(B) "LONG-TERM" FUTURE

https://doi.org/10.1017/S0252921100077277 Published online by Cambridge University Press

LAUNCH VEHICLES OF THE FUTURE: EARTH TO NEAR-EARTH SPACE

G. A. KEYWORTH, II
Director of Research, Hudson Institute, Indianapolis, Indiana, U.S.A.

None of us thought, when this colloquium was scheduled, that the timing would enable it to become a celebration as well. The launch, after years of postponements, of the Hubble Space Telescope, has cast a galactic glow over the proceedings here this week. But at the same time, the frustrating delays caused by the collapse in 1986 and very slow regeneration of the U.S. space launch capabilities since then make this discussion of near-earth access very pointed.

1. Current Situation

As we know, the sheer momentum of the U.S. Space Shuttle Program has dominated our perceptions of space launch for a decade and a half. It reached its peak in the early 1980s when our national policy placed nearly total reliance on the Shuttle as our means of access to space. It was a policy doomed to fail, for obvious and not-so-obvious reasons.

The most obvious is simply the vulnerability of such high reliance on a manned vehicle. Any accident necessitates a review and program of risk-reduction that is as complete as is our respect for the people who fly them.

A second problem with reliance on the Shuttle has been the resulting pressure to come up with a bus-company-like schedule, rather than evolving its flight frequency as experience accrues.

The ability to carry out 24 launches a year looked highly dubious even before the Challenger tragedy. If we could manage even half that number now we'd consider ourselves to be doing well.

A third problem is a strong bias toward large and consequently expensive payloads, a bias that is increasingly anachronistic. Much of the technology developed in the years since the Shuttle was designed, especially in microelectronics, has made smaller payloads more attractive. Smaller, smarter payloads have applications across the board, but they're especially important for space science. We need affordable experiments. We need numerous experiments so that more people can take active roles in them. And we need experiments that can be planned, built, and conducted on much shorter time scales than we've been seeing. That seems to me to be a prerequisite to drawing good young people into space science.

A fourth problem is cost. By any measure current space access is expensive. There are several reasons for that. One is that the kind of rocket technology we use has inherent limitations on performance; those are reflected in the cost. Another is that the sheer complexity of the Shuttle requires an enormous personnel and mechanical infrastructure. Choose your number, but anywhere from 6,000 to 14,000 people are involved, one way or another, in a Shuttle launch. Finally, we don't really

Y. Kondo (ed.), Observatories in Earth Orbit and Beyond, 347–354.
© 1990 *Kluwer Academic Publishers. Printed in The Netherlands.*

https://doi.org/10.1017/S0252921100077277 Published online by Cambridge University Press

know what the real cost of a Shuttle launch is. When you have a state-run enterprise that dominates its market, accurate financial measures are elusive.

2. Future Systems

All that I've described with regard to current systems is reality, and I don't mean to put value judgment on it. It's simply where we are in the stages of development of U.S. space technology. Over twenty years it's no surprise that the system begins to look dated and inflexible. But it has often been awkward, even painful, to address these realities head-on, simply because so much effort has been invested in the current launch systems and because they must continue to serve our needs for years to come. Yet it is timely, in fact overdue, to look at future technologies and systems for space access. And President Bush's real commitment to and call for us to begin the next chapter in space exploration makes it essential.

Let me set out some criteria that future systems should try to meet.

First, and foremost, is cost. A cost on the order of $5000 per pound to put something into low-earth orbit obviously restricts who or what can get there. From the perspective of space science, even a little experiment becomes a big expenditure, especially when compared to the costs of doing other, non-space research. As I'll discuss, we can set as a target a cost of about two orders of magnitude lower.

My second criterion for a new access system is flexibility of payload size. Not everything we want to put into space is the size of the Space Telescope. In the early years of the space program, bigger was better. Not any more. Clever technology makes it increasingly more effective to use multiple deployments of special purpose satellites.

The small satellite, weighing only a few hundred pounds, is the space-borne equivalent of the personal computer; it can be assembled quickly, using readily available hardware, and it can be launched on short notice with little fanfare. Just as computers are shifting away from emphasis on large mainframes to distributed microcomputers tailored to specific applications, satellite users and builders now look to applications that can be met with smaller and cheaper satellites. The billion-dollar multi-ton satellite is the equivalent of the mainframe computer; it takes a decade to build, is so expensive that only a few can be afforded, and has to line up launch times years in advance. In both cases – satellites and computers – the key technologies are being decentralized and put more and more under the affordable control of individual users. The future looks increasingly like it will be moving to "LightSats" and other PC-equivalents in space. And that, of course, was always supposed to be one of the benefits of a "space age."

This is likely to be particularly important for space science aimed at better understanding of the earth itself, where frequency of sampling has often been the missing element in our ability to create realistic models of natural systems. Just to pick a current example, what would offer a better chance to understand the dynamics of global warming – one 25,000-pound satellite, or 250 100-pound satellites? Yet our existing launch systems are highly biased toward the single large satellite.

My third criterion for future space access is a familiar one: We should try to develop routine access to space. Routine access to space is, of course, what we

https://doi.org/10.1017/S0252921100077277 Published online by Cambridge University Press

haven't had for the Space Telescope. Routine access means being able to launch on relatively short notice. And I'll reemphasize that routine access almost certainly goes hand in hand with frequent launches, and that comes back to lower cost as well. Even after forty years of space access with rockets, we're still a long way from that capability, even for unmanned systems. It's eye-opening to compare the way in which manned air flight progressed rapidly to that kind of routine, and safe, operation, compared to space flight.

3. Change

Is there a solution to this set of equations? If there's not, then we may be faced with a demoralizing lid on what we can ever expect to achieve in space, always constrained by high costs that seem as unassaultable as a physical constant. But if that were the case, it would be a rare technology. In fact, in spite of the attention currently focused on extrapolations of Shuttle or current-ELV designs, there are newer, more clever approaches now becoming available. It may be that the greatest barrier we face is not technological, but the inevitable resistance to discontinuities.

While the prospect for rapid improvements in performance would seem to be especially appealing to organizations responsible for space access, it is tempered by the uncertainties – and natural resistence – brought about by rapid change. Machiavelli summed up this universal dilemma centuries ago when he observed that "Change has no constituency." And in those areas where the change promises to be greatest – in changing the cost and operational equations of space access – it creates the most uneasiness among people whose primary mission is to maintain ongoing rocket technology programs and guide its evolution over time.

But if there's anything that's more inevitable than resistance to change, it's change itself. And that's why I'm confident that we're due for some significant breakthroughs. Let me return to those criteria I laid out for future space systems.

My first criterion was cost. And I said we should aim for a two-order-of-magnitude reduction. Call it my Federal Express model for space access. That model cannot be met by most of the follow-on launch systems now being considered. I'm thinking of such options as the Advanced Manned Launch System, essentially a Shuttle follow-on; multi-stage ELVs, which are essentially new faces on old ballistic missile technology; the unmanned heavy lift vehicle, known as Shuttle-C, which would substitute an unmanned payload for the Shuttle's orbiter; or the Advanced Launch System, a next-generation unmanned ELV. Of those, the Advanced Launch System purports to be the most ambitious about taking a big bite out of the cost barrier, with a target rate of about $300 per pound. That's a laudable goal, a one-order-of-magnitude reduction, but few people believe it's realistic – or even close to realistic – based on the kind of technology projected for it.

There is also a class of small expendable launch vehicles, being promoted primarily by small commercial firms. While these don't make significant inroads in cost reduction over larger ELVs, they do address one of the other criteria – flexibility of payload size. In fact, the rapidly intensifying interest in small satellites – in the range from 50 to 1000 pounds – may find that these relatively unsophisticated small launchers fill a role that the larger ones cannot. And as we saw just a few

https://doi.org/10.1017/S0252921100077277 Published online by Cambridge University Press

weeks ago, there was a successful launch of Pegasus, which is in effect a multi-stage ELV in which the first stage is a high-flying aircraft. While such an approach lacks some of the elegance of other systems, it illustrates the kind of flexibility of launch and size of payload that is clearly needed.

A more interesting, and potentially promising, class of launch vehicles are single-stage-to-orbit rockets – such as the Phoenix, being developed by Pacific American Launch Systems. These would take advantage of improvements in materials technology to create a lightweight, reusable launch vehicle. They would also take advantage of some novel engine concepts that permit variable fuel/oxidizer ratios matched to ambient conditions as the vehicle climbs through the atmosphere.

In many ways these represent what could be possible in rocket technology when thinking is unconstrained. In the early days of the space program, it was apparent that weight and engine thrust prohibited a single-stage-to-orbit. So we developed staged rockets and got comfortable with them, sometimes forgetting that a design decision based on technology of the 1950s and 1960s might be worth revisiting every so often.

Some people have, and with promising results. So of all the evolutions of rocket technology, these may well be the most promising route for optimizing it. Rather than build a bigger expendable structure to support a heavier payload, the proponents of single-stage-to-orbit may succeed by fresh thinking about the whole launch system. And at least some of the people pushing these concepts have opted at the start to achieve operating simplicity – which translates into lower cost and more flexible operations – by emulating airplane-like operations, rather than rocket operations.

4. Costs

The incentives to do so are strong. About half the cost of a launch goes not to hardware or fuel, but to operations. So the proponents of single-stage-to-orbit, as do many proponents of small ELVs, hope to achieve a substantial cost reduction by reducing that enormous human operating cost.

I continue to focus on cost for two reasons. One, as I'll discuss, it can be cut substantially. The other is that our view of the future of space programs is distorted by assumptions about cost and demand for access. We've all seen estimates of demand for launch capacity that suggest that the Shuttle, plus the existing military and commercial ELVs available worldwide, will be able to handle the demands that are now projected for the next half dozen years. But that's putting the cart before the horse. At $5000, or more, per pound, there are very high barriers to participation. If I'm a potential user, and someone comes to me with an order book in hand, asking me how much launch I want at $5000 a pound, my answer is going to be a lot different than if someone comes to me and says, "If I can provide launch at $50 a pound, how much might you want?" In fact, there are going to be many, many more users – users who aren't even being considered at today's high prices – who will surface if prices drop. There's an assumption at work that presumes demand for space access is essentially inelastic. I think that's nonsense, yet it continues to drive the dominant space access planning processes at work today.

https://doi.org/10.1017/S0252921100077277 Published online by Cambridge University Press

But I'll take it a step farther. I think a look at easily projected space access needs shows that we're already undersupplied with lift capacity, and it may get worse. When I look to space programs over the next several decades, I see the following.

– Expanded need for space-relayed communications, including a new generation of direct broadcast television satellites.
– A proliferation of local and global navigation and position-locating systems, with systems becoming inexpensive enough for everyday use by individuals.
– A next generation of earth-sensing programs, including the ambitious Mission to Planet Earth. The early impact of Spot Image, the French company selling commercial satellite photos, gives a taste of the kind of market that could develop for the even higher quality services that could be made available.
– There's a backlog of planetary exploration options, and manned missions to Mars will require a whole new space infrastructure, for which low-cost, routine access to low earth orbit will be no less than enabling.
– I expect military need for space access to increase, not decrease, as the nature of international relations changes. As nations reduce defenses, there's a correspondingly higher premium on intelligence – nothing more than the old adage to "trust, but verify."
– I also expect military satellites to take increasing advantage of micro-miniaturization of electronics, leading to smaller packages and an increasing mismatch with existing launch systems.

The list of space programs I've just run through is hardly visionary. In fact, the linearity of the extrapolations I've used displays the dilemma we're in, because we'd be hard put to accomplish a majority of them. And, with the appropriate architecture for satellite systems beginning to look more and more like that of distributed computing, with the much heralded space station beginning to look like the proverbial "lead balloon," and with renewed interest in totally different propulsion systems to take years off the round trip to Mars, our current approach to low earth orbit access is about as relevant as worrying about the vitality of U.S. Steel is to U.S. industrial competitiveness.

5. National Aerospace Plane

Happily, there is one research program, well underway, that tackles this dilemma head on. It's a technology whose potential is well recognized in Japan and in Europe, but here it's often overlooked as an oddity – neither airplane nor rocket. Yet it's one that should be able to provide the kind of space access I set the criteria for when I started this talk. That's the National Aerospace Plane, or the NASP. Ironically, but I hope only temporarily so, the NASP hasn't been seriously included in plans for the post-Shuttle space-launch systems. Some see it as lacking a sponsor, although we've already invested nearly two billion dollars in it. Some see it as an airplane, not as access to space, although that's been the driving goal for a decade. And some see it as futuristic, too far away to be part of today's debate. Yet its first flight in 1996 is sooner than the half-width of the policy debate over the Space Station. Lip service is paid to NASP as a means for space access, but the real attention seems

https://doi.org/10.1017/S0252921100077277 Published online by Cambridge University Press

focused on things like the Advanced Manned Launch System.

There are a lot of institutional reasons behind this hesitancy about the role of the NASP in space launch. I don't intend to go into those, other than to say that I expect them to fade as the NASP gets closer to reality.

Briefly, for those not familiar with the NASP, it's essentially an airplane that is designed to achieve hypersonic speeds, using a sophisticated air-breathing engine. Little additional oxygen is needed to enter low-earth orbit. And it will return to an airport runway under powered flight.

I said earlier that such novel new approaches as the Phoenix single-stage-to-orbit rocket use engine controls to adjust the mixture for optimal performance during powered flight. That's an important advantage over traditional rockets. The NASP takes that advantage even further. It will alter the engines' internal geometry, as well as fuel and air flows, for optimal combustion in a sequence of speed regimes, from takeoff at airplane speeds from a runway, into supersonic ramjet mode, and eventually into hypersonic scramjet mode.

Let me try to frame how the NASP technology, as it could be embodied in the so-called NASP-derived vehicles, would compare with other space launch technologies.

First, cost and payload. Estimates for a NASP-derived vehicle are a cost of two to five million dollars for a payload of perhaps 20,000 to 30,000 pounds to orbit. That compares to the Shuttle, whose cost for a comparable payload is nominally about $150 million. And here's an interesting comparison between the two. Costs for fuel for a Shuttle flight are only about four percent of the launch costs; about half its costs are for hardware that has to be replaced with each flight. For the NASP, about half its launch costs go for fuel, and virtually none of it is for hardware. Costs for a workhorse ELV like a Titan may be somewhat less than the Shuttle, but still on the order of $100 million for a payload somewhat smaller. Small ELVs, like Pegasus and Space Services' commercial Conestoga rocket, offer a launch sized to small payloads, but, while filling a much-needed gap, their costs per pound are still comparable to larger ELVs. And I simply don't find credible the Advanced Launch System's 300-dollar-per-pound goal.

Finally, I have seen estimates for the new single-stage-to-orbit designs that are substantially lower than existing launch costs. Phoenix, for example, a reusable vertical-takeoff and vertical-landing manned vehicle, projects costs for medium-sized payloads far closer to NASP's projected costs than to existing systems. The Phoenix is an intriguing concept with substantial technical merit. However, it is essentially a paper design at this point, though it might be adaptable to fast-track development that would make it a possible alternative during the transition to air-breathing vehicles.

The other criterion of importance is what we've referred to as routine access to space. For those of you who have been waiting for five years for the Space Telescope Launch, routine access may seem less important than predictable, reliable access. Yet in the long run, space science in particular demands frequent access for small payloads. It shouldn't take years from the time you design an experiment until you can conduct it. Yet it does now. You not only have to wait until it can be incorporated into a larger package, but you're then likely to have to wait until the larger package can win a slot in the launch schedule. One of the prime attributes

https://doi.org/10.1017/S0252921100077277 Published online by Cambridge University Press

of the NASP is its fast turnaround and minimal logistics. A single NASP-derived vehicle might be able to make from 40 to 150 flights a year if it can achieve its projected ground turnaround time of 24 hours. And unlike current rockets, and especially the Shuttle with its stringent abort requirements, the NASP shouldn't be affected much more by weather than regular aircraft operations. The NASP also offers the advantage of access to virtually all near-earth orbits. To me, that begins to meet a meaningful definition of routine access.

I am myself highly optimistic about NASP's prospects; other people are understandably skeptical. All of us remember our experience with the projections made for the Shuttle – particularly its mission schedule. But we shouldn't have too long to wait to test many of the projections for the NASP. As it stands now, the NASP program expects to make the first test flight in 1996. Frankly, it could have been sooner, but the development program was stretched out last year, largely for budgetary reasons. Even so, if the test flight program proceeds on schedule, the NASP will be ready to try a single-stage-to-orbit flight in 1998. And we could see a NASP-derived vehicle – that is, an operational version of the NASP – capable of achieving orbit by the year 2000.

But even on a shorter time scale, there will be chances to assess how well the projections are likely to hold up. There have already been several – all encouraging.

6. Prospects of NASP

When NASP was first proposed as a national program in 1985 there were many who were skeptical that materials could be developed to hold up to the extreme operating conditions. The worst of those problems are the nose and leading edge surfaces, whose temperatures would soar at high mach numbers. Yet that potential show-stopper looks like it's essentially solved now. And solved in a way worth noting. Early in the NASP program the major contractors formed a unique consortium that enabled them to share data and research on different approaches to the materials problems. So we now have available an array of materials – high-temperature intermetallics, metal-matrix composites, materials with high-thermal-conductivity and high specific creep-strength, and even practical carbon-carbon composites – that seem to be well able to handle the stress expected in the aircraft. And if I were to point to any single area of technology as the most important that will spin off from the NASP, it would be the technology of engineered materials. Ten years from now when manufacturers are routinely designing three-dimensional materials properties into their products, they'll look back on the NASP as the catalyst for that capability.

The other area of uncertainty is the engine technology, which is the fundamental enabler of the NASP concept. The optimistic design objective is to be able to use the engines themselves – without the addition of rocket assist – to propel the aircraft nearly all the way to orbit. So far that optimism is unshaken. Obviously, there are limits to what can be learned by ground testing – limits in the kinds of operating regimes that wind tunnels and shock tunnels can produce. That will only be overcome with flight tests on the actual X-30 research vehicle.

However, the program has gained substantial confidence in the engine designs

https://doi.org/10.1017/S0252921100077277 Published online by Cambridge University Press

and variable configurations through advanced numerical simulation techniques in combination with available testing data. There's strong confidence already in the engine performance up to about Mach 10 or 12. It's certainly true that skeptics can, and do, await demonstrated performance. But at this point there's strong reason for confidence that the upper limits of the speed range will be gratifyingly high.

It's also important to remember that, although the NASP's initial, and perhaps most important, role will be for space access, it is being developed as one would develop an airplane. As a manned, reusable vehicle, it will have the advantage of repetitive flight testing in which the operating characteristics will be refined, and in which performance will be explored, step by step, into the unknown flight regimes of altitude and speed.

Also unlike a rocket, the NASP can have varying degrees of success. A rocket that doesn't achieve orbital velocity is a failure. But what if NASP can't achieve more than Mach 15 or Mach 20 in air-breathing flight? It will then inject sufficient oxygen into the same engine to get the final boost. The need to store more oxygen on board will cut into the payload, but most of the performance benefits it set out to capture will be preserved. It still has operating flexibility and fast turnaround because it operates from airport runways. And it will still bring enormous cost reductions to space access by eliminating a vast portion of the most expensive rocket flight regime – from the ground up to perhaps 200,000 feet. In effect, even a NASP that doesn't meet complete design expectations will create a launch platform very nearly in orbit to start with.

NASP, like any development program, will encounter problems as it refines the technology. But two things in particular give me confidence in the ultimate success. One is the state of the engine design. It's derived from a reliable concept, and the work so far to extend and demonstrate its range has been gratifyingly successful. That builds confidence. The other is that there have been several different good solutions developed for most of the critical materials problems, and the new materials have been tested and can be manufactured. So even though the NASP represents a totally new approach to space access, it's a technology that's already has some maturity. Many people are surprised when I talk about the NASP primarily as a means of access to low-earth orbit. There's been a perception that NASP would be primarily a hypersonic airplane, intended for high-speed point-to-point transport, or perhaps as an exotic military reconnaissance or rapid deployment aircraft. In truth, it might be those things as well, and it has as-yet unassessed potential impact – very possibly massive impact – on the aircraft industry. But the nearest term impact will be in space access, and that impact will be profound.

https://doi.org/10.1017/S0252921100077277 Published online by Cambridge University Press