$$D + T \rightarrow n\ (14.07\ MeV) + {}^4He\ (3.52\ MeV).$$

Table 2. Summary of steps requested to implement the *Fusion Energy for Space: Feasibility Demonstration Program.*

STEPS	TASK/PROJECT SUMMARY	DATES	FUNDING LEVEL	PURPOSE
	PHASE I: PROGRAM DEFINITION PLANNING			
1.	Initiate program with transfer of Norman Schulze to Code R.	Immediately	$0K	Manage program.
2.	I.1 Conduct Space Fusion Program Planning Conference. I.2 Analyze preliminary reactor approaches for 2 different designs. I.2a Analyze preliminary reactor approaches for more than 2 designs. I.3 Analyze fusion vehicle system requirements. I.4 Perform testing analysis/support limited tests. I.5 Conduct Space Fusion Program Review Conference.	FY 93 FY 94	$230K[1] $300K[1]	Program plan. Travel, reports, misc. Develop analytical and test program.
	NASA REVIEW –Program decisions–			
	PHASE II: SPACE FUSION FEASIBILITY DEMONSTRATION			
3.	II.1. Conduct key concept confirmation tests of 2 recommended concepts.	FY95-97	$10M (total)	Obtain proof-of-principle feasibility, stability, plasma transport rate, and scaling test data.
4.	II.2. Conduct flight system reactor integration analyses.	FY95-6	$3M (total)	Establish system issues and minimum cost program.
5.	Conduct Space Fusion Demonstration Program Planning Conference.	FY96	$50K (total)	Down select to a primary reactor and an option.
6.	II.3. Commence "Space Fusion Propulsion Technology Developmental Analysis and Test Program."	FY97	$50M (FY97)	Commence full scale demonstration.
	II.3. Demonstrate space flight fusion feasibility.	FY98 and subs.	Increases to ~$250M/annum (FY92$)	Analysis/test program to demonstrate flight power levels.

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[1] Level requested. A lesser amount either reduces planning rigor if the schedule is maintained; otherwise it stretches out the planning process.

6.0 PROGRAM IMPLEMENTATION

The program presented herein is a simplified version of that presented in reference 1 which should be consulted for a more detailed understanding. The total program flow is presented in Fig. 3 for an overview.

FUSION ENERGY FOR SPACE: FEASIBILITY DEMONSTRATION PROGRAM

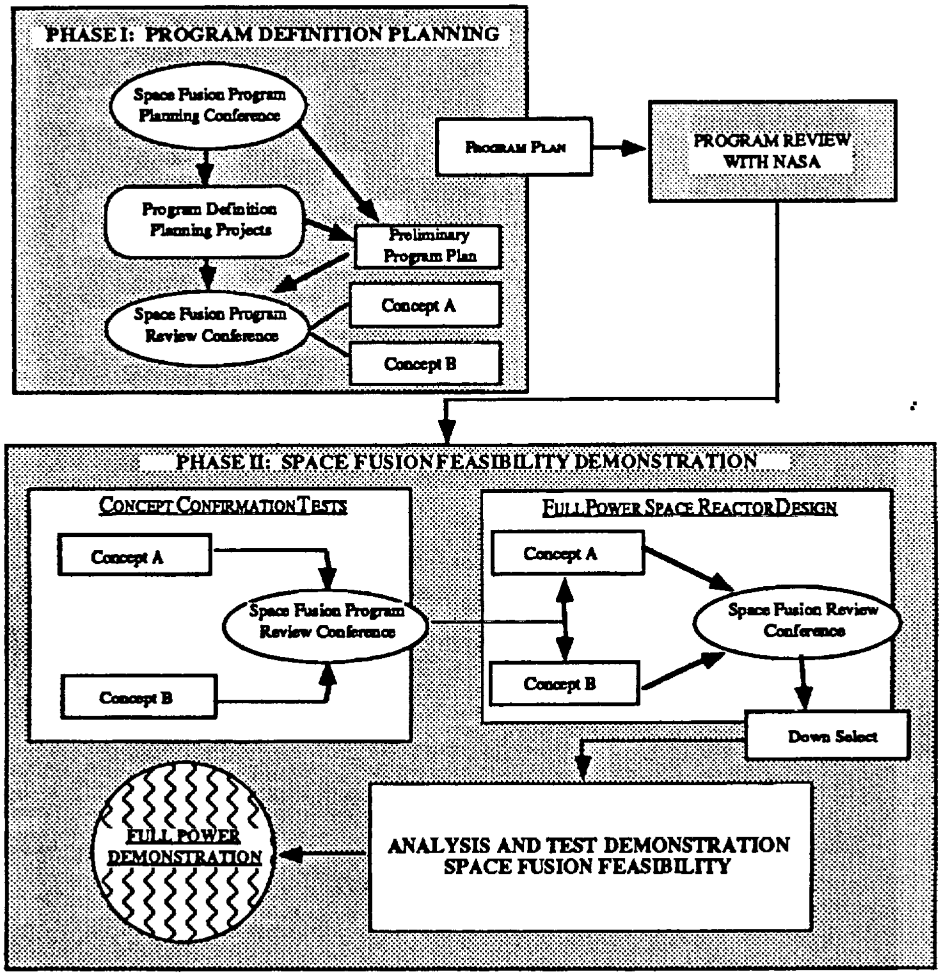


Fig. 3. Total program flow, *Fusion Energy for Space: Feasibility Demonstration.*

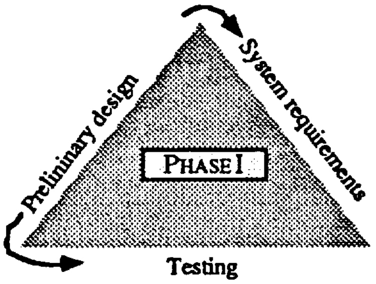
By the completion of *Phase I: Program Definition Planning*, a program plan for the "*Phase II: Space Fusion Feasibility Demonstration*," will be prepared using the best expertise available. This is a space fusion propulsion and power technology development analysis and test program. Issues and key steps necessary to accomplish a space fusion capability will be defined in Phase I. This *Phase I: Program Definition Planning*, thus, produces the managerial planning path to answer the key question, "Can it be done?"

The NASA management team will be established at initially a small level, one individual - Norman Schulze - at Headquarters. Several individuals at the Lewis Research Center are proposed to perform the day-to-day management of the program. Any available, existing staff from the prior NASA-LeRC fusion program would be an asset and would expedite program initiation and the implementation of projects.

Let us now examine the program content in greater depth.

6.1 PHASE I: PROGRAM DEFINITION PLANNING

The *Phase I: Program Definition Planning* projects will accomplish the program planning without necessitating significant reprogramming. The diversity of projects and major reviews will provide NASA management the confidence that the proper events are planned for Phase II. Key elements of Phase I are:



Hence, the planning in Phase I provides the greatest opportunity for meeting with program success in Phase II. Refer to Fig. 4 for a project summary of Phase I. Planning can be accomplished with less funding and reduced rigor or with equivalent rigor stretched out.

PROJECTS
Phase I: Program Definition Planning :
I.1 Space Fusion Program Planning Conference
I.2 Preliminary Space Reactor Design/Analysis
I.3 Space Fusion Vehicle System Requirements Analysis
  –Fusion Mission Conference
I.4 Test Analysis/Support
I.5 Space Fusion Program Review Conference
  –Program Plan

Fig. 4. Project summary for *Phase I: Program Definition Planning.*

The major goal of Phase I is to further define the key elements of an optimal space fusion developmental technology program. That will be accomplished by five projects, Fig. 5, and one major review.

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As a minimum, the desired objective is initially to structure a program plan for the development of fusion energy for space. The development of fusion energy provides NASA with an option non-existent at the present; and, hence, this information is critical to the future of NASA. The information needs to be developed and presented to NASA for review. The projects as defined in Phase I are requested since they will greatly assist in presenting to NASA the impact of the fusion energy option. In the event that funding is severely restricted, a program plan will still be developed but obviously with reduced analysis. The one essential project is the Conference, Project I.1. An additional $20K in FY 93 will facilitate the planning, implementation, and reporting on the results. A program plan will be prepared from the results of the Conference plus other individual efforts.

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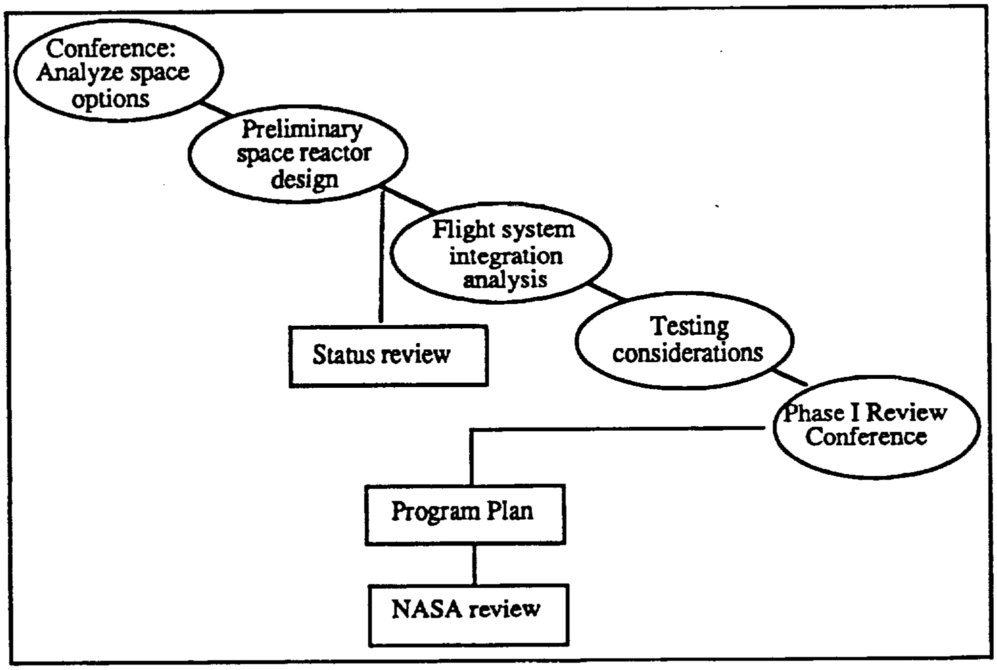

Fig. 5. Phase I projects.

The Phase I work commences with the wide involvement of an advanced fusion focused community in a **Space Fusion Program Planning Conference.** Participants will include individuals having an interest in fusion powered space propulsion systems. The Conference will accomplish its objective using a peer review process of the proposed concepts. This will be done by chartering a Space Fusion Reactor Peer Review Panel. The Panel will also add guidance to the overall planning. Included in this planning will be long term, i.e., greater than 2-3 years, program recommendations.

The timely implementation of Phase I permits the Phase II budget to be incorporated into the 2-year budget cycle such that the Conference can contribute to the FY95 budget process. By accomplishing Phase I, NASA will be in a good position to have a better defined basis for program requirements and funding. The expenditure is very small, but a tremendous return is realized from that small investment. This is a no risk–large potential gain proposition.

The proposed funding and program products are shown in Table 3. The schedule is presented later in Fig. 9a.

Table 3. Budget proposed and products for Phase I.

#	PROJECT	BUDGET		PROGRAM PRODUCTS
		FY93	FY94	
I.1.	Space Fusion Program Planning Conference.	$30,000		1. Report on proceedings. Recommended initial confinement approaches. 2. Briefing to NASA: program options, preferred approaches, and key experiments.
I.2.	Preliminary Space Reactor Design/Analysis	$100,000 each- 2 contracts		1. Technical reports on preliminary space reactor designs and recommended testing for FY94. 2. Preliminary reactor designs – 2 full scale. 3. Brief NASA: reactor designs, program options, preferred approaches, and key experiments.
I.3.	Space Fusion Vehicle System Requirements Analysis.		$100,000	1. Project I.2 reactor-flight vehicle system requirements. Integration of reactor with flight systems. Specific power analysis. 2. Fusion Mission Conference. 3. Report on optimal approach for accelerated space fusion program. Space Reactor Test Program Definition.
I.4.	Test Analysis/Support.		$150,000	1. Test requirements/planning and/or technical reports on key experiments per Project I.1 and I.2 to better define the potential of Phase II's reactors.
I.5.	Space Fusion Program Review Conference.		$50,000	1. Review program results and make recommendations on planning. Recommend minimum of 2 confinement approaches. 2. Brief NASA on Phase I results. Establish a plan for space fusion program options.
Total		$230,000 (up to)	$300,000 (up to)	Program Plan

PHASE I PROJECTS

First Year Activities–FY93:

To initiate *Phase I: Program Definition Planning,* two projects are to be performed: a conference and the analysis of 2 confinement designs:

FY93 Projects:
- Space Fusion Program Planning Conference
- Space Fusion Reactor Design/Analysis
  - –Concept A
  - –Concept B

Details on the work to be performed follow. The Phase I projects are presented in a prioritized order. Tasks are identified which the proposer will directly accomplish as well as which will be contracted out.

PROJECT I.1. SPACE FUSION PROGRAM PLANNING CONFERENCE

Objectives: The determination of viable space reactor concepts and definition of a program is the first task. To accomplish that a **Space Fusion Program Planning Conference** will be held, the first step in Phase I. Prior to the Conference, efforts will be devoted to planning.

By holding this meeting, fusion technologists will be able to freely present ideas for space reactor confinement concepts. These proposals will receive an independent peer review for selecting design preferences. In order to be responsive to the NASA FY95 budgetary process the Conference will be held in the winter of 1992. One major objective will be a documented technical approach for the development of space fusion energy and the Panel's recommendations for the program, including testing, budget, etc.

PROJECT I.1. SPACE FUSION PROGRAM PLANNING CONFERENCE

- Supports program planning.
- Recommends confinement approaches to pursue and tests to be conducted:
  - –Concept A
  - –Concept B
  - –Others?
- Provides long-term strategic program planning basis.

Content: The tasks that will be accomplished preceding the conference consist mainly of planning for the meeting. The location, agenda, date, and participants will be determined. A Space Fusion Reactor Review Panel will be established and convened with the objective of evaluating confinement proposals and determining preferred options. Evaluation criteria will be prepared, and the definition of the Panel's functions will be established to assist the Panel in the performance of their task.

In this planning phase, no concept will be eliminated from consideration. This includes ICF and MCF. The approach is to avoid locking in at this early time on a single design and to have at least one preferred approach and one option.

Analytical understanding and the test background of plasma confinement options will be important factors in making decisions to determine the best approaches. Fuel selection for space use will still be another criteria. Space flight propulsion system level considerations are additional factors to be examined. The Panel will be requested to identify those candidates having potential for further consideration and will be asked to provide a priority rating. The identification of important tests to assist the decision process is another objective of this conference.

Products: The product will be the determination of the most likely confinement designs capable of meeting with success for space applications, listed in a prioritized order, and program approaches to best pursue those designs. There will be proceedings published. Recommendations for confinement configurations as program options are to be provided to NASA. By the judicious use of the participants, we will have the programmatic advantage of starting not from scratch, but having the experience of at least hundreds of man-years of experience, if not more, practically free of cost. NASA management will be exposed to the importance of a space fusion program, to technical feasibility, and to the development program required. A funding level of $30K in FY93 supports the conference and publication of proceedings.

PROJECT I.2. SPACE FUSION REACTOR DESIGN/ANALYSIS

Objectives: With the determination of the best options to pursue completed, we are now in a position to explore their capabilities in greater depth. The program initiates pursuit of the question, "Can it be done?" Thus, the design of a space fusion reactor is sufficiently important that it is assigned as the next priority task:

PROJECT I.2. SPACE FUSION REACTOR DESIGN/ANALYSIS

- Conduct preliminary reactor design analysis of two confinement approaches:
  - –Concept A
  - –Concept B.
- Identify test issues.

Content: Two small, $100K, study contracts will be awarded to conduct a preliminary reactor design and to perform an analysis of two preferred confinement approaches for the space application. That is work which has not been accomplished and is essential to the decision process. For example, a space reactor design of magnetic confinement reactors has not been performed. The analysis will include and emphasize stability of full scale space propulsion reactor designs using current or modified codes. This project will contribute to our planning through the development of a better understanding of the technology needs. This design work is designated as "preliminary" because at this phase we wish to examine concept fundamentals to determine the concept's viability for further pursuit. Later, a detailed analysis will be made which will require over an order of magnitude increase in funding.

One obvious first question is, "Are there any viable space fusion reactor design concepts?"

Options could include the Field Reversed Configuration (FRC), magnetic dipole, electrostatic, Electric Field Bumpy Torus devices, tandem mirror, etc. – any compact torus having high β characteristics – one making efficient use of magnetic fields. A general discussion of ICF versus MCF issues is expected to develop.

Based upon the analysis conducted and reported in reference 1, the Field Reversed Configuration (FRC) appears to be a strong potential candidate fusion reactor for space. In the **Space Fusion Program Definition Planning Conference**, one anticipated recommendation which may evolve from the Peer Review Panel is to perform a design analysis of the FRC for space. The value of plasma stabilizing techniques, such as using neutral beam injection for the FRC, should be established. The potential value of the FRC to the space application generally meets with wide agreement on the part of the fusion community.

Products: One product will be to provide an improved analytical understanding of key reactor level concerns on design concepts having potential for space applications. Recommended important limited testing, to be performed in the FY94 Project I.4 to investigate key testing concerns, will be defined. A technical report describing space reactor designs at the end of the year will be prepared. A briefing will be given to NASA which describes the design options and pros and cons of design approaches. We will also identify key experiments that offer technological leverage and could be accomplished in FY94.

PROJECT I.2a. SPACE FUSION REACTOR DESIGN/ANALYSIS OPTIONS

While the above Project I.2 pursues two highly regarded confinement approaches, there is merit in providing for innovation of new ideas. In the event that additional funding could be made available, the pursuit of a number of small seed contracts, on the order of $75K, would be a valuable asset to the program to provide for the definition of additional design options.

SECOND YEAR ACTIVITIES–FY94:

In the second year of *Phase I: Program Definition Planning*, three projects are to be performed to assist in the program planning process: a space fusion powered vehicle systems analysis, test analysis/support, and a review conference.

FY94 Projects:

- Fusion Powered Vehicle System Requirements Analysis
- Test Analysis/Support
- Space Fusion Program Review Conference

Details on the work to be performed follow.

PROJECT I.3. SPACE FUSION VEHICLE SYSTEM REQUIREMENTS ANALYSIS

Objectives: Having completed a reactor design to address the first key question, the program is now in a position to address the second key question: "Is a fusion powered propulsion system capable of providing a 1 kW/kg to 10 kW/kg specific power system for space flight vehicles?" During FY94 analyses will be performed to determine the vehicle requirements and the propulsion system-flight vehicle integration requirements of the Project I.2 reactor designs:

PROJECT I.3. SPACE FUSION VEHICLE SYSTEM REQUIREMENTS ANALYSIS

- Determine vehicle system design requirements including integration of fusion reactors into flight vehicles.
    - – Fusion Mission Conference.
- Define optimal accelerated fusion development program.

Content: A major goal of Project I.3 is to obtain a better understanding of the potential for meeting the α_p goal of 1 kW/kg to 10 kW/kg. Key areas in need of technological support will be quantified to the greatest degree possible. The flight operational and flight system aspects will be included. For example, this project will address one critical element with regard to α_p, namely, the mass required for restart. Low-mass energy storage systems, such as efficient flywheels and capacitors, are technology areas in need of review and investigation. Ideas are an important part of this project. Another is system thermal control and thermal design for the integration of the propulsion system into the flight vehicle. These understandings are necessary to determine the mass and, hence, α_p capability. Further, an optimal program plan will be included to develop the reactor using an accelerated approach to produce space power levels from a full scale reactor design. A preliminary evaluation will be performed by a $100K contract. The NASA expertise with flight operations and with thermal analysis is to be included as part of the activity.

Soon after the system requirements/integration contract is awarded, a **Fusion Mission Conference** will be held with the science and exploration community. The purpose is to receive user requests for performance and mission capabilities which will assist the integration analysis.

As another product, user understanding and support for the program will be developed, hence, customer advocacy. Reviews of reference 1 by the science community already show that the capability would be expected to revolutionize the conduct of space science.

Products: The products from this project will be a report on analyses concerning the capability to meet 1 kW/kg to 10 kW/kg vehicle system design requirements including integration of fusion reactors into space vehicles, and the definition of an optimal accelerated fusion development program. A Fusion Mission Conference report will also be prepared.

PROJECT I.4. TEST ANALYSIS/SUPPORT

Objectives: Subsequent to the design project performed in Project I.2, the program can move to investigate the third key element in need of better definition, namely, that of testing. With a $150K budget in FY94, only very limited testing support, at best, can be considered. The thought here is to support an on-going activity that will be beneficial to NASA's interest. In the event that cannot be done, the program will devote this project to analytical aspects of testing, the requirements, and the programmatic means (facilities, diagnostics, etc.) to perform a full scale test program including budgeting and scheduling.

Content: Testing support and/or analysis performed will be based upon the proceedings from the **Space Fusion Program Definition Planning Conference** and upon the FY93 Project I.2 design analysis results. The goal is to obtain a better understanding of the potential of a confinement approach(es) or, alternatively, to determine test program requirements and capabilities:

PROJECT I.4. TEST ANALYSIS/SUPPORT

- Perform/support key experiments that will provide support for preliminary feasibility test evaluations.
- Identify test issues, potential resolutions, and capabilities.

Experimental test results constitute the most important product from a space fusion technology development program. The approach recommended is to support key experiments that will assist preliminary feasibility evaluations. The test program content will be identified, and key information concerning reactor viability will be investigated. This project would provide support to a test activity which is beneficial to NASA. It must be clearly understood that without the comprehensive testing to be performed in the *Phase II: Space Fusion Feasibility Demonstration* in no way can we accomplish more than indications of the design approach soundness. There is no shortcut to obtaining the final answer. The ultimate need is for full scale testing which will cost typically several hundred million dollars – a number which is based upon the budget of terrestrial concepts used as a guide.

In the event that good test support is not possible within the time-cost constraints contained herein, the project will be devoted fully to an analysis of testing including requirements and the facilities to meet those requirements. By an analysis of the designs from Project I.2, this project provides better understandings of an optimal test program to proceed to that necessary full power demonstration step.

Products: A report on test issues, requirements, and capabilities will be prepared. Test data will be reported as applicable, and a review of the results will be provided.

PROJECT I.5. SPACE FUSION PROGRAM REVIEW CONFERENCE

Objectives: By the end of FY94 there will be an improved understanding of program options, system requirements, and test needs for the viability for two recommended confinement designs considered capable of potentially meeting space requirements. A follow-up **Space Fusion Program Review Conference** will be held to present to the technical community the results of the work accomplished in the past two years and to revise planning for presentation to NASA management.

PROJECT I.5. SPACE FUSION PROGRAM REVIEW CONFERENCE

- Present results of work accomplished in past two years.
- Revise planning.
- Prepare Phase II Program Plan.

Content: Using the results of the contracted projects plus the civil service staff, a program plan will be prepared for the conference. The conference will be similar to the initial planning conference.

Products: The product will be a document on Conference proceedings and evaluations of the Program Plan to define the Projects for Phase II. The program plan will subsequently be revised to reflect conference inputs.

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At this point, program planning preparations will have been completed, and the program will be in a position to provide NASA management the best recommendations for determining how to demonstrate fusion energy for space.

6.2 PROGRAM REVIEW WITH NASA

A senior level review will be held with NASA to present the results of the *Phase I: Program Definition Planning*. Elements of the agenda will consist of:

PROGRAM REVIEW WITH NASA

1. Assumptions, rationale, and applications of fusion energy for space.
2. The feasibility of developing fusion energy for space.
3. Space fusion reactor requirements.
4. Space fusion flight system requirements.
5. Recommended Program Plan.

The results will be presented to senior NASA management for review and direction. NASA management will be exposed to the importance of a space fusion program, to technical feasibility, and to the development program required.

The NASA action will be to provide direction for *Phase II: Space Fusion Feasibility Demonstration*.

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We now direct our attention to *Phase II: Space Fusion Feasibility Demonstration*.. This anticipates the products from Phase I, that is, the approach without being hardware specific.

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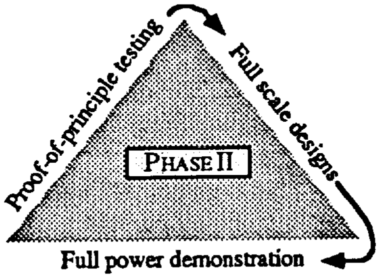
6.3 PHASE II: SPACE FUSION FEASIBILITY DEMONSTRATION

A Space Fusion Propulsion/Power Technology Development Analysis and Test Program

This *Fusion Energy for Space – Feasibility Demonstration Program* is aimed at one broad objective: demonstrate that plasma confinement and control for a space fusion reactor are possible and that fusion propulsion and power using plasma exhaust can be accomplished. A viable Program Plan has been established in Phase I and reviewed with NASA. The program now proceeds to implement the test demonstration. From the available information it is anticipated the plan will be structured along the lines as shown herein. Phase I will provide a determination of program budget and schedule. Further, an understanding of test requirements, including facilities, will have been determined.

The conversion of plasma energy to thrust while maintaining plasma burning is addressed as the major technology development goal. The generation of large electrical power in space is also a highly important technology, the development of which is essential to NASA's future. The three key elements to pursue in the demonstration of fusion for space are:



The Phase II program elements flow to demonstrate objectives of these three elements is presented in Fig. 6.

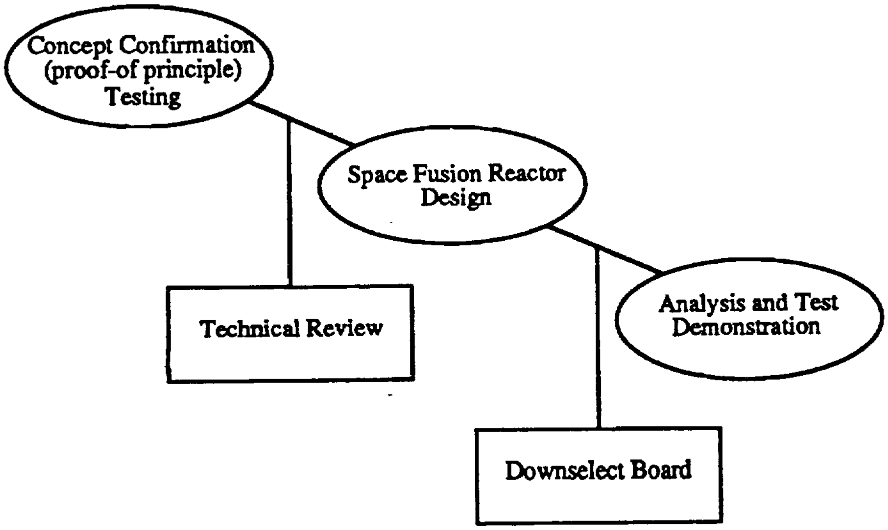


Fig. 6. Phase II program flow.

The text below provides a more detailed discussion of each of the above projects.

PHASE II PROJECTS

PROJECT II.1. CONCEPT CONFIRMATION TESTING

Objectives: In this project we perform analyses and conduct testing to validate claims and assumptions of the confinement techniques recommended for further study by the Peer Review Panel. The objective is to demonstrate concept worthiness for proceeding into a full scale design and test program.

PROJECT II.1. CONCEPT CONFIRMATION TESTING

- Validate proposed designs analyzed in Phase I and planned for Project II.2, design, and Phase II.3, full power testing:
  - –Concept A
  - –Concept B
  - –Additional Concepts?
- Establish test results versus physics predictions
- Verify readiness to proceed to Project II.2.

Content: Prior to the initiation of a full scale test program, it is important to establish the strength(s) and weakness(es) of the proposed concepts by conducting proof-of-principle tests and by analysis. This testing serves as a screen to determine applicability of the proposed concept to space missions. This is work which will not otherwise be performed unless NASA does it.

Confinement design options will have been determined in Phase I. Options for proof-of-principle include, but are not necessarily limited to:

a.) The Field Reversed Configuration. As mentioned, this design holds high promise for meeting space requirements but requires specialized testing to demonstrate that it can be advanced to a performance level that will support the space program. The LSX is an experiment located at the University of Washington (formerly at STI Optronics [Spectra Technology]) but which was recently diverted to support tokamak fueling studies. One programmatic advantage of the FRC is the high funding leverage which it provides. Considerable test history has been obtained; the FRC and its related support equipment exist including: the test facility; an experienced team which had been assembled and may possibly be readily reassembled; and neutral beam injectors which are already in existence at a national laboratory. The cost is small considering all that has been accomplished. Under this proposal the experiment would be redirected to provide important stability data for a high β reactor, that is, one making efficient use of the reactor's magnetic fields and which will consequently provide for a minimum mass configuration. The demonstration of FRC plasma stability is one key data point. Thus, this experiment would be directed toward providing important plasma stability information, thereby leveraging resources through the use of available equipment and facilities. The use of neutral beam injection equipment to stabilize plasma confinement and to increase plasma temperature is a major factor for determining the viability of this concept. Valuable information can be obtained using existing neutral beams. The follow-up work remaining then will be to demonstrate start-up and burning of the plasma.

b.) NASA pursued the Electric Field Bumpy Torus as reported in reference 2. This is possibly another consideration that the Panel could review and recommend.

c.) A new magnetic dipole concept has been recently examined and is reported in reference 1 as still another option.

d.) A plasma focus experiment that could burn completely neutron free fuels is still another option.

e.) ICF has been studied (ref. 5) and reported to offer potential for human Mars exploration missions.

f.) A revised tandem mirror has recently been studied (ref. 6) and reported to offer potential for meeting requirements as discussed herein.

g.) The above are examples; others can be expected to be forwarded as a result of the Phase I projects.

The point to be emphasized here is that there are a number of fusion concept options which can be considered for space. One charge to the Peer Review Panel will be to optimize planning for NASA's focus. We could make excellent use of their learning experience from prior programs.

The pursuit of a minimum of 2 different reactor design approaches is crucial. In Phase I, as mentioned, we may be advised that additional concepts should receive proof-of-principle testing. That is a valid approach and is a program option having merit. In that case, additional funding will be required, subject to a NASA management decision.

Products: The products will be test confirmation and test reports compiled into a technical document correlating physics principles with test results. This project validates concepts which will be selected for the design and feasibility demonstration in Phase II.

PROJECT II.2. SPACE REACTOR DESIGN

Objectives: The purpose of this project is to perform two full power space reactor designs in preparation for the construction of a reactor that will be built and tested in Project II.3:

PROJECT II.2. SPACE REACTOR DESIGN

- Reactor design and analysis:
  - –Concept A
  - –Concept B.
- Full, power feasibility requirements:
  - –80 MW
  - –250 MW.
- Propulsion and power capability.
- Low/no neutrons produced.
- Program Plan for Project II.3.

The objective is to develop two different design approaches which can proceed along an accelerated path to a full scale space propulsion/power reactor. A design option is important until physics principles have been demonstrated at full power.

Content: A competitively bid contract will be awarded to a national laboratory, university, or industry to conduct full-scale reactor design for each of two reactors capable of burning fuels emitting low/no neutrons, e.g., D-^{3}He, at a net propulsion jet power output of ~80 MW, which can be upgraded to the ~250 MW level. This design project will advise on concept feasibility and on key technological developments upon which the program should concentrate. The confinement options will be examined in depth. The actual design concepts considered for this project will be based upon the Panel's recommendations, upon the results from Phase I as agreed in the NASA Review, and from the results of Project II.1. We achieve a minimum cost program by the use of a reactor design capable of full power demonstration and by using a system designed to require a minimal number of steps to proceed to full power. The reactor will be designed to be capable of providing propulsion and/or power.

At the completion of Project II.2 a major program technical feasibility readiness review will be held to assure that planning for Project II.3 is sound.

Products: The products will consist of 2 different designs suitable for space and the design reports on those reactors. The Project Plan for *Project II.3. Analysis and Test Demonstration* will be completed.

Down selection: The down selection will be accomplished using a Down-select Board Review process. At this phase of the program, the candidate reactors will be down selected to one preferred option. A second confinement approach is recommended for development as an option.

PROJECT II.3. ANALYSIS AND TEST DEMONSTRATION

Objectives: This project answers the key question, "Can it be done?" The approach here is to use the Project II.2 design analysis results to fabricate one test reactor concept that will demonstrate burning of a suitable space fuel, like D-^{3}He, at the anticipated power levels for space, ~80 MW and ~250 MW.

PROJECT II.3. ANALYSIS AND TEST DEMONSTRATION

- Test facility modifications.
- Fabricate one reactor: 80 MW/250 MW (jet power).
- NASA-DoE Space Fusion Liaison Council established.
- Prove full power by test demonstration.

Content: For this program phase it will be important to coordinate the NASA Program with the DoE terrestrial program to assure cross fertilization of program activities and test results for mutual benefit. The Peer Review Panel membership will be used as a source for members of a Space Fusion Liaison Council, thereby maintaining technical continuity. A high level council of independent experts will be assembled to periodically meet for reviewing program activities and progress and assuring integration of program results. The relationship is presented in Fig. 7. Membership could consist of experts from universities, national laboratories, DoE, and the Air Force.

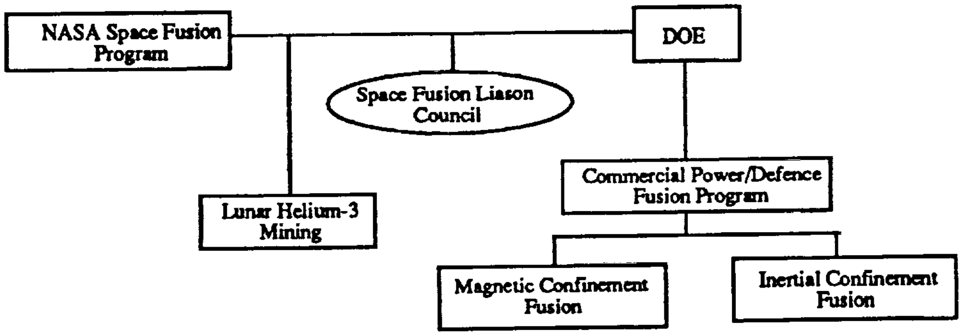


Fig. 7. Space Fusion Liaison Council for the NASA space program and the DoE terrestrial program.

Products: The product of Phase II will be the demonstration of a full scale propulsion reactor. Further, from the experience and knowledge gained in the design, fabrication, and test of that reactor, we will have developed the design criteria for a space flight fusion propulsion system. Specifically then, the project products will be test result documents and design criteria presented in comprehensive manuals and specifications.

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The program duration is not specified since this is a research program, and the rate of progress cannot be extrapolated at this early time. Much will depend on the cooperation of mother nature and on the budget. To provide a feeling, the construction of such a reactor can be expected to require about 2 years to 5 years, if adequately funded. The fabrication time and costs are functions of concepts selected. Testing can commence following reactor construction, and initial answers should follow in a fairly quick time-frame. Major program milestones will be established to assure that progress is being made and that this is not an open-ended program.

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7.0 THE NEXT STEPS

The goal of the Space Fusion Feasibility Program will be complete upon demonstrating the feasibility of a fusion reactor(s) capable of operating in a space environment at space power levels to produce propulsion and power. At the conclusion of Step 1 a major symposium will be held on the program and on the direction of fusion energy for space. Then the program will be in a position to proceed into *Step II, the Fusion Flight Systems Development* to conduct detailed analyses of a fusion powered space ship and to conduct integrated reactor and flight system testing. *Step III, Flight Qualification* testing will follow. The final step is *Step IV, Flight Operations.*

8.0 PROGRAM PRODUCTS

The products to be produced by each of the project tasks are summarized in Fig. 8:

Projects	Objective	Products
PHASE I: PROGRAM DEFINITION PLANNING		
I.1. Program Planning Conference	Obtain peer review of ideas for space reactor design options.	Conference proceedings showing ranked design options.
I.2. Space Reactor Design Analysis	Evaluate characteristics of two most promising designs in greater depth.	Two reports on analyses concerning feasibility.
I.3. Vehicle System Requirements Analysis	Evaluate system feasibility by determining indication of α_p.	Report on system integration aspects of a space fusion vehicle and α_p.
I.4. Test Analysis/Support	Obtain test understandings. Improve test knowledge.	Test planning and evaluations. Report presenting test data/information.
I.5. Review Conference	Develop optimized program.	Program plan and conference proceedings.
NASA REVIEW	Present planning results to NASA management	Decision on proceeding with Phase II.
PHASE II: SPACE FUSION FEASIBILITY DEMONSTRATION		
II.1. Concept Confirmation Testing	Validate physics of recommended options.	Test data and test reports which discuss viability of concepts as space reactors before proceeding to Project II.2.
II.2. Space Reactor Design	Design 2 flight reactors.	Space reactor design for fabrication in Project II.3.
II.3. Analysis and Test Demonstration	Show feasibility: Demonstrate required power levels and propulsion.	Reports on test data demonstrating fusion's viability as a space energy source for propulsion and power.

Fig. 8. Program product summary, Fusion Energy for Space: Feasibility Demonstration Program.

9.0 SCHEDULE

The schedule for *Phase I: Program Definition Planning* and *Phase II: Fusion Feasibility Demonstration* are shown respectively in Fig. 9a and 9b.

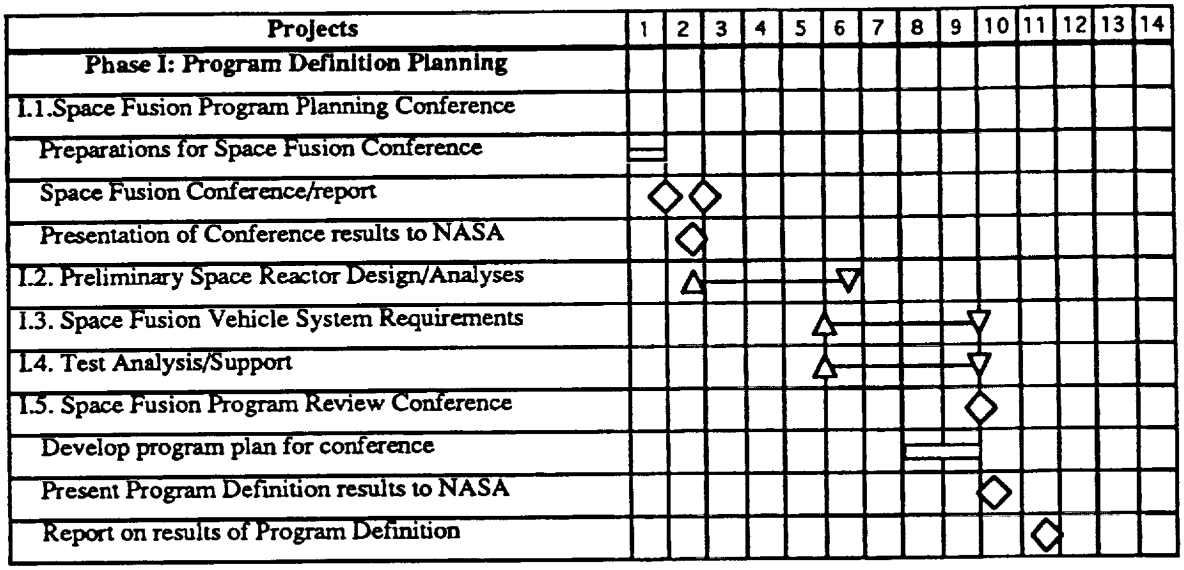

Fig. 9a. Schedule for *Phase I: Program Definition Planning*. (Time shown in quarters of year from start.)

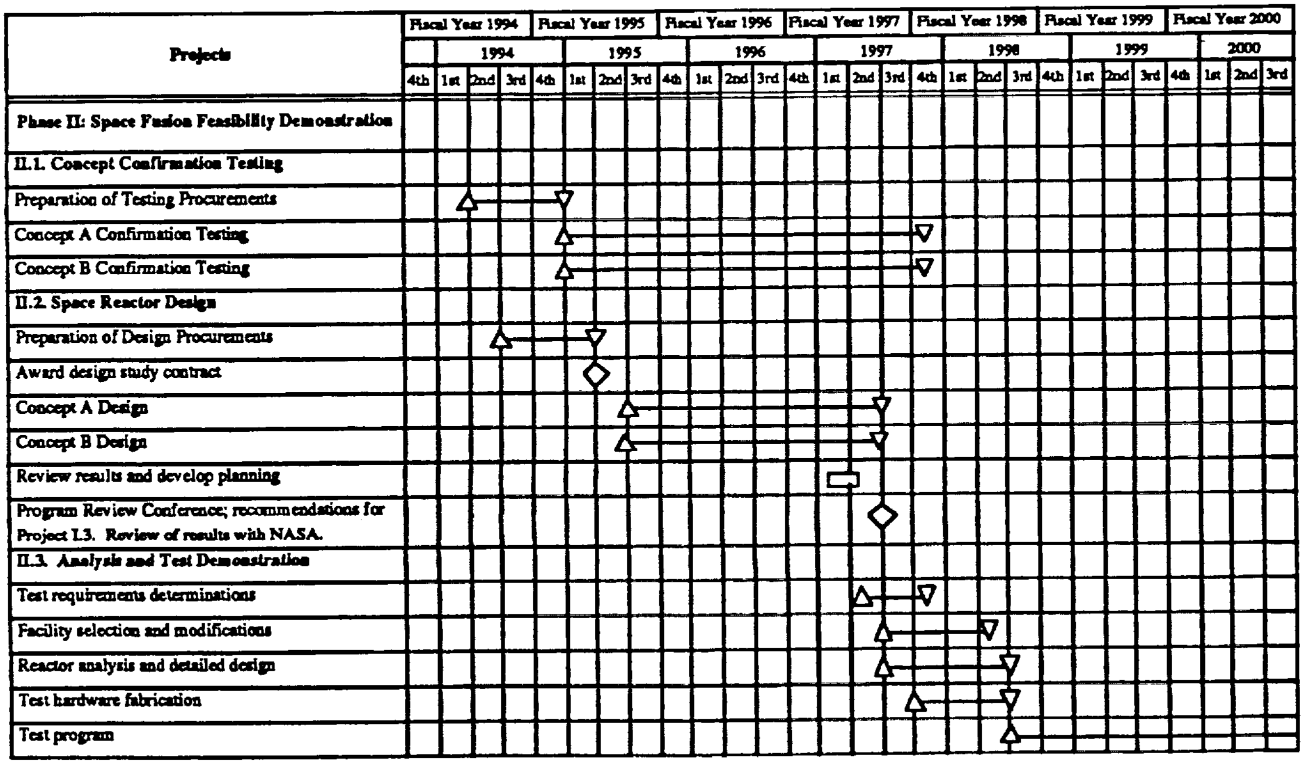

Fig. 9b. Schedule for *Phase II: Space Fusion Feasibility Demonstration.*

The program schedule, as presented, includes several assumptions in order to occur at the indicated pace. First, there will be adequate funding. The anticipated funding is presented in the following section. The funding level is expected to depend to some extent upon the confinement design. The funding level must match work objectives. For Phase I to commence on a sound basis, the **Space Fusion Program Planning Conference** is the program cornerstone. The Conference and design tasks will provide improved tuning of the schedule and costs. The assumption is made that the personnel and equipment are readily available to support the selected concepts as, for example, in the case of the FRC-LSX, if that is an option recommended for space consideration. The analysis and design options of suitable concepts for space are to be performed through a competitively bid process, requiring the NASA staff to prepare the specifications and the procurement packages in a timely manner. To facilitate adherence to the schedule a well integrated NASA-staff with non-NASA staff is envisioned as the optimal management approach. These staff members are assumed to be readily available. Civil servants stationed at experimental facilities will be another important feature. The continuation of a program advisory council throughout Phase II will be a valuable asset to maintain a properly focused activity and will be implemented.

In Phase II testing the use of a space environmental test chamber is not required for reactor technology demonstration purposes. To demonstrate propulsion/power and to qualify the hardware in a flight environment, the vacuum facility is essential. Hence, for the demonstration of fusion propulsion/power principles, the schedule assumption is that the reactor will fit into existing suitable facilities, e.g., the LLNL MFTF-B test chamber or the NASA space test facility at Plumbrook. There may be other available facilities. The assumption is also made that extensive facility modifications and refurbishment are not required. A better understanding of program test approaches and other considerations will be obtained by Project I.4 in which facility requirements and capabilities will be analyzed. Concurrent activity of design with facility modifications is shown in order to quickly place the facility into a state of readiness and to accept the reactor once fabricated. Additional time is allowed to make reactor design changes since test integration activities and testing could prove the need. The schedules are estimates, based upon similar efforts required in the past.

A properly funded program should be providing valuable answers within 10 years.

10.0 BUDGET

The recommended budget estimate presented below in Fig. 10 is expected to approximately fund the program outlined above. The budget process for the feasibility demonstration will be better quantified from the Phase I projects, as discussed in this proposal. The one presented below is considered to be a reasonable first approximation based upon the funding experience gained from the terrestrial power program and the work performed in reference 1.

A structured program approach has been developed in this proposal to minimize program risk. There is a two-year planning activity. Extensive use is made of the available expertise. Concept verification is required prior to committing to a design. Two flight designs are performed to provide a back-up. At least one option is maintained up to the demonstration phase. A coordinating liaison council between the NASA and DoE programs is established. There will be an advisory panel throughout the two phases. During the feasibility demonstration, another option is recommended and could be considered, perhaps proceeding at a slower pace.

The earliest demonstration of the capability will provide the greatest savings from a total Agency perspective, once the high performance mission capability has been attained. Hence, the program uses an accelerated test approach. The main point is that the program places resources where the greatest benefit will result, namely, from the analysis and the testing of those plasma confinement approaches which will be suitable for space propulsion reactors. The importance of full scale testing is reiterated.

The funding level presented is an estimate, as mentioned. A reduced Phase I funding level will still start the program. The impact to Phase II is that we will either proceed on schedule with less planning rigor, make greater use of civil servants, or delay the start of Phase II. The financial disadvantage to NASA of lower funding is that overall higher costs to the Agency will be incurred. The deferral is costly because fusion will provide significant cost returns and safety benefits to NASA. Additional funds will permit more than two concepts to be investigated in the proof-of-principle testing and will permit more than one space fusion confinement approach to be demonstrated in Project II.3. Both are worthwhile. Additional funding will, consequently, lower program risk.

PROGRAM PROJECTS*	FY93	FY94	FY95	FY96	FY97	FY98	FY99	FY00
PHASE I: PROGRAM DEFINITION PLANNING								
I.1. Program Planning Conference	0.030M							
I.2. Space Reactor design Analysis	0.200M							
I.3. Vehicle System Requirements Analysis		0.100M						
I.4. Test Analysis/Support		0.150M						
I.5. Review Conference		0.050M						
PHASE I: PROGRAM DEFINITION PLANNING								
II.1. Concept Confirmation Testing (2 concepts)			5M	5M				
II.2. Space Reactor Design (2 concepts)			1.5M	1.5M				
II.3. Analysis and Test Demonstration (1 concept)					50M	150M	200M	250M
Total	0.230M	0.300M	6.5M	6.5M	50M	150M	200M	250M

* Funding is in FY92 dollars. A lower funding level reduces program planning or deferral of results.

Fig. 10. Estimated budget for space fusion program, *Step I: Fusion Energy for Space – Feasibility Demonstration.*

11.0 CONCLUDING REMARKS

This proposal presents an issue critical to the future of space science and exploration beyond LEO missions. A solution has been proposed to address the issue, and a program has been defined to pursue the solution. The development of this high energy space propulsion and power performance capability will complement chemical propulsion capabilities and not supplant that capability where energy requirements are low. This development is solely NASA's responsibility. Let us act positively to meet the challenge and to provide a sound future for: the scientific understanding of our solar system, space science, exploration, and the commercialization of space.

12.0 QUALIFICATIONS

Mr. Schulze has an academic background in physics (BS, University of Chicago, '58) with over 33 years of practical space experience including propulsion research, applied propulsion development, program management of propulsion systems including the Gemini spacecraft propulsion systems, and the initiation of technology programs. He has conducted a self initiated study on the application of fusion to space and prepared a comprehensive report (ref. 1). He has worked closely with the fusion community where space interests are involved. He resided at the Lawrence Livermore National Laboratory for the study of fusion for space as well as at the University of Illinois and the University of Wisconsin. He has visited and discussed this subject at the Los Alamos National Laboratory and the Princeton Plasma Physics National Laboratory also and with leading fusion researchers throughout the US and abroad. He was an invited speaker for the Second International Aneutronic Fusion Symposium. At the request of the University of California he participated in the evaluation of the ARIES III (D-^{3}He) reactor study. He has discussed this topic with industry, both aerospace and the fusion community, and with the science user community.

13.0 PEER REVIEW

The names of individuals are provided below for further discussion of the proposal contents. Letters of endorsement from experts in the fusion community and the science community support the proposal content as expressed by reviews of *Fusion Energy for Space Missions in the 21st Century* (ref. 1) (draft version). Since that review, numerous requests for reference 1, both nationally and internationally, have been made. Conversations with persons in the space community have been very supportive.

14.0 REFERENCES

1. *Fusion Energy for Space Missions in the* 21^{st} *Century*, Norman R. Schulze, NASA Headquarters, TM 4297 and TM 42981.
2. *The NASA-Lewis Program on Fusion energy for Space Power*, 1958-1978, Norman R. Schulze and J. Reece Roth, American Nuclear Society, *Fusion Technology*, 19, Jan. 1991
3. "Lunar Source of ^{3}He for Commercial Fusion Power, "Fusion *Technology*, 10, 167, L. Wittenberg, J. Santarius, G. Kulcinski (1986)
4. "^{3}He Sources for D-^{3}He Fusion Power," G. H. Miley, *Nuclear Instruments and Methods in Physics Research*, A271, 197-202, North-Holland Amsterdam (1988)
5. "The VISTA Spacecraft–Advantages of ICF for Interplanetary Fusion Propulsion Applications," C. D. Orth, Lawrence Livermore National Laboratory, UCRL-96676, October 2, 1987
6. "Mirror Fusion Propulsion System (MFPS): An Option for the Space Exploration Initiative (SEI)," S. Carpenter and M. Devaney, 43^{rd} Congress of the International Astronautical Federation, Washington, August-September 1992

15.0 DEFINITIONS

^{3}He	Helium 3, light isotope of helium (1 neutron)
α_p	Specific power
AU	Astronomical unit = 93 million miles
B	Boron
β	Ratio of plasma pressure to magnetic field pressure
D	Deuterium, heavy isotope of hydrogen (2 neutrons)
Δv	Velocity change, km/sec
FRC	Field Reversed Configuration
γ	Payload mass fraction
ICF	Inertial confinement fusion
kg	Kilograms
kN	Kilo-Newton's
kW	Kilowatts
LEO	Low Earth Orbit
LLNL	Lawrence Livermore National Laboratory
MCF	Magnetic confinement fusion
MeV	Million electron volts
MFPS	Mirror Fusion Propulsion System
MFTF-B	Magnetic Fusion Test Facility used to test the Tandem Mirror
M_o	Initial vehicle mass
M_p	Propellant mass
MT	Metric ton
MW	Megawatts
NEP	Nuclear Electric Propulsion
P	Proton
P_j	Jet power
SEI	Space Exploration Initiative
SFE	Space Fusion Energy – a term used to designate the special attributes of fusion energy for space propulsion and power
t	Round trip flight time
T	Tritium, heavy isotope of hydrogen (3 neutrons)

16.0 REFERENCES FOR PROPOSAL ENDORSEMENT

The following individuals may be contacted regarding this proposal entitled "Fusion Energy for Space." They have reviewed the proposal and fully support this approach, within the context of their expertise and responsibility.

This is considered a program which is critical to NASA's future and, hence, to the future of the Unites States space program. Consequently, the program funding should be among the highest priority level. The approach is considered sound, and the planning is as reasonable as can be accomplished with the current state of knowledge.

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