Weighing the Future: Strategic Options for U.S. Space Nuclear Leadership

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EXECUTIVE SUMMARY

For more than sixty years, the United States has invested in the development of space nuclear systems. Yet despite sustained interest and technical progress, we have not flown a fission reactor in space since 1965. Today, the field remains active but unfocused. NASA's surface fission power program is making incremental progress under constrained budgets. The Air Force Research Laboratory is proceeding reluctantly with a congressionally directed in-space power demonstration. The NASA-DoD Nuclear Thermal Propulsion (NTP) collaboration has been all but disbanded. Other investments are scattered across government, often disconnected from broader national goals.

The crux of the problem is simple but fundamental: space nuclear programs succeed only when real mission demand and technology maturation reinforce each other in a self-sustaining cycle. Decades of U.S. efforts have faltered because development was attempted in isolation without a committed customer or mission. Without operational need, budgets dry up, support wanes, and programs collapse. Conversely, as the naval reactor program showed, when mission pull is clear and sustained, technology matures steadily. The key insight is that we cannot break this circular "chicken-and-egg" loop by vision alone, or technology development alone. Planners will not adopt what they cannot see working. What is needed is a near-term demonstration that shows mission planners something real, soon, and relevant.

Among all applications, civil, defense, or commercial, in-space, surface, or propulsion, we found one foundational insight: everything starts with power. Power is where demand is strongest, use cases clearest, and progress is most achievable in the near term. Prioritizing power does not preclude propulsion, it enables it. Power reactor systems mature the industrial base, infrastructure, and regulatory scaffolding needed for Nuclear Electric Propulsion (NEP) to follow. Even nuclear thermal propulsion (NTP), which differs technologically from NEP, benefits from these enablers. Should sufficient mission pull, institutional leadership, and near-term funding align, a DoD-led NTP flight program could proceed without delay. But in the current environment, where that alignment is lacking, the strategic priority should be to stabilize the ecosystem through achievable progress: in power. This lays the groundwork upon which more ambitious propulsion programs can be built.

Timing is critical: achieving ground tests by 2028 and flight by 2030 ensures rapid feedback and sustains political momentum, giving the administration a clear, near-term success to champion and strengthening incentives for sufficient funding. Delaying beyond this window risks losing stakeholder interest, missing key decision opportunities, and ceding strategic advantage.

With a 2030 launch date as our hard constraint, the next question is scale. A small system (<100 kWe) is faster, cheaper, and less complex. It can validate fuel handling, power conversion designs, and licensing frameworks on a 3-5 year timeline. But it may not dramatically outperform solar power systems (though later generations will), raising the risk that it appears underwhelming. On the other hand, a larger demo (100–500 kWe) delivers immediate advantage. But it also demands new reactors, advanced power conversion systems, and heavy-lift capacity. And risks failure if not matched by top-down support. The challenge is to avoid overreach without undershooting. The solution we propose is to design backward from what can be ground tested by 2028 and fly by 2030 with the allocated resources. Let schedule and feasibility set the scale, not the other way around.

This leads to three distinct strategic options. The first, "Go Big or Go Home," calls for a 100–500 kWe power or NEP demonstration executed by NASA or DoD with DOE as a partner. It offers the highest potential return, but only if it passes the "Manhattan Project test": centralized top-down leadership, large and stable multi-year funding, and sustained urgency. If these conditions are not met, this pathway is likely to repeat the fate of the NASA JIMO/Prometheus mission, exciting on paper, canceled in practice.

The second option, "Chessmaster's Gambit," assumes that alignment and funding at the Manhattan scale may not be possible. Instead, it proposes two sub-100 kWe demonstrations using fixed-price, milestone-based public-private partnerships: one led by NASA for a power reactor that would operate either in cislunar orbit or on the lunar surface, contingent on the availability of a suitable lander, and one led by DoD for an in-space system. The key is for the government to be technology agnostic, letting commercial entities select technology and fuel enrichment levels that can meet deadlines, safety parameters, and the budget envelope. This approach builds on commercial momentum, carries lower risk, and enables scalable progress.

In parallel, a third option, "Light the Path", serves as a hedge. It proposes a <1 kWe commercially led radioisotope power system (RPS) demonstration. Though limited in scope, an RPS demo helps clarify regulatory roles, build flight heritage, and derisk the broader space nuclear ecosystem, especially if larger efforts encounter delay.

Summary of Principal Options

Option	**Demonstration**	**Scale*** **	**Lead Organizations**	**Rough Cost to Govt.**	**Risk Profile**
Go Big or Go Home*	Power or NEP flight demo by 2030 Ground test by 2028	100–500 kWe class	NASA or DoD with DOE as partner	~$3B over 5 years	High
Chessmaster's Gambit	Two power demos - in-space and surface - by 2030 Ground test by 2028	10-100 kW class	Industry, NASA/DoD, DOE, FAA	~$1B per agency over 5 years	Medium
Potential Light the Path**	Commercial RPS demo by 2028	<1 kWe class	Industry, NASA/DoD, DOE, FAA	~$100M (illustrative)	Low

**Should be pursued only if political, budgetary, and leadership conditions are aligned.*
*** Could proceed in parallel with either major Option. Requires further assessment.*
**** Actual power levels may be driven by lift and lunar lander capacity and other architectural constraints.*

Regardless of the option selected, all forward progress hinges on three non-negotiable pillars: technology maturation, infrastructure development, and regulatory reform. Without concurrent investment in these areas, no system—regardless of scale—will reach the launchpad, nor will any future system follow.

Success also requires resolving one final barrier: institutional fragmentation. The space nuclear domain is sufficiently fragmented, underprioritized, and cross-cutting that success will require centralized top-down coordination. Either option would need a dedicated leader at the White House and/or a Presidential Executive Order to signal priority, and strong Congressional support with budget commitments and deadlines. Any of these mechanisms can accelerate execution by clarifying roles, consolidating fragmented authority, and enabling necessary regulatory reform. What matters is not the specific instrument but the commitment to act.

The global context raises the stakes. Other nations are moving quickly to develop and field space nuclear systems as a foundation for long-term strategic advantage in space. The United States cannot afford to delay while others shape the rules of the road and claim first-mover advantage. In a lunar “race” in particular, it’s not just who lands on the Moon first but who stays powered first. Continuous operations secure our rights and reset the strategic playing field.

This strategy provides a clear roadmap if the right decisions are made and followed with urgency:

- Prioritize fission power as the enabling first step that leads to larger systems for both power and propulsion
- Design backwards from a 2028 ground test and 2030 flight, letting feasibility rather than power level set the scale
- Sequence surface or in-space demonstrations based on execution feasibility
- Advance three critical pillars in parallel: technology maturation, infrastructure, and regulatory reform
- Centralize leadership, avoiding a half-step that underfunds ambition or stalls under fragmentation

With these options in hand, the U.S. government must now move decisively. The window of opportunity is narrowing, as peer competitors accelerate their investments in space nuclear systems and position themselves to define the rules of the road. Meanwhile, U.S. industry stands ready, technologies have matured, and the alignment of policy and economics offers a rare chance to act at speed with confidence. With the right choices and the leadership to carry them forward, space nuclear power and propulsion can become a cornerstone of American space leadership for decades to come.

PROLOGUE

We come to this strategy as experts, deeply familiar with the technical, policy, and institutional dimensions of space nuclear power, but without a vested interest in any particular organization or technology. We do not have a dog in this fight. We do not subscribe to any “religion” when it comes to specific reactor designs, fuels, or propulsion architectures. What we bring instead is a sober view grounded in history, informed by past successes and failures, and motivated by a shared goal: to see a thriving, operational, commercially sustainable space nuclear sector in the United States.

We have studied the repeated starts and stops of past U.S. nuclear space efforts and drawn lessons from more successful models, such as the Manhattan Project, Nautilus, NASA’s Commercial Orbital Transportation Services program, and Operation Warp Speed. Each of these efforts succeeded not because of perfect plans, but because they combined clear mission goals, strong government coordination, milestone-based funding, and (in most cases) eventually an embrace of commercial innovation and speed.

This strategy is rooted in a hard truth: there will be winners and losers. That is the nature of good strategy. It is about making deliberate, often difficult choices, not crafting a plan that tries to make everyone happy. Resources are limited, stakes are high, and time is short. A viable space nuclear strategy must prioritize what matters most, deprioritize what matters less, and move with urgency. The path to space nuclear is not paved by consensus. It is paved by commitment.

We believe in speed, strength, and sovereignty. The government’s job is to unlock private sector investment, not micromanage it. Our approach shifts from endless studies to operational deployment, from bureaucratic gridlock to clear timelines and accountability. If done right, this strategy will unleash private capital, create high-tech American jobs, and put the United States back in the lead, not just in space, but in the future of energy and defense.

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Weighting the Future: Strategic Options for U.S. Space Nuclear Leadership

1. Introduction: The Long Struggle for U.S. Space Nuclear Power

For more than half a century, the United States has recognized the physics-based advantages of nuclear power for space but has failed to field an operational in-space reactor beyond the brief 43-day SNAP 10A mission in 1965. Billions of dollars have been spent, yet today the only operating space nuclear systems remain lightbulb sized ~100-watt plutonium-238 radioisotope thermoelectric generators (RTGs), a government technology largely unchanged since the 1960s.

The pattern has repeated across decades: promising programs launch with technical ambition and political support, only to collapse under budget cuts, technical challenges, shifting national priorities, and fragmented leadership. Time and again, programs launched with ambition and backing have collapsed, because they tried to advance technology in isolation of a committed mission pull, and when that hardware didn't advance in proposed timelines, mission sponsors walked away.

Three Faces of Fission: Power, NEP, and NTP

Fission technologies for space are not a monolith. They come in three distinct flavors, each serving a different strategic role, and each imposing different requirements on mission architecture, safety review, and investment timing.

1. Fission reactor power systems provide steady electricity, whether on the surface of celestial bodies for habitats, mobility, and in situ resource utilization (ISRU), or in-space to power sensors, relays, and defense assets. These systems can be sized from single unit kilowatts to megawatts.
2. Nuclear Electric Propulsion (NEP) systems use fission reactor power systems to power electric propulsion. While NEP offers the highest fuel efficiency, its thrust is low, requiring months of acceleration.
3. Nuclear Thermal Propulsion (NTP) uses fission heat to directly heat propellant, producing thrust 2–3× more efficiently than chemical rockets. NTP requires large extremely high temperature reactors, multi-year storage of liquid hydrogen, and intense thermal control. Hybrid NEP–NTP systems combine both propulsion modes, using NTP for rapid maneuvers and NEP for efficient cruise, potentially offering mission flexibility across a wider range of trajectories. These dual-mode systems demand complex integration and control, management, and precise mode switching protocols.

The SNAP program showed early feasibility but faltered. SP-100 and the Space Power Advanced Reactor (SPAR) programs of the 1980s and 1990s were canceled before flight. Even attempts to leverage Soviet technology through TOPAZ II ended without deployment. In the 2000s, the Prometheus program aimed to deliver a 200 kWe nuclear-electric propulsion system but collapsed under unrealistic expectations and competing priorities. Kilopower successfully demonstrated a 1 kWe fission experiment (KRUSTY) in 2018, but no operational follow-on mission was fielded. More recently, the DARPA-NASA DRACO program was discontinued after significant investment, underscoring the enduring difficulties of executing bold, high-risk space initiatives.

The fundamental value proposition remains unchanged. Nuclear power offers energy density and endurance that chemical batteries or solar panels can't match. It can keep lunar habitats and rovers running through the two-week lunar night and in permanently shadowed craters, sustain long-duration human outposts on Mars, enable continuous deep-space missions, and drive large sensors, communications arrays, and directed-energy systems for national security. Nuclear propulsion delivers performance beyond the reach of chemical rockets: it shortens transit times to Mars and supports roundtrips without in-space refueling, reduces launch mass and cost, minimizes crew radiation and health risks, and lets defense satellites and space platforms reposition rapidly for responsive operations.

Enabling vs. Enhancing: The RORSAT Case for Nuclear-Only Solutions
The Soviet RORSAT program offers a textbook example of a mission where nuclear power wasn't just a nice-to-have, but the *only* viable choice. While the United States launched just one experimental nuclear-powered spacecraft (SNAP-10A) in 1965, which operated for 43 days before a voltage regulator failed, the Soviets launched over 30 space reactors as part of their Radar Ocean Reconnaissance Satellite (RORSAT) program during the Cold War.

Why the heavy investment? The Soviets faced a clear operational challenge: tracking U.S. aircraft carriers and submarines across vast oceans in real time. That required satellites with active radar, which consumes kilowatts of power, far beyond what early compact solar panels could provide. Compounding the challenge, effective radar requires low orbital altitude to generate strong signal returns and differentiate ships from ocean clutter. But flying low increases atmospheric drag. Soviet-developed solar panels large enough to power radar would act like parachutes, pulling the satellite out of orbit prematurely. So, they went nuclear.
Compact space reactors, first BES-5 "Buk," then Topaz, delivered steady high electrical output without the drag penalty of massive solar arrays. Nuclear power wasn't a luxury; it was what made the mission possible.

In contrast, the U.S. had access to superior imaging, SIGINT, and tracking systems that didn't demand such power levels as well as superior solar panels. Nuclear remained, until now, a lab curiosity for us, an *enhancing* technology, not an *enabling* one for all applications other than deep space science, the needs for which were satisfied with RTGs.

Despite these advantages, space nuclear systems have remained trapped in an R&D cycle. NASA's exploration systems directorate has not committed to nuclear propulsion for Mars. The Department of Defense (DoD) remains fragmented and undefined on space nuclear power or propulsion.[a] The commercial sector shows growing interest in space-based computing, in-space manufacturing, and in situ resource utilization (ISRU), but high costs and regulatory uncertainty have prevented commitment. FAA regulatory uncertainty, investor trepidation, incomplete indemnification frameworks, and fragmented agency leadership further stall development. U.S. programs have repeatedly blurred the line between *enhancing* missions (nice-to-have performance gains) and *enabling* missions (impossible without nuclear), intermittently funding the former while postponing the latter, and watching each effort collapse when budgets tightened. This stagnation is now a strategic liability. China and Russia are reportedly collaborating on a 1.5MW space reactor slated for 2036, an effort that, if successful, could leapfrog U.S. capabilities. Without decisive action to overcome systemic failures, the United States risks losing its leadership in space nuclear technology at a moment when global competition is accelerating.

1.1. Goals and Organization

The goal of this report is to break the decades-long cycle of overambitious starts and disappointing cancellations in U.S. space nuclear efforts by providing a pragmatic set of blueprints for fielding operational fission power and propulsion systems. We aim to force clear choices, sequence efforts against real mission needs, and concentrate resources where they deliver the greatest strategic value, civil, commercial, and defense alike.

[a] A clear illustration is the Space Force's muted response to direct Congressional inquiry in the FY 2024 NDAA in which Congress explicitly invited the Service to articulate its needs and aspirations for nuclear propulsion. While acknowledging the promise of NTP and NEP, the response from Space Force to Congress stopped short of expressing interest in near-term development or deployment, offering a cautious and noncommittal reply instead and emphasizing the need for fielding capabilities as part of a system-of-systems. This restrained response underscores the disconnect between external expectations for a strong national security "mission pull" for nuclear propulsion and the DoD's institutional priorities.

What sets our work apart is its integrated perspective. Most prior efforts have focused narrowly, either going deep on power systems alone, such as comparing specific power levels, or on propulsion, often evaluating propulsion architectures in isolation from power (and just as often pitting NTP against NEP). This is the first strategy we know of that takes a step back to consider the full fission landscape, power and propulsion, surface and in-space, civil and national security, and evaluates how choices in one domain shape outcomes in others. Rather than optimizing within silos, we identify the sequence and structure of decisions that can unlock the whole field.

Our analysis draws on over 100 interviews across government, industry, and academia, as well as a deep review of policy, technical, and mission documents, to diagnose why progress has been elusive, and to assess how lessons learned from these prior efforts and new conditions may finally allow the United States to break the cycle. We organize the report as follows:

- Context and Imperatives – In sections 1-3, we trace the history of stalled efforts and lay out the current mission drivers for action, clarifying what's changed, and why space nuclear is once again on the table.
- Demonstration Pathways – In sections 4-5, we define the core problem to solve and offer three strategic options for near-term flight demonstrations, each with distinct goals, risks, and execution models.
- Evaluation and Enablers – In sections 6-7, we compare these options under real-world constraints, surface the trade-offs they entail, and outline the supporting actions, across technology, regulation, and institutions, needed to succeed.

The next section examines why, despite persistent setbacks, there is now a fragile but real opportunity to transition space nuclear power and propulsion from concept to operational reality.

2. A Fragile Turning Point: Why Now?

Although historical efforts have repeatedly stalled, several emerging factors suggest that space nuclear power and propulsion may finally be poised to advance beyond the R&D phase. However, the opportunity remains fragile, uneven, and time sensitive. The 2020s are therefore the *decisive decade*: hardware either flies this cycle or the United States relinquishes leadership by default.

First, the geopolitical environment has shifted. China's ambitious space nuclear plans, including a reported collaboration with Russia on a 1.5 MW class space reactor targeting 2036 and a nuclear-powered lunar base by 2035, provide a clear national security driver for U.S. action. The risk of losing technological leadership in space nuclear power is no longer theoretical. This is not just about prestige, it's about denying adversaries uncontested mobility, power projection, and logistics advantages in Cislunar and deep space. See Sidebar.

> **First Mover Advantage at the Lunar South Pole**
> If China places the first continuously operated reactor at the south pole, it instantly creates "facts on the ground" that justify de facto territorial control. By citing "safety" exclusion zones around its installation, Beijing could carve out an area of exclusive use, undermining the Outer Space Treaty's non-appropriation principle (Article II) without formally claiming sovereignty. That installation would also trigger Article XII consultations, but under Chinese direction: subsequent U.S. or allied missions would have to negotiate access rather than freely operating as the Treaty intended.
>
> Beyond territorial and diplomatic leverage, whoever sets up the first reactor defines the technical and safety norms for lunar nuclear power. If China dictates those standards, every follow-on mission—American, allied, or commercial—must conform to rules crafted in Beijing. By deploying first, the United States secures the agenda for safety zones, emergency protocols, and interoperability requirements, ensuring lunar governance reflects American values of transparency, reciprocity, and non-discrimination.

Second, mission pull is beginning to emerge, unevenly but meaningfully (See Appendix B). NASA has made its first formal commitment to use fission power for human Mars surface operations. Defense requirements remain fragmented, but the Golden Dome initiative signals potential demand for high power space-based systems. Private sector interest is growing in areas such as lunar mining and propellant production, orbital computing, and in space manufacturing, although commercial demand remains contingent on cost, risk reduction, and regulatory reform. There is also growing interest in 750 kWe-2 MWe class systems for propellant production for Earth return and for other surface activities on the surface of Mars.

Third, technological maturation is accelerating. Advances in modeling, simulation, autonomous operations, miniaturization, and the emergence of high assay low enriched uranium (HALEU) based reactor designs are improving technical feasibility and could shorten development timelines. Reductions in launch costs and improvements in space-based assembly and manufacturing further enhance viability. The pattern is classic 'disruptive innovation' (See Sidebar).

Fourth, investments in advanced terrestrial nuclear power are strengthening the supply chain, workforce, and industrial base. These investments, driven by rising terrestrial energy needs and a growing recognition of nuclear's role in energy security, create spillover benefits for space systems.

Fifth, policy clarity has improved. Space Policy Directive-6 (Memorandum on the National Strategy for Space Nuclear Power and Propulsion) and National Security Presidential Memorandum-20 (Presidential Memorandum on Launch of Spacecraft Containing Space Nuclear Systems)[b], among others, provide clearer guidance on launch approval processes and technology pathways, offering greater certainty for government programs and private investors.

Finally, the commercial sector is better positioned to participate. Private companies are increasingly capable of co-investing, co-developing, and fielding space nuclear systems, if regulatory and indemnification barriers are addressed.

Yet these positive developments do not guarantee success. Mission pull remains incomplete. Technology and infrastructure gaps persist. Regulatory frameworks remain fragmented and unclear. Most critically, no dedicated leadership entity exists to integrate budgets, resolve interagency conflicts, and enforce execution timelines. The historical cycle of ambition followed by collapse remains the default unless deliberate reforms are made. Private investment continues to see far too much uncertainty and risk in space nuclear opportunities to allow for a commercially driven program.

This strategy is being developed at a critical inflection point: where sustained action could finally allow the United States to field operational space nuclear systems, or where inaction could once again defer nuclear power in space to another generation, and perhaps another nation.

The sections that follow will map the current mission needs, diagnose the systemic barriers that must be overcome, and outline a strategy to enable U.S. leadership in space nuclear power and propulsion.

[b] Issued in 2019, NSPM-20 establishes a streamlined interagency process for authorizing space nuclear system launches for government and commercial missions. Recognizing that nuclear power is essential for long-duration missions, it replaces outdated approval mechanisms with a tiered risk-based framework: Tier 1: Low-risk missions having the lowest quantities of radionuclides. Tier 2: Moderate-risk systems requiring enhanced review. Tier 3: Novel or high-risk designs needing Presidential authorization.

Space Nuclear as a Disruptive Innovation
Nuclear systems begin in a niche where solar arrays and chemical propulsion still appear cheaper and less complex, much as the earliest automobiles seemed slower and less reliable than horse-drawn transport, or the first mobile phones cost more than landlines. Yet the competitive axis is different: sustained, high density power that operates day and night, far from the Sun, and in contested orbits.

Once fission reactors fly and accrue operating hours, a compounding learning curve takes hold: unit costs decline, regulatory pathways stabilize, and entirely new missions become feasible. What initially looks like an esoteric capability becomes the backbone for persistent cislunar logistics, Mars ISRU, and high power defense platforms. Disruption is seldom an instant breakthrough; it is an accelerating progression, and each year of delay allows others to climb the curve first (Figure 2.1).

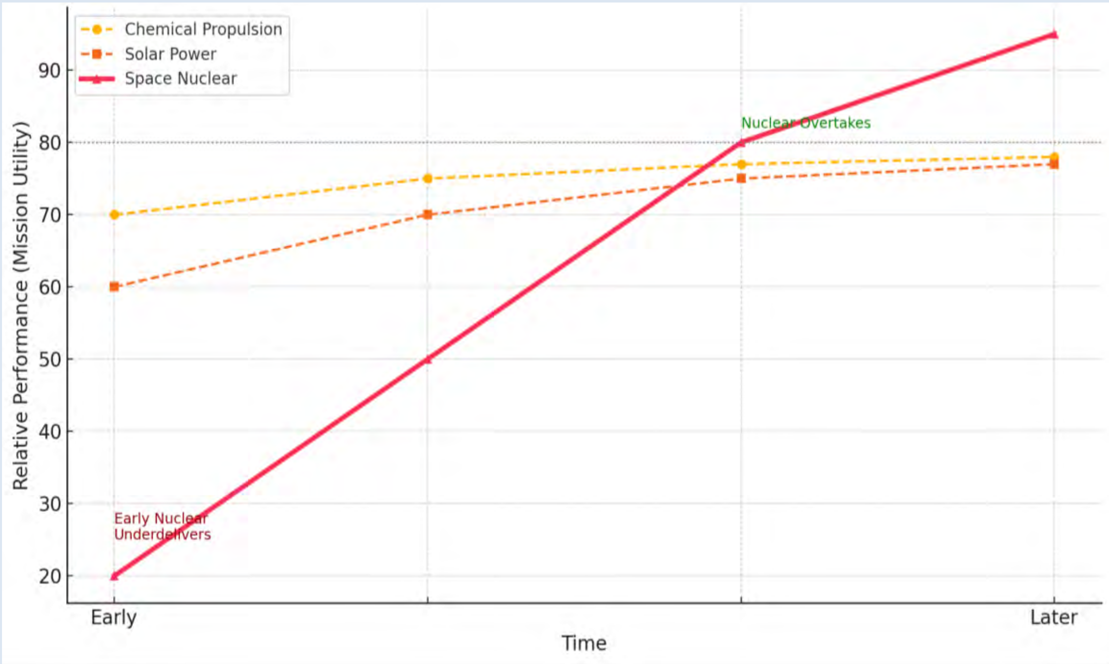


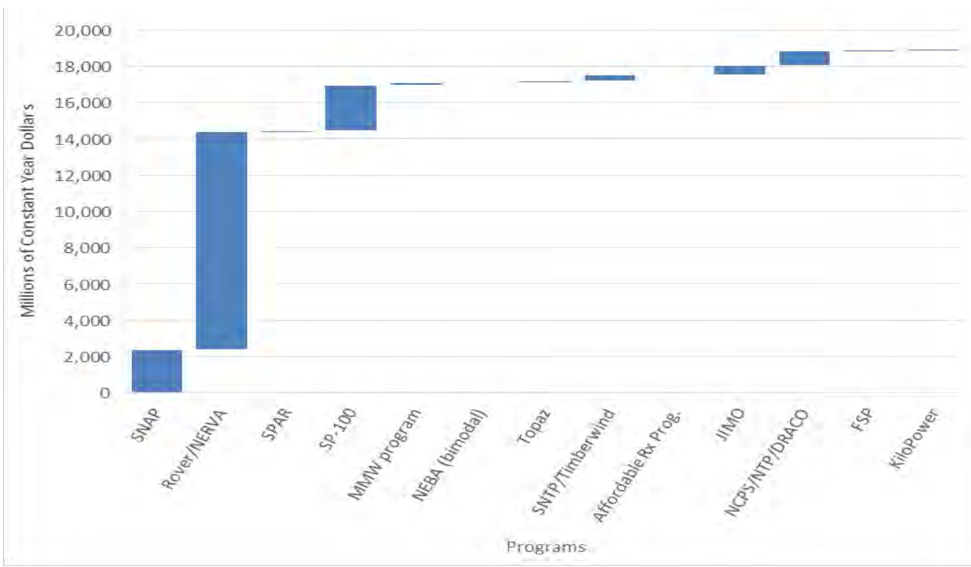
Figure 2.1: Space Nuclear as a Disruptive Innovation?

3. About Our Approach

This strategy is based on a rigorous examination of why decades of U.S. space nuclear efforts have failed to transition from promising concepts to operational systems. We also reviewed successful programs such as the Manhattan Project, Nautilus, NASA COTS/CRS and Operation Warp Speed (See Sidebar). We drew from over 100 interviews with technical experts, policymakers, industry leaders, and regulators (see Appendix A for a partial list of interviewees) comprehensive reviews of past programs; and collaborative workshops. Our goal was not simply to catalog past efforts, but to diagnose the structural and systemic barriers that have repeatedly undermined progress.

3.1. What We Learned

Early programs such as SNAP, SP-100, and Prometheus/JIMO began with ambitious goals and strong political rhetoric. But they collapsed under the weight of shifting priorities, underdeveloped technology, and, most of all, lack of committed customers. Even when technical progress was real, as with KRUSTY in 2018, programs struggled to convert momentum into deployment. Every generation of policymakers has rediscovered the same problems, made the same missteps, and exited the stage leaving space nuclear little closer to flight. Instead of being driven by concrete operational needs, many space nuclear programs have been technology driven efforts, pursuing capability without a committed customer. The result: billions spent on systems that never transition from R&D to deployment. See Figure 3.1.



Source: Poston & McClure (2021) updated with publicly available data on NASA/SNP and DARPA/DRACO

Figure 3.1: Estimated Expenditures on Space Nuclear Systems

Programs like SP-100 failed, in part, because they were designed as generic power systems without a specific mission. Without clear demand, these projects struggled to justify their cost, leading to inconsistent funding and eventual cancellation. Moreover, these systems were often too complex for first deployment, featuring unvalidated high-temperature materials, deployables, or dynamic power conversion elements. Instead of designing for testability and speed, we pursued performance levels we couldn't prove. Here are some of the most critical lessons we learned:

- **Capabilities Without Customers Cannot Survive.** The most consistent failure across U.S. space nuclear history is the absence of *durable mission pull*. Instead of building systems to meet urgent operational needs, we have pursued abstract capability, building reactors in search of a mission. Programs like SP-100 were conceived without a committed specific user, making them easy targets when budgets tightened. Today's initiatives, as exemplified by DRACO, remain at risk unless they are anchored to clearly defined, time-bound mission requirements. Only if there are sustained needs will the initiatives succeed. The lesson here is that mission pull must ultimately be there. Without real customers, civil, defense, or commercial, nuclear systems will never move beyond R&D. This applies equally to architecture: if a reactor has no customer, or a demo has no follow-on, it will not survive. Space nuclear must begin with a user and work backward, not begin with a capability and hope to find one.

- **Overreach Is the Enemy of Progress.** Too often, U.S. programs have prioritized architectural perfection over timely, incremental advancement. Prometheus was supposed to leap straight to a 200 kWe mission, skipping simpler, risk reducing precursors. It collapsed under its own weight. This pursuit of "extensible" solutions has delayed flight and eroded confidence. The lessons here is to start smaller and launch something real. A sub-100 kWe system flown in the next few years would teach us more than another decade of study for a 1 MWe dream. The more complex the system, the more fragile the timeline. Flight demos should begin with low-complexity, low-temperature, low-integration designs, built with TRL 6+ components where possible. And as we scale, improvements should be incremental, added in blocks, not all at once.

- **No One Is in Charge, and That's the Problem.** Unlike the U.S. Naval Reactors program, which has delivered operational reactors for over 70 years under unified leadership, space nuclear has no single accountable entity. DOE, NASA, DoD, and the FAA all hold fragments of authority. But no agency owns end-to-end delivery, and each has its own priorities. The result is fundamental misalignment, fragmented funding, and a lack of institutional memory. Projects drift across administrations and vanish when champions retire or budgets tighten. A lesson here is that leadership cannot be diffused. We need an empowered office, backed by budget, authority, and continuity, that can own this mission through development, deployment, and operation. We must also treat program execution as a discipline. Every successful crash program has shared core traits: centralized authority, clear milestones, rapid test and feedback loops, and organizational accountability. Space nuclear lacks all four.

- **Timelines Kill, and We Ignore Them at Our Peril.** National Academies, NASA and other reports show that using traditional approaches, the development of space nuclear systems will take 10–20 years. But U.S. political and budgetary cycles run 2–5 years. This structural mismatch has doomed every major effort. When timelines outlast political attention spans, programs are restructured, delayed, or canceled, before hardware is ever built. Private sector players, wary of this instability, stay on the sidelines, defer investment, or pivot to other technologies, as companies such as Atomos did. We must align strategy with reality. That means phased development, milestone-based execution, and clear early wins that build staying power across administrations. Each year without visible technical progress erodes confidence. Every delay increases the chance of cancellation. 'Fly as you test, test as you fly' must be the new default.

- **Fragmented Oversight Strangles Progress.** The regulatory regime for nuclear systems has been and remains fragmented, ambiguous, and risk averse. It treats commercial fission in space as an edge case, with overlapping oversight, but no clear path to launch approval or indemnification. Indemnification alone is now viewed as the *single greatest barrier* to commercial participation. FAA licensing is slow and undefined. NRC authority over HALEU and space-qualified systems remains unsettled. DOE is viewed by many as a gatekeeper, not an enabler. Without regulatory clarity, there will be no market. We must create a streamlined, space-specific framework with clear timelines, differentiated risk tiers, and government indemnification for qualified missions. And we must recognize that the regulatory process is part of the technical system: if it cannot be navigated within the program timeline, the program will fail, no matter how elegant the reactor.

- **We Cannot Ignore the Workforce or Infrastructure.** Decades of underinvestment have left the United States with no deep bench of engineers or technicians trained in space nuclear system design, integration, or operation. Each new program must rebuild its team, often from scratch. That delay raises costs and increases technical risk. Without a parallel workforce strategy embedded in space nuclear *system* development, we will continue to lack the people needed to execute. Workforce development must be embedded into system development, not treated as an afterthought. The same is true for facilities such as hot cells, thermal vacuum testbeds, and launch integration, none of them exist at scale. Without parallel infrastructure investment, no strategy is executable.

In sum: Space nuclear has failed not because the technology is too hard, but because we have made it too hard to execute. We have chosen bureaucracy over leadership, perfection over progress, ambiguity over commitment. If we want different outcomes, we must choose differently.

3.2. Principles for Developing a Successful Strategy

Based on these lessons (see also Sidebar on lessons from other programs), we established a core set of guiding principles to shape a successful strategy. These principles respond directly to the root causes of past failures, and serve as the foundation for charting a credible, executable path forward.

1. The strategy should prioritize ruthlessly. No program can serve every interest. The strategy must make deliberate tradeoffs, deciding what to lead with, what to delay, and what to set aside. Resources should be focused on high leverage missions and executable deliverables, not spread thin across unfocused ambition.

2. The strategy should field early systems to learn and build momentum. Demonstration earns credibility. The strategy must prioritize rapid deployment of small, flight ready systems that generate real data, test integration pathways, and unlock user buy-in. Learning by flying, even with limited systems, is more valuable than indefinite design work.

3. The strategy should abandon the study–cancel–repeat cycle. Analysis without execution has failed the field. The strategy must adopt milestone-driven program structures with clear criteria for continuation or pivot. Risk should be accepted and managed, not avoided through paralysis.

4. The strategy should pursue fleet-based development, not one-off flagships (drones over Battlestar Galactica). Rather than single, monolithic systems, the strategy should promote modular, replicable architecture that can be fielded iteratively. Success lies in launching systems often, not in building one perfect spacecraft every decade.

5. The strategy should embed design flexibility from the outset. Given shifting political and agency priorities, space nuclear systems must serve multiple use-cases, civil, defense, and commercial, without requiring reinvention. Pivotable, modular architecture reduces wasted investment and enhances resilience.

6. The strategy should pursue bold efforts only under the right conditions. Large-scale efforts should only be launched when three enabling factors (that we refer to as the “Manhattan Project test”) align: (1) A centralized lead with real budget and milestone authority; (2) Stable, multi-year funding insulated from annual swings; (3) A strategic imperative strong enough to align leadership and unlock institutional will.
7. The strategy should treat delay, not failure, as the dominant risk. The costliest outcome is not a failed demo, but the absence of any demo. The strategy should emphasize timely fielding of “good enough” systems, with the intent to iterate and improve. Timelines should be treated as non-negotiable drivers of confidence and commitment.
8. The strategy should assign clear, centralized leadership and long-term accountability. Diffused responsibility has led to program drift and collapse. The strategy must consolidate execution authority within a single office or coordinating body with control over budget, timelines, and cross-agency alignment. Without centralized leadership, continuity cannot be sustained.
9. The strategy should modernize the regulatory framework. Today’s regulatory environment is fragmented, ambiguous, and a major deterrent to commercial participation. Regardless of the Option selected, the strategy should push for a unified, space-specific framework that clarifies agency roles, streamlines launch approvals, includes risk-tiered licensing, and provides indemnification where needed.
10. The strategy should invest in the entire execution ecosystem, technology, infrastructure, people, and commercial markets. A successful program requires more than a space nuclear system. The strategy must co-invest in technologies, development and test facilities, fuel supply chains, and workforce pipelines, while also laying the groundwork for future commercial transition. Government-led efforts should seed a resilient, self-sustaining space nuclear sector, not create permanent dependency.

Recent developments, such as NASA's commitment to nuclear surface power for Mars and increased commercial interest, provide opportunities, but success depends on clearly aligning nuclear development with near-term mission deliverables, establishing dedicated institutional leadership within agencies, reforming the regulatory framework, and investing in technology, testing and system integration infrastructure and fuel production.

These principles are compressed into four non-negotiable execution pillars, a quick-win flight demonstration, technology maturation, infrastructure build-out, and streamlined regulation, and funnel directly into two main strategic forks detailed in the next sections. But we begin with humility: knowing we’ve seen this movie before, and knowing exactly how it ends, unless we change the script.

Common Lessons from Past Crash Programs

From atomic weapons and nuclear submarines to pandemic vaccines and space reactor false starts, U.S. history offers a clear playbook for how (and how not) to accelerate game-changing technology. Several landmark crash programs offer relevant models: the Manhattan Project (1942–46), Nautilus (1947–54), Operation Warp Speed (OWS, 2020), and JIMO/Prometheus (2003–05). It's true that space nuclear doesn't address an immediate existential threat, unlike nuclear weapons or a global pandemic. But space nuclear does aim to secure U.S. strategic leadership, enable critical national capabilities, and catalyze long-term economic growth in space. That makes the urgency different, but not the need for disciplined, results-driven execution. Across these efforts, existential or not, several execution principles recur:

1. **One Mission, One Owner.** Every success had a singular goal and a central authority. Groves, Rickover, and Slaoui had the power, and the accountability, to drive results. When no one's in charge, nothing ships.
2. **Parallel Paths, Not Perfect Bets.** The Manhattan Project and OWS didn't gamble on a single solution. They funded multiple approaches in parallel and picked winners based on results, not theory.
3. **Milestones with Teeth.** Nautilus ran on weekly tests. OWS paid only for deliverables. The Manhattan Project tracked critical mass, not paperwork. In every case, progress was measured and enforced. No open-ended studies, tie contracts to progress or shut them down.
4. **Field Something Fast.** JIMO tried to go straight to a flagship mission and collapsed. Manhattan and Nautilus built testbeds first. OWS ran clinical trials and manufacturing in parallel. The message: fly something small and useful, then scale.
5. **Stable Budgets, Not Fiscal Ping-Pong.** Every successful program had protected, multi-year funding insulated from annual turbulence. Budget instability killed Prometheus.
6. **Co-located Teams.** The best programs put engineers, operators, and safety experts shoulder to shoulder. No one emailed specs, they solved problems together, in real time.
7. **Fast Does not Mean Reckless.** The Manhattan Project managed unprecedented risk without paralysis. OWS moved fast without cutting corners. Even Nautilus, with its first-of-kind reactor, prioritized safety without delay. The model isn't "reckless speed", it's "disciplined urgency."

The takeaway: Success requires clear goals, empowered leadership, milestone-driven contracts, and the discipline to test, learn, and scale.

4. What Do We Propose and Why?

A sound strategy synchronizes mission pull and technology readiness, sequences effort, and concentrates resources where they deliver maximum strategic value. We cannot chase every technical option in parallel, nor can we postpone the hardest work until "later." Our principles in developing this strategy are simple: make deliberate trade-offs, build foundational capabilities first, and move with disciplined urgency.

This section lays out the conceptual pathways that follow one core principle that we uncovered in our extensive deep dive of the field**: power comes first**. With that foundation in place, we next explain how an initial power demonstration cascades into scaled surface- and in-space power grids, enables NEP, and accelerates NTP. Later sections compare alternative execution pathways, but the purpose here is to establish the logic and sequencing of a power-first approach that embeds the pull-maturity cycle at every step.[c]

4.1. Why Power Comes First, No Matter the End State

Whether our end goal is sustained lunar or Martian presence, orbital infrastructure for national security applications, or missions to Mars, the path starts the same: deploy a flight-proven, scalable fission power system. This principle is the foundation of this strategy: power comes first.

Historically, space nuclear programs often treated power generation as subordinate to propulsion, bundled within integrated vehicle concepts or deferred until other technologies matured. But we found that that sequencing is backward. Nuclear propulsion systems, whether NEP or NTP, are ultimately *reactor systems* with different kinds of propulsion systems added. Their viability depends on similar fundamental reactor core technologies, startup and control capabilities, autonomy, safety systems, development, testing and launch infrastructure, and regulatory streamlining which will be proven first through power-only demonstrations.

This approach also benefits from direct synergies with terrestrial advanced reactors, especially microreactors, under development for defense and remote applications. Many of the core technologies, fuel forms, power conversion cycles, control systems, and safety cases, are shared across terrestrial and space systems. Aligning with these parallel investments allows the space nuclear effort to leverage the momentum of ongoing DoD and DOE work, such as Project Pele or the commercial DOME[d] demonstrations. This alignment accelerates development timelines, lowers costs, and expands the industrial base. These synergies also strengthen the political and budgetary case for early investment, as the same testbeds, fuel processing pathways, and supply chains can serve multiple national priorities.

Across all credible mission classes, high-power availability is the enabling constraint:

- NASA targets 10–40 kWe for lunar and Mars surface operations.
- The Department of Defense requires ≥100 kWe for persistent sensing, directed energy, and Cislunar mobility.

[c] Although this strategy focuses on fission systems, a third, lower-power pathway—commercial radioisotope systems—offers a quick, low-risk means to validate licensing and indemnification; we flag that concept briefly here and return to it in section 5.

[d] Demonstration of Microreactor Experiments (DOME) is a testbed under construction to accelerate the development of advanced nuclear energy technologies.

- All chemical Mars transportation architectures require >750kWe on Mars to produce propellant for the Earth return journey.
- Some deep-space missions will benefit from 20–50 kWe for science and low-thrust NEP.
- Human Mars transit scenarios demand ≥2 MWe NEP-chem hybrids today, and up to 60 MWe in the long term.[e]
- Commercial applications forecast 100–500 kWe near-term needs for lunar resource processing and orbital compute, rising to multi-megawatt scale for Mars propellant production and surface systems.

Solar arrays, RTGs, and fuel cells either stall well below these levels or impose prohibitive penalties in mass, duty cycle, or complexity for these missions. Only fission systems can deliver continuous, high-specific-power operation in shadowed, distant, or contested environments. That's why this strategy makes power the entry point. By isolating the reactor-converter subsystem in a power-first mission, we:

- Validate critical reactor technologies, startup protocols and operations requirements, leveraging the extensive terrestrial experience with power reactors
- Validate radiation-hardened reactor sensors for autonomy, fault recovery, and shutdown logic under in-space conditions
- Provide a qualification path for HEU- and HALEU-fueled systems under NSPM-20-compliant launch and operations frameworks
- Establish and validate critical infrastructure for fuel production, reactor development and testing, space nuclear system integration, and launch site handling
- Establish and demonstrate the new streamlined regulatory framework
- Develop an operational workforce, and supply chain base that can scale with system size
- Directly support exploration missions and near-term commercial opportunities

Power first is thus not just an engineering argument, it is a sequencing imperative. No stakeholder, NASA, DoD, or commercial, can commit to nuclear propulsion or in-space or surface infrastructure without assured access to high-reliability space reactor. And both NEP and NTP are downstream consumers of that power core: each requires reactor validation, autonomous controls, and high-temperature thermal management to proceed.

4.2. Propulsion Follows

Nuclear Electric Propulsion is the natural first beneficiary of a power-first strategy. NEP systems are, at their core, electric power systems with electric thrusters bolted on. Once a power demo proves the flightworthiness of the reactor-converter-radiator stack (validating reactor, dynamic power conversion, thermal transport, high-voltage power management and distribution (PMAD), and radiator deployment under space conditions) everything needed for NEP is already in place. What remains is modular: attach electric propulsion units, integrate thrust vector controls, and test combined operations. This stepwise approach allows early power demonstrations to carry forward nearly wholesale into NEP architectures, compressing development timelines. A megawatt-class NEP system cannot be built until a power system has flown. A successful power-first mission is not a detour on the way to NEP; it is the essential on-ramp.

[e] NASA is still evaluating propulsion architectures for Mars missions and has yet to select among four candidates: all-chemical propulsion, solar-electric plus chemical, nuclear-electric plus chemical, or nuclear-thermal propulsion.

Crucially, the same flight data, on fuels, materials, autonomy, and launch-safety, also shorten the road to NTP, ensuring that power-first accelerates both branches rather than choosing one over the other. The hardest parts of an NTP architecture are not *just* extreme fuel/materials survivability, long-term cryogenic hydrogen storage and full-scale, integrated system ground tests, they're also nuclear subsystems: autonomous control, transient startup, thermal shock resilience, fault-tolerance, and launch authorization. These are precisely the systems exercised in an early fission power or NEP demo.

Moreover, as NEP systems mature, they begin to test mission scenarios, like nuclear system proximity operations, fault detection and management and long-duration operation, that are operationally relevant to NTP even if the reactor regime and propulsion physics differ. Flying a high-power NEP tug reduces many of the risks for NTP development, launch and operations, including in-core thermomechanical modeling tool validation, high-fluence material behavior, establishing the major elements of the development, testing and launch site infrastructure (other than system-level hot-fire), digital twin capability, and nuclear system regulatory pathway demonstration. In this way, a power-first pathway accelerates, not delays, our ability to field an NTP system, and gives policymakers the option to make a go/no-go decision on Mars-class NTP with real hardware heritage in hand.

Figure 4.1 shows that power and propulsion form a single, feed-forward chain rather than parallel tracks, power being the shared bottleneck, the foundational core upon which both propulsion systems and the broader infrastructure depend. While early flight demonstrations validate the reactor, power conversion, thermal transport, and high-voltage distribution, propulsion-specific technologies mature in parallel and are then grafted onto those proven power cores. In this way, each successive power or NEP mission directly leverages the hardware heritage and lessons learned from its predecessors, compressing development timelines and de-risking higher-power and propulsion architectures.

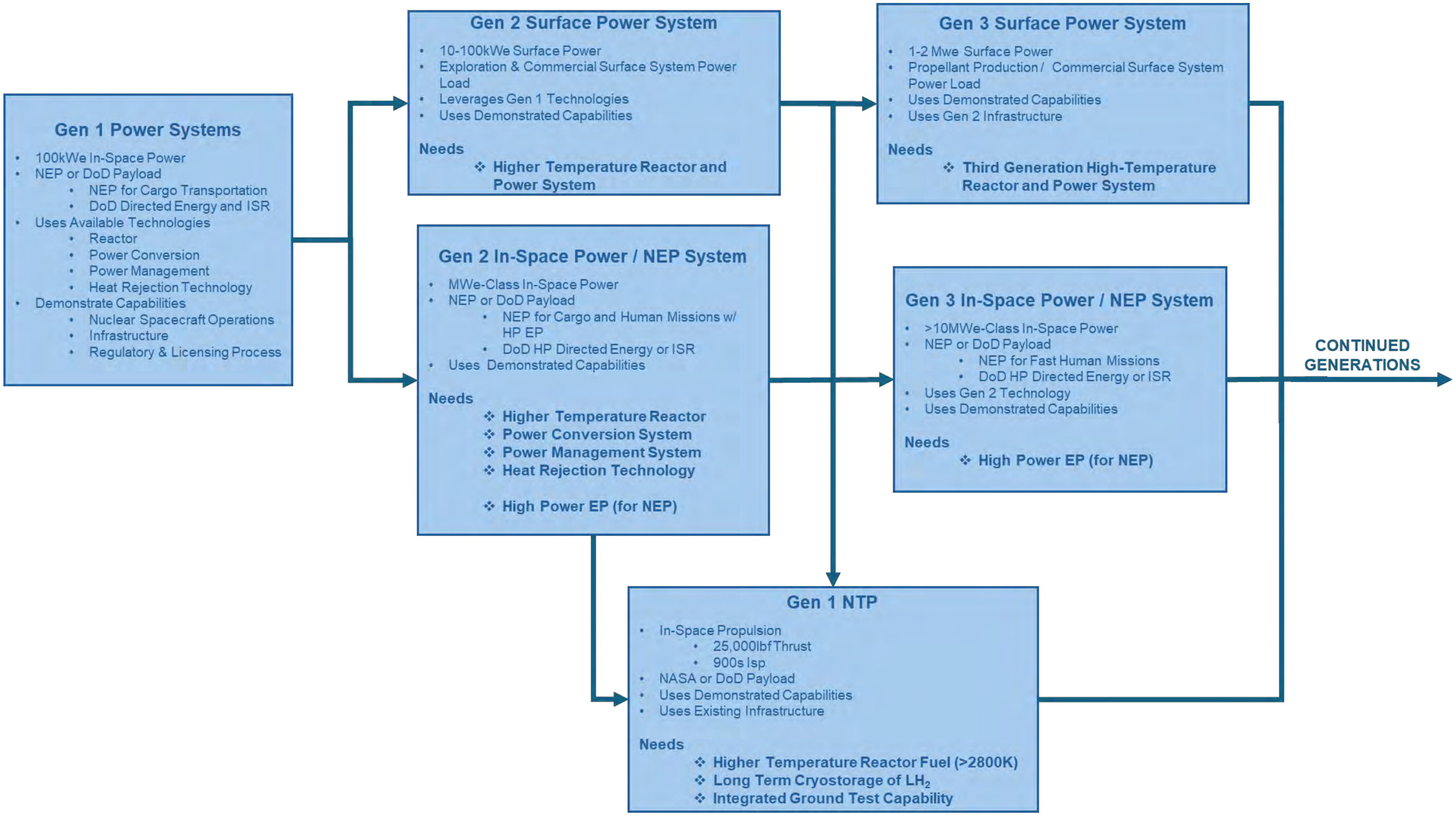


Figure 4.1: Feedforward from Gen 1 Systems

By starting with power, we do not delay NEP or NTP, we de-risk and accelerate them. A flight-proven power core becomes the launch platform for propulsion. It shifts decision-making from aspirational to executable, grounded in real hardware performance, not notional roadmaps.

4.3. Why an Early Demonstration is Essential

As detailed in Section 3, our historical experience underscores the strategic value of early, tangible demonstrations. Quick wins demonstrate progress, validate core technologies, reduce political risk, and drive mission pull. Fielding an early system, rapidly and decisively, is strategically more valuable than indefinitely designing an ideal system that may never fly.[f] An early demonstration provides the momentum required to sustain political support and maintain programmatic continuity. An early demonstration not only proves the technology's operational value, but also creates its own mission pull—stakeholders see concrete benefits and rally around further deployments—making it more valuable than any purely theoretical design. A demonstration, therefore, is not a luxury. It is a survival requirement.

The next logical question becomes: what is the right size for this initial demonstration? Our discussions unearthed a difficult choice between small and medium-scale power demonstrations.

- **Small-scale (<100 kWe):** Validates reactor startup and shutdown, basic control systems, and licensing pathways. Lower cost and risk. Can fly sooner. But limited in strategic and operational relevance.
- **Larger-scale (>100 kWe):** Enables direct feedforward to megawatt-class systems. Supports real operational missions (e.g., surface ISRU, Cislunar logistics). Greater complexity, cost, and lead time, but higher payoff.

Table 4.1**:** Comparing Demonstrations by Power Levels

Factor	Smaller Demo (~10 kWe class)	Larger Demo (~100 kWe class)
Complexity & Risk	Low	Higher
Technical Value	Foundational only	Directly relevant to future missions
Operational Use	Limited	High
Strategic Signaling	Weaker	Strong
Cost/Timeline	Faster, cheaper	Slower, more costly

There is a strategic case for skipping a power-only demo and going directly to an integrated in-space NEP one, which eliminates all the lander constraints on the program and enables a higher power first demonstration. NASA already has flight-qualified electric thrusters that could be made available. There are also commercial options to choose from. The regulatory burden is not substantially greater, and the technical integration is feasible assuming the "Manhattan Project test" conditions discussed in section 3 are met. Doing so enables meaningful operations, such as moving cargo in Cislunar space, and makes the demo operationally and geopolitically relevant from day one. It's a higher-risk, higher-reward move.

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[f] In his 1953 "Paper Reactor" memo (https://whatisnuclear.com/rickover.html) Admiral Hyman G. Rickover contrasts the simplicity and flexibility of theoretical "academic" reactor designs on paper with the complex, costly, and schedule-slipping realities of practical reactor construction, warning decision-makers not to confuse elegant concepts with engineering feasibility.

- System relevance. A 100 kWe NEP system exercises the same Brayton converters, high-voltage PMAD, and radiator architecture needed for future megawatt-class power systems.
- Meaningful thrust. Electric thrusters coupled to 100–500 kWe provide tonne-class cargo transport within Cislunar space, an operationally useful capability.
- Surface feed-forward. Mars ISRU and DoD Cislunar sensors both converge on the 100 kWe-plus power regimes; validating that this power class once is more efficient than validating 10 kWe now and repeating the regulatory gauntlet later.
- Launch-approval overhead. Safety, licensing, and ground-handling requirements are dominated by core criticality, not electrical rating; the bureaucratic lift is nearly identical, so it is smarter to "buy" more power on the same paperwork.
- Political signal. Launching a high-profile 100–500 kWe space nuclear demonstration in the 2020s would signal U.S. leadership, positioning the nation ahead of planned PRC/Roscosmos initiatives in the near-term, and demonstrating leap-ahead capability to allies and competitors alike.

In short, a 100–500 kWe NEP demo could be a step to close the strategic gap. Later sections compare execution pathways and schedule variants, but the sequencing logic begins here: fly a reactor-driven power system of consequential scale, then feed forward to surface grids, cargo NEP tugs, and, ultimately, crewed nuclear propulsion architectures.

To succeed, this bold approach demands what we referred to in section 3 as the "Manhattan Project test":

- A centralized lead with real budget and milestone authority
- Stable, adequate, multi-year funding insulated from annual swings
- A strategic imperative strong enough to align leadership and unlock institutional will

Absent these, we risk repeating the failure cycles of JIMO/Prometheus and other ambitious overreaches. But if these conditions hold, we can vault forward.

The logic in this section is clear: start with power, build upward, layer on complexity when ready. But sequencing alone doesn't dictate tempo. In the right political and technical conditions, it may be possible to compress that sequence, to demonstrate flight-scale power and propulsion together, in a single, high-impact mission in the 100s of kW range.

That's the premise of Option 1, explored in the next section: "Go Big or Go Home." It does not reject the feedforward logic laid out above. It operationalizes it, faster. Rather than walking the path step by step, it attempts to vault forward, validating the same systems while capturing the strategic and reputational value of a flagship achievement. It requires crash-program authority, multi-year funding, and political resilience cannot be secured ("Manhattan Project test"),

If, however, the "Manhattan Project test" isn't met, a second pathway, Option 2, *"Chessmaster's Gambit"*, advances the identical power-first thesis in the sub-100 kWe class. This approach sacrifices headline scale for execution resilience, using smaller but existing systems to build supply chains, prove regulatory pathways, build commercial markets and maintain strategic momentum while larger reactors mature. It's less dramatic, but potentially more resilient.

Both Options build on the foundation laid here. The difference is not the destination, it's the speed, structure, and scale of getting there.

5. Three Ways to Build: Leap, Scale, or Hedge

Section 4 established the foundation: **power comes first.** Fielding reliable, scalable nuclear power, both in space and on the surface of a celestial body, is the essential first step to unlocking key national security capabilities, deeper exploration, commercial growth, and propulsion at scale. The question now is how to act on that principle. What path should the United States pursue to move from a power-first foundation to operational systems that support civil, commercial, and national security needs?

This section presents two primary execution tracks plus a narrower exploratory hedge. These are not competing visions, but contingent execution tracks. Each one starts from the same premise: nuclear power systems must be demonstrated early and credibly. Each one reflects a different level of political alignment, institutional capacity, and funding availability.

- **Option 1: Go Big or Go Home**[g] leapfrogs intermediate steps to demonstrate a 100-500kW power system with or without an electric propulsion payload, in the latter case using an NEP flight demonstration to establish U.S. leadership in space nuclear power and propulsion by 2030. It assumes extraordinary political alignment and executes power and propulsion in a single mission.
- **Option 2: Chessmaster's Gambit**[h] takes a more deliberate, modular path, fielding smaller-scale power systems by 2030 through milestone-based public-private partnerships (PPPs), before scaling to propulsion. It mirrors past U.S. approaches like Surveyor-to-Apollo: field early, learn fast, and scale from strength. It is the base case under current constraints and lays the groundwork for propulsion.

Whichever Option is chosen (decision heuristics are discussed in Section 6), the United States should secure an early operational victory by flying a sub-kilowatt commercial non plutonium (Pu-238) radioisotope system (RPS). The mission would test regulatory pathways, build public familiarity with nuclear power on the Moon, and keep momentum alive while larger reactors mature.

- **Proposed Option 3 to Evaluate: Light the Path** is not a substitute for fission-class systems. It is a low-cost, near-term hedge: a commercial radioisotope power system (RPS) demo that establishes regulatory precedent, tests integration processes, and signals seriousness to the private sector.

All three options follow the same feedforward logic: power systems enable critical exploration missions, create commercial growth opportunities, and de-risk larger power and propulsion systems. Power enables critical NASA and defense missions, commercial markets, and NEP. Progress in both accelerates NTP. Each Option builds on that architecture, differing not in direction but in tempo, risk tolerance, and degree of political insulation. Power-first is non-negotiable.

- Option 1 is the ceiling, viable only with extraordinary political alignment.

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[g] This approach is not new to the United States. Fission was discovered in 1938; the $30B+ Manhattan Project detonated a working bomb less than seven years later while building three green-field industrial sites in parallel. Option 1 could be the "mini" Manhattan project to get Americans living and working on Mars soonest as long as it meets the "Manhattan Project test." Source of cost: https://www.nps.gov/mapr/faqs.htm#:~:text=The%20Manhattan%20Project%20cost%20approximately,War%20Powers%20Act%20of%201941

[h] In chess, a gambit is when a player sacrifices something early (like a pawn) to gain a strategic advantage later—tempo, position, or initiative. In this context, it refers to trading short-term flash for long-term impact—getting on the board now, learning from flight experience, and putting the U.S. in a stronger position to scale up quickly and responsibly. The "sacrifice" is not aiming for the biggest, most ambitious flight experiment upfront. Instead, it's accepting a more modest start to gain long-term leverage.

- Option 2 is the floor - it enables steady forward momentum and builds the commercial market.
- Option 3 (once validated) buys time, credibility, and momentum, no matter what.

All Options first establish the foundational capabilities in the first generation (Gen 1) flight demonstration, while in parallel developing the technologies and infrastructure required for follow-on systems with improved capabilities (Gen 2 and beyond). See Figure 5.1. All Options must also confront a set of national bottlenecks: expansion of launch/test/fuel infrastructure, supply chain robustness, regulatory streamlining, and workforce/industrial base development. These must be prioritized as first-order deliverables, not left to program detail. The rest of this section lays out these three pathways including what they require, besides a flight program, at a strategic level. In section 6, we evaluate which pathway, or combination, is best suited to current realities.

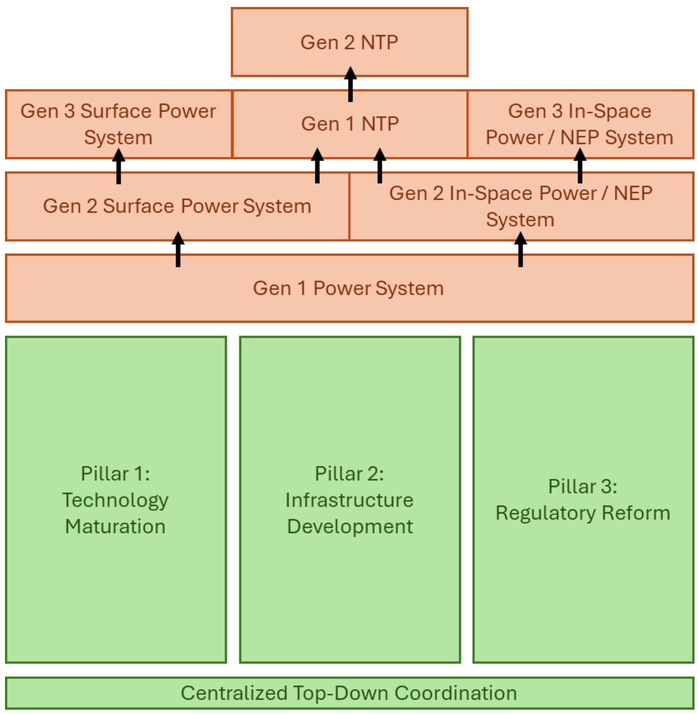

Figure 5.1: Relationship Among the Flight Demonstration Programs and Supporting Pillars

5.1. Option 1 - Go Big or Go Home: Flight Demonstration by 2030

If centralized leadership, stable multi-year funding, and a sense of urgency ("Manhattan Project test") can be secured, the United States should pursue a crash program to field a 100–500 kWe Nuclear Electric Propulsion (NEP) system by ~2030. This is not a detour from the power-first principle, it is a high-power reactor power system that includes a propulsion system. *As with the Manhattan Project or Operation Warp Speed, Option 1 should only be pursued if the enabling conditions are firmly in place.* Absent these conditions, this approach is high-risk and likely to fail. Each Option has four parts – a Gen 1 flight demonstration (followed by Gen 2 and others), and three supporting pillars (Figure 5.1).

5.1.1. Flight Demo

The goal of the Option 1 flight demo would be to demonstrate a 100–500 kWe power or NEP system in space by ~2030 to validate integrated high-power reactor, power conversion, thermal management and propulsion subsystems under realistic mission conditions, de-risk critical technologies, and cement U.S. strategic leadership. The actual power level of the demo would be driven by not only the availability of a reactor in the timeframe of interest, but also by the lift capacity of launch vehicles available at the time. Doing an in-space demonstration eliminates any constraints imposed by lunar lander availability in the proposed timeframe but prepares the nuclear power system for surface delivery as soon as the lander is ready. There are at least three major structural Options to deliver this demo:

- Option 1A (NASA or DoD-led): NASA or DoD conduct a flight demonstration of a 100-500 KWe system, leading all aspects of the mission, including reactor authority, and reaching out to DOE and other Departments and Agencies including National Labs as it sees fit. For NASA, this approach may require statutory changes to the Atomic Energy Act (AEA), though there might be other options to evaluate (e.g., use of Public Law 85-804, a program like naval reactors), to grant NASA nuclear licensing and indemnification powers. For NASA, it also demands significant new internal NASA capacity, creating a high administrative, legal, and political burden.
- Option 1B (DoD-DOE or NASA-DOE partnerships): A joint NASA–DOE or DoD-DOE program, with DOE funding reactor, fuel, nuclear testing and safety analysis, and NASA or DoD funding the mission design, spacecraft, power conversion, thermal management, propulsion systems, system integration and test launch and mission operations. Collaboration with DOE avoids potential AEA amendments, retains DOE's nuclear oversight and safety analysis, enables cost-sharing, and could be more politically tractable.
- Option 1C (NASA-DOE–DoD partnership): A 100–500 kWe nuclear electric propulsion (NEP) demonstrator by uniting NASA's mission expertise, DOE's reactor development, and DoD's mission pull. This is an *opportunistic* model: it proceeds only if and when DoD's operational needs align, and it commits substantial in-kind or financial resources.

Option 1B may be preferable over Option 1A for several reasons. First, Option 1B may avoid the need to amend the Atomic Energy Act (AEA), allowing DOE to retain reactor licensing authority and indemnification responsibility while DoD or NASA focuses on the non-nuclear systems, spacecraft integration and mission design and operations. This eliminates one of the most difficult political and legal hurdles. Second, assuming all of DOE's work is done *with* DOE funding and not transfer of DoD or NASA funds, Option 1B enables cost- and capability-sharing with DOE. Instead of DoD or NASA needing to seek full appropriation from Congress to stand up their own independent nuclear capability (a heavy lift in the current fiscal environment), costs and responsibilities are distributed across both agencies.

Reactor development and testing experience and facilities at several DOE national laboratories can be leveraged. The key is to use a system currently being built to reduce the program's technical and schedule risks and make the program more fiscally sustainable.[i] Option 1B is in principle faster, cleaner, lower risk administratively, and more fundable, while still achieving the same strategic goals as Option 1A.

As discussed in section 4, there is a case for aiming high. A 100-500 kWe-class experiment would deliver a transformative leap in capability, demonstrating that the United States is serious about fielding power-rich infrastructure in space. If successful, it could *skip* incremental steps and deliver the scale required for demanding exploration and defense missions and demonstrate power systems required for in-situ propellant production and large-scale surface operations on the Moon and Mars. However, it <u>must</u> meet the "Manhattan Project test":

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[i] While we based the analysis of Option 1 on government funded reactors currently in build, should commercial alternatives be available which meet the technical, cost, and schedule imperatives for this Option, they should be evaluated. The critical constraints are the ability to develop, build and integrate a 100-500kWe flight system in the timeframe (2028 ground test and pre-2030 launch) which can be launched to a nuclear safe orbit using available launch vehicles.

- Creative Unorthodox Leadership. A program of this scale must operate like a crash effort, with a unified program office spanning budget, procurement, safety, and technical integration, and a mandate to make bold bets across agencies.
- Large and Stable Funding. This approach is only viable with multi-year, protected investment, on the order of $3B over 5 years (Table 5.1 provides an order of magnitude budget that includes launch). Annual appropriations and fragmented budgets will not sustain a system of this scale and complexity.
- Singular Focus. General Leslie Groves drove the Manhattan Project by uniting scientists, industry, and the military under a unified command. Admiral Hyman Rickover delivered the nuclear navy by demanding discipline, technical rigor, and programmatic control. Moncef Slaoui led Operation Warp Speed by integrating private companies, regulatory agencies, and government funding into a single coordinated effort. If the U.S. chooses to go big, it must empower leadership with real authority and political backing.

Figure 5.2 below shows the aggressive timeline needed for the 100kWe-class flight demonstration. It builds to a major system ground demonstration in 2028 followed by flight in 2030. This is enabled by leveraging the progress made in reactors currently being built, along with major investments in the non-nuclear power system elements, spacecraft, and system and vehicle integration. The upgraded Gen 2 system (last line in Table 5.1) leverages the progress made by the Gen 1 program as well as the parallel developments in technology maturation, infrastructure, and policy and regulation.

In addition to the reactor demonstration itself, selecting a high-visibility, mission-aligned payload (see Sidebar) will be critical to building support and demonstrating nuclear's transformative potential. The mission must be both operationally meaningful, addressing logistics, science, or defense needs, and publicly compelling. A range of candidate payloads is outlined in Appendix C to guide discussion and evaluation.

The supporting pillars shown in Figure 5.1 are essential for the flight demo to succeed. The technology isn't ready, and the regulations and infrastructure are outdated/inadequate, not only for the Gen 1 flight demo, but especially for what comes after the first demonstration. As discussed in Section 4, ~100 kWe isn't the end game, but a steppingstone to multi-megawatt systems.

Without parallel investment in technology, regulatory reform, development and testing capacity, even the best-designed program will stall. These pillars will support not just the power/NEP demonstration and subsequent improvements but also–in the long term–

Payload Selection Without Operational Risk

To maximize impact, the demo mission should include a small, compelling payload that sparks public imagination, analogous to the placement of the Ingenuity helicopter on the Mars Perseverance mission, without compromising the demo's core objectives. This could include a high-resolution, lighting-independent surface mapping radar on a nuclear-powered orbital or aerial platform. Recent lunar lander failures (e.g., IM-2's tipped landing in February 2025) underscore the limitations of current terrain navigation systems, especially near the lunar South Pole. A fission-powered orbital platform or low-flying vehicle could deliver continuous, high-resolution, and shadow-agnostic mapping of polar terrain using radar, LIDAR, or multi-spectral imaging. This persistent sensing capability would dramatically improve autonomous landing precision, derisk human and robotic missions, and support long-term lunar base site selection. Other examples are included in Appendix C. The key is to choose a payload that is self-contained, low-mass, and non-intrusive, yet visually or scientifically captivating, enhancing public support and program visibility. Past efforts like Prometheus over-weighted the destination science goal, leading to architectural complexity. In contrast, this demo should prioritize tech validation first, with payloads that enrich the story, not drive the mission.

development, larger scale surface power systems, NEP and NTP. These pillars are not a menu of options to choose from. They are a package deal: tech maturation, infrastructure improvement, and regulatory overhaul, all at once. We are not building one system; we are laying tracks for a scalable "railroad" of space power and propulsion. All pillars must move in parallel if we want to escape the cycle of false starts.

> **A Note on Commercial Readiness for Space Nuclear**
> It is tempting to believe that the commercial sector already has space nuclear in hand, and that all the government needs to do is "get out of the way." But this view is incomplete. Many companies have promising designs and bold visions, but nearly all still face technical hurdles that require significant support. Fuel forms remain unqualified. Brayton and Stirling power conversion systems need flight-relevant validation. We need a nuclear safe launch integration facility at the Kennedy Space Center (KSC) to enable launch vehicle system integration and test for these large power systems. Ground test infrastructure, such as hot cells, zero-power test facilities, power system test capability and nuclear capable thermal vacuum facilities, is aging, insufficient, or unavailable to commercial users. Market uncertainty prevents commercial capital from funding these alone. Regulatory streamlining is essential, but it does not substitute for enabling technology, infrastructure or public-private partnerships that drive systems from TRL 4 to TRL 9. Without parallel investment in tech maturation, infrastructure, and early procurements, regulation reform alone will not bring commercial space nuclear to life.

5.1.2. Supporting Pillar 1: Focused Technology Maturation

This pillar adopts the Manhattan Project's model of "parallel bets", where multiple reactor types and conversion systems are pursued in parallel, not sequenced, to hedge technical risk and maximize learning. Equally critical is maintaining team continuity across funding cycles, an explicit correction to the team churn that undermined Prometheus.

Beyond maturing reactor concepts currently being built, the government needs to invest decisively in critical nuclear technologies needed for both the flight demonstration and scalable operational follow-on systems. Focus areas include advanced reactor fuels (e.g., higher-temperature HALEU), high-temperature materials, modular reactor and shielding designs, power conversion systems, radiation-hardened, long-life PMAD components, heat rejection systems, high-thrust electric propulsion technologies and system/spacecraft integration. Investments must be made at NASA, DOE, national labs, and industry in parallel. Results from government funded programs must be available to all potential U.S. industry providers.

Focused, stable R&D is essential to bridging the "valley of death", advancing lab-proven technologies to operational flight systems. Targeted investment will reduce technical risk, enable scalability beyond a single demo, and cultivate the specialized workforce needed to sustain a space nuclear enterprise.

5.1.3. Supporting Pillar 2: Enhanced Infrastructure Capability

Following the Nautilus model, test before you fly, we must invest now in the ground-based facilities required to develop, validate and integrate space nuclear systems at scale. While critical nuclear irradiation facilities exist at national laboratories (Sandia National Laboratory, Oak Ridge National Laboratory, Idaho National Laboratory, National Institute of Standards and Technology, White Sands Test Facility) and universities (MIT, Ohio State University, North Carolina State University, and others), they generally have a long wait time for access, or do not provide the specific capabilities required for space nuclear power systems. Additionally, there are several other critical development and testing capabilities that currently either do not exist or are inadequate. Without modern, purpose-built infrastructure, even a successful demonstration will stall, delaying future deployment. Most critically, the

U.S. currently lacks a nuclear-capable launch processing facility for fission systems at Kennedy Space Center (KSC), Cape Canaveral Space Force Station, or any other national launch site, a foundational gap that risks delaying any fielded reactor, regardless of technical readiness. In parallel, expansion of fuel production capacity (HEU and HALEU) and securing multiple suppliers for key reactor subsystems (reflectors, heat pipes, power conversion) are strategic imperatives, not supporting details. Infrastructure investment must encompass the entire chain: launch processing, fuel production, development/test beds, and rapid integration capabilities.

Two of the most time-critical investments are: first, establishing a launch site nuclear-capable launch processing facility at KSC. This capability is very different from the current Pu-238 RTG launches, and it is necessary to ensure that a safe, secure, and fully capable site is available for final processing and integration of the space nuclear power system with the spacecraft and launch vehicle. Second, we must establish critical space nuclear R&D facilities including a space reactor and fuels R&D center, a space reactor hot-cell and post irradiation evaluation (PIE) facility; space nuclear power system integration test facility; and nuclear-rated thermal vacuum facility.

These investments directly address critical barriers to development, production and operational deployment of space nuclear systems. The goal is to build facilities that serve not just the demo, but a fleet of future space nuclear systems, power for in-space and surface systems as well as propulsion. We could build the pad while we build the reactor. Our research showed that the Manhattan Project, SNAP, Rover, and Starbase all erected test stands in parallel with hardware, not after it.

In this strategy, infrastructure investment is non-negotiable. Without it, we will bottleneck both the flight demonstration and all follow-on systems. Facilities must be modular, scalable, and accessible to multiple users across government and industry.

5.1.4. Supporting Pillar 3: Streamlined Policy and Regulatory Reform

Regulatory reform is a critical enabler, not an afterthought. Without it, even modest reactor flight demonstrations will be delayed or derailed. The current regulatory framework is fragmented and slow. No single agency owns the full lifecycle of a space nuclear system, from ground safety and launch authorization to orbital operation and end-of-life disposal. Mandates overlap, accountability is diffuse, and review queues stretch into years. The result is bureaucratic inertia, risk-averse behavior, and massive uncertainty for both government and commercial actors. To convert regulation from roadblock to runway, we will need:

- Time-bound licensing decisions: Establish integrated review timelines for nuclear payloads, no more than 12–18 months from proposal to final launch authorization.
- Expansion of NSPM-20 “left” of launch: Today’s NSPM-20 framework governs nuclear launch approvals but says little about pre-launch activities, which tend to be burdened by excessive bureaucracy and red tape. Some experts believe NSPM-20 could be expanded to cover development testing, production activities, system testing, transportation, and launch site assembly, integration and testing prior to launch.
- Space-specific indemnification structure: The Price-Anderson Act currently covers DOE contractors. A tailored indemnification model is needed for space nuclear systems.
- Agency role clarity: There is no agency assigned roles for operational approval and disposal of space nuclear systems - either in-space or on the Moon or Mars. Interagency coordination cannot be left to ad hoc MOU negotiations. It must be built into the execution model.

Following the Operation Warp Speed model, one approach toward a crash program could be to embed regulators with the development teams and create a centralized regulatory execution cell, likely White House-led, with authority across DOE, NRC, and NASA.

Bureaucratic delays, redundant reviews, and fragmented authority have killed more nuclear space programs than technical failure. Fast, disciplined regulatory pathways will be as important to success for the Go Big program as reactor or thruster design. The sidebar below lays out a greenfield approach to developing and launching space nuclear systems.

5.1.5. Centralized Coordination

Delivering Option 1's aggressive 2028 ground-test schedule, and building the three supporting pillars, requires more than agency-level efforts. No single department can field a complete space-nuclear system on its own. Only a unified leadership office, vested with authority to align budgets, harmonize requirements, and enforce cross-agency accountability, can cut through silos, synchronize NASA, DOE, DoD, FAA, and other stakeholders, and drive this program to the pad on time.

In addition to single empowered points of contact at DoD, NASA and DOE, it will be necessary to have a designated single point of responsibility at the White House, either within the National Space Council, the National Security Council, or the Office of Science and Technology Policy. This individual must report directly to the President's National Security Advisor and the Director of OSTP (or the Executive Secretary of the National Space Council who can speak on behalf of the Vice President), with a mandate to coordinate space nuclear activities across NASA, DOE, DoD, FAA, and other relevant agencies.

This individual must have real budget coordination authority, not just convening power. Without control over budgets and milestones, past efforts at interagency coordination have failed to overcome inertia. Specifically, the White House space nuclear lead would be empowered to:

- Set and enforce interagency timelines for space nuclear initiatives
- Align and deconflict budget requests across DoD, NASA and DOE
- Track progress on critical technology and infrastructure deliverables
- Resolve policy and regulatory conflicts quickly, including launch licensing, indemnification, and fuel availability
- Drive forward common standards for nuclear system safety, mission authorization, and international consultations

Historical lessons are clear: crash programs like the Manhattan Project, Nautilus, and Operation Warp Speed succeeded because of strong, centralized leadership empowered to cut through bureaucracy and hold teams accountable for results. Space nuclear requires the same model today.

Without this leadership, fragmented budget requests, regulatory delays, and interagency turf battles will undermine execution of Option 1.

5.1.6. Agency Roles and Appropriations

Delivering space nuclear capability in Option 1 may require both a combined program office and distinct roles for NASA, DOE, and the National Laboratories, each aligned with their comparative strengths. These roles must be clearly defined, formalized, and separately appropriated by Congress to ensure accountability and sustained progress.

- DoD/NASA: Lead for mission design, spacecraft, systems integration, power conversion, PMAD, propulsion, non-nuclear system testing, launch and mission operations.
- DOE: Lead for reactor design, fuel and reactor production, nuclear ground testing, and other safety related roles per the Atomic Energy Act, NSPM-20 and other relevant policies.
- National Laboratories: Provide test infrastructure, nuclear validation, materials expertise, and support for commercial access to facilities.

These roles must be codified in statute or executive agreement, not left to informal MOUs. Agencies must make the mission an explicit priority. Clear authorities and appropriations reduce duplication and turf conflicts, while strengthening interagency coordination. Program offices must physically co-locate engineering, contracting, and regulatory staff to replicate the real-time problem-solving model of Nautilus and Operation Warp Speed.

5.1.7. Budget and Timeline for Option 1B

We have estimated a rough order-of-magnitude budget of about $3B total over five years to get through the in-space demonstration and start work on the second-generation system.[j] Only this level of budget will enable a credible, first-of-its-kind NEP system in orbit, while building the foundation for follow-on megawatt-class systems. Each line item reflects a necessary investment to bridge from Gen 1 demonstration to a sustainable, scalable space fission enterprise.

Table 5.1: Rough Order of Magnitude Budget for Option 1B

	USD (Million)
Flight Demonstration System - 100kWe-class	1,170
Gen 2 System (500kWe leverages Gen 1 & new tech, yr 5 only)	160
Pillar 1: Tech Maturation	1,060
Pillar 2: Infrastructure	470
Pillar 3: Policy and Regulation	75
Total	**~3B**

[j] For reference, the JIMO/Prometheus program was costed at ~$21B. Source: NASA (2005). JIMO/Prometheus Project Final Report. Page 178. The proposed flight demo is very different from JIMO/Prometheus.

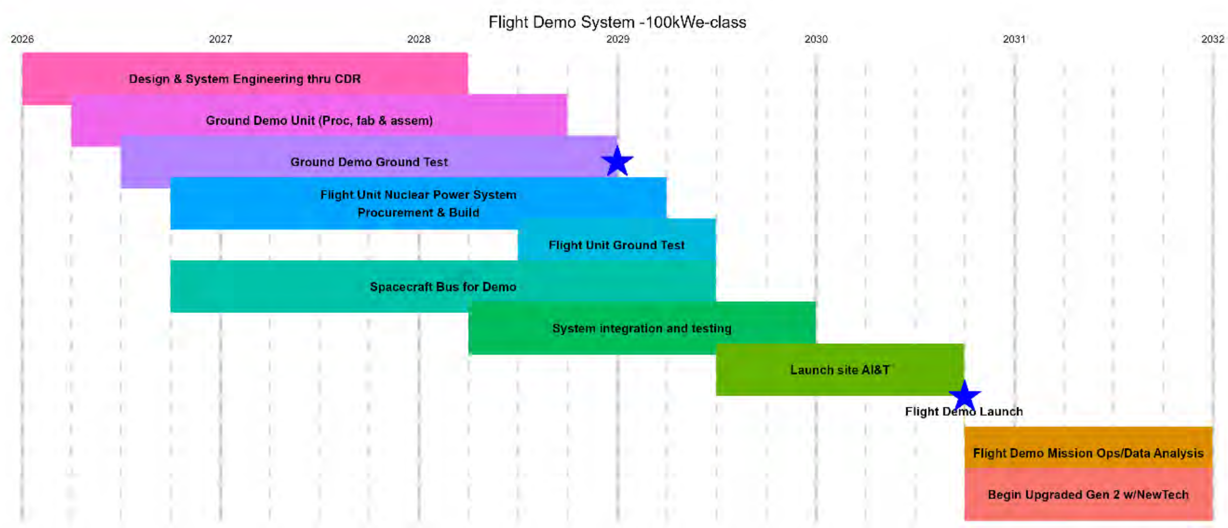

Figure 5.2: Timeline for Implementing Option 1B

The two blue stars refer to the ground test (2028) and flight test (2030)

To meet the 2030 flight target, system design, licensing, infrastructure, and integration must begin by 2026. This is a crash-program budget, intended to reflect the urgency and ambition of establishing U.S. leadership in nuclear electric propulsion.

5.2. Option 2: Chessmaster's Gambit: NASA and DoD PPPs

If the funding level and extraordinary alignment required for Option 1 is absent, Option 2 pursues a smaller power-first pathway: Gen 1 systems (10-100 kWe) that can demonstrate a ground test by 2028 and fly no later than 2030, followed by Gen 2 systems (≥100 kWe) that scale up in the early-to-mid-2030s through public-private partnerships (PPPs). This execution track mirrors past U.S. success stories, small, fast, milestone-driven projects like Mercury before Apollo, Falcon 1 before Starship, and the Ingenuity helicopter before a Mars helicopter fleet.

The goal is not to wait until perfect systems can be built, but to field early, learn fast, create momentum, and lay the foundation for larger commercial ambitions. Rather than betting on a single mega-project, this approach lays "chess pieces" across the board, small but useful systems that build confidence, validate regulatory pathways, and seed commercial markets. If executed well, before 2030, the U.S. would have one or more nuclear power systems in space, creating mission pull, commercial capability, and political durability.

This track embraces deliberate sequencing: fly first-generation power systems, while simultaneously maturing the technologies, infrastructure, and regulatory frameworks needed for larger systems. The same maturation pathway as in Figure 5.1 applies (except the pillars focus on slightly different activities).

This stepwise approach is not slow, it is strategic, fast, and resilient. This is how Apollo won. NASA didn't start with Saturn V. It started with Mercury and Gemini, building operational experience and confidence. Similarly, Falcon 9 didn't emerge overnight, it was preceded by Falcon 1. Space nuclear must follow the same logic: field operational systems fast to build a durable path to scalable power and propulsion. The model is the Wright Flyer, not the SR71.
As with Option 1, Option 2 is underpinned by a tightly integrated set of supporting pillars: technology maturation, infrastructure build-out, and streamlined policy reform. And as with Option 1, the pillars are a package deal: tech maturation, infrastructure improvement, and regulatory overhaul, all at once.

Examining the Concept of Extensibility
Extensibility in a space-nuclear context goes beyond raw power scaling. Even a modest system demonstration, say, a microreactor on a robotic lander or an Earth-orbit power module, delivers outsized value not only by providing kW-class power that unlocks missions not feasible with batteries and solar cells but also by clarifying regulatory pathways, refining payload-integration procedures, and validating payload-processing workflows through real-flight thermal-management and radiation-shielding demonstrations. These operational precedents shorten approval timelines, ease interagency coordination, and prove modular packaging and rapid refueling protocols, shrinking uncertainties and accelerating follow-on architectures across the portfolio.
Just as the first transistor radio delivered simple, battery-powered amplification, revolutionizing portable audio even though its basic circuitry would be replaced within a few product generations, a focused nuclear demo can yield immediate, mission-critical insights without waiting for a perfect, multi-hundred kilowatt system. By treating each demonstration as a standalone "extensibility enabler," we de-risk the hardest questions early and pave the way for larger systems.

5.2.1. Flight Demos - NASA and DoD

Option 2 centers on launching two parallel flight demonstrations delivering 10-100 kWe:

- NASA-led surface power PPP for the lunar surface by ~2030 (with a ground test by ~2028). Power levels for the NASA system will be driven by lander payload weight landing constraints.
- DoD-led in-space power PPP by ~2030 (with a ground test by ~2028). The power levels will still be limited by the lift capacity of the launch vehicles available at the time of launch.

Each PPP should support at least two suppliers through a full-scale nuclear power system ground test with a down select to a single flight system provider (one each for NASA and DoD). The PPPs should have milestone-based disbursements, guaranteed flight opportunities, and long-term procurement visibility to follow-on systems. Agency Requests for Proposals should specify key demonstration capability objectives while remaining technology-agnostic, allowing diverse reactor designs, conversion technologies (e.g., Stirling, Brayton, thermionic), and fuel types (HALEU, HEU) to compete based on achieving the desired mission and system capabilities at the lowest cost to the government.

Separate PPPs for DoD and NASA are necessary: NASA's requirements for rugged, dust-tolerant, mobile surface power that protects humans from radiation differ fundamentally from DoD's needs for autonomous orbital systems. Diverging missions, timelines, and operational environments demand tailored execution models, avoiding the pitfalls of "one-size-fits-all" programs. DoD may also wish to use HEU instead of HALEU for its PPP (a decision that should also be left up to the commercial entities that bid for the award).

This structure intentionally mirrors the successful sequencing in other fields: Mercury before Apollo, Falcon 1 before Falcon 9, Ingenuity before operational Mars helicopters. It also builds on the experience from the NASA Commercial Orbital Transportation Services (COTS) and Commercial Resupply (CRS) programs, both of which had multiple providers and used milestone-based contracts to drive program performance. The logic is not to delay capability, but to move fast, learn fast, and build scalability through early operational experience.

For Option 2 to succeed, the government must clearly articulate high-level mission requirements, scope, scale, and timeline, without over-specifying design solutions. Specifically, agencies should define strategic intent by stating: (1) the expected total investment envelope, (2) desired launch timeframe, (3) required system lifetime, and (4) target power class (e.g., 10-100 kWe). However, they must avoid prematurely dictating technical pathways such as specific power levels, conversion methods (e.g., Brayton vs. Stirling) or fuel enrichment levels (HEU vs. HALEU). These design trades should be left to the bidders to optimize against cost, performance, and lifetime. Imposing rigid or mismatched technical metrics too early risks undercutting innovation, inflating costs, and selecting solutions misaligned with system-level performance. In short: strategic clarity, not technical micromanagement, is essential.

NASA and DoD must also learn lessons from recent PPPs that underscore that simply adopting fixed-price contracts or milestone payments does not constitute a true public-private partnership. Programs like the Gateway Power and Propulsion Element (PPE) faltered because NASA retained traditional oversight, imposed bespoke requirements, and failed to relinquish meaningful control, undermining the very commercial practices they sought to emulate. The result was cost overruns, vendor distress, and eroded trust. For Option 2 to succeed, the government must design partnerships that go beyond contract structure to align incentives, authority, and accountability. This includes matching contract type to market maturity, enabling real competition or managing its absence transparently, and ensuring internal champions and institutional memory sustain commercial best practices. A partial or hybridized model risks repeating past failures and jeopardizing the long-term viability of the space nuclear ecosystem.

Why Space Nuclear PPPs Are Harder Than COTS
A critical question is why a commercially-led nuclear PPP would work. In nearly 75 years of space nuclear system development, there is no precedent for commercial development - the dozen or so programs the government has funded to-date were all traditional cost-plus contracts. For technically-ambitious, one-of-a-kind programs where NASA is the only customer, production is limited to only one (or a few) of the systems, and which are dependent on significant technology development, a traditional approach is considered more appropriate. The highly successful NASA COTS/CRS programs, that used a PPP approach, met four criteria: (1) No technology breakthroughs were required – we were not pushing the technological state of the art by flying cargo and people to and from low Earth orbit; (2) There were very real prospects of other customers beyond NASA – spaceflight participants and sovereign clients were, even the start of the program, existing markets with substantial growth potential; (3) Government foundational customer base – the International Space Station represents a long term, repeatable customer base: (4) Strong industrial base – several U.S. companies have demonstrated the capability to develop safe and reliable cargo and crew transportation system(s). Some might argue that nuclear meets none of these criteria.

Space nuclear does not meet all 4 criteria above: (1) no, nobody has done space nuclear before, so there will be some technology development required to meet the performance, lifetime, mass and reliability requirements. (2) yes, there are commercial space applications for space nuclear, even though they are in the future; (3) no, but NASA and the Space Force are likely reliable customers, and if the programs are designed well, multiple customers would establish power purchase agreements or purchase multiple smaller units (fleets) to support their power grids rather than one-off large battlestar galactica sized systems: (4) no, but the industrial base is growing, largely driven by terrestrial Micro/Modular reactor work.

This means the PPPs would need significant initial government support to become successful. In general, the four areas of risk must be addressed for PPPs to work and begin the transition to a fully commercial marketplace for space nuclear power systems. This is conceptually shown in Figure 5.3, which shows the relative magnitudes of government and private funding as the risk levels from market, technology, infrastructure access, and policy and regulatory barriers are reduced over time.

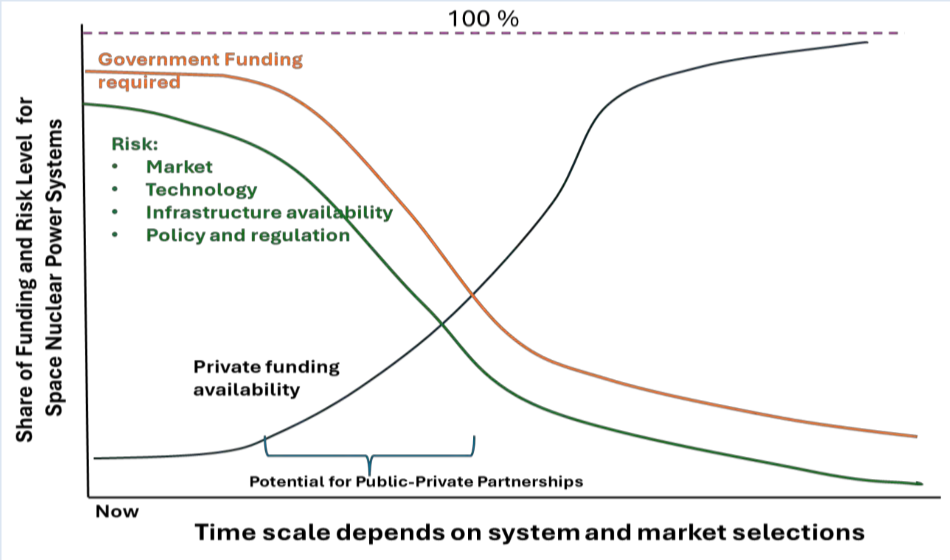

Figure 5.3: Impact of Risk on PPP Funding by Time

Building on Gen 1 flight data as well as the parallel technology maturation, infrastructure and regulatory reform investments, each PPP will compete for a follow-on Gen 2 award to deliver ≥100 kWe systems capable of modular clustering to MW-class power. Each of the pillars is discussed below.

5.2.2. Supporting Pillar 1: Targeted Technology Maturation

A tightly focused, milestone-driven technology maturation program must run in parallel to support flight demos and future scaling for operational systems. This program, led by NASA, DoD, and DOE in collaboration with industry and national labs, should prioritize critical nuclear power system technologies needed for both the experiment and scalable follow-on systems. Similar to Option 1, the focus areas include advanced reactor fuels (e.g., higher-temperature fuel including but not limited to HALEU), high-temperature materials, modular reactor and shielding designs, power conversion systems, radiation-hardened, long-life PMAD components, heat rejection systems, and system/spacecraft integration. For future use in NEP systems, high power electric propulsion systems also require investment, though this could be delayed by a year or two in this Option. Investments must be made at NASA, DOE, national labs, and industry in parallel. Results from government funded programs must be available to all potential U.S. industry providers.

The goal is not to fund open-ended research, but to bridge the "valley of death" between promising concepts (TRL 3–4) and flight-ready systems (TRL 7–9). Investments must directly support the flight demos and operational systems but also lay the groundwork for scaling to larger megawatt-class systems over time. We need focused programs that directly address the risks holding back private investment so that we can, over time, create a commercially viable space nuclear enterprise.

Parallel bets across multiple designs, fuel forms, and power conversion methods should be pursued, hedging technical risk rather than picking a single winner prematurely. This deliberate parallelism reflects lessons from successful crash programs like the Manhattan Project and Operation Warp Speed: multiple parallel paths at early stages speed overall success, not delay it.

5.2.3. Supporting Pillar 2: Infrastructure Capability and Access

As with Option 1, infrastructure investment is critical for Option 2 to work. Similar to Option 1, areas of investment include:

1. Fuel production: Depending on commercial proposals' needs, expand HEU and HALEU availability and develop space-qualified fuel forms. Existing supply may be insufficient to support even one full-scale program. Without near-term investment in enrichment, fabrication, and encapsulation capabilities, both PPPs and any future megawatt-class missions will stall.
2. Development and Test facilities: Modernize and expand reactor development and test capabilities, thermal vacuum chambers rated for nuclear systems, zero-power critical test capabilities, hot cell and post-irradiation evaluation (PIE) labs, system integration and testing capabilities. Most existing facilities are either at capacity, not nuclear-rated, or inaccessible to commercial teams.
3. Launch site support: Develop nuclear-capable payload handling and integration facilities at KSC, Cape Canaveral Space Force Station, and alternate launch sites. This includes radiation-safe integration areas, security zones, and rapid transport logistics, none of which currently exist at scale for fission-class systems.

These capabilities must be built with dual-use flexibility, supporting both PPP demos and eventually high-power and propulsion systems. Investments in fuel production, expanded test capabilities, and ground handling facilities enable both power and propulsion architecture and reduce per-mission cost over time.

Infrastructure must also be designed for shared access, modular, government-owned but commercially available, so that follow-on systems from new entrants can leverage existing assets without duplicating cost. As with Option 1, we could build the pad at the same time as we build the reactor, the Manhattan Project, SNAP, Rover, and Starbase all erected test stands in parallel with hardware, not after it.

Without targeted infrastructure investments, even the most promising PPPs will face long delays and high risk, dramatically reducing the potential for commercial investment. Critical components cannot be validated, integrated systems cannot be tested, and testing, transportation, and launch approvals will be delayed due to safety review backlogs.

5.2.4. Supporting Pillar 3: Streamlined Policy and Regulatory Reform

Policy and regulatory reform for commercially oriented partnerships is not an afterthought; it is a technical enabler. Much of the groundwork mirrors Option 1, but the commercial context requires sharper timelines and a lighter federal touch. Immediate priorities include expanding NSPM-20 processes to cover ground processing and in-space operation, and establishing time-bound, integrated licensing pathways. A tailored indemnification model is needed especially if PPPs are expected to operate under commercial licensing regimes. Without it, insurance markets will not engage, and developers will balk.

Given the outsized role of the FAA's Office of Commercial Space Transportation (AST) per NSPM-20, regulatory reform is critical at FAA. FAA is a pacing item and should not replicate nuclear safety credentials that DoD and other agencies have.

5.2.5. Centralized White House Coordination and Oversight

As with Option 1, success will require empowered, centralized leadership, not interagency consensus-building. The flight demo and the supporting pillars intersect multiple missions, authorities, and budgets across NASA, DoD, DOE, FAA, and more. Without a single point of coordination, past efforts have collapsed under their own fragmentation.

The White House must designate a space nuclear executive who would be charged with driving alignment across agencies. This official must have authority to:

- Align budgets, timelines, and technical milestones across NASA, DOE, DoD, and FAA.
- Resolve interagency disputes over safety, procurement, and execution.
- Track progress on infrastructure, licensing, and PPP delivery.
- Oversee international consultation, norms development, and foreign engagement.

This is not a bureaucratic preference, it is a structural requirement. Without this, delays multiply, risk aversion dominates, and mission goals decay into process. If we want space nuclear fielded this decade, someone must own it.

5.2.6. Agency Roles and Appropriations

For Option 2 to succeed, each participating agency must have clearly defined roles, formal responsibilities, and distinct appropriations, aligned with its comparative strengths and statutory authority (and without exchange of funds). The space nuclear capability must be an explicit objective for each agency to ensure the program receives the appropriate priority.

- NASA[k]: Lead for the lunar surface power PPP, power conversion R&D, PMAD and thermal systems, spacecraft integration, and non-nuclear test facility management and access controls.
- DoD[6]: Lead for the in-space PPP, power conversion R&D, PMAD and thermal systems, spacecraft integration, and non-nuclear test facility management and access controls.
- DOE and National Laboratories: Responsible for supporting private companies in designing reactors, HALEU and HEU fuel production, nuclear system ground testing, nuclear test facility management and access controls, and launch safety approvals. Provide critical nuclear infrastructure, reactor and fuel development labs, hot cells, PIE labs, zero-power test facilities, plus support for materials qualification, radiation testing, and component validation.

Given the launches would be commercial, FAA (the Federal Aviation Administration), through its Office of Commercial Space Transportation (AST), plays a critical role in approving any space nuclear launch. While DOE and NRC oversee nuclear safety on the ground, FAA holds exclusive launch licensing authority, and must be integrated into early program planning, not treated as an after-the-fact reviewer, to ensure speedy granting of the launch license.

These agency roles must be codified through executive memoranda, interagency agreements, and distinct Congressional appropriations. Without clarity and statutory backing, turf battles and budget uncertainty will undercut execution. Most importantly, there should be no transfer of funds across Agencies. Every Agency must bring its own resources to the table to fulfill its role in the ecosystem.

5.2.7. Rough Timeline and Budget Overview for Option 2A

Figure 5.4 illustrates the implementation timeline for Option 2A (NASA) and 2B (DoD), highlighting key milestones including the ground test (~2028) and flight demonstration (~2030). While less compressed than Option 1, this schedule remains ambitious, delivering operational space nuclear power systems within five years while building the regulatory, infrastructure, and technical foundations required for long-term scalability.

[k] While there appears to be duplication here between the efforts at NASA and DoD, it is crucial to recognize that the missions and environments they are focused on are very different, with NASA focused on surface power for both the Moon and Mars, and DoD focused on in-space. It will be critical to have effective coordination between the two agencies on their efforts to avoid duplication. This is also consistent with the strategy of supporting multiple parallel bets to ensure dissimilar redundancy.

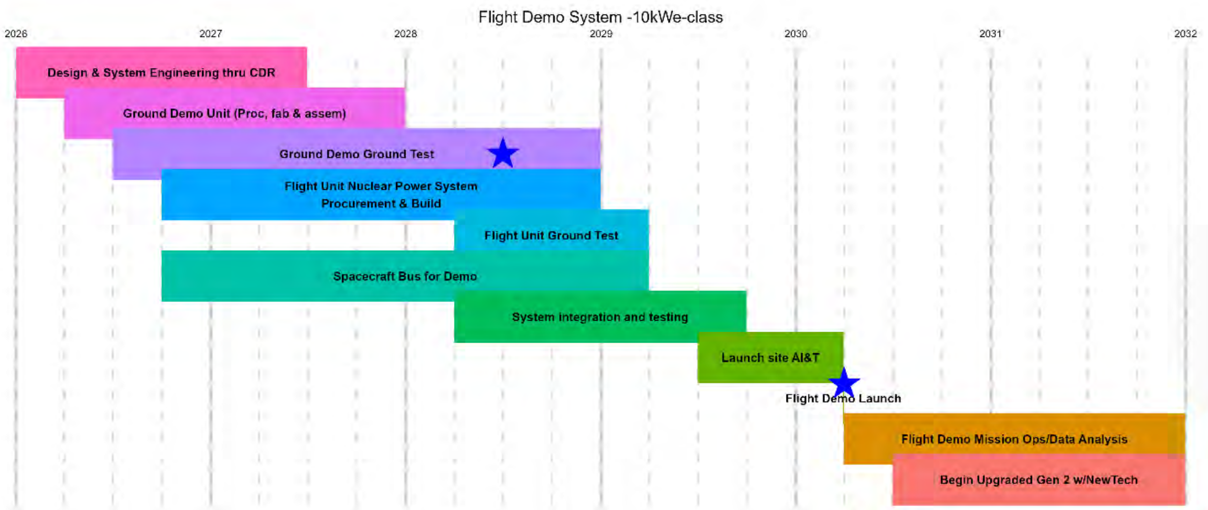

Figure 5.4: Timeline for Implementing Option 2A and 2B
Option 2 uses data from INL to inform designs
The two blue stars refer to the ground test (2028) and flight test (2030)

Over five years, Option 2 proposes about $2 billion in total public funding for space nuclear power development, split between NASA and DoD (Table 5.2). This funding results in 2 space system flight demonstrations. Each agency supports a public-private partnership (PPP) with two providers through ground testing, down-selecting to one provider for a flight demo by 2030. Gen 1 demos are cost-shared at a 75/25 public-private split. In parallel, the government fully funds technology maturation, infrastructure, and policy and regulatory reform.

Table 5.2: Rough Order of Magnitude *Government Only** Budget for Implementing Options 2A (NASA) and 2B (DoD) - combined

	USD (Millions)
Total 5-year Flight programs funding (Gen1+Gen2)	525x2
Total 5-year Technology Maturation budget	600
Total 5-year Space Nuclear Infrastructure budget	470
Total 5-year Space Nuclear Policy and Regulation budget	45
Total	**~2B**

**Assumes 75-25 split Gen 1 50-50 split Gen 2*

This is a somewhat more modest investment relative to Option 1, yet it delivers multiple fielded commercial nuclear systems, necessary technology, infrastructure and regulatory frameworks, and commercial momentum, all by ~2030. Option 2 provides a lower risk path to a successful space nuclear enterprise. It is a prudent, achievable, and strategically powerful path under current political and fiscal realities. It allows the United States to:

- Field a ground test by ~2028
- Field two operational (Gen 1) nuclear systems by ~2030.

- Build user and funder confidence.
- Establish the required technology and infrastructure for subsequent systems
- Establish regulatory precedents for future nuclear systems.
- Catalyze a commercial space nuclear market.
- Lay the groundwork for Gen 2 and scalable megawatt-class systems in the 2030s.

If we cannot Go Big, we must go smart and go now.

> **Surface Reactor Payloads for NASA**
> As with any first-of-kind demo, the power plant, not the payload, is the star. Any attached system must be modest in size, risk, and integration complexity. The goal is to generate real operating hours, not maximize science return.
>
> Options for payloads include: a lightweight wireless or cabled recharge station that could demonstrate persistent power delivery to a mobile system, validating the hub-and-spoke model for lunar logistics or a small autonomous hauler, like a "lunar golf cart", could move inert cargo between landers and habitats.
>
> Demonstrating even basic autonomy under real power constraints would mark a leap forward. The Moon isn't Mars, but it's close enough to test what matters: installation, shielding, startup, operation, fault detection, control, and thermal rejection in a real, dusty, low-gravity environment. Each hour of fault-free operation on the Moon raises confidence for future missions for Mars.

5.3. Evaluate Parallel Option 3. Light the Path - Commercial RPS to the Moon

This Option, once validated, proposes a fast-executing, low-cost demonstration, a commercial radioisotope power system (RPS) flown to the lunar surface by 2028, likely in the sub-kilowatt class. It is designed not to transform capability, but to illuminate the regulatory, procurement, and integration hurdles to commercial systems that must be resolved before more ambitious systems can succeed. Like the fission-class pathways, this Option requires targeted investment in infrastructure, regulatory reform, and streamlined procurement to succeed at scale. Even a low-power demo cannot launch without solving national-level bottlenecks in approval and integration.

This pathway is exploratory, not yet deeply developed across government, and rests on multiple uncertainties. But if executed well, it could function as a proving ground: helping NASA, FAA, DOE, and industry navigate licensing, export control, indemnification, and launch safety for real missions, on real timelines. It would also provide NASA and commercial suppliers with early operational data on alternative isotopes to Pu-238 (such as Am-241 or Sr-90), serve as a signal to commercial innovators, and potentially support early lunar science or infrastructure. Several experts interviewed believe that these alternative isotopes could provide dramatic cost reductions (by >10X) over the current Pu-238 RTGs, as well as higher availability. Critical infrastructure required to support these alternative isotopes includes establishing a reliable U.S. supply of Am-241 which is not domestically available in the quantities required to support an on-going series of mission applications.

The concept is feasible with a commercial payload delivery via NASA's CLPS program or through independent partnerships with companies like Firefly, Astrobotic, Blue Origin, or Intuitive Machines. While not as transformational as a 10- or 100 kWe-class fission reactor power system demo, it offers a visible, quick, and credible win in a domain that has long suffered from over-promising and under-delivering.

Time constraints did not permit a data-driven analysis of this Option. Instead, we propose in section 7 several near-term steps, centered on radioisotope fuel production and supply chains, flight authorization pathways, and public-private engagement, that could enable such a demonstration if political and programmatic alignment emerges. Until such analysis is complete, Option 3 remains exploratory and is *not* included in aggregate cost totals.

If successful, this Option could de-risk future fission-class missions by generating institutional experience with launch approval, radiation safety planning, and operational integration. In short: Option 3 is a hedge. It does not deliver high power, but it does deliver fast critical capabilities to survive the lunar night, learning, credibility, and momentum, at relatively low cost and political risk.

Section 6 next compares both fission Options head-to-head with a decision matrix that incorporates cost, risk, and political feasibility. It also highlights the key structural tensions that program executives must manage, through leadership, funding structures, program design, and relentless focus.

6. Choosing Between Options

Each of the three strategic Options outlined in section 5 offers a different balance of ambition, risk, and realism. Option 1 would deliver transformational capability but demands extraordinary alignment and resources. Option 2 offers a more executable pathway grounded in near-term missions, existing agency strengths, while leveraging the rapidly advancing commercial nuclear sector. Option 3 is low hanging fruit but requires further analysis before it can be responsibly implemented.

Indeed, it is worth thinking about two additional sub-options within Option 1. If the full NEP demo becomes infeasible, due to integration risk, cost escalation, or loss of political momentum, a viable pivot is to fly the same 100–500 kWe reactor and power system but without propulsion. A power-only demo reduces complexity (no thrusters, plume interaction, or trajectory planning) while still proving the most critical, high-risk elements: reactor and power system startup and control in space as well as the radiator deployment and thermal management and preparing the nuclear power system for surface operation. This keeps the program in the 100–500 kWe class, essential for strategic value, but buys schedule and cost relief. Alternatively, if a lander capable of delivering a >100kW system to the lunar surface is available on time, a high-power surface demonstration could be completed, setting the stage for large scale surface operations.

Such a demo could power a Cislunar logistics node, a future DoD platform, or act as a precursor for surface power systems. It maintains U.S. credibility in flight reactor capability, preserves the industrial and regulatory advances made under Option 1, and avoids total program collapse if propulsion coupling proves too burdensome. It’s not failure, it’s a controlled decoupling that salvages strategic value.
The government could also pursue a hybrid approach, executing both Option 1 and Option 2 in parallel, as was done in the Manhattan Project, to hedge risk through redundancy. By running a flagship NEP demo alongside smaller PPPs, the United States increases the chance that at least one effort succeeds, while leveraging shared infrastructure, regulatory reform, and tech maturation to avoid fully additive costs. Only the flight demo costs would rise; the supporting pillars remain largely common.

This section provides a comparative analysis of Options 1 and 2. The goal is not to pick a winner, but to illuminate trade-offs clearly, so policymakers can match ambition with executable commitment.

6.1. Decision Heuristics

Table 6.1 lays out a side-by-side view of the cost, risk and schedule trade-offs of the two principal options. The choice between Option 1 and Option 2 hinges on several conditional factors.

Table 6.1**:** Comparative Snapshot of Options

Criterion	Option 1: "Go Big or Go Home"	Option 2: "Chessmaster's Gambit"
Core Objective	Field a 100–500 kWe power or NEP demo by ~2030 (ground test by 2028)	Field 10-100 kWe surface and in-space power demos by ~2030 (ground test by 2028)
Strategic Impact	Demonstrates undisputed U.S. leadership in space	Builds operational credibility; seeds commercial base, demonstrates leadership in space
Technical Maturity	Pushes frontier technologies	Leverages more mature components and modular designs
Execution Risk	High, requires centralized crash leadership and sustained funding	Moderate, requires multi-agency coordination and PPP management
Cost & Schedule Realism	~$3B over 5 years; susceptible to disruption	~$2B over 5 years for two demos; more achievable under current constraints
Workforce and Talent	Rapid surge in hiring; risks team loss if program stalls	More distributed development; better retention and growth potential
Industrial Base Activation	Strong demand pull for advanced systems and infrastructure	Steady ramp-up; primes commercial entities
System Extensibility	One-off system may lack reuse unless follow-ons are scoped from the start; most valuable if paired with surface readiness	Modular designs support multiple missions and users; compatible with decoupled cargo and crew delivery strategies
International Signaling	Unmistakable leadership signal; risk of strategic overreaction	Could support norm-building and allied engagement
Public Perception Risk	High visibility and high stakes; potential backlash on failure	Lower profile; better managed public and political expectations
Interagency Complexity	Requires singular authority to avoid bureaucratic drift	Fits within existing authorities; less brittle structurally
Architectural Sequencing	Prioritizes in-space transport capability; assumes surface systems will follow or be developed in parallel	Enables early deployment and testing of surface infrastructure; aligns with need to pre-position assets for Mars missions

The following "if-then" heuristics can help guide strategic selection:

- If stable, multi-year funding (~$3B), strong White House backing, and a designated, empowered program authority can be secured, then Option 1 may be viable. This approach delivers the most strategic and reputational upside, but only if execution conditions are tightly controlled.
- If political support is moderate, funding streams are uncertain, and decentralized agency execution is more realistic, then Option 2 is better aligned. It emphasizes resilience, early wins, and long-term extensibility while managing risk through milestone-based PPPs.

- If propulsion is a near-term imperative (e.g., for Golden Dome applications or Mars transit missions), then Option 1 offers the only path that meaningfully advances that timeline.
- If broader government or commercial demand for space power must be catalyzed first, then Option 2 creates the ecosystem conditions for propulsion systems to succeed later.
- If failure of a highly visible mission would damage trust in the broader nuclear agenda, then Option 2 offers a more reputationally conservative starting point.
- If agencies are unwilling to yield control to a central program lead, then Option 2 is more compatible with current structures.

In addition to the comparative snapshot above, several architectural considerations deserve emphasis. These factors do not tilt the decision toward a single option, but they clarify what each option enables, and what it risks deferring. For NASA is particular:

- **Sequencing of Surface Infrastructure.** Mars missions, regardless of their in-space transportation system, require that surface assets, power, ISRU, habitats, be deployed and validated well in advance of crew arrival. That infrastructure can be delivered with today's chemical propulsion systems and near-term landers. Option 2 allows surface power systems to be tested on the Moon, iterated upon, and adapted for Mars delivery. This approach aligns with how most crewed Mars architectures will unfold: long pre-positioning timelines and independent cargo delivery. Option 1, by contrast, prioritizes in-space propulsion and high-power transit while avoiding lander dependencies. If successful, it signals undeniable leadership in space nuclear power and propulsion technology. However, there is a risk that it will shift funding and focus shifts away from surface systems required for exploration missions and sustained presence on the Moon and Mars. The risk is not technical failure, but architectural imbalance: a transport system without a destination ready to receive it.
- **Decoupling Cargo and Crew Systems.** Historically, many architectures assumed that the same system would deliver both cargo and crew. That assumption is changing. It is increasingly plausible that cargo systems will continue to use chemical propulsion for the foreseeable future, while crew missions evolve toward nuclear options. Option 2 supports this differentiated architecture by maturing power systems that can be delivered and operated independently. The power levels for Option 1 drive its mass to levels which will limit its flexibility for near-term surface applications supporting crewed missions to the Moon and Mars.
- **Strategic Signaling.** Each Option also signals a different type of U.S. leadership. Option 1 is a bet on technological ambition, a bold claim to unmatched power or propulsion capability. It is a moonshot in the classic sense, and success would reinforce U.S. status in deep space. Option 2 offers a more distributed signal: leadership in deployment, flexibility, and architectural realism. It may resonate more with international partners and private actors focused on the near term, and it will provide the United States with a continuous presence on the Moon. But it lacks the singular, high-visibility impact of a flagship mission.

These architectural tradeoffs reinforce the central point: neither Option is inherently better. Each excels in certain areas and omits others. A successful strategy must choose not only what to pursue, but what to postpone, and why.

6.2. Navigating Inherent Tensions in the Strategy

This strategy requires decision makers to deliberate choices. But even the best choices create tradeoffs. Some tensions are technical. Others are institutional, cultural, or political. Ignoring them invites failure. Naming them early equips leaders to navigate, or resolve, them. Below, we highlight six key tensions that must be actively managed. These are not flaws in the strategy, but realities of execution:

1. Speed vs. Scalability. Option 2 accelerates early fielding through sub-100 kWe systems. It prioritizes fast wins to demonstrate feasibility and build user confidence. But early deployment can lead to architectural lock-in. Systems designed for speed may later constrain scalability, complicating the transition to 500 kWe or megawatt-class systems. Conversely, Option 1 delays early fielding to pursue scale directly, raising the opposite risk: a program so ambitious, it never flies. The tension lies between shipping something now and designing for tomorrow.

2. Demonstration vs. Deployment. A successful flight demonstration is only valuable if it leads to sustained production and operational use. Option 1 places a large bet on one transformative system, which, if not immediately extensible, could be seen as a one-off. Option 2 emphasizes iterative demos, but without clear procurement follow-through, risks becoming a cycle of isolated tests. The question isn't just whether we fly, it's whether we field.

3. Commercial Agility vs. Government Accountability. Public-private partnerships harness commercial speed and risk tolerance. But they also amplify the challenge of public oversight. Agencies must weigh how to delegate authority without losing visibility, and how to allow failure while maintaining public trust. The inherent cultural mismatch between entrepreneurial pace and government scrutiny plays out in procurement choices, milestone enforcement, and risk-sharing structures.

4. Centralized Coordination vs. Agency Turf. Option 1 requires a singular program office to cut across bureaucratic silos. But strong centralization often provokes resistance from entrenched agencies with statutory mandates. Option 2, which leans into distributed execution, may be more politically feasible, but risks incoherence without strong White House coordination. No structure is perfect: the tradeoff is between decisive integration and durable buy-in.

5. Regulatory Reform vs. Institutional Inertia. Both Options demand faster, clearer regulatory pathways. But reforms often require the very agencies being reformed to act against their default incentives. Licensing timelines, indemnification policies, and launch authorization processes remain ambiguous. In Option 1, delays can sink the timeline; in Option 2, ambiguity discourages private investment. Reform must be swift, but bureaucracy rarely is.

6. Power-First Strategy vs. Congressional Preference for Propulsion. This strategy is anchored in the premise that power must precede propulsion. Congress has focused on NTP, which carries historical momentum and, at least for the moment--political resonance. Option 1 tries to bridge this gap somewhat by bundling propulsion into the first demo. Option 2 postpones propulsion, risking disconnect from NTP advocates. The tension is between technical sequencing and political capital.

These tensions are not flaws in the strategy; they are the strategy. Naming them makes them manageable. They must be managed by leadership, funding structures, program design, and relentless focus.

6.3. Recommendation

The U.S. government should pursue Option 1 (“Go Big”) only if funding, centralized crash leadership, and long-term political insulation are assured. It offers a transformational leap in U.S. space capability, demonstrating serious leadership in high-power propulsion and infrastructure. However, success demands fulfillment of the “Manhattan Project test”: centralized crash-program execution, and stability across administrations if the flight experiment does not occur before 2029.

If these conditions cannot be guaranteed, the government should move decisively with Option 2 (“Chessmaster’s Gambit”), the faster, lower-risk pathway that can deliver a demo by 2030, build user and investor confidence, establish regulatory precedents, and catalyze commercial market growth. This is our recommendation.

What must be avoided is a half-step toward both, initiating a large demo without stable funding or delaying smaller PPPs for lack of urgency. That is the path of past failures.
Regardless of the fission path chosen, Parallel Option 3 ("Light the Path"), once validated, should be treated as a candidate for targeted planning. It could help deliver near-term wins, accelerate regulatory and operational experience, and signal serious U.S. intent in deploying nuclear technologies on the Moon.

Both Option 1 and Option 2 are viable, but under different conditions. The decision is not about preference; it is about alignment. Leaders must assess political will, funding stability, risk tolerance, and institutional capacity to choose a path that is executable now, and extensible later.

7. Summary and Conclusion

7.1. Summary

Since the launch of SNAP 10A, the United States has pursued space nuclear systems development without ever fielding an operational fission reactor. This strategy breaks the chicken-and-egg loop by embedding mission pull and technology readiness in every step, moving from paper to [launch] pad through five pillars: a flight demonstration, technology maturation, infrastructure build-out, regulatory reform, and centralized top-down coordination. It presents two executable tracks and one exploratory hedge, each suited to different political, technical, and institutional conditions. Table 7.1 summarizes the options.

Option 1: Go Big or Go Home calls for a bold, government-led 100–500 kWe power or NEP flight demonstration by 2030. This pathway delivers the highest strategic value: signaling U.S. leadership in space and building credibility in high-power fission. But it demands a rare convergence of political will, centralized crash-program execution, large levels of sustained funding, and continuity across administrations. Crucially, it also requires a set of national preconditions—accelerated regulatory approvals, expanded launch and test infrastructure, robust fuel supply chains, milestone-based procurement, and a strengthened workforce/industrial base—without which even full political alignment will not ensure success. Within Option 1:

- **Option 1A** (NASA or DoD-led NEP demo) involves DoD or NASA leading all aspects of the program, including licensing and indemnification, working with DOE, national laboratories and other organizations as needed.
- **Option 1B** (DoD/NASA led NEP demo in partnership with DOE) reduces regulatory and cost burdens through shared agency roles. The Option assumes funding streams at both DoD/NASA and DOE.
- **Option 1C** (NASA–DOE–DoD NEP demo) could be opportunistically pursued if defense needs and cost-sharing emerge.
- **Option 1D** (in-space or surface power demo) focuses on the power system only, with any of the agency alignments mentioned above. This option is preferred if there are alternative payloads ready to be flown with a 100-500kWe power system: either DoD or NASA payloads could be accommodated, depending on which is ready. The option preserves strategic value by flying or landing on the Moon a power-only demo using the same 100–500 kWe reactor system, de-risking the hardest elements while shedding electric propulsion complexity. If it becomes a surface demonstration, this provides a clean path to high power surface infrastructure required for exploration and commercial opportunities.
- **Option 1E** executes the flagship demo *and* both PPP power demos in parallel, enabling flexibility to drop from Option 1 to Option 2, providing Manhattan Project style redundancy if budgets allow.

Option 2: Chessmaster's Gambit offers a faster, lower-risk approach focused on 10-100 kWe power public-private partnerships (PPPs) led by industry. NASA would fund a surface fission power PPP where power levels are driven by lift and lander capacity and other architectural constraints, while DoD would fund an in-space power PPP where power levels are driven primarily by lift constraints only. These systems, targeted for flight by 2030, would be cheaper, more modular, and more resilient to political volatility. They would build early operational credibility, catalyze commercial ecosystems, and pave the way for higher-power systems in the 2030s.

Parallel Option 3: Light the Path is an exploratory hedge. It envisions a commercial-led RPS demo to the lunar surface (before 2028) at low cost. This could validate launch authorization pathways, test new isotopes, and signal near-term U.S. action in space nuclear. However, analysis on this Option in this report is underdeveloped and it requires further analysis before elevation to strategic status.

While Option 1 delivers strategic signaling, Option 2 offers a distributed and extensible power-first path. Option 3 provides a low-cost hedge.

The strategy is not prescriptive; it is conditional. The right choice depends on political alignment, risk appetite, and execution capacity. Early power demos under Option 2 can build the ecosystem for Option 1-style propulsion later. Option 1D ensures Option 1 does not collapse under its own weight, and if a lander is available, it enables more rapid development of surface power systems for the Moon and Mars. Option 1E allows a pivot from Option 1 to Option 2 if necessary. This combined approach echoes Manhattan Project's use of parallel technical bets, increasing the probability of success while supporting investments (in infrastructure, regulation, and technology maturation) largely unified. Option 3 creates a foundation for commercial micro-nuclear missions. These are not mutually exclusive; they are sequenced bets. Despite differences, the options reinforce each other. Common themes include:

- Flight, not study: Each path focuses on demonstration systems by 2030.
- Technology maturation and infrastructure: All options require fuel production, development and test beds, PIE facilities, system integration environments, and launch site facilities.
- Regulatory clarity: Regardless of option, launch and licensing pathways must be defined and streamlined.
- Cross-agency coordination: NASA, DOE, and DoD all play roles. Close White House leadership is necessary to maintain strategic cohesion.
- Future extensibility: All demos will seed platforms, procedures, and policies that support Gen-2 and Gen-3 systems.

Lastly, while this strategy prioritizes fission power and its applications for in-space, surface, and NEP, there are cross-cutting benefits to reactor core qualification, fuel development, thermal systems, reactor control systems, infrastructure and regulatory streamlining that will support future NTP efforts. These overlaps ensure that early investments in power will reduce technical and institutional risk across the broader space nuclear ecosystem.

Table 7.1: Summary of Flight Demo Options and Sub-Options

Option	Demonstration Goal	Power Scale (kWe) ***	Lead/Executing Entities	Execution Model	Target First Flight	Rough *Public-* Funding Estimate
1A/B/C “Go Big or Go Home”*	NEP flight demo	100-500	NASA or DoD + DOE	Government crash program	~2030	~$3B (five-year total)
1D “Anchor Power”*	Power-only (in-space or surface) demo	100- 500	NASA or DoD + DOE	Government crash program	~2030	-
1E “Hybrid Manhattan”	Flagship + PPP	100-500 (NEP) + 10-100 (power)	NASA/DoD + Industry/DoD or Industry/NASA + DOE	Crash program and PPPs run concurrently	~2030	Addl funding but not additive (pillars are similar)
2A “Chessmaster’s Gambit” (lunar Surface)	lunar surface demo	10-100	Industry/NASA + DOE	Fixed-price, tech agnostic, milestone PPP	~2030	~$1B
2B “Chessmaster’s Gambit” (In-Space)	In-space demo	10-100	Industry/DoD + DOE	Fixed-price, tech agnostic, milestone PPP	~2030	~$1B
3 “Light the Path”**	Commercial RPS demo	< 1	Industry/NASA/DoD + DOE	Fixed-price, tech agnostic, milestone PPP	2028	~$100 M (illustrative)

**Only pursued if political, budgetary, and leadership conditions are aligned.*
*** Could proceed in parallel with either major Option. Requires further assessment.*
**** Actual power levels will be driven by launch vehicle, lander capacity, and other architectural constraints*

7.2. Next Steps

The next steps are laid out in two parts: those that focus on additional tasks not conducted in this study (subsections 7.2.1-7.2.5), and those that focus on the next steps on the strategy itself (subsection 7.2.6).

7.2.1. Laying the Groundwork for a Commercial RPS

While Options 1 and 2 are supported by analysis, stakeholder input, and implementation detail, Option 3 remains preliminary. The idea of fielding a small ***commercial*** radioisotope power source in the near term, has intuitive appeal but lacks the institutional grounding, technical specificity, and agency alignment necessary for execution today. To make Option 3 viable as a national strategy pathway, the following work is needed:

- Assess ongoing efforts within NASA, DOE, and FAA related to small-scale nuclear systems for commercial use, including any funded technology maturation or licensing activities.
- Map potential use cases for small radioisotope heating units (RHUs) and RTGs that would justify a flight demo, e.g., CLPS landers, lunar infrastructure experiments, or standalone navigation/science payloads.
- Engage with commercial actors (e.g., lander providers, payload developers) to gauge appetite, technical readiness, and potential for private co-investment.
- Clarify the regulatory environment, including how the launch approval process (under NSPM-20) would apply to non-government, low-power nuclear systems, and what gaps exist in FAA, DOE, or NRC roles.
- Examine isotopic fuel availability, particularly Am-241, Sr-90, or other alternatives to Pu-238, and develop a snapshot of supply chain, handling, and licensing issues and pathways to address them.
- Analyze international implications, including potential export control challenges, coordination under the Artemis Accords, and opportunities for allied collaboration.

Once these questions are addressed, Option 3 be elevated to a strategic alternative. Until then, it should be treated as a promising concept in need of further definition.

7.2.2. Exploring Fusion Technologies

Although this strategy has focused on fission-based systems, the emergence of fusion power and propulsion technologies warrants structured investigation. Several private firms are making early but ambitious progress on compact, in-space fusion concepts, some of which are working toward key demonstrations by the end of the decade. This timeline means that, if successful, fusion systems could compete with Gen 2 or 3 fission systems. However, the readiness, feasibility, and implications of these systems remain uncertain. Before fusion power or propulsion can be incorporated into a national space strategy, key questions must be answered.

- Assess Technical Viability: Current fusion power and propulsion concepts remain largely in the early prototype or simulation stage. DOE and NASA should conduct a neutral assessment of readiness levels across leading fusion concepts. The key milestone will be demonstrating breakeven power generation - net power gain from fusion with an approach that is scalable to space applications.

- Clarify Regulatory and Safety Implications: Fusion systems appear to face much lower regulatory and safety barriers than fission systems because there are no radioactive products until after significant operation, and there is zero criticality risk. The risk of damage from a launch accident is no different than a non-nuclear launch so there is no nuclear indemnification issue. Nevertheless, it's unclear whether existing nuclear launch guidance (e.g., NSPM-20) or the Price-Anderson Act applies to fusion devices. Recent terrestrial licensing successes indicate relatively simple processes. Further legal analysis may be needed to clarify the appropriate regulatory framework for space fusion systems.
- Compare with Alternatives: The benefits of fusion propulsion, faster transit times, higher specific impulse, need to be weighed against simpler, nearer-term alternatives like solar electric propulsion or advanced NEP systems. Objective side-by-side analysis is needed to determine where fusion adds differentiated value and where it does not.
- Define Strategic Role and Timelines: Fusion propulsion is often invoked for deep space or interplanetary missions, but there is no shared view of which missions would justify its cost, mass, and development complexity. Strategic mission modeling could clarify when fusion systems might become essential, if at all.
- Evaluate Funding and Public-Private Models: Some firms are backed by venture funding and partnerships. But long-term development for space applications will likely require federal R&D and possibly milestone-based PPPs. A structured roadmap is needed to define how fusion propulsion fits into broader government priorities.

In short, fusion power and propulsion remain an exciting but speculative area. Serious technical and strategic evaluation, not just hope or hype, is needed to determine whether and how fusion technologies should be supported in the coming decade.

7.2.3. Assessing Regulatory Streamlining Scenarios

There is a persistent sense that stripping away regulatory hurdles will dramatically accelerate the flight demos' schedules. A data-driven analysis may help identify how targeted regulatory reforms could not only compress the demo timelines but also help establish a cost-effective future space nuclear enterprise. Steps could be:

- Map all launch, licensing, environmental and indemnification rules, marking which can be changed by agency rule versus those requiring changes to current statutes.
- Quantify, to the extent feasible, schedule and cost savings from time-boxed reviews, single-window approvals, or consolidated safety assessments against the baseline process.
- Prioritize the regulations whose reform yields the biggest reductions in program risk and calendar.
- Recommend concrete administrative and legislative actions, while safeguarding safety and public confidence, to streamline permitting.

By modeling "what-if" scenarios for regulatory elimination or acceleration, leadership can rank policy reforms by impact and ensure regulatory work moves in lockstep with reactor development.

7.2.4. Assessing Opportunities for International Collaborations

This project did not examine the international dimension of space nuclear development. A targeted analytical effort could help determine whether international collaborations offer a net benefit, and under what conditions such an effort would be viable, effective, and aligned with long-term U.S. interests. Focused follow-on analysis could clarify whether such collaborations would be strategically and operationally beneficial. Key areas for research include:

- An assessment of allied technical, regulatory, and industrial capabilities, including in the UK, France, Australia, India, and Japan, could help identify feasible divisions of labor in areas such as reactor development, fuel production, system integration, and testing infrastructure.
- Comparative analysis of frameworks such as AUKUS, the Artemis Accords, or relevant NATO initiatives could inform design options for cooperation, ranging from joint R&D to coordinated regulatory processes or shared flight demonstrations.
- Export controls (e.g., ITAR, EAR), nuclear nonproliferation regimes, and classification rules may constrain technology sharing. Research could identify precedents that enable meaningful collaboration without compromising national security.
- Further analysis could weigh the benefits of cost-sharing and industrial scaling against potential risks—such as loss of U.S. primacy, political pushback, or complexity in program execution—especially if major system elements are co-developed.
- Research could assess how such partnerships would be perceived by China, Russia, and non-aligned states, and whether they reinforce or undermine broader U.S. strategic objectives.

7.2.5. Examining Need for a Space Specific Executive Order

In May 2025, the White House released four executive orders (EOs) to accelerate progress for terrestrial nuclear energy. They are intended to streamline NRC licensing, expand DOE testing, revitalize the industrial base, and support rapid reactor deployment for national security. If implemented well, these EOs can deliver broad benefits (e.g., faster licensing, a more robust fuel supply, and improved infrastructure) with some spillover effects for space nuclear efforts. Whether a space-specific EO is necessary or optimal remains an open question and would be worth exploring in a follow-on activity.

As this report has shown, space nuclear missions face distinct regulatory and institutional hurdles (especially related to launch authorization, indemnification for spaceflight, interagency coordination, and commercial licensing) that these terrestrially oriented EOs do not directly address. A dedicated space nuclear executive order could close these gaps by clarifying agency roles, streamlining mission approval, and de-risking commercial participation. Such a targeted EO could resolve ambiguity, accelerate timelines, and catalyze private investment.

On the other hand, a space-specific EO risks adding policy complexity or duplicating/conflicting with terrestrial reforms if not carefully integrated. It could also be seen as politicizing space nuclear. More analysis is needed to weigh the benefits, risks, and implementation challenges of a space nuclear executive order in light of these recent, sweeping reforms. Decision makers should assess where the new EOs fall short for space and whether targeted executive action would deliver net strategic value.

7.2.6. Next Steps for Strategy Execution

The critical first step is to resolve whether the U.S. government will pursue Option 1 (Go Big) or Option 2 (Chessmaster's Gambit). This decision will determine the direction for leadership structure, funding, legislative efforts, and execution approach.

- Key Stakeholders: White House, NASA, DoD, DOE, Congressional leadership, and industry/commercial partners.
- Objective: Assess the political will, funding viability, and commercial and institutional readiness for the selected option.
- Sub-actions:
  - Host White House-level meeting with Congressional participation to assess the political and funding feasibility for Option 1 (bold government-led power/NEP demo) and Option 2 (lower-risk power-first PPPs).
  - Assess if Option 1B is realistic in that DOE will be able to be a funding partner, with technology and independent funding on the nuclear side.
  - Evaluate if the required centralized leadership (for Option 1) or dual-track public-private partnership model (for Option 2) aligns with current agency capabilities and political support.
    - Option 1 ("Go Big"): If "Manhattan Project test" conditions ((1) A centralized lead with real budget and milestone authority; (2) Stable, multi-year funding insulated from annual swings; (3) A strategic imperative strong enough to align leadership and unlock institutional will) are met, proceed with a bold government-led 100–500 kWe power-only or NEP experiment by 2030, ground test by 2028. This is a high risk, high reward program.
    - Option 2 ("Chessmaster's Gambit"): If political will or resources are insufficient for Option 1, opt for the lower-risk, tech agnostic, milestone and fixed price driven faster public-private partnership path (power-only systems, 10–100 kWe class, by 2030, ground test by 2028).

Once the decision between Option 1 (Go Big) and Option 2 (Chessmaster's Gambit) is made, the next steps must be tailored to the selected approach.

If Option 1 is chosen, a new space nuclear point person in the White House or an Executive Order may be needed to formalize agency roles and ensure the necessary political commitment, funding, and centralized leadership. This executive action will commit funding and outline NASA, DOE, and DoD's responsibilities, enabling coordination across agencies.

The strategy will focus on establishing a unified crash program to execute the power or NEP demo, with parallel targeted investments in reactors and power system technologies and system integration. Infrastructure development will also be prioritized, particularly for launch site nuclear system handling, reactor development, PIE facilities, nuclear safe thermal vacuum chambers, and facilities that support large-scale systems.

If Option 2 is chosen, the option we recommend, a different organizational approach or executive order could formalize the parallel public-private partnerships led by NASA and DoD. In this case, the government will invest in smaller, quicker power systems that build momentum toward larger goals, while ensuring that parallel technology and infrastructure developments will be ready to support 2nd and 3rd generation higher power systems.

For both Option 1 and Option 2, immediate actions must be taken to establish clear and efficient space nuclear regulatory pathways for ground operations, launch and operations. This could include broadening NSPM-20 to encompass ground phases of development, production and testing, as well as identifying and empowering the appropriate regulatory agency for commercial operation. Clarifying the role of the FAA will be crucial for Option 2. In parallel, the White House (or delegated authority) should lead a national program to install the non-negotiable execution pillars: technology maturation, infrastructure build-out, and streamlined regulation. These actions must be started immediately, as no demo can proceed without them.

Both Options require congressional action to secure funding for long-term success. For Option 1, this will focus on ensuring multi-year, protected investments, while Option 2 will aim for sustainable funding streams for the PPPs. Technology and infrastructure investment will be necessary in both cases to ensure that fuel production, reactor development, reactor and system testing and launch facilities are equipped to handle these new systems.

Specific actions within NASA will also be tailored to the specific approach once the decision between Option 1 and Option 2 is made. To ensure a smooth transition, all ongoing NASA space-nuclear projects may need to be realigned to whichever option is adopted, and organizational structures set up for a flight demo within an organization that has experience with flight programs. In either path, no effort is lost; each current program is repositioned to deliver maximum value inside the unified execution framework, including both the flight demonstration and the supporting technology maturation, infrastructure and regulatory streamlining pillars.

Likewise, all ongoing Department of Defense space-nuclear activities, including AFRL's JETSON power-system work, Space Force high-power feasibility studies, and service-specific initiatives, may need to be strengthened and/or steered into the chosen path.

Option 3, a low cost commercial RPS using non-plutonium isotopes is not yet ready for execution. After further foundational analysis and stakeholder engagement to determine its viability, Option 3 can move forward as a formal part of the national strategy.

7.3. Conclusion

The United States has long pursued space nuclear systems without ever breaking through the institutional barriers that stalled every crash effort since 1965. This strategy confronts those barriers head-on by making and justifying one clear choice: power comes first. Proven, scalable fission-power systems are the indispensable foundation for every downstream capability, whether long-duration lunar or Mars habitats, high-power national-security sensors, or the high-delta-v tugs and rapid-response platforms.

To turn choice into reality, three pillars must rise together: technology maturation, infrastructure buildout, and regulatory reform. Only by investing in all three can we ensure that a first demonstration reaches orbit, or the lunar surface, and creates the institutional momentum to support follow-on missions.

From this foundation flow three execution paths. "Go Big or Go Home" pursues a 100–500 kWe demo under government leadership. "Chessmaster's Gambit" uses two fixed-price, milestone-driven public-private partnerships for sub-100 kWe surface and in-space power. And “Light the Path” hedges with a commercial sub-kWe RPS demonstration. Each path balances different risk, cost, and timing considerations, but all require the same decisive leadership: a single, empowered office to align budgets, harmonize requirements, and enforce cross-agency accountability.

This is more than another study. It is a call to end the old pattern of parallel starts and stalled stops. The choice is ours: pick power as the starting point, marshal the resources and authority behind it, and see it through before the decade is out. If we act now, America will reclaim its edge in space nuclear power and propulsion. Delay, by contrast, invites lost momentum, rising foreign competition, and another generation of unmet promise. The moment to decide is now.

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Appendix A

Partial List of Interviewees

Last Name	First Name	Affiliation	Date(s)
Ainsworth	Tom	DoD	28-Feb
Andrus	Jason	INL	29-Apr
Bahran	Rian	DOE	31-Jan
Baranwal	Rita	Westinghouse	12-Feb
Bell	Randy	Aerospace Corp	6-Feb
Bernauer	Doug	Radiant Nuclear	13-Mar
Bilardo	Vince	Intuitive Machines	25-Feb
Boston	Robert D.	INL	26-Feb
Bramble	Jordan	Antares	9-Feb
Brown	Celeste	DoD	30-Apr
Calomino	Anthony	NASA	24-Jan
Carlson	Joshua	Space Force	17-Mar
Cassady	Joe	L3 Harris	21-Feb
Chambers	Rob	Lockheed Martin	20-May
Chapman	Travis	BWXT	5-May
Charania	AC	NASA	10-Mar
Cichan	Timothy	Lockheed Martin	12-Mar
Cirtain	Jonathan	BWXT	6-Jan
Coleman	Justin	INL	various
Corbisiero	Sebastian	INL	various
Cuellar	Sarah	DIU	3-Mar
Cummings	Nick	SpaceX	19-Feb
Daniels	Matt	DoD	27-Jan

Dankanich	John	NASA	2-Apr
Desai	Harsh	Zeno Power	2-Apr
DIU	Team	DIU	27-Feb
Dodson	Tabitha	DARPA	26-Jan
Doty	Kate	PNNL	14-Mar
Eades	Michael	Formerly USNC	23-Jan
Earl	Steph	FAA/AST	27-Jan
El-Genk	Mohamed	UNM	9-Apr
Faibish	Ron	General Atomics	27-Mar
Freeze	Brent	Aerojet Rocketdyne	21-Feb
Frye	Peter	Westinghouse	26-Mar
Folton	John	Sandia National Labs	17-Mar
Garretson	Peter	AFPC	2-Feb
Gedmark	John	Astranis	Various
Gilbert	Alex	Zeno Power	28-Apr
Goswami	Namrata	Space Policy Expert	2-Feb
Griffin	Mike	Former DoD/NASA	4-Mar
Harrison	Todd	AEI	4-Mar
Helton	Don	NASA	12-Feb
Helvajian	Henry	Aerospace	4-Feb
Hendrikson	Dan	Astrobotic	19-Feb
Houts	Mike	NASA	24-Jan
Isaacman	Jared	NASA Nominee	14-Apr
Jarrell	Josh	INL	20-Feb
Kaldon	Lindsay	NASA	10-Mar
Kammerer	Ann Marie	US Army	25-Feb
Kennedy	Fred	Dark Fission	17-Mar

Kim	Moon	NASA	22-Mar
Klein	Spencer	NEI	2-Apr
Lenyk	Chris	DoD	21-Apr
Lintner	Stephane	Helicity Space	17-Mar
Loverro	Doug	Former DoD	15-Mar
Martin	Billy	Sandia National Labs	1-May
Martin	Tom	Blue Origin	28-Feb
Mason	Lee	NASA	18-Mar
Mathews	Jake	Zeno Power	10-Dec
McAdams	Doug	Balerion Space Ventures	10-Mar
McAlister	Phil	NASA	20-Mar
McClure	Pat	Space Nukes	22-Feb
Melroy	Pam	Former NASA	27-Jan
Merancy	Nujoud	NASA	28-Jan
Meyerson	Rob	Interlune	17-Jan
Millard	Lindsay	NASA	22-Feb
Morrison	Chris	TerraPower	27-Jan
Mounce	Gabe	DoD/AFRL	26-Feb
Mueller	Tom	Impulse Space	6-Feb
Nayak	Orbit	DARPA	21-Feb
Olson	John	DoD	12-Feb
Polzin	Kurt	NASA	8-Apr
Porter	Lisa	Former DoD	4-Mar
Pratt	Bill	Lockheed Martin	20-May
Presby	Andrew	NASA	Various
Putzu	Frank	PNNL	14-Mar
Rao	DV	INL	Various

Reichert	Bailey	Aegis Space Law	7-Feb
Reuter	Jim	Former NASA	12-Feb
Roma	Amy	Hogan Lovells	1-Apr
Rucker	Michelle	Former NASA	24-Feb
Sambora	Matt	DARPA	16-Apr
Schenewerk	Caryn	Consultant	12-Feb
Schleeper	David	R&S	14-Apr
Shank	Chris	Former DoD	12-Feb
Shofner	William	DoD	18-Mar
Sholtis	Joe	Consultant	28-Jan
Shumlak	Uri	Zap Energy	12-Mar
Smith	Mike	DOE	28-Feb
Steinke	Lee	Cislunar Industries	23-Feb
Stockham	Erik	DoD/Space Force	28-Feb
Sullivan	Greg	Consultant	30-Mar
Tolley	Zack	Blue Origin	10-Mar
Tournear	Derek	DoD/SDA	20-May
Umstattd	Ryan	Zap Energy	12-Mar
Venneri	Paolo	Amazon	20-Mar
Vogel	Kurt	Former NASA	28-Jan
Voss	Susan	Voss Consulting	24-Jan
Waksman	Jeff	DoD	28-Jan
Whitley	Ryan	NASA	25-Mar
Williams	Stacie	Space Force	20-Feb
Witter	Jonathan	BWXT	4-May

Appendix B

Mission Pull for Space Nuclear Systems

Review

For more than half a century, the United States has invested billions of dollars in developing nuclear power for space, yet today, the only operational system in use remains the ~100-watt plutonium-fueled radioisotope thermoelectric generator (RTG), a technology that has changed little since the 1960s. This stagnation represents a puzzling paradox: despite clear physics-based advantages of nuclear power in space that have been recognized since the dawn of the space age, the nation has failed to field an operational space nuclear reactor since the brief 43-day mission of SNAP-10A in 1965.

The cyclical pattern has become familiar: ambitious programs launch with technological promise and political enthusiasm, only to collapse under budget constraints, technical challenges, and shifting national priorities. The Systems for Nuclear Auxiliary Power (SNAP) program demonstrated early feasibility but ultimately faltered. The Space Power Advanced Reactor (SPAR) and SP-100 programs of the 1980s and 1990s sought to develop high-power nuclear reactors but were canceled long before producing operational systems. Even attempts to leverage Soviet technology through the TOPAZ-II program ended in termination before deployment.

The 21st century brought no change to this pattern. The Prometheus program of the early 2000s aimed to deliver a 200-kWe nuclear-electric propulsion system for the Jupiter Icy Moons Orbiter but collapsed under unrealistic expectations, ballooning costs, and competing priorities. More recently, the Kilopower project successfully demonstrated a 1-kWe fission experiment in a controlled ground test environment (KRUSTY) in 2018, but despite this achievement, no operational system has been built or deployed on a mission.[1]

The value proposition for space nuclear power and propulsion remains as compelling today as it was in the 1960s, the fundamental physics hasn't changed. Nuclear power offers energy density and longevity unmatched by chemical or solar alternatives, particularly for missions beyond Mars, the Martian surface, or in permanently shadowed regions of the Moon. Nuclear propulsion promises reduced transit times to destinations like Mars, minimizing crew exposure to space radiation and microgravity, and reduces by at least a factor of two the number of launches required per mission to Mars, reducing mission risk and environmental impacts.

Yet despite these clear advantages, NASA has made no firm commitment to using nuclear propulsion for Mars missions. While there is acknowledged demand for surface power for both lunar and Martian operations (with recent commitments to power systems for Mars), specific requirements and implementation timelines remain vague. A clear need for MWe on Mars to produce propellant for Earth return is emerging, though the timing is unclear.

The Department of Defense (DoD) landscape appears equally fragmented. While DoD has articulated clear requirements for small terrestrial reactors, its efforts seem scattered across services with limited

[1] The KRUSTY (Kilopower Reactor Using Stirling Technology) experiment was led by NASA's Glenn Research Center in partnership with the National Nuclear Security Administration and tested at the Nevada National Security Site from November 2017 through March 2018. It operated a 1 kWe uranium-235 reactor using passive sodium heat pipes and Stirling converters under space-simulated conditions, validating startup, steady-state, transient operations, and safety systems for the first ground test of a space fission reactor in over 50 years.

coordination. More concerning is the lack of clear requirements for space nuclear applications, whether for propulsion or power, despite seemingly obvious potential military advantages.
In the commercial sector, demand is emerging for high-power applications such as space-based computing, lunar resource mining, and in-situ resource utilization (ISRU) on the Moon. However, this demand has not yet reached a tipping point due to the high cost and perceived risk of space nuclear systems. If commercial markets are tapped for Mars propellant production, then that could provide mission pull, though the ultimate customer would of course initially be NASA until commercial markets mature.

Current technology and infrastructure challenges increase uncertainty and risk. There are no spaceflight qualified reactors, power conversion systems, or large-scale high temperature radiators nor are there adequate development, test, system integration and launch site assembly facilities. The gaps for initial flight systems are manageable, but the challenges increase rapidly as mission capability needs increase. Additionally, the regulatory frameworks are viewed by many as increasing cost uncertainty and timeline risks. This regulatory ambiguity, which impacts ground testing, launch and space operations, prevents investors from confidently assessing return on investment, creating a significant barrier to private capital mobilization.

Further complicating matters is growing concern about the Federal Aviation Administration's emerging role in regulating private-sector space nuclear activities, which some industry participants view as potential regulatory overreach that could further impede development.
Despite this challenging historical and current landscape, several emerging factors suggest the potential for breaking the decades-long cycle of failure:

1. **National Power Imperatives**: Great power competition, particularly China's ambitious space nuclear plans including nuclear-powered infrastructure on the Moon, provides a new geopolitical driver for U.S. investment. The national security implications of falling behind in this domain are also becoming increasingly difficult to ignore.
2. **Defense Initiatives**: The Golden Dome Executive Order has created a national security argument for high-power space-based assets that may require nuclear power, providing a more durable mission pull than previous efforts. Specifically, mission use cases for directed energy and new ISR capabilities will drive to much higher in-space power systems.
3. **Private Sector Evolution**: Growing capabilities, funding sources, and potential users of space nuclear technology in the private sector create a commercial ecosystem that could help sustain development through government priority shifts.
4. **Technological Maturation**: Advances in technology have improved prospects for reliable autonomous operations and better modeling and simulation. Adjacent technological developments such as dramatic reductions in launch costs, improved electronics/miniaturization, and innovations allowing the use of HALEU instead of HEU may collectively (or perhaps eventually) reduce costs and accelerate timelines.
5. **Terrestrial Nuclear Renaissance**: Increasing terrestrial power needs are driving substantial investments in advanced reactors for a wide range of applications. These investments are improving microreactor and system technology, reducing technology risk and uncertainty, strengthening nuclear supply chains, and increasing the depth of the workforce, with potential spillover benefits for space applications.

6. **National Policy Clarification:** The release of both Space Policy Directive 6 and National Security Presidential Memorandum 20 has increased the clarity of the technology approaches and launch approval processes, increasing the certainty for private investors in this area.

However, a core issue persists, nuclear power in space continues to be treated as a "luxury" rather than a necessity. Without embedding nuclear development into a broader strategy which addresses the broad scope of emerging national security, exploration, and commercial capability requirements these programs remain vulnerable to budget cuts and policy reversals.

As we look toward formulating an effective strategy for space nuclear development, understanding this complex history and the current landscape is essential. The repeated failures of the past provide valuable lessons, while emerging changes in the geopolitical, commercial, and technological environments offer new opportunities to finally realize the long-promised potential of nuclear power in space.

To better understand the path forward, Section 2 below examines both the mission pull and system needs for space nuclear power and propulsion across key sectors, national security, human exploration, robotic space science, and commercial development. It outlines where and why nuclear power is needed, the technical performance requirements that systems must meet, and the gaps that must be addressed to ensure feasibility and long-term viability.

This Appendix lays the groundwork for the broader strategic framework outlined in the main text of this report, identifying key enablers and potential solutions to ensure that space nuclear power and propulsion can finally transition from concept to operational reality.

Introduction

Requirements for what we must be able to do in space are growing as our Nation's security, exploration, robotic space science, and commercial development plans take shape for the coming decades. Our national security needs for advanced, modular, mobile power systems span both terrestrial and space applications, with potential uses including high-power long-range sensors, new defensive and offensive weapons systems, sensor fusion and data management systems, and high speed, enhanced mobility to confuse the enemy and enable rapid response to crises. These national security needs are driven in large measure by the growing capabilities demonstrated by China, which has publicly stated plans to develop advanced terrestrial and space nuclear power systems in the 2030s, including a reported collaboration with Russia on a 1.5MW reactor targeting 2036 deployment.

For human exploration, sustained human presence on the Moon and Mars, part of national efforts such as Artemis and beyond, will require highly reliable power sources to support surface systems, and may be required if we decide to produce propellants needed for return from those destinations. Power requirements may start with relatively low levels but must increase over time as our surface operations expand.

For robotic space science, furthering our understanding of the solar system and its potential for life demands higher-power, longer-lived spacecraft operating far from the sun. These missions could conclusively answer the question "is there life elsewhere in the solar system?" by conducting in situ research and/or returning samples from bodies like Europa and Enceladus.

Finally, the continued economic expansion of the United States will increasingly rely on space assets, both in space and on the surfaces of the Moon, Mars, and potentially asteroids. Currently, the space economy is dominated by Earth-orbit applications (communications, Earth observation, position and navigation, data management). Over the next 15 to 20 years, as critical technologies mature and initial infrastructure is established to lower the cost barrier for new entrants, other forms of space-based economic activity will emerge, fueling further growth. It is clear that new power systems technologies, including nuclear systems, will be required to support this expansion.

Nuclear power systems offer high power in compact form for both terrestrial and space applications. The primary distinction between terrestrial and space nuclear power lies in waste-heat rejection (evaporative/convective for terrestrial vs. radiative for space or lunar surfaces, and possibly convective on Mars). Terrestrial systems must also meet a different set of security and safety requirements as compared with space. Although some level of commonality between terrestrial and space systems exists, and indeed is desirable (for reducing cost, strengthening supply chains, leveraging infrastructure, and building a skilled workforce), there will inevitably be differences at the system level.

Space nuclear power systems excel where solar is limited: prolonged darkness or the outer solar system, and where compact, high-density 24-hr sources of power are required. They also scale well to hundreds of kilowatts and beyond without the complexity of large solar arrays. Consequently, interest from both government and the private sector is growing, resulting in increased development and application of space nuclear power systems.

The mission requirements for space nuclear power systems hinge not only on power level but also on system mass, reliability, and lifetime. The full "system" includes the reactor and shielding, power conversion, heat rejection, and electrical power management/distribution. Power level and mass often combine into the parameter α (specific mass, kg/kW), making four Key Performance Parameters (KPPs): power level, α, reliability, and lifetime. After initial trades, other factors, cost, volume, complexity, must be considered. Whether for surface or in-space use, low α, high reliability, and long lifetime are universally important. While mass is less of a factor in terrestrial use, mobile nuclear power systems still emphasize mass reduction and share the need for reliability and longevity. In all cases, mission-level trades (e.g., nuclear vs. solar) will guide adoption, factoring in development, demonstration, production, and overall mission cost.

Space nuclear systems include radioisotope, fission reactor-based, and fusion technologies. Radioisotope Power Systems (RPS) challenges have been discussed elsewhere though it's worth noting that commercial RPS development faces significant issues beyond just Plutonium-238 supply. Several companies are now exploring alternative isotopes including Americium-241, Strontium-90, Cobalt-60, and even Europium-152, each with their own technical, supply, and regulatory considerations.

Meanwhile, fission reactor-based systems offer a near-term path to scalable power and propulsion for surface operations on the Moon and Mars, high-power electric propulsion, and deep-space exploration, but they have received less attention over the last two decades, and the U.S. flew its only reactor in space in 1965.

Fusion, though potentially transformative, remains low TRL for either terrestrial or space applications, with only a single large scale ground test demonstrating scientific breakeven. Until engineering breakeven is achieved, when the system power cycle is closed with net power generation, fusion will remain speculative. Challenges include plasma confinement, energy conversion, and miniaturizing reactors for space environments. Despite rising private investment in terrestrial fusion, viable fusion for space appears unlikely in the near term. Four major sectors shape the mission pull for space nuclear systems: national security, human exploration, robotic space science, and economic development. Appendix A provides further details on the current missions pulls in each sector as well as potential future applications, based on over 100 interviews with stakeholders from these communities and a thorough review of relevant literature.

National Security

Existing Mission Pull/Capability Needs for Terrestrial Nuclear

To the best of our knowledge, though we will continue to verify, there is currently no explicitly stated mission pull for terrestrial fission power in defense activities. Instead, ongoing technology push efforts are advancing system development in anticipation of future demand. However, the growing emphasis on energy resilience and distributed power solutions suggests that this may shift in the near future, particularly in response to increasing threats to critical infrastructure and the rising logistical burden of fuel transport.

One potential application is forward base power, which is currently supplied by diesel generators that rely on fuel convoys, operations that are both costly and high-risk. A 2018 study (Vitalli, 2018) indicates that small mobile nuclear reactors could significantly reduce both cost and risk. In line with this, the Strategic Capabilities Office (SCO) is developing Project PELE, a 1–5 MWe mobile microreactor designed to provide power in forward operating environments. The progress made in this program could also inform future space-based nuclear applications.

Beyond traditional defense applications, the modularity and rapid deployability of these reactors suggest potential utility for disaster relief operations, temporary power stations, and remote installations that require secure, continuous energy without dependence on vulnerable fuel supply chains.

Another area of interest is grid-independent power for missile silos and other remote military installations. These systems, envisioned in the 2–7 MWe range, must be transportable by truck or rail and compact enough to fit within a standard shipping container, ensuring flexibility and rapid deployment in various operational scenarios. As threats to energy infrastructure increase, mobile nuclear power systems could provide critical redundancy, ensuring uninterrupted operations in both peacetime and conflict scenarios.

The increasing digitization of military operations also drives new power demands. Future command and control centers, sensor networks, and electronic warfare capabilities will require significantly more energy than current systems, particularly in contested environments where grid infrastructure may be compromised.

The autonomy and low-maintenance operation of these systems further enhances their viability for high-risk environments where personnel exposure must be minimized. As defense strategies increasingly emphasize logistics reduction, operational resilience, and energy independence, the case for deploying mobile, rapidly deployable nuclear power solutions will likely strengthen.

There are potential tech development activities underway, but we need to learn more about that.

Mission Pull for National Security Space

Although the space dimension is receiving more attention, there are no *current* immediate, formally stated DoD requirements (as of early 2025) for nuclear power in space. Interviews with more than a dozen senior members of the national security space establishment confirm a lack of clearly identified missions that require space nuclear systems.

Preliminary indications suggest growing interest in specific capability areas. The Space Force is beginning to evaluate potential mission applications, and the DoD's "Golden Dome" initiative, with a goal of deploying capabilities before 2029, represents a potential significant near-term pull for high-power systems in space.

Potential Capability Pull for National Security Space

Although there are no explicit near-term requirements for nuclear power in national security space applications, DoD’s evolving strategic landscape suggests emerging needs that could be significantly enhanced or enabled by fission systems.

One key area is high-power space systems, where nuclear power could support persistent surveillance, space domain awareness, electronic warfare, directed energy weapons, secure communications, autonomous spacecraft operations, and even neutron generation for interrogating adversary satellites. Traditional solar power faces limitations in deep-space environments or extended shadow conditions, whereas fission reactors can provide continuous, high-output power in a compact form, particularly valuable for operations in Cislunar space and beyond. The power demands for such applications range from tens to hundreds of kilowatts, with some directed energy systems potentially requiring megawatt-class power levels.

Another critical need lies in orbital logistics and on-demand power, as future autonomous satellites may require rapid station-keeping and power scalability. Nuclear systems offer resilience, mobility, and security advantages over large solar arrays, making them particularly useful in contested space environments.

A key capability of interest is "maneuver without regret" more recently referred to as “sustained maneuvering” or space littoral observations, the ability to execute multiple orbital adjustments with far less concern about depleting propellant. This flexibility would enable more dynamic operations and support the use of non-Keplerian orbits, which could make U.S. spacecraft more difficult to track and target. Nuclear propulsion remains a long-term enabler for this kind of space superiority, with growing research into both Nuclear Thermal Propulsion (NTP) and Nuclear Electric Propulsion (NEP).

While the April 2024 Space Force report on NTP and NEP acknowledged no immediate plans for incorporating these systems, historical programs such as ROVER and NERVA demonstrated that nuclear propulsion could be essential for defense applications requiring high-thrust, high-efficiency maneuvering. Chemical propulsion remains constrained by low specific impulse, while electric propulsion, though efficient, provides minimal thrust. NTP offers approximately twice the efficiency of chemical systems while still generating sufficient thrust for time-sensitive maneuvers. NEP provides even greater efficiency (5-10x) but lower thrust, making it more suitable for long-duration station-keeping and deep-space mobility.

Recognizing the potential of these technologies, DARPA’s Demonstration Rocket for Agile Cislunar Operations (DRACO) program was collaborating with NASA to develop an NTP system for space operations. Per Space Force documents (2024), DRACO aimed to execute the first on-orbit demonstration of an NTP system and is currently assigned to the National Security Space Launch (NSSL) mission USSF-25, scheduled for launch in FY 2027 (though that seems unlikely given more recent technical and infrastructure related challenges; multiple sources indicate DRACO faces significant technical challenges, particularly with hydrogen storage and loading. The program has reportedly taken a "strategic pause" to address these issues). If successful, DRACO could provide the foundation for high-thrust, high-efficiency propulsion capabilities critical for maneuverability in Cislunar space and beyond. However, the effort appears moribund as of May 2025.

Complementing these efforts, the Air Force Research Laboratory’s (AFRL) Joint Emergent Technology Supplying On-Orbit Nuclear Power (JETSON) project focuses on developing component-level technologies for an overarching NEP spacecraft system design. JETSON is expected to reach its preliminary design review milestone in FY 2026, positioning it as a key enabler of future high-power space operations. Again, it is unclear where JETSON is heading.

These programs reflect a growing recognition of nuclear power and propulsion as critical enablers for future national security space operations.

Human Exploration

Existing Mission Pull

NASA’s Moon-to-Mars program (REF) plans for crewed lunar landings starting in 2028, leading to a sustained human presence on the Moon and, ultimately, missions to Mars. In November 2024, NASA’s Exploration Systems Development Mission Directorate (ESDMD) issued the “Mars Surface Power Technology Decision White Paper,” declaring:

“NASA has selected nuclear fission power as the primary surface power generation technology for crewed missions to Mars.”
This statement marks the first clear mission pull for nuclear power in decades, informed by analyses of Mars surface power requirements and the available options. No formal decision has been reached for lunar surface power, though multiple architectures (beamed solar, nuclear, etc.) are under study. The current plan is to demonstrate nuclear technology on the Moon before using it on Mars. NASA’s Mars Surface-Power Decision Memo (Nov 2024) notes that a purely solar solution would “require >4 000 t of arrays, storage, and dust-mitigation systems, well beyond the current Artemis cargo model.”

- Mars Surface Power:
    - For an all-chemical architecture, 750 kWe–2 MWe will be needed to produce propellant for Earth return.
    - Could be many smaller units (e.g., 100 x 10 kWe systems = 1 MWe).
    - Interviews suggest power needs of 10 kWe by 2028 to 2 MWe in the mid-2030s. Lower end suits a small habitat; higher end supports propellant production for Earth return.
    - Commercial partners, particularly SpaceX, stating a desire for 100kW-class reactors for Mars by 2028 and megawatt-class systems as soon as possible thereafter.

Potential Capability Pull

On the lunar surface, NASA has not officially designated nuclear power as essential (as it has for Mars), but its potential contributions are clear. If available, nuclear systems could support key activities such as in-situ resource utilization (ISRU), enabling resource extraction and processing. They would also provide reliable power for lunar habitats and ensure survival during the 14-day lunar night, a major challenge for both human and robotic operations.
Commercial lunar landers, including those from Intuitive Machines and Astrobotic, have expressed interest in small nuclear systems (10–50 We) to enable night survival. Some of these companies plan to fly Stirling-based radioisotope power systems (RPS) as early as 2028, demonstrating a tangible near-term demand for compact, reliable nuclear power.
In Mars transportation architectures, multiple propulsion options are still under study, including all-chemical, solar electric-chemical hybrids, and two nuclear-based approaches (NTP and NEP-Chem). Emerging fully reusable heavy-lift vehicles with rapid turnaround and in-space refueling may challenge early nuclear concepts in terms of transit efficiency. However, these analyses have yet to fully consider factors such as launch and refueling cycles, abort scenarios, and the feasibility of propellant production on Mars. Regardless of the propulsion choice, high-power (1–2 MWe) systems will be critical for ISRU operations to produce the fuel necessary for Earth return missions. If a chemical propulsion architecture is ultimately selected, nuclear power may still be the most practical solution for large-scale energy generation on Mars, given the vast amounts of propellant required, potentially thousands of metric tons, which would necessitate a robust and continuous power source.
The path forward for nuclear propulsion remains uncertain, as NASA has not committed to any single propulsion architecture. The Exploration Systems Development Mission Directorate (ESDMD) continues to study all four primary options, and without a firm mission requirement, NASA is unlikely to make much progress on Nuclear Thermal Propulsion (NTP) despite Congressional mandate.
Ongoing expert discussions suggest that fission surface power for the Moon and Mars should be our nation’s top priority, given its immediate applicability and shared technology base with other nuclear systems. Progress in fission surface power would also directly support the development of small robotic-scale Nuclear Electric Propulsion (NEP), as the two share common technologies and subsystems.
Given budget constraints, careful sequencing will be essential, prioritizing efforts that deliver near-term impact while laying the groundwork for more advanced capabilities. This is where coordinated efforts

between NASA, the Department of Energy (DOE), and the Department of Defense (DoD) will be crucial. Ongoing discussions continue, and this draft strategy will weigh in on the most effective path forward.

Robotic Space Science

Existing Mission Pull

The National Academy of Sciences Decadal Survey sets robotic space science priorities. Although white papers on nuclear electric propulsion (NEP) were submitted in the last two Surveys, no nuclear-based missions emerged as high-priority flagship projects. Instead, the Surveys recommended further study of nuclear-enabled missions. In other words, there is no NASA science mission, current or proposed, that we are aware of that has specifically requested fission power or NEP.

Potential Capability Pull

Nuclear power and propulsion technologies have the potential to unlock groundbreaking scientific discoveries by addressing some of the most persistent challenges in deep-space exploration.

One major advantage is the ability to reduce trip times by more than 50%, significantly improving mission efficiency. Current deep-space missions suffer from extended flight durations and restrictive launch windows, limiting when and how often such missions can be executed. For example, the Cassini mission took nearly seven years and required multiple gravity assists to reach Saturn. With Nuclear Thermal Propulsion (NTP) or Nuclear Electric Propulsion (NEP), travel time to Saturn and beyond could be drastically shortened, easing launch constraints and accelerating the return of scientific data.

Another key capability nuclear propulsion can enable is enhanced maneuverability. Past missions like Galileo and Cassini ended primarily due to a lack of available propellant, restricting their ability to continue exploring. With high-performance NTP or 20–40 kWe-class NEP systems, spacecraft could carry significantly more maneuvering propellant, allowing for deeper and more comprehensive exploration of planetary systems. This would enable extended orbital operations, repeated flybys of moons, and even precision maneuvers that are currently impossible with chemical propulsion.

Nuclear power would also allow for long-duration missions spanning 10 to 20 years, which is critical for gathering multi-season data on outer planets and their moons. Missions like Galileo and Cassini provided only a snapshot, roughly a single season, of Jupiter and Saturn's complex atmospheres and magnetospheres. Since these gas giants have much longer years than Earth, the lack of seasonal data leaves significant gaps in our understanding of their dynamic systems. Additionally, these missions performed only flybys of key moons like Europa, Enceladus, and Titan, limiting our ability to fully characterize these worlds. More extended operations enabled by nuclear power could allow long-term studies, revealing how these planetary systems evolve over time.

Another transformative advantage is the ability to increase data transmission rates by 10 to 100 times. Deep-space missions like New Horizons took 18 months to send back all of its Pluto flyby data, constrained by its low-power (~12 W) radioisotope thermoelectric generator (RTG). A nuclear system capable of generating more than 10 kWe could vastly improve downlink speeds, enabling real-time or near-real-time science return. This would not only enhance mission productivity but also allow scientists to dynamically adjust observation strategies based on newly acquired data.

For the most ambitious science goals, life-detection missions with sample return to ocean worlds like Europa and Enceladus will require robust and persistent power sources. The best chance of finding extraterrestrial life within our solar system lies beneath the ice-covered oceans of these moons. However, landing, operating, and successfully returning samples from these extreme environments will demand power and propulsion capabilities far beyond what solar panels or chemical propulsion can provide. Only nuclear systems offer the necessary energy density and longevity to sustain these missions from surface operations to sample retrieval and return to Earth.

Despite their immense potential, robotic science missions using nuclear power and propulsion must remain cost-effective within existing funding frameworks. Flagship missions typically have budgets under $5 billion, with propulsion and power ideally consuming no more than 10% of the total cost (~$500 million). Achieving this affordability will be challenging unless nuclear system production is shared across multiple missions and leveraged for broader space and defense applications. If scalable production models are established, a wide array of groundbreaking missions could become feasible, including:

- A sample-return mission from Enceladus, retrieving materials from its subsurface ocean
- A Neptune orbiter with a full lunar tour, studying its moon Triton in depth
- A Triton lander and potential return mission, exploring its active cryovolcanism
- A Mercury night-side lander and sample return, surviving the extreme temperature fluctuations
- A Europa lander, conducting in situ analysis for possible biosignatures

Each of these destinations presents extreme environmental challenges, low sunlight, harsh radiation, or vast energy demands, that make nuclear power not just an advantage, but a prerequisite for achieving high-impact science. By investing in nuclear power and propulsion, space exploration can push beyond current limitations, unlocking transformative discoveries across our solar system.

Commercial Development

Existing Mission Pull

Interviews with 18 private companies revealed a strong interest in nuclear systems to support commercially viable space ventures. One major area of focus is orbital data processing and storage, which advocates say will require multiple megawatts of electric power to sustain high-performance computing and data transmission in space.

Another promising application is in-space manufacturing, where initial power demands are expected to be in the hundreds of kilowatts but will scale to megawatt levels as production capabilities expand.

lunar resource extraction is another sector claimed to benefit from nuclear power. The company we spoke with noted that early operations will require tens of kilowatts, but by the early 2030s, their energy demand may rise to approximately 150 kWe, as extraction and processing activities increase. These findings highlight the growing need for reliable, high-power energy solutions to enable long-term, sustainable commercial operations beyond Earth. These claims may be unverifiable.

Potential Capability Pull

A growing number of Moon-focused ventures are exploring opportunities in lunar resource extraction, power generation, and industrial development, with nuclear power emerging as a critical enabler.

One notable company, Interlune, is pursuing the extraction of Helium-3 from the Moon, not for fusion energy, as often speculated, but for use in quantum computing on Earth. Unlike many other lunar ventures, Interlune does not plan to rely on government contracts as an anchor customer. According to the company, the potential market for He-3 is significant, with a current value of approximately $20 million per kilogram or $2,800 per liter, and projected demand by 2040 estimated at 4,000 kg per year, far exceeding the current global supply of only 3 kg annually.

Beyond helium extraction, other companies are developing commercial propellant production, power generation, and lunar resource mining ventures, many of which plan to serve government contracts initially before transitioning to private markets. These activities will demand increasing amounts of power, with estimates ranging from 10 kWe over the next five years to 150 kWe within a decade, and likely even higher as operations expand. A recent demonstration of lunar water extraction technologies (LUWEX project) revealed that small-scale extraction requires 100–400 W of power, achieving an efficiency of over 50 grams of water per kilowatt-hour. However, industrial-scale water extraction, which is essential for producing propellant or supporting a sustained human presence, will demand vastly greater power, far beyond what solar arrays can provide alone.

As lunar infrastructure expands, industrial-scale mining, refining, and manufacturing operations may eventually require continuous, high-output power that only nuclear systems can reliably supply. Companies such as Cislunar Industries are actively developing lunar foundries to process metal resources extracted from the Moon, while others are investigating the feasibility of lunar rail systems for transporting materials. These operations will require stable, high-power solutions, particularly for activities that must continue through the 14-day lunar night, when temperatures plunge to approximately 30 Kelvin (-243°C). Even outside permanently shadowed regions, where solar power is intermittently available, large-scale industrial operations will depend on nuclear power for uninterrupted, long-term functionality.

As commercial activity on the Moon accelerates, nuclear power will play an increasingly vital role in enabling these ventures, whether through surface reactors, radioisotope power systems, or other nuclear technologies designed for lunar conditions. The long-term success of these enterprises may hinge on the ability to deploy scalable, reliable nuclear power solutions to sustain a growing lunar economy.

With respect to Mars, SpaceX considers systems below 10kWe to be "in the noise" and believes 80-100kWe class systems are where nuclear becomes interesting. SpaceX expressed confidence in landing on Mars as early as 2026 and desires a 100kWe-class reactor by 2028, followed by MW-class reactors "as soon as possible thereafter."

Enabling vs Enhancing

When Is Nuclear Power the Only Solution? (Enabling Capabilities)

There are specific missions and operational environments where nuclear power is not just advantageous but the only viable solution due to its unmatched energy density, reliability, and ability to function in extreme conditions. These include:

- Sustained Mars Surface Operations and Propellant Production: Large-scale In-Situ Resource Utilization (ISRU) on Mars, particularly fuel production for Earth return missions, requires continuous, high-power output. Given Mars' distance from the Sun and frequent dust storms, solar power alone cannot provide the required energy levels. Nuclear fission is the only power source capable of supporting multi-megawatt-scale operations essential for crew survival and mission sustainability.
- Deep-Space Exploration Beyond Solar Reach: Missions venturing beyond Jupiter, such as orbiters, landers, and potential sample return missions to Neptune, Triton, Enceladus, or Europa, require long-duration, high-power systems. Solar power is ineffective at these distances, making nuclear power the only viable option for continuous operation and high-data-rate transmissions.
- Continuous lunar Night Operations at Scale: The 14-day lunar night presents a fundamental challenge for power generation. While small-scale systems can survive using batteries or alternative energy storage, large-scale operations (e.g., industrial resource extraction, ISRU, or permanent habitation) require a continuous, high-output power source. Without nuclear, infrastructure would have to rely on excessively massive solar arrays and storage systems, which would be infeasible at megawatt scales.
- High-Power Defense Applications in Space: Directed energy weapons, space-based electronic warfare, and persistent deep-space surveillance require continuous, high-power generation that exceeds solar and chemical energy capabilities. If the U.S. intends to field megawatt-class military assets in Cislunar space or beyond, nuclear power is the only practical energy source.

- Cislunar Maneuverability and "Sustained Maneuvering": For U.S. spacecraft to maintain unpredictable, non-Keplerian orbits (i.e., orbits that do not strictly follow gravitational paths), continuous, high-efficiency propulsion is required. Chemical and solar-electric propulsion cannot provide the necessary energy density or flexibility for sustained maneuvering, whereas nuclear propulsion (NTP or NEP) could allow maneuver without regret (space littoral operations), making U.S. assets harder to track and target.

These cases highlight areas where no alternative can match nuclear power's ability to provide sustained, high-output energy in extreme environments. Without it, these missions would be either infeasible or dramatically limited in scope.

When Is Nuclear Power a Major Enabler? (Enhancing Capabilities)

While not always strictly necessary, nuclear power enhances many missions by improving efficiency, reducing risk, or lowering operational constraints. In these cases, alternative technologies could be used, but nuclear offers a superior solution:

- Scaling Beyond Kilowatt-Class Power: For megawatt-scale power needs in orbit or on planetary surfaces, for high power systems, nuclear provides a far more compact and mass-efficient solution than solar. Solar-powered systems would require enormous arrays, heavy batteries, and complex power management architectures, making deployment and maintenance increasingly challenging at scale.
- Faster Interplanetary Transit & More Frequent Launch Windows: Nuclear Thermal Propulsion (NTP) or Nuclear Electric Propulsion (NEP) can cut transit times to Mars or the outer planets in half, reducing radiation exposure for crewed missions and enabling more frequent and flexible launch opportunities. *However, transit could still be achieved using chemical propulsion, albeit with higher fuel requirements, longer flight times, and narrower launch windows.*
- Military Space Operations: Enhancing Low Observability and Mobility. While nuclear propulsion may be viewed as the only enabler of continuous sustained maneuvering, it also enhances mobility for satellites or spacecraft operating in contested environments. Solar-electric and chemical systems can still provide some maneuverability, but nuclear allows greater flexibility, unpredictability, and resilience in dynamic orbits.
- Commercial Ventures: lunar and Asteroid Mining, Space-Based Factories. Industrial-scale operations in space, such as helium-3 extraction, asteroid mining, or orbital manufacturing, will require stable, high-power systems. Solar may suffice for early, small-scale activities, but at larger scales, nuclear significantly reduces system complexity by eliminating the need for expansive solar arrays and large-scale battery storage.

In these applications, nuclear power is not strictly required, but it removes constraints, improves mission flexibility, and reduces operational risks, making it the preferred choice for high-power, long-duration space activities.

Summary of Mission Pull Data

While nuclear power and propulsion offer clear enabling and enhancing benefits for a wide range of defense and space missions, firm mission pull remains limited. However, geopolitical shifts, evolving military doctrine, commercial space expansion, and the increasing demand for resilient energy infrastructure suggest that this could change in the near future.

- **National Security (Terrestrial) – Technology Push, No Mission Pull (Yet).** Despite the development of terrestrial nuclear reactors for defense applications, such as Project PELE and grid-independent power for remote military installations, there is no explicit mission pull for nuclear power in terrestrial defense today. The ongoing efforts are technology-driven, advancing system capabilities in anticipation of potential future demand (2-7 MWe) rather than in response to defined operational requirements. However, as the military prioritizes operational energy resilience and as the vulnerability of fuel supply chains becomes more apparent, these reactors may transition from a speculative capability to an operational necessity.
- **National Security (Space) – No Immediate Mission Pull, But Emerging Interest.** There is currently no formally stated requirement within the Department of Defense (DoD) or U.S. Space Force for space nuclear power or propulsion. While some exploratory work is underway, such as DARPA's DRACO (NTP demonstration) and AFRL's JETSON (NEP, RPS), these are not linked to defined mission needs. The Golden Dome initiative, while often cited as a potential driver for high-power space-based assets, does not currently require nuclear power. However, if directed energy weapons, persistent ISR (Intelligence, Surveillance, and Reconnaissance), or space-based computing become core elements of future military space operations, nuclear power could rapidly become the only viable solution to sustain these systems.
- **Human Exploration – First Clear Mission Pull (Mars Surface Power), lunar Still TBD.** NASA's November 2024 decision to use nuclear fission for Mars surface power marks the first unambiguous mission pull for nuclear space systems in decades. The scale and timeline remain uncertain, but Mars mission architectures explicitly include nuclear power as a necessity for long-term operations. Optimists predict 2028 for the first infrastructure landing (NASA's timeline suggests 2037); either way, the goal is to include a nuclear power system (10–40 kWe) for habitat/equipment power in the early landings and 750 kWe–2 MWe to enable propellant production for Earth return. For the Moon, NASA has not yet committed to nuclear power. Current lunar power studies focus on solar with batteries and beamed power, with nuclear considered as a precursor demonstration for Mars rather than an operational necessity. However, if lunar activities expand, nuclear power could transition from an option to a requirement, particularly as operations scale beyond kilowatt-class power levels and into megawatt-class industrial capabilities.
- **Robotic Space Science – No Current Mission Pull But Recognized Benefits.** The National Academies' Decadal Survey has not prioritized nuclear fission power or nuclear electric propulsion (NEP) for any upcoming flagship science missions. However, there is growing recognition that nuclear propulsion and power (20-40 kWe range) could enable higher-efficiency transit, greater maneuverability, and expanded deep-space operations. Future missions, such as a Neptune orbiter, Europa lander, or Enceladus sample return, could drive demand for nuclear power, but there is no current mission requirement. A shift in priorities, such as an increased

emphasis on rapid deep-space exploration or high-powered instruments, could generate a clearer pull for nuclear-enabled robotic science missions.

- **Economic Development – Growing Interest, But No Committed Demand for Nuclear.** The commercial sector has expressed strong interest in nuclear power for lunar mining, in-space manufacturing, and orbital computing, with power estimates ranging from 10 kWe in the near term to 150 kWe–2 MWe in the 2030s. However, this remains an emerging interest, not a committed demand. Cost, risk, and regulatory barriers still prevent nuclear power from being an established commercial requirement.

For nuclear to move from a theoretical enabler to an indispensable capability, the challenges that have historically stalled its development must be addressed. In some cases, mission pull has not emerged because decision-makers do not yet recognize nuclear's full potential, or alternative technologies are perceived as sufficient for the time being. In other cases, even where nuclear is acknowledged as a game-changing capability, deep-rooted challenges, ranging from infrastructure gaps and regulatory hurdles to fragmented leadership and inconsistent funding, have made investment in nuclear systems too complex, costly, or uncertain to sustain.

Across civil exploration, national security, and science, the decisive inflection arrives in FY 28-30; without nuclear, the United States either de-scopes missions or cedes first-mover advantage to adversaries. Without addressing these systemic barriers, space nuclear power and propulsion will remain stuck in an R&D cycle.

Appendix C

Illustrative List of NEP Demo Payload Concepts

Apophis Rapid Intercept and Reconnaissance – Use NEP to intercept asteroid Apophis during its 2029 Earth flyby, enabling extended close observation or delivery of scientific instruments on a time-critical trajectory.

Particle-Beam Asteroid Deflector - Use the reactor's power to drive a sustained high-energy particle beam or ion jet against a small near-Earth object for weeks, demonstrating true deep-space hazard mitigation via continuous thrust.

Rapid Mars Cargo Loop - Dispatch a cargo module on a ~0.3 AU outbound burn, coast to apoapsis, then return to Earth orbit within a year, proving high-ΔV, multi-leg autonomy for future uncrewed resupply.

High-ΔV Cislunar Transfer Tug - Ferry multi-ton payloads (rovers, habitat modules, propellant) from LEO to NRHO or low-lunar orbit in weeks instead of months, cutting propellant mass by >50 % vs. chemical stages.

Orbital Propellant Depot Resupply & Relocation - Demonstrate rendezvous, capture, and repositioning of a large LOX/LH_2 tanker from LEO to a Cislunar depot, then return to LEO for reload, showing infinite-life station keeping and rapid-turn services.

Debris Removal Sweep - Perform a multi-target drag-and-deorbit campaign: attach to several defunct LEO objects over a 3-month mission and spiral them safely into atmosphere.

Deep-Space SmallSat Delivery - Deliver 6U–12U CubeSats to Mars or asteroid targets: NEP provides >10 km/s ΔV to escape Earth and inject into interplanetary trajectory, validating deep-space deployment.

Gateway-to-Surface Lander Tug - Tow a fully fueled lander from the lunar Gateway NRHO down to a near-pole landing site, then return the tug to NRHO for reuse, showing precision descent support.

Phobos/Deimos Recon Tug - Transport sensors and mini-landers between Phobos and Deimos orbits, showcasing flexible, multi-body mission support with high cumulative ΔV.

Power in Cislunar Orbit. High-power radar or communications payloads in Cislunar or high-inclination LEO orbits, where eclipse duration limits solar viability, and steady power draw is required. Persistent platform with power as the central service for plug and play modular payloads.