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The Value Proposition of Multi-Megawatt Electric Power/Propulsion for the Human Exploration of Mars

John H. Scott[a]*

[a] *Propulsion and Power Division, Lyndon B. Johnson Space Center, National Aeronautics and Space Administration (NASA), Code EP1, Houston, Texas 77058, United States.* john.h.scott@nasa.gov
* Corresponding Author

Abstract

NASA and other space agencies have been offering stakeholders an architecture for the human exploration of Mars that has remained essentially unchanged since the 1960's. The mission duration and launch mass are the two first order figures of merit that drive the total cost of such an architecture. The options studied to date center on the "conjunction class" or "Long Stay" mission, in which surface infrastructure elements are sent ahead on uncrewed, slow trajectories requiring a minimum amount of propellant, and the crewed elements are sent at the synotic cycle's shortest trajectory, stay on Mars until the next close alignment, and then return to Earth. Total crewed mission durations for these architectures range from 900 to 1100 days, with variations driven primarily by trades between the amount of propellant launched and the effective specific mass of the propulsion technology assumed (e.g., chemical, nuclear thermal, solar electric, nuclear electric). Mars transit propulsion systems assumed in mission architectures studied to date have all resulted in architectural figures of merit that drive the cost to a level of "too much." However, nuclear electric propulsion (NEP) technology offers a "knob" that might be turned to enable a radically different Mars architecture, whose launch mass and mission duration may enable a value proposition more palatable to mission stakeholders. Mars architectures studied to date have assumed an NEP system providing 2.5 MW_e at a specific mass of no less than 20 kg/kW_e. This has often been seen as obtainable with a moderately high temperature fission reactor. An NEP system providing 15 MW_e at specific mass of ~1 kg/kW_e, though, could enable a short stay (30 days on surface) Mars mission, requiring only two or three SLS-class launch vehicles and a total mission duration of under one calendar year. However, turning this NEP "knob" would require a high risk development program driving innovation on the order of that delivered by the Manhattan Project, but for a fraction of the cost. Any technology option that might offer such a capability at such a development cost now stands at a low Technology Readiness Level (TRL) 3, would be based on a nuclear energy source (likely fusion), and would require an extremely high risk (and rapid) development effort. The aggressive, parallel path project management paradigm exemplified by the original Manhattan Project might have the best chance of success. An energy source developed in this manner may also have a major impact on the terrestrial power industry.

Keywords: propulsion, power, project management, Mars, human exploration, nuclear, fission, fusion

Nomenclature

ΔV - "Delta-V" The total change in velocity that must be provided via propulsion for a given mission payload. ΔV drives the total energy an in-space propulsion system must provide.

α - specific mass of an in-space electric power and propulsion system, kg/kW_e.

I_{sp} – specific impulse (thrust per unit propellant mass flow of a rocket engine), s

F - thrust (N)

P – Power (W)

Acronyms/Abbreviations

LEO – Low Earth Orbit (generally 200-1200 km orbital altitude)

IMLEO – Initial Mass to LEO

EMC – Evolvable Mars Campaign

TRL – Technology Readiness Level

FOM – Figure of Merit

DRA – Design Reference Architecture

DDT&E – Design, Development, Test, and Evaluation.

SLS – Space Launch System

ITER – International Tokamak Experimental Reactor

1. Introduction

When one speaks of "high power" in human spaceflight applications, one refers to anything beyond the 150 kW_e available on the International Space Station. However, there is a strong value proposition in efficiently developing low-α, MW_e-scale power sources for spacecraft. Such could not only accelerate the human exploration of Mars and beyond but also, if the right technologies are chosen for development attempts, spin off disruptive energy sources for terrestrial commercial applications.

NASA and other governmental space agencies have been studying crewed missions to Mars for decades. The constraints of orbital mechanics and a need to limit the amount of energy (often expressed as ΔV) that an in-space propulsion system must provide, and therefore the amount of propellant that must be launched, have led mission architects to focus planning around the periodic alignment "opportunities," at which the required energy is at a local minimum during the 26 month the Earth-Mars synotic cycle. Planners also note that these local minima vary significantly over a sixteen year cycle define by the eccentricity of Mars orbit.

In developing options for a given opportunity, Mars mission architects trade between two classes of mission: The "conjunction class" or "long stay" mission and the "opposition class" or "short stay" mission. The former represents the global minimum energy trajectory for a given opportunity, allowing surface stays of around 500 days, with the total mission duration of 900-1100 days, driven by the length of the synotic cycle. The latter allows only up to 60 days on the Martian surface, but offers potentially shorter mission durations at the expense of higher energy input (and thus greater propellant mass requirements).

A key assumption mission architects must make in designing such missions is the capability of the Mars transit propulsion technology, for which common first order FOMs include α and P for continuous low thrust trajectories and I_{sp} and F for high thrust impulse trajectories. This assumption ultimately drives total mission duration and the total mass that must be launched from Earth. These last are in turn the first order figures of merit (FOMs) that drive any estimate of mission cost.

Integrated human Mars mission studies published since the beginnings of human spaceflight have almost always assumed Mars transit propulsion systems of TRL greater than 4 or 5, for which the cost and risk of development are reasonably well understood. Studies have also assumed an effectively constant cost of launch from Earth. As a result, these mission studies have offered stakeholders the same Mars mission options for decades. While the first order FOMs drive mission cost predictions which have varied widely, all such costs estimates to date have been "too much." Thus, a human mission to Mars has yet to be mounted.

In a first order assessment there are two "knobs" that mission architects can "turn" to lower the cost of a human mission to Mars. One involves a drastic reduction in the cost of launching vehicles and propellant from Earth to an assembly orbit. Industrial concerns funded by private capital are openly pursuing this and claiming some progress [1], [2]. However, turning this particular knob results primarily impacts only space exploration and commerce.

The other knob requires the development of a lower-α, high P, low cost Mars transit propulsion system. Full success in this direction might not only enable a human expedition to Mars but also disrupt the terrestrial energy industry. This paper explores how this α and P "knob" has been set in selected historical Mars mission architectures and what means may be required to "turn" it further.

2. Historical Mars Architecture Studies

Human Mars missions have been studied under NASA's auspices since the 1960's. A selection of these studies reveals how effective α and P levels affect the mission duration, launch mass and, therefore, the cost.

3.1 Boeing Integrated Manned Interplanetary Spacecraft Concept Definition (1968)

This is one of the first end-to-end studies sponsored by NASA to consider human missions to Mars and, in this case, Venus. [3]

Typical of the Mars mission architectures developed under this study is that diagrammed in Fig. 1. This "long stay" mission assumed ten launches of either Saturn V or Saturn 1B vehicles to boost spacecraft elements, assembly crews, and mission crews to a staging point in low Earth orbit (LEO). It offered a ~500 day mission in Mars orbit, which included only a 30 day crewed excursion to the surface. The mission would have utilized nuclear thermal propulsion, then under active development, with three firings during Mars transit. Key FOMs for this architecture are summarized in Table 1.

3.2 Design Reference Architecture (DRA) 5.0 (2009-2014)

This transit propulsion initially assumed for this Mars architecture study was nuclear thermal, of which active development had stalled in the 1970's, but the study was later extended to consider how more advanced transit propulsion technologies would affect the primary FOMs of a crewed Mars mission [4], [5]. The study extension also assumed availability of the

Space Launch System (SLS), a heavy lift launch vehicle under development by NASA and Boeing since 2011.

Like in the 1968 Boeing study, the selected basis for comparison was a "long stay" mission, but with the crew spending from 300 to over 500 days on the Martian surface. The study architecture assumed two Martian landers, one carrying cargo for a long stay outpost, the other carrying the crew and other cargo. These landers and their Mars transit craft were to be boosted to a staging orbit around the Earth using multiple SLS vehicles. This architecture is diagrammed in Fig. 2.

The more advanced Mars transit propulsion technologies compared in this study included nuclear electric propulsion (NEP) and hybrid chemical and solar electric propulsion (SEP), both of which had been under study and low level development since the 1980's. The key FOMs for these technologies and the effects they have on mission architecture FOMs are summarized in Table 2.

Table 2 shows that considerably more mass would be required to be launched to a LEO assembly orbit for these DRA 5.0 missions than for the 1968 Boeing mission. Of course, DRA 5.0 places about twice as much mass into Martian orbit. More noteworthy is the degree to which the NEP option lowers total mission cost FOMs below those of the nuclear thermal option. The NEP option requires 120 t less mass to the staging orbit, removing the equivalent of an SLS launch from the total mission cost. Though this comes at the price of a longer total mission duration with less time for the crew on the surface, total mission cost is less sensitive to these. SEP/Chemical hybrid propulsion was included in the study as an option requiring notably less cost to develop than either nuclear thermal or NEP. Note that this option offers roughly the same mass to the staging orbit as the NEP option, but at the price of significantly longer mission duration and significantly shorter time on the surface. In any case, the top level FOMs of all of the DRA 5.0 mission options yielded a mission cost that remained "too much."

3.3 The Evolvable Mars Campaign (EMC) (2015)

The EMC study developed a Mars architecture that was to fit into a constrained annual budget by minimizing technology development cost and risk and by limiting the number of SLS launches each year [6]. Technology development cost projections were kept low by assuming a hybrid SEP/chemical Mars transit system of power much lower than even that of DRA 5.0 [7]. Limiting the annual number of SLS launches resulted in a "long stay" architecture with cargo being delivered to the Mars surface in smaller increments, in turn resulting in a requirement to send five small landers to Mars, as opposed to the two large landers of DRA 5.0. When combined with relatively high α Mars transit propulsion, this resulted in an effective mass to LEO requirement much larger than that of the DRA 5.0 options. The EMC architecture is diagrammed in Fig. 3, and FOMs are detailed in Table 3.

3.4 Trend Analysis of Architectures

Examination of these selected Mars mission studies reveals a trend whereby mission architects have worked to avoid requirements for high *P*, low α Mars transit propulsion, which would involve large and risky investment for development (particularly in the case of nuclear fission options). This has resulted in mission concepts that remain at around 1000 days duration but require more and more mass to be launched from Earth. Considering the first order FOMs driving Mars mission cost, these studies would appear to be pointing in the wrong direction.

4. Mars Mission NEP Parametrics on α and *P*

The architectural FOMs resulting from the NEP case of DRA 5.0 (Table 2) reveal possible knobs to turn in lowering the total cost of a Mars mission to a degree sufficient to encourage an organization's stakeholders to fund it. In the DRA 5.0 "long stay" architecture, the NEP option's lower α yields a mission duration significantly lower than that of the SEP case. Further reduction in α should yield further improvements in the mission FOMs.

Because of the many secondary assumptions that must be made to define a Mars architecture (e.g., limits on perihelion passage inside the orbit of Venus), parametric studies on the degree to which α and *P* levels can affect the first order FOMs driving Mars mission cost are rare. A study published in 1993, however, can yield useful approximations. In this study, George *et al* [8] present parametric curves for both "long stay" and "short stay" missions based on NEP for Mars transit (See Figs 4 and 5).

Both mission types include two trajectories to Mars: one for cargo and one for crew, that for the latter based on the ΔV of the 2016 alignment. The "long stay" mission would deliver four landers to Martian orbit, and the "short stay" only one. FOMs for different levels of α and *P*, graphically extrapolated from Figures 4 and 5, are displayed in Table 4.

4.1 Improving α for Fission NEP

At the first order, lowering α for a fission NEP system is accomplished by increasing the operating temperature of the reactor core, thereby increasing the Carnot efficiency of whatever heat engine is used (which lowers full system mass linearly) and increasing the heat rejection temperature of the system radiators (which lowers radiator mass exponentially).

The NEP system assumed of the first two columns in Table 4, defined in a 2011 design study [9], achieves a lower α than that of DRA 5.0 (Table 2) primarily by assuming a reactor temperature of 1500 K, as opposed to 1200 K as in the DRA 5.0 NEP system. This temperature increase would incur many engineering challenges, but the resulting α would enable a "long stay" mission with essentially the same duration as in DRA 5.0 but with IMLEO lower by the equivalent two SLS launches. Also enabled is a "short stay" mission of similar IMLEO but total duration under 18 months.

Per a 1991 design study [10] a reactor temperature of 2000 K likely represents the ultimate limit of solid core fission reactor technology. This study estimates that such a reactor, perhaps with a solid state thermionic heat engine and under clearly severe engineering challenges, might enable a 10 MW_e NEP system with α of 5 kg/kW_e. As shown in Table 4, such an α and *P* enables a "short stay" mission requiring only three SLS launches and just under 14 months duration.

In the limiting case of Table 4, if an NEP system could be developed with α of 1 kg/kW_e and 15 MW_e, then a mission with 30-days on the Mars surface could be carried out with only two SLS launches and with crew on mission for less than a year. A fission-based option which might approach such power at $\alpha < 5$ kg/kW_e has been studied conceptually [11], [12]. It would draw power for electric propulsion from charged fission fragments by passing them through a direct energy conversion device. Concepts for generating thrust directly from energetic fission fragments have also been studied [13]. Such likely represent the limits in performance for fission-based Mars transit systems.

While the basic physics of fission power have been long understood, the engineering challenges of NEP systems increase dramatically as the desired α decreases. Moreover, the safety and proliferation risks associated with a fission system make the development of any new fission reactor of even modest performance extremely expensive. Cost estimates for development of a fission NEP system, even with the relatively high α assumed for DRA 5.0, vary widely. However, they, like the costs of Mars architectures themselves, have all been seen by NASA and other government agencies as "too much."

4.2 Fusion and Other Advanced Options for Low-α Power and Propulsion

If the cost of the crewed Mars mission concepts offered for decades remains "too much", and the estimated cost of developing the fission-based NEP technology which might enable a radically cheaper Mars mission remains "too much", other options might be investigated. In order to be attractive, such mission architectures would need to utilize power and propulsion technology offering low α and multi-MW *P* but whose development cost from existence proof (i.e., TRL 3) forward to flight is no more than that for SEP or chemical propulsion.

Power and propulsion systems which harness certain nuclear fusion reactions may meet this requirement. However, relatively little investment has gone into the technology harness those reactions.

The vast bulk of funding for nuclear fusion development has been directed towards harnessing the deuterium-tritium (D-T) reaction for terrestrial grid power applications. This reaction releases most of its energy in the form of high speed neutrons which must be captured in a cooling blanket in order to produce energy in a form amenable to conversion into electricity. Efforts to harness this reaction, which involve confinement of a hot equilibrium plasma, have for many years been focused on the International Experimental Tokamak Reactor (ITER) project [14], which is projected to someday yield net fusion power at the GW_e scale in a nuclear core weighing many tons. Other efforts are attempting to enable net fusion power with a much smaller core by means of the high magnetic fields enabled by high-temperature superconductors, e.g., [15]. However, for any of these fusion options, the balance of plant for conversion to electricity still faces the limitations of the Carnot cycle, just as in the case of a fission reactor, and the need for superconducting magnets to contain the fusion plasma would result in a cycle topping temperature rather lower than that which could be available from a fission reactor. Thus, though such a reactor produces waste with only a low level of activation, rendering any development program much cheaper than for a fission reactor, the α of the resulting system would not be sufficiently low to enable a Mars mission architecture with first order FOMs anywhere close to those of the limiting case in Table 4.

Concepts using a D-T fusion plasma as a high I_{sp} propellant, providing direct propulsion in a manner analogous to a fission thermal rocket, have also been studied [16]. Such a systems might offer low α propulsion but only at a GW scale, thus requiring IMLEO much higher than the attractive options for Mars described Table 4.

Fusion reactions around which a propulsion system could be engineered for α in the low single digits include the aneutronic fusion reactions: Deuterium-Helium-3 (D-^{3}He) and Proton-Boron-11 (p-^{11}B). Plasma confinements that can support these reactions are much more difficult to maintain than those required for D-T fusion. D-^{3}He fusion requires an equilibrium plasma an order of magnitude hotter than that needed for D-T. The collision energy required for p-^{11}B fusion is in fact so high that an equilibrium plasma at that temperature would have losses exceeding the energy produced by the fusion reaction. Achieving net power from p-^{11}B fusion will require confinement of a non-equilibrium "colliding beam" plasma, research into which has received drastically less funding than that into equilibrium plasma confinement [17].

While the plasma confinement challenges of D-^{3}He and p-^{11}B fusion greatly exceed those of D-T fusion, which themselves have yet to be met, the balance of plant required to convert the energy released into a form useful for space propulsion is potentially much lighter and simpler than the heat engines required for fission or D-T fusion. These aneutronic reactions release the bulk of their energy by means of high speed charged particles: beta particles for D-^{3}He and alpha particles for p-^{11}B. Slowing these particles in an electric field creates an electric potential than can drive current into power for electric thrusters. The efficiency of such conversion is not subject to the constraints of the Carnot cycle, and the entire power plant can be thus be much lighter than a heat engine. Further, if the energetic fusion products are applied directly to heating plasma in a thruster, the even step of conversion to electricity is eliminated. Such a system can conceivably reach an α near 1 kg/kW$_e$, thus enabling the rapid, low-IMLEO mission described in the right-most column of Table 4 [11].

Aneutronic fusion power is perhaps the highest TRL option for a Mars transit propulsion system that meets the criteria described at the beginning of this section (low single-digit α, multi-MW, cost from TRL 3 to flight equivalent to that of a large SEP system). At the lower end of the TRL spectrum are truly exotic concepts such as quantum vacuum propulsion [18] and the Mach effect [19]. When considering which such concepts to pursue, it is important to realize that, if the low TRL research effort is managed properly, these technologies may not be decades away. As will be argued below, such physics breakthrough can be understood to be either six years away or never. However, by the same logic, it will take three years and tens of millions of dollars to determine which outcome might result for a given concept.

5. "Blitzscaling" and the Manhattan Project

A project management paradigm offering a possibility of achieving a breakthrough technology, at controllable cost and finite risk, is known in current business literature as "blitzscaling" [20]. An historical example in which this paradigm achieved success is the World War II Manhattan Project [21].

At first assessment, the Manhattan Project can never be seen as a "low cost" undertaking. The effort to go from Niels Bohr's 1939 theoretical physics paper to the atomic bomb required six years and 1945US$2.2 billion [22] (2018US$26 billion [23]). However, the effort to go from Bohr's paper to proof that a chain reaction can be controlled in uranium (TRL 3, the Chicago Pile experiment in December 1942) required around three years, one of which contributed no progress, and 1942US$1 million (2018US$12 million). Two aspects of the Manhattan Project's management paradigm might be applied to enable success in obtaining a multi-MW, low-α Mars transit propulsion system and, perhaps, much more.

5.1 Parallel Paths

First, the Manhattan Project was massively parallel path. Under the two top–level paths to produce a weapon (one based on ^{235}U and the other ^{239}Pu), there were multiple paths toward manufacturing each material: Four to enrich uranium and two to produce plutonium. This structure was not set up in order to inspire a sense of competition between diverse teams driven toward unified goal. Only the top managers of each path even knew that other paths were being pursued. This structure was created due to the extreme schedule pressure under which the effort was conducted. The top technical personnel in the U. S. were well aware of the capabilities of their German counterparts and, knowing that they also started from the same physics theory, had every reason to be terrified that the Germans would produce a weapon first. This schedule pressure inspired the managers of the Manhattan Project to pursue every conceivable option at once and to pursue each option with as much money as could practically be spent on it, only eliminating paths when they clearly "hit a wall" or were demonstrably much less efficient than others.

For example, there were initially two parallel paths (two neutron moderator options) pursued for the existence proof of a fission chain reaction and, subsequently, for building the reactors needed to produce ^{239}Pu. Graphite was found to have a neutron absorption cross section close to that of more-difficult-to-obtain heavy water and, as it enabled the Chicago Pile experiment quickly, was soon chosen over heavy water as the moderator for ^{239}Pu production piles at the Hanford site*. Of the four uranium enrichment methods (centrifuge, liquid thermal diffusion, gaseous diffusion, and magnetic separation), only the centrifuge was eliminated, and the other three were found at the Oak Ridge site to provide enriched uranium most quickly by being linked in cascade. Moreover, such was the schedule pressure that all four uranium enrichment methods were pursued with pilot plants starting almost a year before the Chicago Pile confirmed that a chain reaction could even be managed. The U. S. Army Corps of Engineers even acquired the land that became Oak Ridge three months before the Chicago Pile experiment. Plant designs for ^{239}Pu production began to be investigated at the same time, less than a year after Glenn Seaborg's first isolation of the element (in µg samples) in March 1941, and the Hanford site was acquired seven weeks after the Chicago Pile went critical.

**A lesson in the cost of prematurely cutting off a path comes from Nazi Germany's atomic bomb effort. Werner Heisenberg eliminated the graphite moderator path due to what turned out to be a technical mistake by Walther Bothe's team in measuring the neutron absorption cross section of carbon. German scientists then focused only on heavy water as the moderator for their first reactor, intended to produce plutonium for a bomb. Heavy water turned out to be so difficult to obtain in quantity that a chain reaction existence proof was severely delayed, and Albert Speer effectively cancelled Germany's bomb effort about five months before the Chicago Pile experiment took place in the U. S. The world might be very different today if a Heidelberg laboratory had not had a little too much boron in its graphite samples.*

If a critical path waterfall project management paradigm had been implemented for the Manhattan Project, focusing resources only on the path which looked to have the lowest risk when first assessed, the project timeline would have exceeded that of its motivation. It would thus be unlikely that nuclear weapons or nuclear power would be available even today (at least in the United States. The Soviet Union started a bomb effort in 1939 as well). The investment made in whatever path might have been assessed as lowest risk would have been money wasted.

5.2 Avoiding "Unobtainium"

Second, it was understood from 1939 onward that a working weapon would have to be created from what was essentially "unobtainium" (^{235}U, ^{239}Pu). Over 90% of the funds spent on the Manhattan Project, and most all of the project's schedule risk mitigation investment, went into building the plant required to obtain this unobtainium.

The theoretical energy source options from which a low-α propulsion effort might begin could be chosen from among several which, if their fundamentals turn out to be understood correctly, require no such unobtainium. For example, the fuel in p-^{11}B fusion meets this criterion. Examining the experience of getting to the Chicago Pile, working from theoretical physics path to an existence proof (i.e., TRL 3) is not so expensive. Arbitrarily tripling the cost in today's money of the path to the Chicago Pile, the fundamental physics of any given option might be proven, or eliminated, in an aggressive, three year effort costing some $30 million. This amount of money and time should either prove or firmly eliminate the option, either of which is valuable.

5.3 "Blitzscaling" to Mars…and to a New World Energy Economy

Fig. 6 lays out a notional program to develop a low α, multi-MW$_e$ Mars transit propulsion system. The initial parallel paths should be selected during a fast Phase A study from among solutions that promise low-α at an integrated system level while avoiding the massive DDT&E costs associated with producing any substantial amount of activated waste. The cost from TRL 3 to flight for any such option is difficult to credibly predict, but, if no unobtainium is required and no high-level activated waste must be managed, the qualification and flight development cost should be comparable to that of a multi-MW$_e$ SEP system.

Phase 1 of this notional program would then consist of aggressive pursuit of, perhaps, five options toward TRL 3 existence proof (i.e., a Chicago Pile equivalent). Down-selection among these options would be made based on success in existence proof and projection of DDT&E cost. Further down-selection into Phase 3 would be based on what should be a better understanding of DDT&E cost onward to flight, and perhaps two options would be pursued to TRL 6. A concept to pursue to flight would be selected then.

It is important to consider that any energy source option that would meet the criteria described above would also be carbon-free, require only a small amount of ubiquitous fuel, and produce no high-level activated waste. Such an energy source would have implications well beyond that of a human mission to Mars, perhaps disrupting the terrestrial energy economy..

6. Conclusions

Funding stakeholders have been offered essentially the same crewed Mars mission concept since the 1960's. They have not bought it yet and look unlikely to do so in the future. However, developing a crewed Mars exploration program that such stakeholders would fund would require gambling ~$100 million on a set of parallel path, three-year development efforts, each of which might have a one-in-three chance of success. However, if there is to be human exploration of Mars within this century, such a gamble is required. A "win" would not only enable a new age of human

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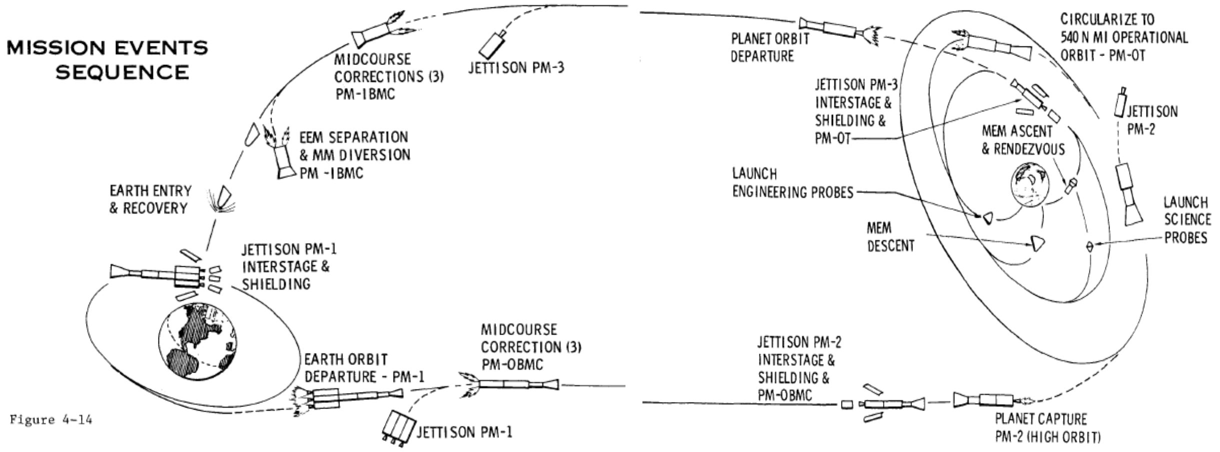

Fig. 1. Boeing Concept Mars Mission Events Sequence

Table 1. FOMs: Boeing Mars Concept (1968)

Mars transit propulsion	Nuclear thermal
Mission Class	Long Stay
Alignment opportunity	1986
Trajectory type	Impulse
Mars transit propulsion I_{sp}, F	850 s, 870 kN
Mars surface duration	30 days
Mass to land from Mars orbit*	43 t
Mass launched to LEO	435 t
Total mission duration	1040 days

incl. descent/ascent propellant

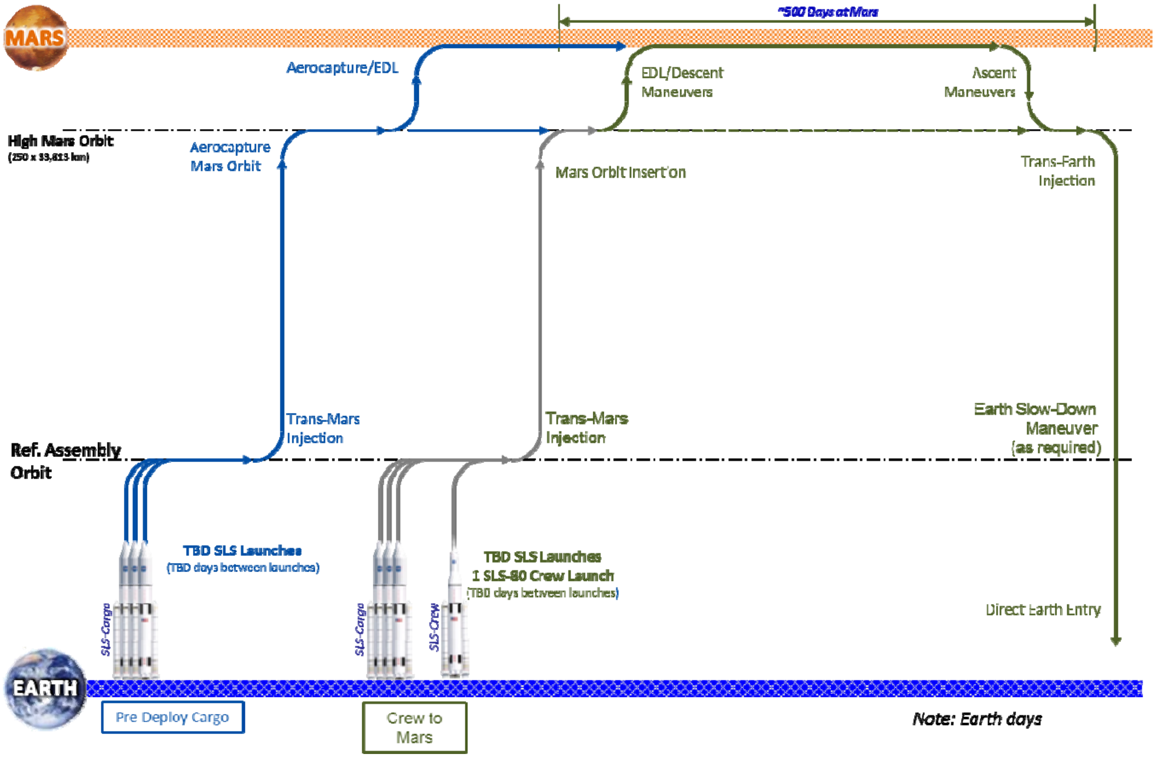

Fig. 2. DRA 5.0 Mars Architecture

Table 2. FOMs for DRA 5.0 (2009)

Mars transit propulsion	Nuclear Thermal	NEP	Hybrid SEP/Chemical
Mission Class	Long Stay	Long Stay	Long Stay
Alignment opportunity	2037	2037	2037
Trajectory type	Impulse	Continuous	Continuous/Impulse
Mars transit propulsion I_{sp}, F	900 s, 330 kN		327 s, -
Mars transit propulsion α, P		20 kg/kW$_e$, 2500 kW	22 kg/kW$_e$, 1000 kW$_e$ (@1 A.U., BOL)
Mars surface duration	539 days	400 days	300 days
Mass to land from Mars orbit*	200 t	200 t	200 t
Mass to LEO	890 t	770 t	780 t
Total mission duration	914 days	980 days	1065 days

incl. descent/ascent propellant

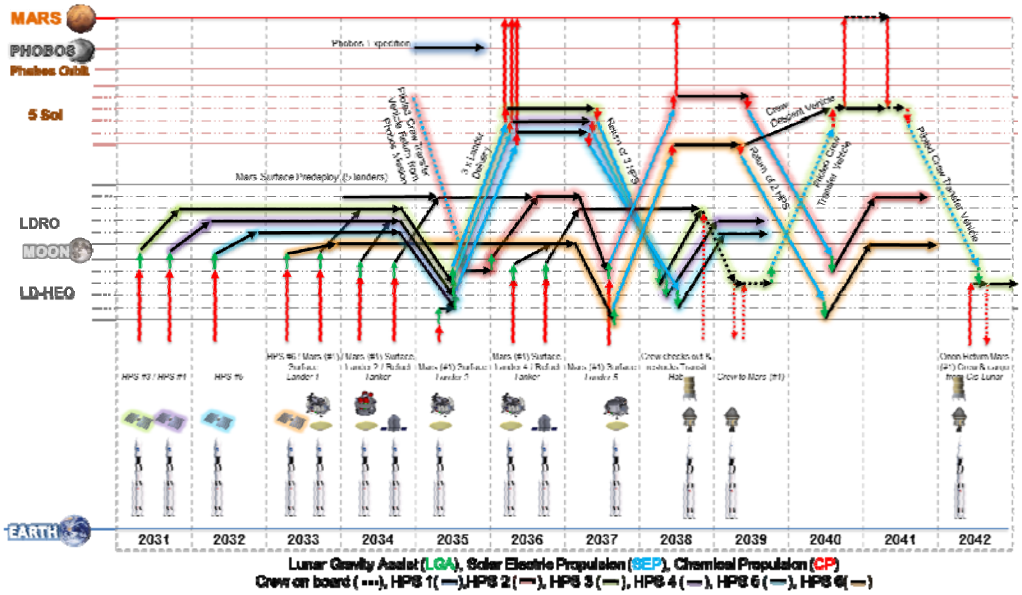

Fig. 3. EMC Mars Architecture (1st Crewed Landing) [7].

Table 3. FOMs: Evolvable Mars Campaign (2015)

Mars transit propulsion	SEP/Chemical hybrid
Mission Class	Long Stay
Alignment opportunity	2035_{cargo}, 2037_{cargo}, 2039_{crew}
Trajectory type	Impulse/Continuous
Mars transit prop. (chem) I_{sp}/F	303 s, 890 N
Mars transit prop. (EP) α, P	35 kg/kW$_e$, 435 kW$_e$ (@1 A.U., BOL)
Mars surface duration	300 days
Mass to land from Mars orbit*	220 t
Equivalent Mass to LEO**	1860 t
Total mission duration	1060 days

**incl. descent/ascent propellant*
***570 t to EMC staging orbit at Lunar distance high Earth orbit*

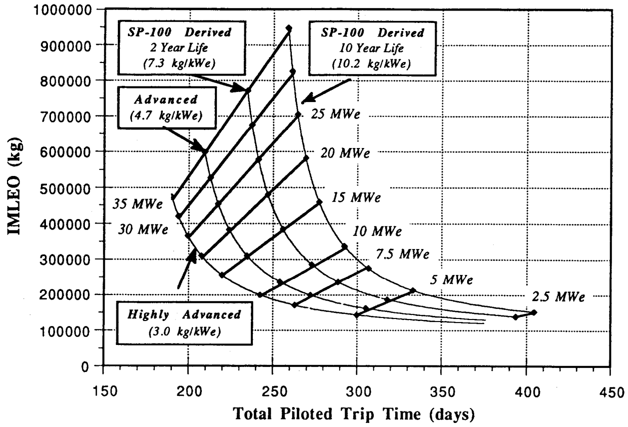

Fig. 4. Piloted Trip Time and Initial Mass to Low Earth Orbit (IMLEO) for "Long Stay" Missions [8].

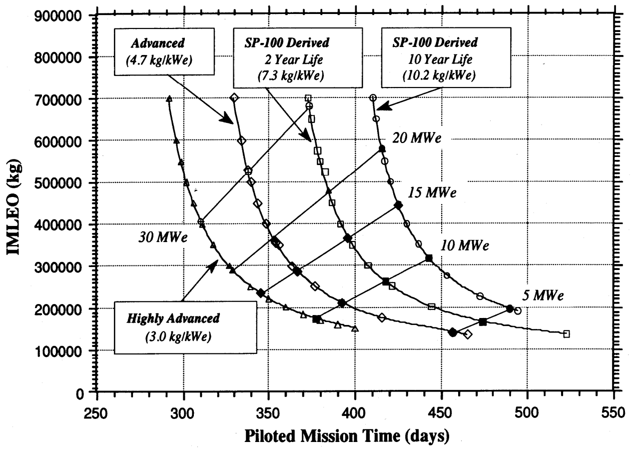

Fig. 5. Piloted Mission Duration and IMLEO for "Short Stay" Missions [8]

Table 4. Parametric Results for Continuous Thrust NEP Mission FOMs as a Function of α and P

Mars transit propulsion α Mars transit propulsion P	13 kg/kW$_e$ 5 MW$_e$	13 kg/kW$_e$ 5 MW$_e$	5 kg/kW$_e$ 10 MW$_e$	1 kg/kW$_e$ 15 MW$_e$
Mission Class	Long Stay	Short Stay	Short Stay	Short Stay
Mars surface duration	600 days	30 days	30 days	30 days
Mass to land from Mars orbit[**]	324 t	84 t	84 t	84 t
Mass to LEO	500 t	480 t	360 t	260 t
Total mission duration	960 days	510 days	400 days	320 days

[*] *incl. descent/ascent propellant*

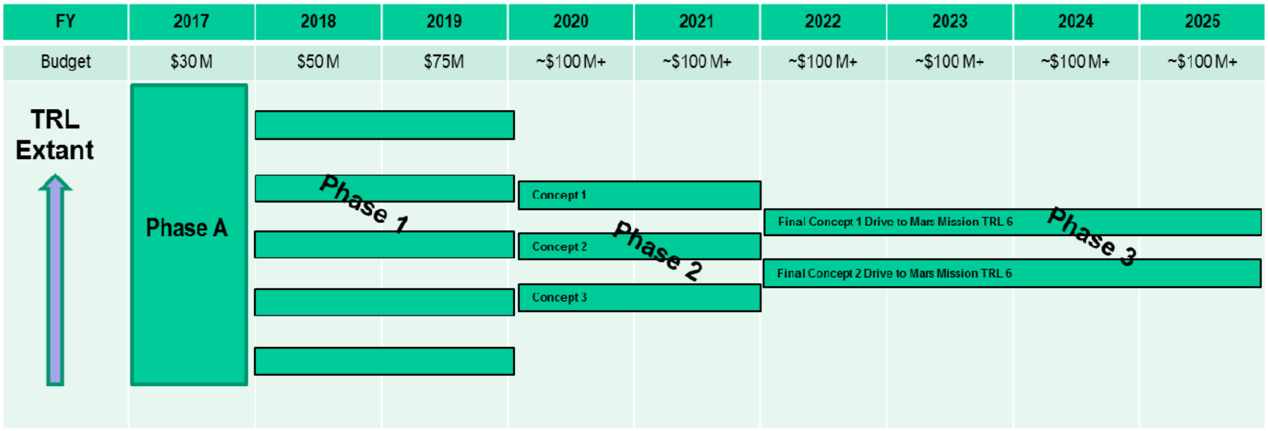

Fig. 6. Aggressive Development Effort for Low-α, Multi-MW$_e$ Mars Transit Propulsion