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JASON LASER PROPULSION STUDY, SUMMER 1977

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Laser propulsion is an idea that may produce a revolution in space technology. A single laser facility on the ground can in theory launch single-stage vehicles into low or high earth orbit. The payload can be 20% or 30% of the vehicle take-off weight. It is far more economical in the use of mass and energy than chemical propulsion, and it is far more flexible in putting identical vehicles into a variety of orbits.

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ABSTRACT

Laser propulsion is an idea that may produce a revolution in space technology. A single laser facility on the ground can in theory launch single-stage vehicles into low or high earth orbit. The payload can be 20% to 30% of the vehicle take-off weight. It is far more economical in the use of mass and energy than chemical propulsion, and it is far more flexible in putting identical vehicles into a variety of orbits.

CONTENTS

JASON LASER PROPULSION STUDY, SUMMER 1977

1. Background

Laser propulsion is an idea that may produce a revolution in space technology. A single laser facility on the ground can in theory launch single-stage vehicles into low or high earth orbit. The payload can be 20% or 30% of the vehicle take-off weight. The rocket propellant may be ordinary water. The system offers theoretically two decisive advantages over chemical propulsion. It is far more economical in the use of mass and energy, and it is far more flexible in putting identical vehicles into a variety of orbits.

If very high power (megawatt) infrared lasers are postulated, then laser propulsion changes from a theoretical dream into a practical possibility. An operational launch facility would require a battery of lasers with a total output of about 1000 MW. Such a facility could probably be built for about a billion dollars. An experimental feasibility study, using existing lasers to find out whether laser rocket engines can really be built so as to do what the theory predicts, would cost a million dollars a year or less. The major technical uncertainties could probably be resolved by such a study within 3 to 5 years.

ARPA has funded a preliminary feasibility study during the years 1975-77, using two contractors, AVCO Everett Research Laboratory (hereafter called AVCO) and Physical Sciences Inc. (PSI). JASON was asked to review the work of the contractors and make recommendations for future action. JASON members heard briefings from AVCO[1] and PSI[2] about their work for ARPA, and briefings from LOCKHEED[3] and PSI[4] about work they have done on laser propulsion for NASA. We read numerous documents,[5] including papers from Soviet journals.[6] We were greatly assisted by Lt. Colonel G. Canavan of ARPA in organizing the study and arranging briefings.

2. Brief Summary

(1) We recommend that ARPA fund a modest feasibility study of laser propulsion at a level of a million dollars a year or less, with continuity of funding for 3 or 5 years.

(2) The objective of the program should be to provide DoD with the option of developing an alternative to chemical propulsion for use during the late 1980's or later.

(3) It makes no sense to tie the laser propulsion study to any particular future mission, but the capabilities of laser propulsion would be well matched to the mission of deploying military space systems made invulnerable to enemy action by proliferation of vehicles. (For details see Section 8 below, and the Appendix by R. LeLevier).

(4) We recommend that both contractors be retained in the program until one or the other of the two design concepts is proved superior. (See Section 4.)

(5) The next phase of the program should concentrate on building small laser reaction engines and testing them with existing lasers (Section 4).

(6) The program should be based on infra-red rather than visible lasers (Section 5).

(7) The program should continue to focus on ground-launch rather than orbit-to-orbit systems (Section 7).

(8) ARPA should proceed with feasibility studies independently of NASA (Section 7).

3. Detailed Recommendations

(1) We recommend that ARPA continue to support laser-propulsion feasibility studies for 3 or 5 years. The program should concentrate on the design, construction and testing of small laser-rocket engines. Until it has been shown that an engine will work, further theoretical studies of missions or of atmospheric propagation are not required. We recommend that both contractors be funded for the next phase of the program so that they can each prepare a small reaction engine for testing. We cannot at

present predict which of the two engine concepts is likelier to succeed. If there is not enough money to fund both programs at the requested level, it would still be better to divide the money and let both programs continue at reduced speed rather than to make a premature choice. The time to choose between the two concepts is after they have both been subjected to endurance tests. Both contractors should be required to design model engines that can be scaled up to full operational size with an acceptable thrust-to-weight ratio, so that the model tests are relevant to operational systems. (See Section 4 below for details.)

(2) We recommend that the laser propulsion program continue to be based on infra-red lasers. One of the major technical uncertainties of the program is the ability of the optical control system to propagate an adequately focused beam through a turbulent atmosphere. The optical effects of atmospheric turbulence are far worse at visible than at 10-micron wavelengths. No matter how good the compensation of turbulence by adaptive optics may be, there will always be a range of weather conditions that a 10-micron system can handle but a visible laser system cannot. We do not recommend that studies of adaptive optics be funded under this program, since adaptive optical systems are already being developed for other purposes. (See Section 5.)

(3) The bad-weather capability of a laser launch system depends on the ability of a 10-micron beam to bore a clean hole through clouds or

rain. Experiments to study hole-boring,[8] important for other reasons, should be coordinated with the laser-propulsion program. (See Section 6.)

(4) The ARPA decision to concentrate the laser propulsion effort on ground-launch systems was correct and should be maintained. The prime objective should be a system launching directly from ground to high earth orbit. The NASA-funded studies of laser propulsion[3,4] deal only with orbit-to-orbit missions using low-thrust engines. These studies are unrealistic and irrelevant to ARPA objectives. In the long run we may hope that NASA and DoD will collaborate in the development of laser ground-launch systems. But for the next few years, NASA will be occupied with the deployment of the Space Shuttle, and ARPA should plan to pursue laser propulsion independently of NASA. (See Section 7.)

(5) Our final recommendation is that ARPA should not plan to make a final Go/No-Go decision on laser propulsion within any fixed period of time. The state of the art in laser technology, in adaptive optics and in rocketry will change as the years go by. Mission requirements will also change. The objective of the ARPA program should be to keep open the option of developing a laser propulsion system whenever DoD may have a need for it. It makes no sense to force an early decision whether or not the option is worth exploiting. Whatever we decide in the next two years, the possibilities of laser propulsion will not go away.

4. The Two Contractor Programs

On the whole, we are impressed by the technical competence and dedication of both contractor teams. We did not find any technical mistakes in their work. Both experimental programs failed to produce definitive results, but this was not the fault of the contractors. They did not have enough time or enough continuity of funding to carry through a well-controlled series of experiments.

The PSI team concentrated its effort narrowly upon one experimental program and was successful in producing superficially favorable results before the deadline imposed by ARPA. The AVCO group spread its effort over orbit studies, optical propagation studies and hydrodynamical analysis, and was forced to finish its experimental work in a hurry with company (IR&D) funds. Both groups came to us with experimental results which were obtained under pressure of time and need further elucidation. We do not consider the evident disarray of the AVCO experiments at the time of our inquiry as a reason for giving preference in funding to PSI. The high values of Specific Impulse reported by PSI, and the low values reported by AVCO, are both subject to change and reinterpretation in the light of further experiments. We judged the quality of the two teams not by the latest Specific Impulse numbers which they quoted but by their ability to understand what they observed. By this criterion the two teams seemed to us to be of equal merit.

The relative merit of the two contractor teams is not so important to the future of the program as the relative merit of the two design concepts which they are pursuing. We are not able to say, on the basis of the work done up to now, that one of the two concepts has a higher probability of succeeding than the other. Each of the two contractors has a design concept for a reaction engine which has a fair chance of working and can be tested with small-scale models. However, each of the designs has weak points which may cause it to fail.

The PSI design, using a parabolic metal structure to focus the incident radiation and channel the outgoing propellant, has two obvious weak points.

(1) The engine operates rather like a reciprocating internal combustion engine, with hot gas close to the metal surface during a substantial fraction of the duty cycle. If the engine is to deliver a specific impulse substantially higher than a chemical rocket, the gas temperature must be of the order of 10,000°K. It is not clear whether the metal structure can be cooled well enough to preserve both its structural strength and its optical reflecting quality. It is not clear whether the gas will remain sufficiently hot as it flows down the cooled nozzle.

(2) Even if the structure can be cooled, it is not clear whether the thing can be built light enough to give an adequate thrust-to-weight ratio. The parabolic surface has to be quite large (about 2 meters diameter), rugged enough to withstand strong impulsive hoop stresses, and

it has to carry the plumbing and pumps that are needed to keep it cool. The PSI team has not produced any detailed designs of structures from which one could make reliable estimates of the weight required.

These two weak points of the PSI design are not present, at least to the same degree, in the AVCO design. In the AVCO design the hot propellant gas is in contact with the vehicle for a much smaller fraction of the duty cycle. For example, if the contact lasts 100 microseconds for each pulse and the pulse-rate is 100 per second, the contact occupies only 1% of the cycle. The exposed surface is mainly a flat plate which does not need to withstand hoop stresses and does not need to be of optical quality.

The AVCO design, however, has its own weak points which are not present to the same extent in the PSI design.

(1) The system for feeding a precisely measured small quantity of propellant through the flat plate so as to form a thin uniform layer on the exposed surface has never been described in detail. If the feed is by transpiration through small channels, it is of crucial importance to study whether the channels stay open under repeated bombardment with laser pulses. Does the flow of propellant adequately cool the surface of the plate without additional plumbing? What is the effect of local inhomogeneities in the distribution of propellant? Will such inhomogeneities tend to grow with time in an unstable fashion?

(2) An alternative, which avoids the problems of transpiring fluid propellant through a plate, is to use a solid propellant of which a thin layer is ablated by each laser pulse. There are then serious problems concerning the uniformity of the ablation over the propellant surface, the possible spalling or mechanical failure of the propellant under repeated shock loading, and the possible bulk heating of the propellant leading to mechanical deterioration.

(3) Whether the propellant is fluid or solid, the AVCO flat-plate design implies that a laser pulse arriving off-center will give the vehicle a large impulsive torque. It is easy to estimate that a badly off-center laser beam will set the vehicle tumbling in one or two tenths of a second. The AVCO design therefore imposes very severe requirements on the accuracy of tracking and guidance of the beam. In principle, it should be possible to place a simple sensor of impulsive tilt on the vehicle and use its output in a feed-back loop to keep the beam centered. The AVCO group should define such a feed-back system in detail and examine whether it will operate stably and reliably under operational conditions. They should also examine whether the tumbling problem can be cured or alleviated by spinning the vehicle about its axis.

One of the main virtues of the PSI design is that the axis of the propellant thrust is insensitive to errors in the beam guidance.

It is our opinion that the weak points of either design may well turn out to be irremediable. For this reason we consider it most unwise at the present moment to eliminate one design and concentrate all efforts on the other. This is the basis of our recommendation (1). We consider it likely that many of the uncertainties could be resolved if both teams were able to build model engines and subject them to endurance tests with the high-repetition-rate pulsed laser (CCEBL) which will become available in the next two years. Such endurance tests would help to decide whether the first weak point of PSI or the first two weak points of AVCO can be overcome. The remaining weak points of each design cannot be resolved by model experiments, but require a thorough engineering analysis. For the PSI design, we require a complete structural analysis of a full-scale engine and the experimental model should be similar enough so that scaling laws can be applied. For the AVCO design, we require a complete specification and analysis of the tracking and guidance system to resolve the tumbling problem.

The pulse-repetition rate of the CCEBL laser is in the right range for realistic tests of the AVCO design, but they are a factor of 100 too slow for the PSI design. Tests of the PSI design with this laser will not correctly simulate the gas flow through the nozzle. For a fully realistic test of the PSI design it would be necessary to find a laser delivering about 100 Joules per pulse at a repetition-rate in the 10 KHz range. ARPA and PSI should explore the possibility of modifying an existing laser so that it can operate in this range.

The programs proposed by both the contractors for the next phase of their work seem to us reasonable, if money is available. If not enough money is available to fund these programs, then the time-table for both of them should be stretched out equally. We find that the rivalry between the two teams, following different ideas with roughly equal support, has a salutary effect on the quality of their work. But ARPA should be careful to insist that the competition between them remains a competition in quality and not a competition in haste. No prizes should be given for quick and inconclusive experiments.

We suggest that after two or three years, when model endurance tests of both concepts have been completed and analyzed, the whole laser-propulsion program should be reassessed. At that time it may be appropriate to choose one concept for further development and to attempt to fix the parameters of a feasible operational system.

5. Effects of Atmospheric Turbulence

In any laser-propulsion system the heating of the atmosphere by the laser beam produces a defocusing of the beam (thermal blooming) which must be compensated by adaptive optics. The compensation is feasible provided that the beam diameter D (in meters) and the average beam power P (in megawatts) satisfy a condition[7]:

$$D > aP^{1/3}$$

where a is a number of order 1. The precise value of a depends on wind conditions and on the laser wavelength. But the dependence on wavelength is slight. Roughly speaking, we can say that a beam diameter of 20 meters allows adaptive optical control at a power-level of 1 gigawatt, independent of wavelength. The adaptive optics needs to operate with a response-time of the order of 0.1 second. It seems clear that adaptive optical control of thermal blooming can be achieved. So far as thermal blooming is concerned, there is no particular advantage in using 10-micron lasers rather than visible lasers.

The situation is very different when we consider adaptive optical compensation of effects of atmospheric turbulence. Here the 10-micron laser has a decisive advantage. The essential point is that the angular defocusing of the beam produced by turbulence is roughly independent of wavelength. In fact the angle decreases slowly as the wavelength increases, but the rate of decrease is poorly measured and depends upon the detailed structure of the turbulence. Neglecting the decrease of angle with wavelength (which anyway makes compensation easier at longer wavelengths) we may assume as an example that on an average day, the atmosphere produces a defocusing of the beam by 2 seconds of arc at all wavelengths. Since 2 seconds of arc is 10^{-5} radians, this degree of defocusing means that rays leaving the tracking mirror remain in phase only over a patch of size about 10^5 wavelengths. The size of this "isoplanatic patch" determines the size of the mirror elements that must be independently adjusted by the adaptive optical system. If we have a beam of diameter 20 meters, we have for a 10-micron laser.

Patch size $d = 1$ meter,

Number of elements $N = (D/d)^2 = 400$.

For a visible laser with wavelength ½ micron, we have

$d = 5$ cm , $N = 160,000$.

Furthermore, the speed of response of the optics must also be much faster for the visible laser. For example, if the wind velocity in the turbulent layer is $v = 50$ m/sec, then the system response time must be $t = (d/v) = 2 \cdot 10^{-2}$ second for the 10-micron laser, $t = 10^{-3}$ second for the visible laser.

Clearly, if a visible laser is used, the compensation of atmospheric turbulence under 2-second-of-arc seeing conditions (which are not particularly bad or uncommon) stretches the limits of possibility for adaptive optical systems. Anything much worse than 2-second-of-arc seeing would defeat the system completely.

In assessing the effects of atmospheric turbulence on system performance, it is important to remember that turbulence tends to be highly sporadic and patchy. This fact implies that the adaptive optics in a laser propulsion system must satisfy much more severe requirements than the adaptive optics in a telescope system. In a telescope system, we can tolerate a loss of focus lasting a minute or two whenever a particularly bad patch of air passes by. In a laser propulsion system, sporadic loss of focus of the beam during a launch might be disastrous. The adaptive optics has to work reliably for five minutes, or else it is no good.

We conclude from the foregoing discussion that a laser propulsion system using visible lasers is basically unreliable. At best it could be available only under conditions of good seeing. Certainly the problem is enormously easier at 10 microns. It seems likely that under normal weather conditions at a mountain-top site the effects of atmospheric turbulence on a 10-micron laser system can be compensated. Before any launch-site is considered for an operational system, we shall need systematic measurements of the local atmospheric seeing at 10-micron wavelength, to see how frequently conditions may arise that the adaptive optics cannot handle. We particularly need information concerning the occurrence of "bad patches" which do not receive attention when data are aggregated and averaged. But the collection of seeing data will be necessary only at a later stage of the program, and we are not recommending that ARPA devote funds to this purpose during the next two years.

6. Hole-Boring Studies

We discussed with members of the AVCO team the work which they have done and are proposing to do[8] on the vaporization of cloud and rain droplets by 10-micron laser radiation. They have facilities for studying the vaporization process in detail in the laboratory. At present they have no funding for further experiments. If they or some other contractors receive support for continuation of this work, the objective will be to find out whether a 10-micron laser beam could bore through clouds and make a channel with good enough optical quality for a subsequent visible laser beam to penetrate. The visible laser beam may be degraded by vapor patches from vaporized droplets which do not seriously affect the 10-micron beam. For this and other reasons, the objective of boring a hole for a visible laser beam seems to be more difficult than the objective of boring a hole for a 10-micron beam. If the hole-boring for a visible beam turns out to be feasible, then it is likely that the same technique would be successful in allowing a 10-micron laser-propulsion beam to penetrate cloud.

The experiments and theoretical analyses that have been published leave a very wide margin of uncertainty concerning the flux-levels required for hole-boring. The most optimistic estimates say that the laser beam need only deliver energy comparable with the energy of vaporization of the droplets. If these estimates are close to the truth, a laser-propulsion beam will have little difficulty in boring a hole for itself under most weather conditions. If the most pessimistic estimates are correct, a beam intense enough to evaporate droplets would heat the air so strongly as to defocus itself by thermal blooming, and hole-boring would be practically impossible.

We do not recommend that a research program on droplet evaporation be funded as part of the laser propulsion program. We only suggest that, if the evaporation research is supported for other reasons, the hole-boring and propulsion programs be coordinated so that the evaporation experiments provide as much information as possible relevant to the laser-propulsion objective.

7. Interaction with NASA

We listened to presentations by LOCKHEED[3] and PSI[4] of work they have done for NASA studying low-thrust laser-propulsion concepts which might be applied to orbit-to-orbit missions. The PSI study dealt with the physics of a small engine, using hydrogen as propellant and a 10-micron CW laser as power-source. The LOCKHEED study dealt with the overall systems analysis of an orbit-to-orbit transportation system, powered either by orbiting or ground-based CW lasers.

Both these studies left us with an impression of unreality. The PSI investigators concluded forthrightly that the engine configurations specified by NASA had little chance of working satisfactorily. The LOCKHEED investigators concluded that the system specified by NASA would be cost-effective, if we could assume the perfect functioning of optical components of a size and precision going far beyond the present state of the art. It was obvious to us, and to at least some of the investigators, that NASA has no serious interest in bridging the gap between these theoretical studies and the realities of NASA operations.

The basic ground-rule of the NASA study contracts is that laser propulsion must be used as an adjunct to the Space Shuttle and not as a replacement for the Shuttle. This means that laser propulsion must be used to move from a low earth orbit (LEO) to a high earth orbit (HEO) but must not be used for launch from the ground. Now it happens that, for two simple reasons, LEO is a peculiarly awkward place at which to apply laser propulsion. First, an object in LEO is within the line-of-sight from any ground-based laser for a very short time. Second, an object in LEO is moving very fast relative to any laser that is not in precisely the same orbit. These two facts have the consequence that a laser propulsion system applied to an object in LEO must work at extremely long ranges (of the order of 10,000 km) and must either use a system of optical relays to bring the beam from a single laser to the right place at the right time, or else must use many lasers distributed in various orbits in space. For the system to work with visible lasers, optical reflectors of the order of 10 meters diameter must maintain their shape within a tolerance of a few parts in 10^8. If 10-micron lasers are used, the mirror diameters become of the order of 100 meters.

Compared with these extreme requirements imposed by the geometry of LEO operations on the optical components of a LEO laser propulsion system, the optical requirements of a ground-launch system almost seem easy to satisfy. The ground-launch system needs only a single large mirror, working at a maximum range of 1000 km. At 10-micron wavelength a mirror of 20 meters diameter with optical tolerances of a few parts in 10^7 will be adequate. There is one major factor which favors LEO over ground-launch propulsion. A ground-launch system needs high thrust and a laser power in the gigawatt range, whereas LEO propulsion uses low thrust and megawatt-range lasers. But when the systems as a whole are compared, it seems likely that it may be cheaper and easier to build a gigawatt battery of 10-micron lasers on the ground than to generate and transport megawatt beams around the earth as required for the LEO systems.

Both types of laser propulsion systems, ground-launch and LEO, require a high rate of traffic in order to be cost-effective. We suspect that, if the rate of traffic becomes high enough to make either system economical, it may well turn out that it is cheaper to launch a payload from the ground directly into HEO than to move the same payload from LEO to HEO. This is a possibility which the NASA studies refuse to contemplate.

We conclude from our examination of the NASA studies that the ARPA policy of concentrating attention on ground-launch systems is correct. If laser-propulsion has any military importance, it can only be as a cheap rapid-fire method of launching large numbers of payloads from the ground into a variety of orbits. So long as NASA is not interested in ground-launch systems, no useful collaboration between ARPA and NASA in this area will be possible.

After the Space Shuttle deployment is completed, it is possible that NASA will be willing to give serious attention to ground-launch laser propulsion as a follow-on system. There are many NASA missions, especially the small unmanned scientific and interplanetary missions, which are ill-served by the Space Shuttle. In the long run a ground-launch laser propulsion system might be the most cost-effective for such missions, although the volume of traffic which they represent would not by itself be large enough to justify the laser system. It is also possible that NASA will become involved in large-scale projects of space industrialization, solar power stations in orbit, or mining of minerals from the moon and the asteroids. Any big industrial projects in space would require launching from ground into HEO of large quantities of construction materials. A ground-launch laser-propulsion system might make a decisive contribution to the economic feasibility of such projects. If activities of this kind ever become part of the NASA program, joint planning and collaboration between NASA and DoD will be essential.

8. Missions for a Laser-Propulsion Launch System

If laser propulsion is feasible, it offers an alternative to chemical boosters and the Space Shuttle for launching spacecraft from the ground into orbit. The advantages of the system are (1) easy accessibility of a variety of orbits including geosynchronous orbits, and (2) very low cost per launch provided that the number of launches is large. The disadvantages are (1) high capital cost of the launcher, and (2) the payload that can be carried by each launch is limited. The payload limit is about 1 ton for the "nominal system" using a 1-gigawatt laser beam.

It is clear that DoD has no immediate requirement for a launch system of this character. The system could in any case hardly be available before the late 1980's.

In recent years the constant tendency of space operations, both civilian and military, has been towards larger, more complicated, more reliable, and more expensive payloads. The cost per pound of payload has been increasing while the cost per pound of launching things into orbit has been decreasing. Payload costs now often exceed launch costs, even for the geosynchronous orbits which are the most costly to reach. The Space Shuttle will bring launch costs down further in the next 5 years. So long as payload costs dominate launch costs, there is no requirement for a system providing cheap launches at high traffic-rates.

There are some military missions which require unit payloads of many tons. For these the Space Shuttle or other chemical launch systems may continue to be used. Ultimately, there is no reason why a laser launch system need be limited to one gigawatt of power. The "nominal system" using one gigawatt of power and launching a one-ton payload was chosen because it is the smallest and cheapest system that appears to be technically feasible. Larger systems could be built. They would still be more flexible than competing systems, and cheaper when the volume of traffic is high.

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5. For example, R. E. Lo, "Propulsion by Laser Energy Transmission," Report IAF-76-165, published by the International Academy of Astronautics, Paris, France, October 1976.

6. F. V. Bunkin and A. M. Prokhorov, "Use of a Laser Energy Source in Producing a Reactive Thrust," Soviet Phys. Uspekhi, 19, 561 (1977), provide a useful survey of Soviet work in this area, with a good bibliography of western and Soviet publications.

7. The rate of heating of air in the beam is proportional to P/D^2. The residence-time of air in the beam is proportional to D. Therefore the change of optical path-length along the beam produced by the heating is proportional to P/D. This change of optical path-length must not exceed the depth $(D^2/4H)$ of a concave mirror with diameter D and focal length H, where H is the scale-height of the atmosphere. Thus (P/D^3) must be less than a bound depending on the properties of the atmosphere. This rough argument is confirmed by the detailed calculations of beam propagation made at Lincoln Laboratories and reported by AVCO (Ref. 1).

8. Avco Everett Research Laboratory, Inc. "Proposal for CO_2 Fog Clearing for Visible Laser Propagation," Draft Proposal, July 1977.

APPENDIX

COMMENTS ON THE LASER PROPULSION STUDY

We would like to make two comments on the impact of a "cheap" launch system on the military. The first is rather obvious -- survivability through proliferation. The second, not unrelated to the first, is the possible relaxing of military specifications. Let us use communications satellites as an example.

Current and planned DoD communications satellites such as FLTSATCOM, DCSS II and the follow-on DCSS III represent single, high value, complex structures. The table gives the weights of some of these satellites:

FLTSATCOM	1950 lbs
DCSS II	1240 lbs
DCSS III	2000 lbs
INTELSAT IV	3100 lbs

Consider a large number (≥100) of small (-100 lbs) proliferated and netted, low earth orbit communications satellites. Using ARPA packet switching technology such a system could connect any two (or more) points in the world and, by virtue of the large numbers involved, would be very resistive to attack and if attacked, would degrade quite gracefully. Noting that the area of the world is 5×10^8 km^2, the satellite net would have about 1,000 km separation between neighbors and, hence, would require one-on-one attack. This system, with a total weight of 5 to 10 tons, would require multiple launches because of possible volume constraints. Thus, a cheap launch system is highly desirable.

However, a second virtue of a cheap launch system is that, unlike current systems which must be extremely reliable and long lived, the small proliferated platforms would not require such a high degree of reliability. Therefore, in principle, they should be cheaper to construct. If one of them dies, you simply talk around him. After enough die, you simply replace them with newer, more modern versions which incorporate any new advances in technology.

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