San Diego, California, June 5–9, 2005
Paper 1074

# Lessons Learned (?) From 50 Years of U.S. Space Fission Power Development

*Sherrell R. Greene*
*Oak Ridge National Laboratory*
*1 Bethel Valley Road*
*Oak Ridge, Tennessee 37831*
*Tel: (865) 547-0626, fax: (865) 574-7879, e-mail: greenesr@ornl.gov*

**Abstract**—*The President's announcement of the Vision For Space Exploration has spawned new interest in space fission power technology. The United States' efforts to employ fission power systems for aerospace propulsion and power generation began in the late 1940s. Yet, during the ensuing 50+ years, and after several program starts and terminations, the United States has actually flown only one space fission power system—the SNAP-10A in 1965. Why? This paper presents one technologist's and amateur historian's assessment of the technical, programmatic, institutional, and cultural lessons that may be gleaned from this experience. The author posits that incorporation of these lessons into current and future space fission power and propulsion programs will reduce technical and programmatic risk and enhance the probability of programmatic and mission success as the United States once again reaches for the moon, Mars, and beyond.*

## I. INTRODUCTION

The Aircraft Nuclear Propulsion (ANP) program, had its birth in the late 1940s, and may truly be characterized as the first "aero-space" nuclear program. During the ensuing 50 years, the United States has pursued a number of space nuclear power and propulsion programs, such as the Systems for Nuclear Auxiliary Power (SNAP), Rover/Nuclear Engine for Rocket Vehicle Application (NERVA), and more recently, the SP-100 program that terminated in the early 1990s. Nevertheless, the United States has actually flown only one space reactor power system—the SNAP-10A launched in 1965. The President's announcement of the Vision For Space Exploration[1] (VSE) has spawned new interest in space fission power technology. Given the current resurgence of interest in space power and propulsion reactor technology, it is both prudent and timely to briefly reflect on prior experience and to discuss the key lessons and observations from these efforts that may be of value in guiding current and future efforts. This paper provides a very brief summary of some key observations and lessons-learned from previous U.S. space power and propulsion programs. These lessons can be summarized in three "mega-lessons:"

- A viable, long-term, sustained technology development approach is required.
- A fresh look is needed in the areas of surface power and nuclear propulsion.
- System performance requirements must be based on sound engineering.

## II. A SUSTAINED, LONG-TERM PROGRAM IS REQUIRED FOR SUCCESS

### *II.A. The Technology Performance Box*

More often than not, system designers finds themselves constrained by the performance capabilities of materials. The U.S. space reactor design community has been operating within the same, narrowly defined space power reactor technology "performance box" since the mid-1960s. The performance box is principally defined by (1) the performance capabilities (temperature, radiation, and chemical compatibility) of "entry-level" refractory metal structural alloys such as Nb-1Zr, (2) the structural mechanical properties of lithium hydride (LiH) shielding material (principally the 600–800 K temperature operating limit) for the shield, and (3) the fuel performance limits of $UO_2$ and UN fuel. In many respects, the constraints posed by structural and shield materials are more severe than those posed by $UO_2$ and UN fuels—the principal near-term candidates for space fission power system applications. These three considerations present the principal design constraints to space surface power and NEP system designers today. This realization was apparent as long ago as 1966 (Ref. 2). The neutronic, thermo-mechanical, and chemical performance limits of the "best-available" fuels, structural materials, and shielding materials have remained unchanged from forty years principally because the lack of sustained development focus has left the design community with same materials options that existed in the mid-1960s. Materials limitations have also greatly inhibited the development of compact, high-temperature dynamic power conversion technologies that will be required for high-power applications.

***Lesson: The development of improved materials (structural and shielding) should be given very high priority in any new space reactor development program.***

*II.B. The "Requirements Merry Go Round" and "Reaching Too Far Too Soon"*

Space reactor technology development is a long-term, resource-intensive activity that has proven to be difficult to maintain in the absence of very strong mission pull. The history of U.S. space reactor technology development during the past several decades has been characterized by periods of intense activity interspersed by lengthy periods during which programmatic, institutional, human, and physical infrastructure decayed and regressed. This cycle is depicted in Fig. 1. Many factors have contributed to this experience. Perhaps one of the most important contributing factors is the tendency to require very substantial performance enhancements (relative to contemporary technology capabilities) for the very first mission application in order to justify the significant investment required to field the system (Fig. 2). This tendency was recognized and articulated in the 1960s (Ref. 3):

> *Realization of U.S. space program potentials in many areas is dependent upon the timely achievement of nuclear space capabilities. This implies an orderly development and qualification program over a period of years, adequately supported to ensure that the necessary capabilities will, in fact, exist at the time when they are needed.*

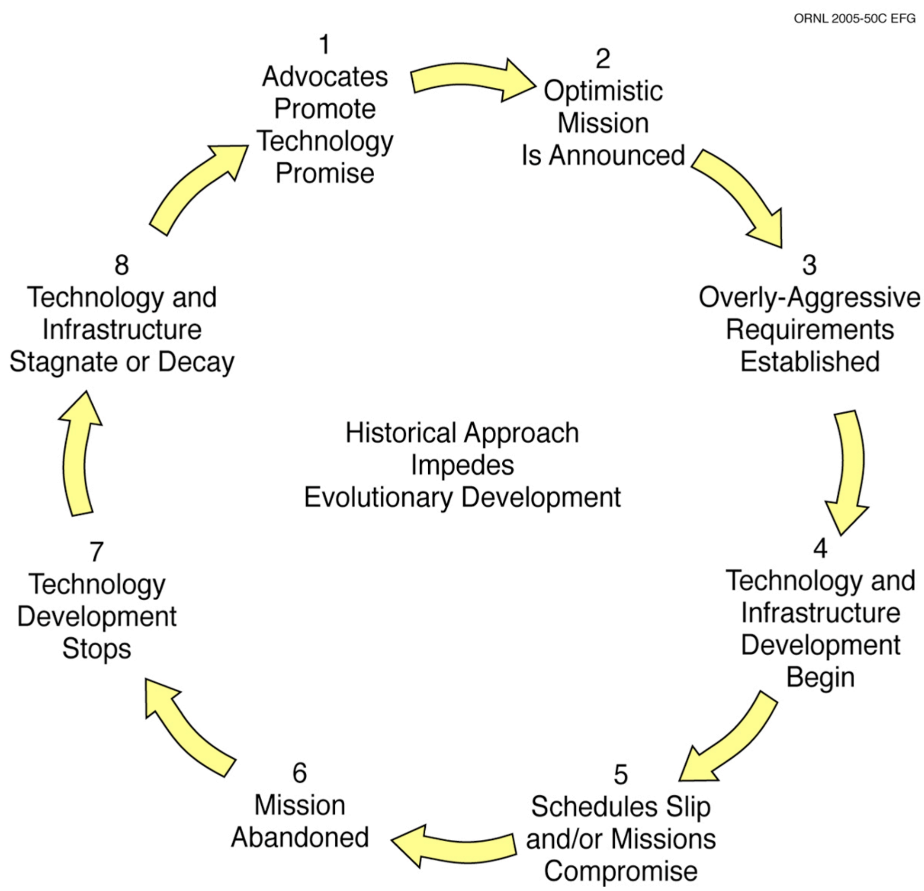


**Fig. 1. Historical cycle impedes technology development.**

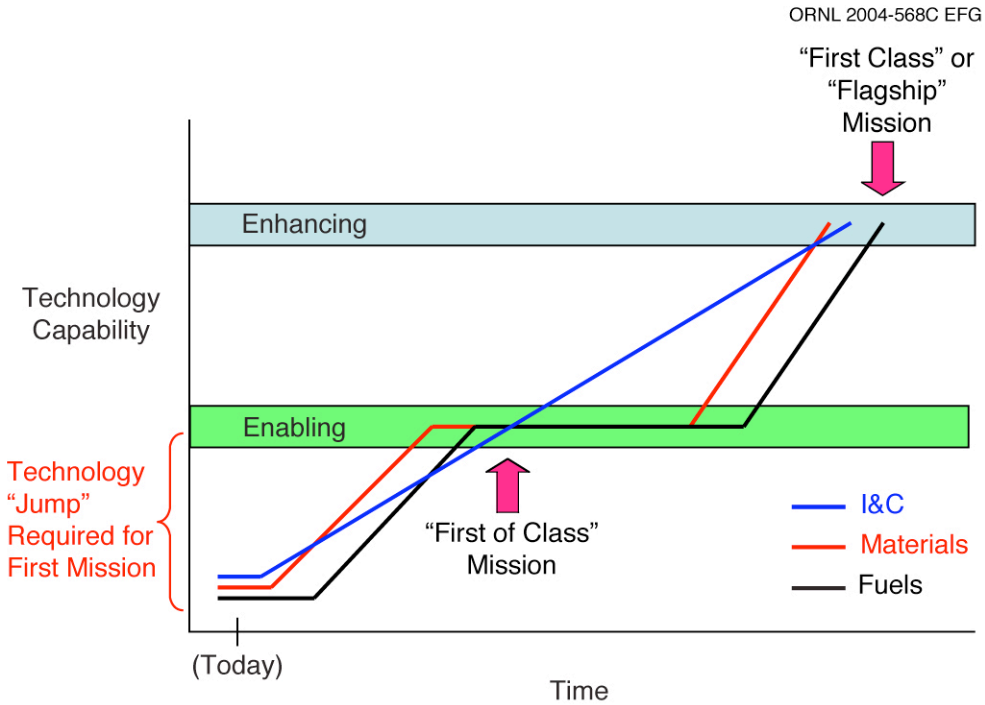


**Fig. 2. Technology advancements enable missions.**

*The basic problem, which has been characterized by Congressman Melvin Price and Commissioner James T. Ramey of the AEC as the "requirements merry-go-round," is to find some means of bringing these capabilities along to the point where user agencies have sufficient confidence in them to establish a requirement for their use in operational systems. The developing agency, and more specifically the AEC, has had difficulties each year during the annual budget cycle in justifying the money required to bring nuclear capabilities to this stage because no firm requirement could be cited. . . .*

*A major difficulty in the past which continues to be a problem in current programs, has been over-emphasis on the ultimate or "design point" performance desired in the eventual operational system and the lack of appreciation for and support of the intermediate steps in the developmental process necessary to the achievement of this final goal. In attempts to justify the funds needed for continued development, and to prove that the system being developed will be "useful" in an operational sense, there has been a tendency to set system requirements which are too advanced in terms of the status and pace of the technological effort. The basic problem has been that the supporting technology development essential to realization of these requirements could not be achieved on the schedule established for the operational system without the benefit of prior development cycle experience.*

***Lesson: The tendency to reach too far too fast—to set the initial performance requirements for the "first-of-class" mission far above those of the present state of technology and to establish mission schedules that fail to facilitate anything other than a series of successful outcomes within highly challenging technology development programs must be avoided if acceptable levels of programmatic risk are to be maintained.***

### *II.C. Mission Pull and Technology Push*

Voss and Dix[4] recount in an interesting chronicle of the SNAP program, that in the final analysis, the mission and mission pull for SNAP-10A vanished, and the SNAP-10A actually flew as a technology demonstrator because the funding authority within Congress demanded that it fly.[4] A cursory review of the history of U.S. space reactor technology development would conclude that "base technology" programs tend to be short-lived in the absence of specific missions, and unfortunately, tend to dwindle and die away under the financial competition posed by missions once the missions are announced. This reality relates closely to the "Requirements Merry-Go-Round" issue previously discussed. What is needed is "mission pull" for near-term technologies that enable "first-of-class" missions and commitment to develop the longer-term technologies that enhance mission options or open entirely new classes of missions—even in the absence of an announced set or suite of official missions.

As noted by Layton, Grey, et al.[5]

> *"The 35-year history of compact fission reactors reveals two essential lessons: (1) there is no substitute for continuity of analysis, research, and technology efforts to define prospective areas for applications of advanced nuclear systems, and (2) development of operational prototypes should not proceed unless and until specific missions are approved to which nuclear reactor systems are most effectively applicable. In any case, before a mission commitment is made, a sound technology base must be available; otherwise uncertainties will probably drive costs and risks to unreasonable levels... Although an analysis, research, and technology program for advanced nuclear systems can and should be initiated with modest funding, constancy of financial support for 10 to 15 years is essential."*

The United States will, at some point in the future, return to the moon, and journey to Mars at a later date. The foundation for success of that program must be laid well in advance of its advent. The Wright brothers did not expect or demand their Flyer soar from Kitty Hawk to Dayton on it's first flight. Powered flight began with a 105-ft., 3.5 s flight down Kill Devil Hill. The U.S. has accomplished remarkable feats with the relatively meager (a few to several hundred We) power levels afforded by radioisotope power systems. How might science and human exploration be furthered if a robust and compact 20 kW(e) fission power system were available? Might a low-power space reactor system that employs available, fabricable and well-understood materials and technologies, serve as a good point of departure for long-term evolutionary development of space fission power and propulsion systems? Given our radioisotope power system experience, one has to wonder if applications not yet envisioned might emerge if such a system were available. Such an approach is plausible if an evolutionary "long view" is taken. If, however, truly revolutionary performance levels are required of the very first systems that are fielded, evolutionary development paradigms will not yield the required success.

***Lesson: A way must be found to provide adequate long-term, stable support for space reactor technology development that is not dependent on the presence of a specific announced official mission by a mission agency and is not threatened by the financial competition posed by such missions once they are announced. Modest investments over longer periods are necessary if the United States is to break out of the "Requirements Merry-Go-Round."***

### *II.D. Technology Decay and Regression*

As previously noted, the history of U.S. space reactor development is characterized by episodic or cyclical periods of intense activity interspersed with long periods of inactivity. The result of this phenomenon is that every new space reactor program must expend significant effort and resources simply to <u>recapture</u> critical technologies and capabilities that have decayed or vanished since the last program ended. For example, the U.S. cannot currently fabricate the fuel employed in the SNAP-10A. Some of the refractory metal alloys developed in the 1960s and 1970s have not been successfully reproduced during the past two decades—even though significant efforts were expended to do so. Modern-day electronic components are, in many cases, <u>less</u> radiation tolerant than older instrumentation and control technologies—technologies that are no longer produced in the United States. Technology decay is a real phenomenon:[6]

> *... In the future, steps must be taken to preserve in a better way the results of analysis, research and technology work which may not be used directly or immediately in ongoing programs. The idea that technology brought to a certain readiness can be placed on the shelf against the day when it will be needed has again been shown to be fallacious. Analysis and technology are most perishable commodities.*
>
> *Nevertheless, proper planning can mitigate the loss significantly, and can indeed be affective in preserving the essential knowledge... A revival of interest in so important a subject as Nuclear Space Power Reactors has been publicized widely in recent months, but the return of the dollars spent on past efforts, is so immodestly low that practically the entire analysis, research, technology and development basis for reinitiating the work has been wasted.*

Modern engineering standards have become much more rigorous in many instances—particularly with regard to documentation. Standards of documentation that were "standard practice" two or three decades ago are no longer considered acceptable or in some cases even very useful.

***Lesson: Critical space reactor technologies and technical infrastructure should be identified, and some "lifeline" level of support should be provided to preserve and maintain essential knowledge and capabilities once they are recaptured or developed. Substantial priority should be given to documentation and archival of RD&D***

***results and products in a form that will be secure and accessible to future programs.***

### *II.E. The Human Element*

Engineers turn dreams into reality. "Engineering is the art or science of making practical."[7]"Engineering is the art of organizing and directing men and controlling the forces and materials of nature for the benefit of the human race."[8] This is nowhere more true than in the field of space nuclear power engineering. Engineering is more than a collection of information, data, methods, and logic. There are innumerable examples of cases in which two groups are given identical specifications and documents, and one group fails while the other flourishes. Why? The reason is often, at least in part, that *all vital knowledge cannot be written down*. The written word, valuable as it is, is no substitute for "hands-on" experience. Owning a recipe does not make one a cook. Experience builds intuitive abilities and catalyzes creativity in a manner that "book learning" alone cannot. This is the "art" and "craft" in engineering. Layton, Grey, et al. indicated in their 1982 report,[5]

> *"... It is evident, further, that there exists a wealth of information available from research, technology and development programs undertaken in the past, and that that information is exorably disappearing as publications become less available and personnel who were involved in these activities retire or move on to other fields of endeavor."*

Forty years has passed since SNAP-10A was launched and the NERVA/Rover NTP engines were built and tested. Twenty years have passed since the demise of SP-100 program. The number of truly experienced space reactor veterans is dwindling year by year. Space reactor technology recapture and evolution becomes more difficult with every stilled heart. Action is required on an urgent basis to mentor and maintain a new generation of space reactor engineers if the United States is to avoid costly missteps and mistakes in its next space reactor development program.

***Lesson: An effort should be initiated to identify and engage veteran space reactor engineers, designers, and technicians (many of who are retired) to document their knowledge and to transfer their knowledge and skill to a new generation of space reactor engineers. This will necessitate sustained long-term funding, and design and testing activities will be required to maintain the skills of this core group and position the United States to be successful in future space reactor development programs.***

## III. A FRESH LOOK IS IN ORDER

### *III.A. The Pursuit of Higher Temperatures*

The single most important space power system performance parameter is arguably "specific mass" (typically expressed in units of kg/kW(e)—particularly for nuclear electric propulsion systems. The history of space reactor power system development is perhaps best characterized as the pursuit of lower and lower specific masses. The most commonly pursued approach for reducing system-specific mass is to increase system operating temperatures. The implications of this approach were characterized in one 1983 study[9] as follows:

> *The incentive for achieving higher operating temperatures is the higher Carnot efficiency and higher system power-to-mass ratio generally associated with higher temperatures. Historically, however, extracting added performance by using extreme cycle temperatures has given rise to more technical and programmatic problems than any other design variable. Higher operating temperatures are the sworn enemy of highly reliable, long-lifetime systems. . . .*
>
> *A desirable target for the development of reactor and power conversion subsystems for the foreseeable future (the next 5 to 10 years) is in the temperature range of 920 to 1250 K (1200 to 1800°F).*

System operating temperature also has a strong impact of heat rejection system (radiator) size and mass. (Higher rejection temperatures enable lower mass radiators.) One practical implication of this quest for higher and higher temperatures is that it typically drives one to employ more and more exotic structural (refractory metal) materials. These alloys are often very intolerant of oxygen and require very specialized and expensive ground production, fabrication, and testing facilities.

***Lesson: The connection between system operating temperature; technology requirements; and developmental costs, schedules, and risks must be clearly understood. The programmatic risk associated with pursuing every-increasing operating temperatures must be aggressively managed if future space fission power programs are to be successful.***

### *III.B. Fast vs Thermal Spectrum Reactors and the Lack of Fast-Spectrum Ground Test Reactors*

Practically all modern space power reactor concepts employ fast-neutron-spectrum core designs. A key infrastructure element in reactor system development is traditionally a ground-test reactor of appropriate spectral quality in which prototypic (enrichments, morphologies, dimensions, etc.) fuels and materials are irradiated at prototypic operating conditions (power densities, temperatures, etc.). While "sheathed" or "flux-tailored" capsule experiments in thermal reactors can provide some of the correct conditions, it is generally not possible to obtain truly prototypic fast reactor conditions (nuclear, thermal, thermomechanical) in thermal-spectrum test reactors. Thus, a traditional approach for the development of fast-spectrum space reactors would require a fast reactor ground test infrastructure (reactor and post-irradiation examination facilities).

The United States no longer has an operating fast-spectrum reactor. (Multiple DOE facilities are capable of performing irradiated fuels and materials examination and characterization.) *Thus, the continued pursuit of fast-spectrum space reactor technology in the absence of a plan for reestablishment of a fast-flux testing and irradiation capability is a potentially major source of programmatic risk.*

The only space reactor that the United States has ever flown (SNAP-10A) was a small, lightweight thermal/epi-thermal-spectrum reactor. Why then, have fast-spectrum concepts come to dominate the space reactor design space? The answer is related to the pursuit of low-mass, high-power, long-life systems. With respect to the mass consideration:

1. Low mass requires high Carnot efficiencies.
2. High Carnot efficiencies require high temperatures.
3. High temperatures require refractory metal structural materials (and often liquid alkali metal coolants).
4. Traditional refractory metals (such as Nb-1Zr) and alkali metal coolants have high thermal neutron cross-sections but lower fast-neutron cross-sections.
5. Therefore, use of fast-spectrum reactors facilitates high temperatures and reduced system masses.

This trend was recognized early in the development of space reactor power systems.[2]

> *This high-temperature trend has been reflected in the development of new materials; much metallurgical activity has been directed towards developing materials with high strength properties at higher and higher temperatures. But for reactor applications, these materials must be compatible with fissionable fuels as well as with high performance coolants. The number of materials applicable to reactor technology becomes small as the temperature requirements increase; it becomes vanishingly small when constrained by the additional requirements for low neutron absorption properties. Unfortunately, the major elements in high-temperature alloys tend to have significant high thermal neutron absorption cross sections. Consequently, fast reactors are becoming the dominant technology for space applications because they are relatively insensitive to thermal neutron absorption effects, thereby allowing materials selection on the basis of metallurgical properties alone."*

With respect to achieving higher powers and long-life:

1. Higher power and longer life requires increased integrated energy production.
2. Increased energy production requires more fissions.
3. Increased total fissions are facilitated by either increasing the fractional percentage of the fissile atoms in the fuel that are fissioned (i.e., increasing the "burnup" limit of the fuel) or adding more fissile atoms (fuel)—or both.
4. Because the maximum burnup limit of traditional fuels has remained relatively constant, the approach to increasing power and lifetime has been to add fuel to the reactor.
5. For the same fuel inventory, thermal reactors (which require moderator materials) will have higher mass than fast reactors (which have no moderator) and will be somewhat larger.
6. Larger (physically) reactors require larger (and heavier) shields.

Another advantage to the use of fast-spectrum reactors is the viability of "simple" spectral-shift methods for assurance of water-immersion subcriticality in the event of a launch accident.

The successful development of a fast-flux space reactor system in the absence of a fast-flux test reactor infrastructure will be a significant challenge. From a programmatic risk perspective, a decision with respect to continued pursuit of fast reactor technology involves balancing overall programmatic risk (system performance, mission success, cost, and schedule) of the fast reactor and thermal reactor approach. Is it "worth" sacrificing some reactor system performance and mass (using thermal reactors) if the need for an expensive infrastructure element (a fast-flux ground test reactor) could be avoided? What are the cost and schedule (and cost and schedule risk) implications of placing a fast-flux test reactor capability in place? What are the technical and programmatic risks associated with using approaches other than fast ground test reactors to supply fast spectrum space reactor developmental testing and qualification functions? The potential options include: (1) use accelerator-based neutron sources within the United States, (2) utilize one or more of the dwindling number of foreign fast-flux test reactors, and (3) explore the use of "flux-tailored" experiments in domestic thermal-spectrum reactors.

***Lesson: A serious evaluation of the technical and programmatic risk implications of the three options noted in the preceding paragraph for development of fast-spectrum space reactors should be conducted in the very near future. The programmatic risks of pursuing fast reactors in the absence of a domestic fast flux test reactor infrastructure should be weighed against the programmatic risks of utilizing thermal-spectrum reactors for which a domestic development infrastructure does exist. The results of the evaluation should directly influence the technical direction pursued for development of the next generation of space fission power systems.***

### *III.C. The Historical Focus on In-space Rather Than Surface Power Systems*

Even a cursory literature review of the space nuclear power arena over the past several decades will reveal that much more attention and focus has been placed on in-space power systems than surface power systems. While a number of pre-conceptual or notional mission architecture studies have been performed, very limited detailed engineering design for lunar and Mars surface reactor power systems has been performed in the past. The limited analyses available have highlighted the pervasive nature of two key surface power-specific design considerations (shielding and waste heat rejection) on overall system design and mission architecture. Significant near-term work will be required to raise our understanding of the specific issues associated with design of a truly functional lunar or Martian surface power system to the level required to understand key system adaptability issues. The belief that in-space systems can be easily and readily adapted to planetary surface applications is perhaps appropriately termed "an act of faith" until such time as these detailed analyses are completed. Furthermore, the radically-different chemical environments of the Moon and Mars will present interesting design challenges if a single common surface power system is desired for use in both environments. The relative abundance of oxygen and the aggressive chemical nature of the Martian environment may preclude the use of materials systems that might be very acceptable on the moon.

***Lesson: Much more detailed engineering evaluations of surface power system architectures and requirements are required to inform decisions with respect to the adaptability of in-space systems for planetary surface applications and the cross-adaptability of lunar and Martian surface power systems.***

### *III.D. The Forgotten Shield*

When developing space reactor power systems, there is a natural and understandable tendency to focus on the reactor itself. This history of U.S. space power programs supports the conclusion that there is a tendency on the part of the reactor designer to view the shield as an "accessory" to the reactor that is engineered after the fact to comply with the reactor system. There is also a tendency to view the shield as a simple structure. Not so. In addition to performing the central shielding function, the shield often serves as a structural member whose integration with the overall power system and vehicle design is a complex task. The SNAP-10A shield was a relatively simple component. However, subsequent (higher power) in-space power reactor designs have required increasingly complex shield designs. A review of SP-100 shield drawings from late in the program indicates that the shield was to be comprised of more than 600 individual piece/parts. That said, recent analysis of SP-100 closeout documentation indicates that the shield design was much less mature than is commonly believed. LiH has been the mainstay neutron shielding material for space reactor shields since the dawn of their development. However, due to the physical properties of that material, its temperature must be maintained between 600–800 K if undesirable embrittlement or swelling behaviors are to be avoided. Alternative shielding materials such as beryllium have related behavioral problems. It is likely that MMW-class power system shields will have to be actively cooled. Shielding approaches for surface missions have rarely been given very serious attention, and most are notional at best. Shielding and heat rejection considerations will have a profound impact on system mass, mission operations, and the overall system and mission architectures for Lunar and Mars surface power missions.

***Lesson: A systems engineering approach is necessary for the reactor power or reactor propulsion system. This is particularly true for the coupled reactor, shield, and heat rejection segments. Early and continuous collaborative engineering of the reactor power and propulsion <u>system</u> will be required if the technical and programmatic risk associated with development of future space power and propulsion systems is to be managed to acceptable levels.***

### *III.E. Maximizing and Leveraging Common Technologies*

The VSE embodies an approach to space exploration that implies evolutionary improvements in space propulsion and power system performance based on a "go as you can pay" philosophy. These performance improvements are generally manifested as increasing power levels, reduced system masses, longer system operational lifetimes, etc. System performance is, of course, an artifact of the specific technologies and architectures employed in the systems. Due to the cost and time required to develop core nuclear technologies such as fuels, structural materials, etc., there is an obvious desire and need to leverage these investments to the maximum extent possible. *Technologies are much more adaptable than complete systems*, just as bricks, mortar, lumber, and steel are much more adaptable than a completed structure comprised of these technologies. However, every technology carries with it an inherent set of performance limitations. In many instances, near-term technologies that could be readily fielded may not provide the performance required for even early missions. Technologies that are capable of yielding the performance required for early missions will not, in many instances, provide the performance required for later missions.

The challenge for the space reactor technologist is to identify the critical technologies that define the fundamental performance constraints on the system and to engineer solutions that relieve these constraints or achieve the desired performance within the constraints. *Multiuse technologies that support multiple reactor system concepts and missions are particularly desirable.* An example of two key technologies of this type would be a high-temperature, oxygen-tolerant, fabricable structural material that could be used in-space and on Mars, in NEP, surface power and NTP systems; and a fuel that could serve both surface power/NEP and NTP applications.

***Lesson: System developers should seek to identify and develop "high-payoff" technologies that offer the potential for multiuse, multiconcept, multimission capabilities. In many cases these technologies could be characterized as "high-risk, high-payoff" technologies. An optimal development program would include a balanced portfolio of R&D activities—both long-lead or critical-path technology development for potential near-term mission applications, longer-term "high-risk, high-payoff" R&D that support longer-term missions, and "breakthrough" or "revolutionary" performance enhancements that fundamentally change the "design box" or enable entire new classes of missions.***

## IV. SYSTEM PERFORMANCE REQUIREMENTS MUST BE BASED ON SOUND ENGINEERING

### *IV.A. The Quest For Very Low Mass Reactors*

Mass rules in the space reactor business. Alpha (specific mass) is "King." Light is good. Lighter is better. This focus on lightweight systems is, of course, appropriate, and is driven by the harsh realities of launch vehicle payload mass limitations and the cost of inserting payloads into low earth orbit. However, the history of space reactor development is filled with examples in which programs established unrealistically small specific mass requirements early in the program only to see them immediately escalate upward to intolerable levels as true engineering design efforts commenced. It was not uncommon to see designers aggressively seeking and proposing dramatic design changes late in the life of these programs in an effort to justify significant mass improvements and so make the option more attractive on a mass basis. Redundancy is reduced. Margins are shaved. A case in point is the relatively recent SP-100 program. The initial design requirements issued for the SP-100 in 1984 specified a specific mass of 30 kg/kW(e). Industry teams responded with affirmation that such a goal was achievable for the 100-kW(e) system. A review of program history indicates, however, that the actual specific mass of the system escalated to twice that original design specification and that values of 40–45 kg/kW(e) were claimed in the closing days of the program. The focus on reducing reactor system mass is appropriate. But a balance must be maintained between setting "aggressive requirements" on the one hand and providing system designers a viable design space on the other.

***Lesson: Programs must establish reasonable, yet aggressive mass budgets for every element of the payload, and develop and maintain contingency plans in the event engineering reality does not provide the desired performance in all areas. There must be a "real" and achievable design space within which the reactor system designers can work.***

### *IV.B. The Quest For The "One-Size-Fits-All" Reactor*

The development of space reactor power systems is an expensive undertaking. There is a natural and healthy desire to leverage the investments in any such technology to the maximum extent possible. An "ideal reactor system" (reactor system here is defined to be the integrated reactor, primary heat transport subsystem, shield subsystem, power conversion subsystem, and heat rejection subsystem) might be characterized as indicated in Table 1.

**Table 1. Ideal reactor system characteristics**

| |
|---|
| • The reactor system is inexpensive. |
| • The reactor system is extremely low mass. |
| • The reactor system is available now. |
| • The reactor system can supply power over a continuous range of a few kilowatts to megawatts in an optimally configured package. |
| • The reactor system is directly scalable to much higher |

power levels.

- The reactor system can function in space, in micro-gravity, high vacuum, and a relatively benign chemical environment.
- The reactor system can be landed, deployed, and function on a lunar or planetary surface, in a gravitation field, in a dynamic atmosphere, and in a hostile chemical environment.
- The reactor system is highly reliable, and system performance degrades in a "graceful" manner.
- The reactor system can support both robotic and human missions.
- The reactor system will function for a lifetime of 3–20 years with no maintenance.
- The reactor system is completely testable on earth at a point early enough in the program such that lessons learned can be incorporated in design changes to the flight system without compromising the desired launch date.

Unfortunately, such reactor systems exist on only paper. Reactor system design is an art of careful compromise—a balancing act that must distinguish between "requirements," "desirements," "must-haves," and "nice-to-haves." Specialization and adaptation are keys to prosperity in nature and in most human-engineered systems.

***Lesson: Specialization and adaptation are keys to success in nature and human-engineered systems. Technologies are more adaptable than systems and architectures. The temptation to pursue the "one-size-fits-all' reactor system must be avoided. The development of adaptable technologies, employed in mission-optimized architectures and systems is a key to success.***

## V. SUMMARY

Space fission power and propulsion technologies promise to revolutionize scientific and human exploration of space. The U.S. has pursued and abandoned the development of many space fission systems since the mid-1940s. The history of U.S. space reactor development is ripe with lessons from which we can benefit. A sustained program of investment in technology, infrastructure, and people is required. A fresh look is in order in some cases. New visions and missions may, in some cases, best be enabled by new technologies and approaches. A systems engineering approach to nuclear power and propulsion system design and development should be adopted and system performance requirements should be carefully and realistically crafted. If adopted and internalized, these lessons will provide a strong foundation for the success of current and future space fission power and propulsion development programs.

## NOMENCLATURE

| | |
|---|---|
| AEC | Atomic Energy Commission |
| ANP | Aircraft Nuclear Propulsion Program |
| MMW | multi-mega watt |
| NEP | Nuclear Electric Propulsion |
| NERVA | Nuclear Engine for Rocket Vehicle Application |
| NTP | Nuclear Thermal Propulsion |
| SNAP | Systems for Nuclear Auxiliary Power |
| VSE | Vision for Space Exploration |

## REFERENCES

1. G. W. BUSH, 2004, "President Bush Announces New Vision for Space Exploration," January 14, 2004, at http://www.whitehouse.gov/news/releases/2004/01/20040114-3.html.

2. D. G. OTT, "Space Applications of Fast Reactors," ANS-100, 1966.

3. Atomic Industrial Forum, "Capability Requirements for Nuclear Space Systems," February 1966.

4. S. S. VOSS and G. P. DIX, "The Pied Pieper—A Historical Overview of the U.S. Space Power Reactor Program," presented at the First Symposium on Space Nuclear Power Systems, Albuquerque, New Mexico, January 11, 1984.

5. J. P. LAYTON, J. GREY, et al., "A Survey of High Temperature, Compact Nuclear Fission Reactors," T-SAC Report No. 82-001, September 17, 1982, pages VIII-1 through VIII-13.

6. J. P. LAYTON and J. GREY, "A Compilation of Selected Nuclear Space Power Reactor Characteristics," Techno-Systems Analysis Corporation, T-SAC Report 83-003, 10/31/83, as incorporated in "SP-100 Nuclear Space Power Source Book—Nuclear Energy Sources," G. A. Klein, Jet Propulsion Laboratory, May 15, 1984.

7. S. C. FLORMAN, "The Introspective Engineer," St. Martin's Press, New York (1996).

8. H. G. Stott, 1907.

9. Rockwell International, "Space Reactor Electric Systems Subsystem Technology Assessment," ESG-DOE-13398, March 29, 1983.