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# The State of Space Propulsion Research

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## TECHNICAL MEMORANDUM

### THE STATE OF SPACE PROPULSION RESEARCH

#### 1. INTRODUCTION

As the Nation attempts to reinvigorate its space technology programs and prepares to embark on a new era of space development and exploration, it is an appropriate time to reconsider future long-term research and technology investment plans and seek better alignment with newly established goals and visions for the new space systems that will be needed for implementation. It is particularly important to examine how space propulsion technology has progressed over the intervening years since the last era of exploration, as epitomized by the Apollo program; to understand current gaps and needs; and to make alterations and adjustments as appropriate.

Space propulsion deserves special attention in this regard since the fundamental technical obstacles to broader human engagement with space are the limitations in state-of-the-art transportation capabilities for both launch and deep space penetration. More directly to the point, traditional propulsion system performance is approaching fundamental theoretical limits that cannot be overcome through further investment, and the specific energy and specific power characteristics of traditional systems are, in fact, simply too low to ever support a robust and vigorous exploration agenda throughout the solar system. Objective consideration of these fundamental limitations leads to one overriding conclusion: Revolutionary advancements in space transportation will only emerge from sustained basic research on highly energetic propulsion methods.

This Technical Memorandum (TM) discusses some basic issues and impediments that have hampered or prevented the effective pursuit of such innovative technological solutions over the years and that will continue to hamper and impede progress in the future unless some changes are implemented. To address these concerns, specific recommendations and a practical plan of action are suggested.



## 2. WHITHER SPACE PROPULSION INNOVATION

Where is U.S. space flight today, and how did we get here? After 40 years, why are we slowly converging on a slightly updated Apollo architecture? Why is there no Moore's law analogy for rocketry? Clearly, we have arrived at a watershed moment in U.S. space flight history, and it is essential that we reflect on such questions in a forthright way. Decisions are now being made that could set our future course in space for decades to come, and it is appropriate that we examine the logic that brought space transportation full circle almost back to where we started.

The circumstances leading to this crossroad are complex, but in large part, the current situation can be attributed to inadequate or ineffective past investments in basic space propulsion research. Generally speaking, the Nation has invested in various space vehicle hardware development programs, but nothing seems to have transitioned to flight application. The history of high-speed hypersonic X-vehicles, as depicted in figure 1, is a prime example. The lesson, which we seem unable to heed, is that a good conceptual idea will not mature and become practical without sound underlying research and hard-won solutions to critical technical issues. At the risk of oversimplification, it is propulsion technology more than any other single factor that governs space transportation system architecture. The simple fact is that there has been no quantum-like leap in space propulsion capability over the last 40 years that would radically change our options and enable truly routine, safe transport to Earth orbit and into the solar system. Despite the expenditure of money on programs aimed at technological advancement, we continue to find ourselves bound by the limits of traditional chemical rocket propulsion technologies.

Thus, we are faced with an uncomfortable proposition: Does the fact that there is currently not a clear path to a breakthrough solution imply that none exists because the ultimate technological limits of propulsion truly have been reached? Virtually everyone, even the most entrenched technologist, would disavow this conclusion; however, there is real, vehement disagreement within the propulsion community on how best to address the obvious gaps. In general, the majority opinion is tilted toward investment in applied research aimed at evolutionary improvement of traditional technologies until a clearly definable alternative with a simple development path can be identified. It is our position, however, that no innovative alternative will ever emerge without a significant level of investment in basic research. Because such investments are inherently high risk and may provide inefficient or negative returns, private investors, unless possessed with inordinate vision and wealth, are unable to justify the costs. The public sector, on the other hand, has been so constrained by risk aversion and so focused on near-term operations that it has proven unable to commit to a sustained, long-term, forward-looking program of basic propulsion research. We believe that this current imbalance is readily correctable, but it will require a critical reevaluation of research and technology focus and a deliberate reemphasis on research that can move us well beyond established technological capability.

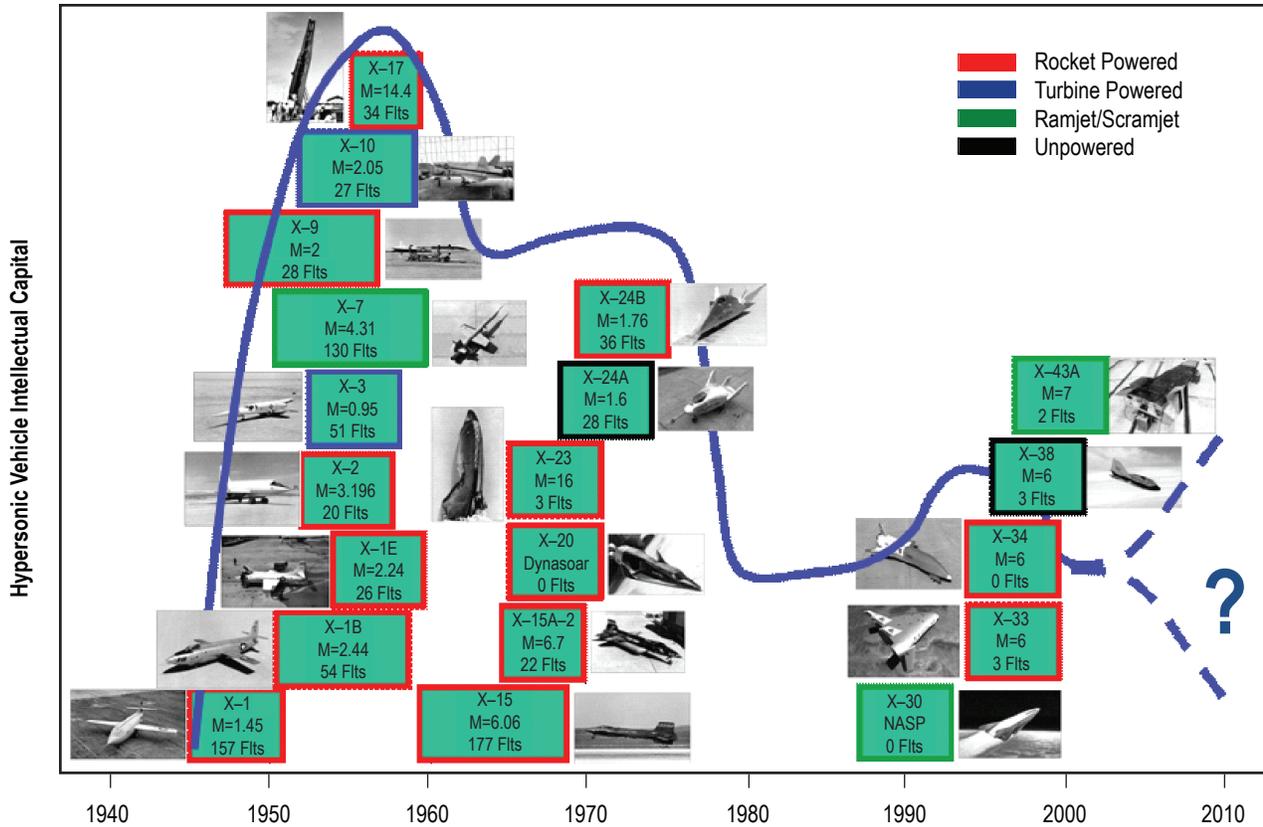


Figure 1. Summary of national intellectual capital investment in high-speed/hypersonic X-vehicles.

This should not be viewed as a call to arms for unrestrained research funding, but rather as a calm and clear statement of the need to achieve a balanced research and technology investment approach. To be sure, the expenditure of taxpayer dollars should be undertaken in a thoughtful and prudent manner, but a hardnosed, short-sighted investment strategy that avoids all elements of risk is technologically sterile. Without question, maintaining operational space access capacity is of highest priority, but we also need the foresight to look to the future and to attempt the difficult and seemingly impossible tasks that will create new possibilities. These dual objectives require opposing mindsets and are generally in direct philosophical conflict, which inevitably leads to a difficult struggle when both goals must coexist within the same competitive environment. In all but the most extraordinary cases, the mainstream high-profile operations activities quickly achieve dominance and naturally subjugate and subsume the immature, far-reaching research efforts that offer the only real hope of changing the status quo. The significant challenge, therefore, is to create circumstances where both objectives can coexist and thrive. Our purpose is to suggest a course of action that could help generate these circumstances and thereby reinvigorate space propulsion innovation based upon far reaching research and innovation.

### 3. ENABLING THE SPACE EXPLORATION VISION

In simplest terms, the Space Exploration Vision is concerned with expansion of human ecology from Earth and into the cosmos. If properly framed and executed, it will be a quest not of pure adventurism but of a determined outward expansion of human presence and activity. Ultimately, it is about going to stay and live. This stands in stark contrast to the traditional view of science-based exploration, which has been primarily concerned with the acquisition of fundamental understanding and knowledge through unmanned autonomous missions. We are now entering an era when these previously separate objectives will be conjoined and intertwined in ways heretofore unimagined, hopefully, to the betterment of both.

The initial phases of this bold, long-term agenda, including establishment of the first permanently manned lunar base and mounting the first human expedition to Mars, will require a long-term sustainable program. Moreover, this program cannot be viewed as a simple matter of systems engineering since the technologies, knowledge, and infrastructure required to accomplish these goals do not currently exist. Most critical, among the many needs to enable meeting these ambitious goals, will be new high-performance space transportation systems for efficient heavy lift launch and rapid movement of large masses and people across vast distances of interplanetary space. Full realization of this highly ambitious agenda will therefore demand space propulsion performance beyond the realm of current capability.

Consequently, if we truly wish to implement a sustained and affordable human and robotic exploration program and desire to extend human presence throughout the solar system, we must first acknowledge the basic shortcomings and then pursue a course of action that could lead to revolutionary technological solutions and quantum-like leaps in space transportation capability. Otherwise, human space expeditions will continue to be viewed as unsustainable feats of romanticism, and real ecology change will forever remain unrealistic. Most desperately needed are innovative methods to effect order-of-magnitude or more increases in propulsion energetics, as defined by system-specific energy and power. It is difficult to imagine how such dramatic gains can ever be attained, however, unless we mount a serious program of basic research now. Even then, success will only be achievable through the combined commitment of public, private, and academic entities and by unprecedented cooperation on an international scale.



#### 4. THE NEED FOR A SPACE PROPULSION RESEARCH INITIATIVE

Currently, there is no coordinated basic research program for space propulsion technology. There has been and continues to be a modest level of program support for applied research and advanced technology development, but not for basic research. This is a serious long-term limitation since long-term realization of the exploration vision will depend on revolutionary advancements in space propulsion capability leading to entirely new transportation systems. Thus, there is a real need for a more balanced investment approach in space propulsion research, and this longstanding need has become even more critical and obvious in light of the outstanding technical challenges ahead.

Basic research proper, whereby we probe the edge of existing knowledge and technical know-how, is inherently a slow and inefficient process and must be undertaken with a long-term perspective and a high tolerance for failure. This is an exceedingly difficult position to sustain within the modern era of the monolithic professional manager, in which extraneous administrative abilities and image projection are more valued and prized than in-depth knowledge and competence within the domain of responsibility. This trend, when coupled with the Nation's tendency to fund Research & Development (R&D) on a fragmented year-to-year basis, while imposing stifling and costly oversight, goes a long way towards explaining the absence of a strong and healthy basic research program as well as our current deficit in research capitol, which would normally serve as the wellspring for technical innovation.

What is most needed, if we hope to meet the needs of the future, is a stable and protected research environment with the capacity, strength, and technical backbone to support worthy high-risk projects and to sustain that support to a conclusive outcome. Most essential is a sustained funding commitment independent of budgetary crisis in mainline programs, missions, and operations and the wherewithal to maintain support over the long haul. A long-term perspective will be absolutely necessary since, from a historical perspective, the life cycle for the development and fielding of new space propulsion technology can be measured in decades. Therefore, the availability of new propulsion technology for some future space transportation system must be predicated upon significant up-front research and development, as illustrated by an idealized program life cycle in figure 2.

It is our contention that the Nation should initiate, organize, and administer a space propulsion research initiative that will meet these critical needs. Because future space transportation requirements, particularly as they relate to deep space exploration, go far beyond the needs of more conventional Earth orbital spacecraft, it is clearly the public sector's responsibility in this arena. One can hope that private sector efforts will help fill the shortfall, but it is difficult to imagine any impact in this regard since the inherent high risk and poor returns of long-term research dissuades commercial enterprises from making any significant investments.

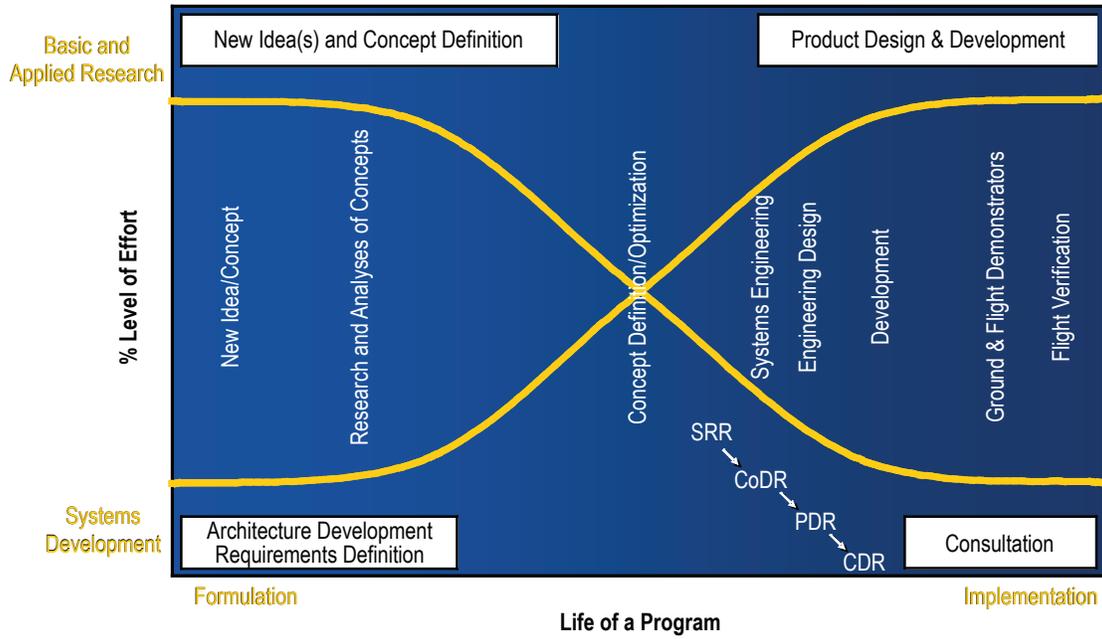


Figure 2. Idealized program life cycle illustrating relative distribution of R&D efforts over time.

## 5. STRATEGIC FRAMEWORK

The most important step in moving forward with a Space Propulsion Research Initiative is establishing a long-term, sustainable framework. This framework would define the focus, scope of activities, goals and objectives, and guiding principles for practical implementation of a meaningful and effective program. We proffer the following thoughts and suggestions.

To insure that long-term needs are addressed without losing near-term relevance, it is suggested that such an initiative encompass both basic and applied research in support of the Nation's space propulsion needs. Heaviest emphasis would be placed upon new science and revolutionary technology to enable voyages and commercial ventures that are not currently feasible, but the initiative should also address special innovative solutions and technical improvements having nearer term potential for flight system utilization. The inclusion of some applied research is considered vital as a means of maintaining a link to broader exploration program objectives and developing a programmatic reputation as an innovative problem solver and practical contributor. One cannot live on dreams alone.

Ideally, the basic research component of the proposed initiative would be structured to conduct fundamental feasibility assessments and demonstrate scientific proof-of-principle of highly enabling propulsion concepts. It is envisioned that the technological scope would embrace innovative solutions applicable to both launch and deep-space transport systems. By emphasizing a longer term, higher pay-off strategy, it is hoped that the Nation will be better positioned to define and fill future technology gaps and maintain a more balanced investment portfolio that avoids the classic down-selection process whereby promising but premature ideas are strangled in favor of well-defined low-risk approaches based on existing technology. To be effective, these basic research investments must be rooted in sound technical analysis and follow a sequential tract encompassing scientific feasibility, technical maturation, relevant demonstrations, and transition to practice.

History tells us that such highly aimed research is bound to be controversial and subject to intense criticism by various detractors. Thus, successful execution and long-term survival of the initiative will require credibility and integrity beyond reproach. Of foremost importance will be the establishment of a culture dedicated to "excellence in research" and a staunch commitment to "good science" with the widest possible dissemination of results and complete openness and respect for peer-driven critiques and assessments.

There has been and will continue to be intense debate over the proper placement of R&D responsibilities. On one hand, there are the overt extramuralists, who would prefer to transfer all research to academia and all development to industry while promoting a "leave the administering to us" mantra. On the other hand, there are the overt intramuralists, who would generally prefer to hold complete command over R&D activities despite the susceptibility to over-centralized control and the "not invented here" syndrome. In our considered opinion, neither of these extreme views is sensible or desirable.

Rather, proper stewardship of propulsion R&D will require Government administrators possessing in-depth technical knowledge and competence over their domains of responsibility and the good sense to seek expert contribution at its source, external and internal to the Government.

As a strategic principle, it is suggested that the proposed research initiative be organized to include both extramural and intramural elements, with separate competitions for each sector. Implementation of such an approach will require a small and technically strong project office capable of understanding the detailed technical issues associated with a particular line of research and using this understanding to set priorities and develop focused lines of attack. The goal would be to structure packages of individual research tasks that, as a whole, exhibit a cohesive and concerted movement towards a desired objective.

As a guiding principle, unique expertise, facilities, and capabilities should be utilized to the maximum extent possible. This should include NASA, Department of Defense, and Department of Energy laboratories, universities, private sector entities, and international collaborations and partnerships. The extramural component of the initiative should also include efforts aimed at stimulating education and extending graduate research opportunities for future scientists and engineers. From an intramural perspective, it would be highly desirable to enhance and develop NASA's in-house expertise and capabilities, beyond applied systems engineering. This type of in-house investment is direly needed to maintain technical competency and remain a world-class contributor to space propulsion innovation.

It should be noted in passing that many universities around the Nation have managed to initiate and sustain some excellent space propulsion research, despite the lack of reliable funding and support. As a result, these activities have tended to suffer at the mercy of year-to-year fluctuations in funding and underappreciation of their contributions, which has made it difficult to maintain continuity and cohesiveness in their programs. In our opinion, this valuable resource is too often overlooked as a major source of new ideas and innovative solutions to our most difficult technical problems, and any attempt to erect a new research initiative should build on this existing capability to the maximum extent possible.

## 6. TECHNICAL FOCUS

The central technical shortfall for better space transportation is the general unavailability of highly energetic propulsion technologies. That is, the specific energy and specific power characteristics of traditional space propulsion systems are simply too low to effect dramatic improvements in mission capability. The energy content of chemical fuels, for instance, has reached its natural plateau, beyond which only marginal improvements can be expected, and this fundamental limitation places severe constraints on the amount of payload that can be delivered for a given vehicle size. Even with energy densities equivalent to solid core nuclear rocket performance, one should note that conventional thermal propulsion is fundamentally constrained by definite material temperature limits, as illustrated in figure 3. Moreover, the low thrust-to-weight ratio and high specific-mass characteristics associated with available low-power electric propulsion invariably yields excessively long interplanetary trip times. There is, therefore, a broad technical gap between the presently available level of propulsion system performance and the level that will ultimately be required to fulfill the exploration agenda.

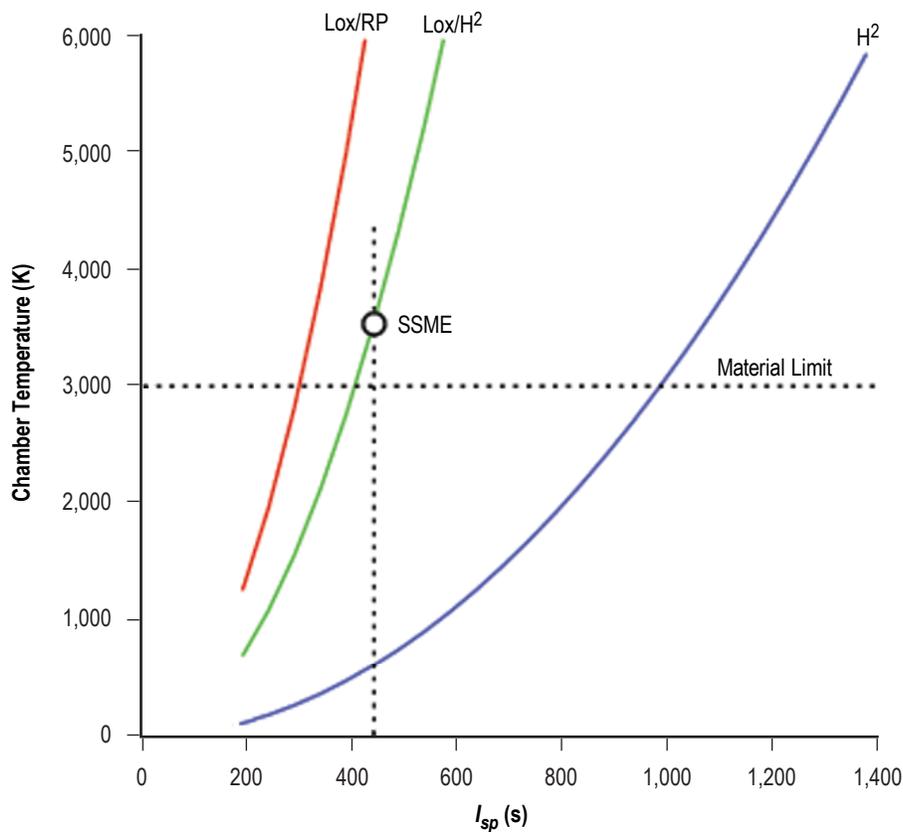


Figure 3. Limits of conventional thermal propulsion performance. Innovative methods are needed to bypass solid state thermal constraints.

To address this shortcoming, it is recommended that basic research be technically focused on high specific-energy/high-power propulsion and power. The broad scope of coverage should include advanced chemical propulsion emphasizing high energy-density matter and advanced engine cycles; advanced high-power electric/plasma propulsion emphasizing MW-class thrusters, high-temperature technologies, electromagnetics, and flight-weight magnetic systems; utilization of nuclear energy sources emphasizing high-temperature fission-based thermal propulsion methods, low specific-mass fission-based space power plants, and fusion propulsion; and advanced energetics emphasizing off-board resources, beamed power, and ultra-energy storage. As a hedge, the proposed program of research should also contain a low level of activity targeted on new scientific discoveries and fundamental physics breakthroughs with revolutionary relevance to space transportation.

History has repeatedly shown that when a technology has matured to a performance plateau for which evolutionary improvements yield diminishing returns, as graphically illustrated in figure 4, a revolutionary breakthrough is required to obtain a quantum-like leap in capability. This general rule should be expected to apply to space propulsion technology, as well.

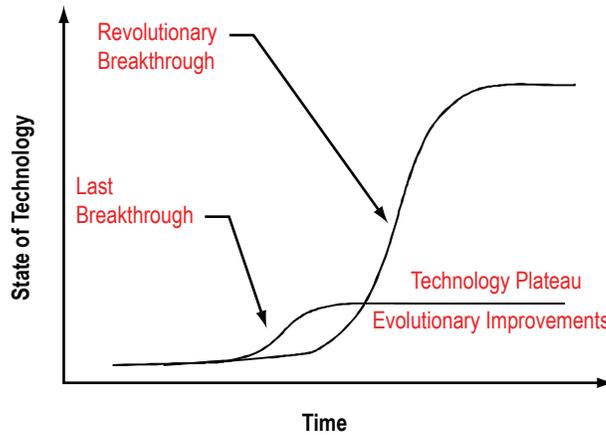


Figure 4. Illustration of technology maturation with plateau of diminishing returns and quantum-like leap in improvement through revolutionary breakthrough.

As a means of maintaining relevance and credibility, the proposed initiative should also include an applied research component to address special innovative solutions and technical improvements having nearer term potential for flight system utilization. Because technologies are often pressed into service before full understanding has been established, so-called mature systems often experience recurring problems and performance anomalies that are not clearly understood. In this sense, applied research can be viewed as means of bridging up technical gaps by identifying, assessing, and promoting modern technological improvements to legacy systems. Recent revolutionary advancements in information technologies, for example, offer tremendous opportunities for autonomous fault detection and correction.

From a more practical perspective, applied research is an important key to reducing total system development costs. Based on historical experience, propulsion system development normally proceeds through a repetitive cycle whereby hardware test failures result in redesign and repair or replacement

of the failed components followed by subsequent test to failure. Gradually, as the number of “test-fail-fix-test” cycles grow, our knowledge and understanding improve and we rise up a learning curve leading to a final optimized design. Consequently, hardware costs tend to drive the overall cost of any engine development program.

Analysis of historic detailed cost distributions on major development programs tends to confirm this basic conclusion. These results clearly show that roughly half of the development cost is for hardware with the remaining half split between test, engineering, and management. Thus, our tragic flaw is a repeated failure to conduct up-front applied research before embarking into major development activities. By incorporating some applied research into the proposed initiative, it is our intent to help encourage a transition towards more cost effective integration of research with mainstream systems development, as illustrated in figure 5.

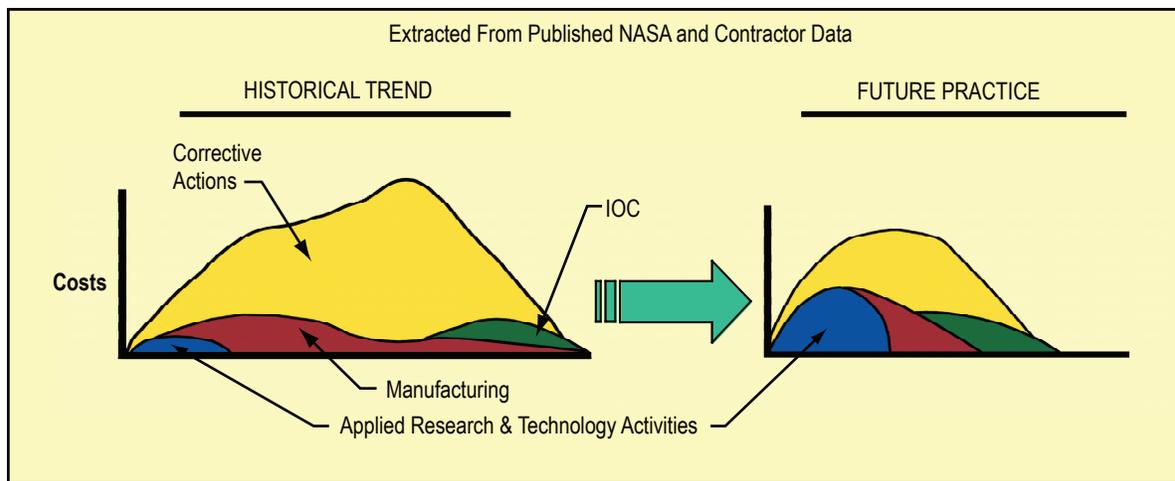


Figure 5. Historical and future trends in cost distributions for propulsion system development.

## 7. CONCLUSIONS AND RECOMMENDATIONS

The current state of space propulsion research, based on thoughtful and candid consideration, is dismal. The simple fact is that full realization of the Nation's space exploration goals can never be accomplished without revolutionary advancement in space transportation capability. Moreover, even the earliest lunar exploration goals of this bold agenda will require some modest propulsion system advancement. Despite the desperate need, however, there is no coordinated basic and applied research program for space propulsion technologies.

To address this critical need, a Space Propulsion Research Initiative ought to be established, which would run parallel with exploration systems development and include a significant basic research component. As an implementation approach, we recommend the establishment of sustained funding for a National Space Propulsion Research Initiative. This would create a direct link to future space exploration needs and serve to revitalize the Nation's traditional R&D focus and heritage of technical innovation. It is believed that such an initiative would result in a more balanced portfolio of basic and applied research and yield the innovative solutions that will be required to enable a robust, exciting, and sustainable human and robotic space exploration program for years to come.

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