# Small Solar System Body Mitigation: A Realist's Approach
# - Appendix -

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## INTRODUCTION

On the request of a number of participants of the PDC'09, to whomever it may concern, this appendix to the conference contribution entitled 'Small Solar System Body Mitigation: A Realist's Approach' provides the translation of the conclusions summary of the author's Diploma Thesis [48] and Second Student Project [47] prepared as required for the successful completion of the Aerospace Engineering University Diploma Course at the RWTH Technical University of Aachen. In the local system of courses, a diploma thesis was at the time to be completed within six months; a student project was at the time not strictly limited in duration, but expected to take several weeks to a few months to complete.
Ref. [48] was the first iteration of the content presented in this conference contribution, abstract # 1548763, and poster P.206. The diploma thesis "Betrachtung des Missionsszenarios zur Verhinderung von Einschlägen von Asteroiden auf die Erde unter Berücksichtigung des Bedrohungspotentials und der technischen Möglichkeiten" (Contemplation of the Mission Scenario for the Prevention of Impacts of Asteroids on the Earth with respect to the Threat Potential and Technical Capabilities) was written under the supervision of Univ.-Prof. Dr.-Ing. W. Koschel at the Research Department for Operational Properties of Jet Propulsion of the Institute of Jet Propulsion and Turbomachinery of the RWTH Technical University of Aachen, and submitted on September 29th, 2006. Additional tables and a few clarifications were introduced in a slightly revised version ported to pdf format by August 29th, 2007, to which an Appendix B with updates and related information was provided.
Ref. [47] was the second of two mandatory extended projects required as part of all mechanical engineering courses at the RWTH. The student project "Zuverlässigkeit von Raumflugkörpern" (Reliability of Spacecraft) was written under the supervision of Prof. Dr. D. Jacob and Dr.-Ing. G. Neuwerth at the Institute of Aeronautics and Astronautics of the RWTH, and submitted on November 30th, 2005. A few clarifications were introduced in a slightly revised version ported to pdf format by March 4th, 2007, to which an Appendix B with updates and related information was provided.
The translations are intentionally kept as close as possible to the original wording and structure in German. Emphasis in the original is represented here as well in *italics*. Clarifications standing in for more detailed content of the full text, later updates, additions to the original content developed as a result of the PDC'09, and comments introduced for this translation are marked in dark blue.

## TRANSLATIONS OF REF. [48]

### Brief

The natural impacts of small bodies of the solar system can cause disasters that are relevant to human civilization due to the combination of their frequency and scale. The effects of these disasters increase with the spread of human settlement on Earth. Astronomical surveys showed a hitherto unexpected and rising number of potential impactors.
New technologies, public interest stipulated by the media, and competition on research policies lead to many different and often very expensive concepts to mitigate the threat. Usually, their high cost inhibits the realization of such projects, resulting in a nearly complete lack of practical experience. Scientific investigation is restricted to well known bodies or those on precisely known orbits within easy reach, which in this respect are unlikely properties of an impactor. Experience regarding controversial technologies teaches that investments will only be made when an inevitable necessity is publicly received, except for volunteered contributions by interested individuals. The inclination to invest in this sector withered with broad public interest before science could agree on a comprehensive strategy to mitigate the threat.

This poses the question whether the defence against a threatening object is possible entirely without dedicated efforts. If this is not the case, the question is, in which areas additional efforts are indispensable, on which scale, and whether they can be combined synergistically with existing institutions.
Among others, the following topics shall be addressed:

- Literature research on the threat of impacting asteroids
- Synopsis of the small bodies of the solar system and their classes
- Analysis of presently available spaceflight systems and their actual availability
- Computation and discussion of mission scenarios
- Limits of the technological capabilities
- Cost-efficiency analysis
- Selection of cost-effective methods
- Documentation of the results

**Outline**

The initial topic of this work was first set to answer frequently asked questions resulting from notorious movie science related to asteroid hazards mitigation. The subset of the spacecraft reliability complex later to be addressed in [47] was identified as being more urgent due to its relevance for commercial spaceflight, and consequently spun off as a preceding project. The initial primary objective set before this decision was to demonstrate that there was no need for government spending related to asteroids and the NEO hazard in general, at all.
The rephrased brief then made non-existence of prior spending the set condition for the mitigation scenario, instead. To identify the lacking parts of the end-to-end scenario from discovery of an impactor to its successful deflection, the flow of required activities was first traced forward phase by phase, and then backwards for the required timescales, deflection performances, and accuracies. Some additional emphasis was put on the emergence and historical development of threat estimates and the understanding of impact-related effects on the ground. The objective was not to compute exact effects and efficiencies of mitigation methods or deflection mission scenarios focused on one object or a representative set of specific objects. (Such calculations were done independently at the same time in great detail for [1].) Instead, the focus was set on a general comprehensive study of all aspects required for the successful prevention of impacts on the Earth without prior spending and without standing defences, integrated as a general scenario. Initially, analysis was to be based on the state of the reference literature that became available after the Asteroids, Comets, Meteors 2002 conference in Berlin, and the Meteoritical Society and Large Impact Crater meetings in Münster and Nördlingen the following year. Due to the precedence taken by [47] and its unexpected results and very extended duration, it became possible to take the results of later Planetary Defense Workshops into account, as well; e.g. [24,38].

**Conclusions Summary**

*The data and case reports presented show with virtual certainty:*

**1.)** It is urgently required to improve the characterization of the properties of Near Earth Objects (NEOs) and their populations with regard to the preparation for objects eventually to be deflected. In particular, knowledge on the relation of absolute brightness, H, and the object's mass has to be improved vastly, for example by spectroscopic observations that are directly linked to discovery and follow-up observations. Also, the relationship of these properties to all damaging effects of impacts has to be established. It is possible that some effects are still unknown, and some are currently underestimated or unclear in terms of yield efficiency, effects propagation, or energy deposition. Examples are a number of features of Tunguska-like airbursts and impact-generated tsunamis.
**2.)** The orbital data of objects recognized as possible Potentially Hazardous Objects (PHO, here not limited to the current PHA definition of an absolute magnitude $H \leq 22$ mag and an Earth minimum orbit intersection distance (Emoid) $\leq 0.05$ AU) larger than about 20 m in diameter, and in particular of objects larger than about 60 m in diameter passing by very closely, should be refined during the night of discovery by means of automatic delay-Doppler-radar observations, to avoid their loss and to ensure early warning for the thereby begun synodic cycle. (A synodic cycle encompasses the interval between synodic encounters of the same character, and is a multiple of the synodic period. Small objects may be invisible at most synodic encounters within one cycle.) [a]
**3.)** This requires the installation of a coordinated network of at least four radar telescopes that are quickly and fully steerable. Half of them should be situated in the southern hemisphere, alternately placed with those on the northern hemisphere at an offset of 90° or 360°/n of longitude. The detection and astrometrical performance of these radar telescopes should at least approach the performance of the asteroid radar at Arecibo. These radar telescopes should be coordinated synchronously worldwide, amongst themselves as well as with the (most efficient) optical installations nearby. This requires especially in the southern hemisphere automatic survey installations that are in performance equal to those in the northern hemisphere. On both hemispheres, the number of optical survey sites should exceed the number of radar installations by a factor that compensates for weather-related restrictions of observation. The optical surveys should all operate according to the most efficient method established so far; see next paragraph below. Outside the local optical observation periods, and beyond immediate post-discovery tracking requests resulting from local and other

optical observations, radar follow-up of previous discoveries on the longest possible orbital arcs should take precedence over other radar- or radio-astronomical observations.

**4.)** The existing NEO survey and follow-up programmes have to be intensified, to discover within an interval shorter than the time between Mt-scale Tunguska-like events as many as possible of these objects which pose an elevated risk because of the nature of the airbursts they cause on impact. Survey equipment and data processing should be improved sufficiently to reliably close the Blind Zone [b] and enable the detection of Mt-scale Tunguska-like class objects at effective distances with respect to existing deflection or civil defence capabilities. With regard also to the high resistance of nickel-iron small bodies to influences of the Earth's atmosphere, this requires at least an extension of the surveys down to H = 26 mag or an average expected diameter of approximately Ø = 20 m. Survey sites equipped with telescopes equivalent in sensitivity and resolution to a diffraction-limited main mirror diameter of 3.5 m, of the same number per site and operated in the same manner as LINEAR, should suffice to achieve this goal. Extending the search to even smaller objects requires correspondingly larger telescopes in an otherwise identical arrangement. A design goal of H = 28 mag or Ø = 10 m is recommended, also to achieve a reasonable safety margin.

**5.)** Design documentation related to flown products that could be relevant for interplanetary missions must not be destroyed. All documents which are required to maintain the capability to copy-build relevant products once test-flown should be preserved for an indefinite time.

**5a.)** A pool of practically trained proficient experts in interplanetary spaceflight has to be maintained. Interplanetary spaceflight in this context includes the ground and launch segments, and a flourishing planetary science environment. The required status of expertise can only be achieved if several complete missions can be experienced hands on by one person during a lifetime career, including at least a few witnessed 'from cradle to grave' in conjunction with all related political and management decisionmaking processes. Friendly and cooperative but determined competition in interplanetary spaceflight is the best way to create an environment that is able to sustain said expertise while providing synergetic benefits to science and engineering in general. Flight experience should be passed on and shared in the most open manner, while with great care any type of 'blame culture' is avoided. (cf. [47])

**6.)** All these measures are cost-effective. The total effort required is far less than the cost of the damage caused by a single Tunguska-like [c] event over inhabited territory, and still considerably less than that caused over uninhabited territory. The total effort required is negligible compared to the consequences of an event of a similar type but considerably smaller than Tunguska [d] over densely inhabited countryside or a metropolitan area.

*The data and case reports presented show clearly:*

**1.)** Knowledge of small bodies on orbits of the Sun partly and fully Interior to the Earth's Orbit (IEO) is incomplete. There is an unused capability for transit observation at solar observatories. Such observations could reach to similar distances as asteroid radar observations, but with a search beam at least an order of magnitude wider that could enable small body transit surveys and triangulation using high frame rate high resolution cameras at several sites in parallel to normal solar observations.

**2.)** Another unused observation capability resides in the camera systems of interplanetary probes, especially those travelling inside the Earth's orbit. From their vantage point, NEOs and IEOs outside the probe's orbit are observable much more easily than from Earth, in opposition. The synergetic use of satellite and interplanetary probe cameras in general is promising, also outside the Earth's orbit, from within a planet's shadow, and during other idle times. Local opposition observations and on-board autonomous surveys can serve to improve population models. This option exists for and includes all suitable existing as well as specialized Earth-orbiting satellites.

**3.)** Real practical testing of all deflection methods related to small solar system bodies is at least recommended, and in most cases urgently required. There are mostly large uncertainties concerning the high-energy methods discussed, kinetic impact and nuclear radiative ablation, regarding their precise efficiency and possible unwelcome side effects. For the low-energy methods discussed, thruster landers and solar concentrator evaporation, more substantiated knowledge on the interceptor-target interaction and the actual efficiency is required.

**4.)** There is a need for heavy launch vehicles with upper stages that can achieve very high Earth-escape velocities with payloads that are very small for this class of vehicles. The capability to stack cryogenic upper stages within or below the fairing appears an efficient solution to further increase Earth-escape velocities, and is therefore desirable. Such vehicles would also be useful for purely scientific missions.

*The following appears likely according to the data and case reports presented:*

**1.)** Small and medium-size PHOs can be deflected using existing technology and commercial infrastructures at current flight cadence.

**2.)** The deflection of larger objects requires longer preparation time, or the fundamental readiness to quickly use nuclear explosive deflection payloads and their *previous, comprehensive and complete practical testing independent of a specific threat or deflection mission*.

**2a.)** At the very upper end of deflection decision lead times to be expected at the given precision of astrometry for high probability positive impact predictions of large objects, it may occasionally be possible to split the deflection campaign into two volleys, a precursor to test nuclear explosive deflection efficiency, and an accordingly scaled main

deflection campaign employing a minimum number of nuclear devices. However, the deflection effect required will be higher in the main campaign than if all deflection payloads were launched at the time of the precursor campaign, due to the shorter time remaining in which the deflected object can accumulate miss distance by drifting off collision course. This may more than offset the reduction of the number of nuclear devices achieved by better characterization of their effects on the specific object.
**2b.)** The longer lead times required by low energy methods other than those discussed, or by much larger objects, can not be achieved at the level of precision required for deflection decisions by current astrometry. The required combination of lead time and precision may become achievable for a considerable part of the threatening bodies if they are observed over very long timescales or by other methods that enable the determination of non-gravitational effects on the orbit, including spacecraft rendezvous, landers, and sample-return.
**2c.)** Interceptor spacecraft applying low energy methods other than those previously discussed within the current and announced capabilities of launch vehicles are usually not sufficient to carry a deflection campaign alone. They may however prove useful as shepherding verniers, to guide an already deflected object past secondary orbital keyholes during later phases of the campaign and even after the date of the averted impact. With ample warning time, the very best of current orbit determination, and early decisive action given, such low-energy deflection spacecraft may occasionally be able to deflect an object on their own if their effects are amplified by a later very strong planetary flyby of the object to be deflected. (An example would be the deflection of Apophis using gravity tractors of the size of current interplanetary spacecraft that would have to arrive approximately a decade before its 2029 close encounter with Earth which would then amplify the accumulated weak deflection; cf. [52,53,54]) It may also be possible to accelerate the secular evolution of the orbit of an object to push it over a zero-Emoid period in the distant future; cf. [13].
**3.)** The use of nuclear explosive deflection payloads can be reduced within limits through the practical testing of other methods. The most promising approaches are solar-thermal methods because they do not have to carry the deflection energy along, and may use commercially available carrier spacecraft.

*The data and case reports presented suggest:*

**1.)** The true risk posed by impacts has still not arrived in the public mind. In large parts of the public, the impression has been created that either the most important and dangerous problems are by and large solved after the discovery of 90 % of the 1-km-NEAs or the achievement of other similar goals, or that deflection is technically impossible anyway.
**2.)** As of mid-2006, the population of NEOs, and of PHOs in particular, still seems to be significantly larger than previously conjectured or estimated. An important factor is the addition of new groups of objects, such as IEOs, co-orbital NEOs, high-inclination NEOs, etc. An increase of the estimated population within any class of sizes by almost an order of magnitude, as within the past years and decades, is very unlikely in general, although it may still be possible for some of these newly recognized groups of objects due to the very low number of objects observed so far and the uncertainty of observational biases.
**3.)** Solar-electric propulsion has great potential as cruise propulsion in the inner solar system, especially for inclination change and transfer orbits that are closing in on the Sun at first. This applies in particular if, for the most efficient use of on-board electrical power, thrust and specific impulse are variable. High thrust, low specific impulse operation can then support deep space manoeuvres and planetary flybys, while operation in a low thrust, high specific impulse mode is used for continuous thrusting and deflection after landing on a small body.

## TRANSLATIONS OF REF. [47]

### Brief

The planning and design of satellite constellations to establish communication, navigation, and Earth observation systems in Earth orbit, as well as of fleets of interplanetary probes for the continuous scientific investigation of objects of the solar system over several synodic oppositions, requires the consideration of expected failure rates in various mission phases with respect to the completion of mission goals and cost optimization. The available data are, for historical, political, and legal reasons, very heterogeneous and patchy in detail and scope. Consequently, the need for spare vehicles and the growth of experience in serialized production are not satisfactorily documented, as well as the mission risk of vehicle groups which address various tasks. Also, the recognition of elementary clusters of certain problems by statistical methods is made very difficult by the lack of a consistent database, as well as predictions of the future markets in spaceflight.
In the frame of this student project, to create an improved foundation for planning, it shall be investigated whether sufficiently detailed data can be extracted from the available published documentation.
Among others, the following topics shall be addressed:

- An as extensive as possible literature research within the given time frame
- Selection and classification of the resulting data according to scope, degree of detail, professional quality, and reliability of the source
- Determination of statistically discernible modes of failure and their development over time
- Comparison with similarly demanding parts of aviation technology
- Consideration of cost development of spaceflight systems

- Extensive documentation of the tasks addressed

**Outline**

The initial primary objective set for this work was to give a mathematical function to determine a likely failure factor for commercial satellite constellations akin to Iridium or Teledesic. The author also expected to use this failure factor in [48] for the flotilla of interceptors that was at the time thought necessary to deflect large km-scale PHOs. Other important initial objectives set were to demonstrate beyond doubt: the expected significantly lower reliability of crewed spacecraft, their expected out-of-proportion cost, and a firm link between both of the former; the expected higher reliability and suitability of robotic spacecraft in particular on missions presently assigned to crewed systems; and the expected reliability advantage of purely commercial developments over governmental and in particular military or military-heritage systems. After two iterations attempting to extract statistical data from the raw data collected, it was realized that due to the unique nature of every single flight test imposed by minor modifications of even the most prolific serialized vehicles, it is rigorously impossible to give statistical answers because non-identical systems are always part of the set. Also, it became apparent through the data collected that none of the initial objectives would be demonstrated, despite a very substantial extension of the project duration, scope, and depth of analysis. Instead of dropping the project as suggested, the author decided to apply to the data a method gleaned from Lewis [6] and Chladni [49,50] and the reporting of the meteorite fall of L'Aigle [51] that underscores the immeasurable value of unedited direct eyewitness and hands-on-witness reports that was congenially demonstrated by the report on the Carancas event given at this conference which showed the value of minute, and sometimes at the first look amusing or irrelevant detail convolved in individual oral histories of unexpected events. [30]
In the third and final iteration, 106944 failure events of 39675 flight experiments were extracted from available literature in the form of summary and per-flight reports. These were complied in 5602 failure event data sets, from which 1521975 information units suitable for automatic processing were derived. 1077 sorted result extracts were compiled, in addition to 16 selective extracts for important families of spacecraft. Available information allowed 2998 failure events to be fully classified in all relevant aspects. This set was analyzed for typical failure patterns. The patterns recognized were backed up by case analysis of well reported failures, grouped into similar causes of failure and mitigation. Pattern distributions within the incompletely classifiable failure events were consistent with the fully classifiable subset. By the quality of the information provided on the course of specific failures in spaceflight, emphasis was put on historical literature and works critical of spaceflight and crewed spaceflight. Still, none of the initial objectives could be demonstrated even for selected subsets of the data available after three years. Following the across the board unexpected results, it was suggested to rephrase the topic of [48] and address it within a different institutional framework.

**Conclusions Summary**

*The data and case reports presented show with virtual certainty:*

**1.)** Experience founded on practice is the indispensable prerequisite for the achievement of high reliability in spacecraft. [e] It can not be gained in studies and simulations; in testing on a component level and by the testing of incomplete vehicles, it can only be gained within strong limits. The cancellation of projects and the disruption of lines of development completely devaluate the overwhelming part of the experience gained so far immediately or on very short timescales compared to the time it took to accumulate this experience. The transplantation of experience gained into the context of completely new developments or strongly constrained replacement projects is always of low efficiency, mostly difficult, and frequently impossible.
**2.)** There exists for every phase or era of the development of spaceflight an upper limit of the complexity of a spacecraft; increasing complexity beyond this limit results in a dramatic reduction of reliability. This limit can be exceeded by robotic spacecraft only within tight limits, and if at all, only in an effort of practical testing which is out of proportion in terms of expenses and time required. This explicitly includes the number, variety, and complexity of the payloads of a vehicle. From all these aspects follows an out-of-proportion increase of project cost.
**3.)** The limit of complexity can only be expanded by the continued build-up and maintenance of the experience gained, within the project environment and spaceflight in general. The most favourable foundation to achieve this is a busy flight schedule that is directly connected with the preparation of further missions and the analysis of previous flights. It helps to design vehicles and components for an as universal use as possible, to develop them in an incremental manner, to test them comprehensively and in a practical manner on the ground and in real use, and to use them as frequently as possible in flight, even though this may sometimes involve additional expenses. Simulations designed as closely as possible to real flight conditions can be useful in terms of preparation, but are correspondingly expensive.
**4.)** The variety and depth of the options to mitigate failures [f] during the mission, and the high flexibility of the methods used, and in particular the density and scope of the flow of information on the operational conditions aboard, are crucial for the reliability of spacecraft. The efficiency of failure mitigation increases with the amount of information available, and with the directness of the available modes of intervention. Automatic modes of intervention designed into spacecraft based on expected failures contribute at relatively low efficiency to the mitigation of failure events. [g] Compared to automatic modes of intervention, the contribution to the mitigation of expected and unexpected failures by

ground control interventions is four times more efficient; the contribution by the integration of the complete experience gained into follow-up missions is six times more efficient, and the contribution by an on-board crew is twenty to fifty times more efficient.

**5.)** When conducted at the same scales and for the same purpose and with comparable mission goals, crewed spaceflight is always more cost-efficient than robotic missions. This is the result of the very high failure mitigation capability of the crew on board which allows to raise the practically sustainable limit of complexity far beyond that which can be achieved for even well tested robotic spacecraft, despite the lower frequency of crewed flights required. Only by this offsetting of the complexity by the increased failure mitigation capability, it becomes possible to bundle a large number of experiments in a single mission with a high net mass of experiments and low specific cost.

**6.)** The scientific benefit and yield of scientific missions depends overwhelmingly on the readiness not to cut back on the operational, post-processing, technology transfer, and flight experience propagation budgets which are relatively small when compared to the initial investment required to achieve the capability of conducting such a mission. Especially if the option is available to use spare vehicles or components for follow-up or similar missions, the experience gained and infrastructure established at the expense of the initial investment and previous operational costs can be used without friction losses to yield quality results, which quickly and dramatically increases cost efficiency.

*The data and case reports presented show clearly:*

**1.)** A positive effect on the reliability of spacecraft, of purely organizational schemes and management methods which are one level removed from the product is not documented in the available literature analyzed. Specifically, no contribution of purely organizational schemes and management methods that led to the prevention or mitigation of a single failure event was documented. In all failure event cases that were documented in sufficient detail, all dedicated or institutionalized measures of quality assurance that were not exclusively limited to the product level and production level were completely ineffective in terms of the prevention or mitigation of failure events. Exact documentation of the material, temporal, and manufacturing provenience of parts directly on the respective product level can help to accelerate the post-mortem investigation of failure events.

**2.)** A tested and effective way of project management with the intention to quickly and cost-effectively create reliable spacecraft is to concentrate planning, design, manufacturing, testing, and operation in one site, with an as small as possible project work force, and with completely unrestricted flow of information within it. From this project work force, all activities on a level removed from that of the product itself have to be purged, by simple and clear methods of documentation, and by the delegation of decision responsibility towards the product-handling individuals. The flow of information through internal and external levels of hierarchy removed from the product has to be avoided at all costs. New developments have to be reduced to the minimum absolutely required, by the use and, if necessary, goal-oriented testing of available components. Within a tight but sufficient budget to be provided, the project work force has to have practical and technical freedom of decision up to and including the practical and comprehensive testing of the product. To this end, simple and clear goals have to be provided by the customer, and an accordingly very limited reporting scheme accepted, instead of extensive and detailed technical specifications and requirements structures. (cf. "Kelly's Rules", [21], pp. 53ff., 295, 304)

*The following appears likely according to the data and case reports presented:*

**1.)** The further spreading of until recently or currently still inaccessible information on failures can contribute greatly to the prevention of accidents. To a great extent, this also appears possible for systems [h] that differ in technical detail as well as for systems that were developed completely independently from another, which would allow the maintenance of military and commercial secrecy requirements in the reporting of such information. For the course of some failure events, the manner of reporting used in the technology-historical works analyzed for this study seems suitable for the filtering and anonymization of information to be protected. Information anonymized by a technical categorization similar or identical to that used in this study can contribute in general to increasing the reliability of spacecraft, by directing attention towards patterns of failure events without passing on protected information.

**2.)** The high cost and unfavourable cost-efficiency of many Western space projects results mainly from the practised procedures of external control and review and budget release. Contributions to excessive cost are the cancellation of projects after a single failure; the assignment of mishap investigation to external institutions, or institutions not usually involved in the related technologies and sciences, e.g. offices of budget or management; the habit to initiate, after some time and starting with a blank sheet, completely new developments to address tasks very similar to previous missions; the cancellation of developments nearing completion due to non-technical considerations; projects being kept on the back burner for a long time for non-technical reasons; the preference of very detailed theoretical studies free of practical risks of demonstrated failure over step-by-step developments with the attached risks of practical testing on the ground and in flight; the bundling of separable projects into large and complex, in particular robotic missions; the habit to fire, disperse, or retire early the established work forces when projects are cancelled or fail; the accordingly reduced build-up of experience, much hindered by the very low number of missions per lifelong career.

**3.)** In projects that are conducted in the form of a cooperation of several otherwise independent governmental or commercial partners, and for which the distribution of the work to various sites and workforce sizes is not exclusively determined according to technical and practical requirements, the total expenses rise out of proportion with the number of institutions involved. (Some sources suggest a power law in which the resulting expenses are proportional to the indispensable technical expenses to the power of the number of partners.) These additional expenses do not contribute to the end product or its quality, and the separation of sites in particular has an adverse effect on reliability due to the increased need for long distance, i.e. non-face-to-face communication, and the necessity of inter-institutional communication through assigned channels or levels of hierarchy in the chosen arrangement.

*The data and case reports presented suggest:*

**1.)** The after the Cold War also in non-military spaceflight increasing popularity of small satellites, with a small number of instrument payloads per satellite for a firmly defined and limited task or mission, can become a very efficient contribution to increase cost-efficiency as well as reliability of robotic spacecraft. Small satellites on low Earth orbits can, in particular in the shape of constellations, bypass the systemic structural weaknesses of heavy long-life satellites in higher orbits that have so far been an impediment to investment in space, and enable a faster and more continuous development of technology by easing practical flight-testing. The transition to a larger number of smaller satellites in lower orbits does not necessarily carry new structural risks for spaceflight in general, as long as technological progress, orbit geometry, and constellation replenishment frequency are commonly kept in tune with the self-cleansing capability of the upper atmosphere.

**2.)** Complex robotic missions with a high number of bundled instrument payloads may be useful for the scientific investigation of targets difficult to reach, like the outer planets, if an uninterrupted comprehensive long-term practical base of experience is accessible to conduct such missions. In such missions, the long cruise fight phases lift operational cost to the same order of magnitude as the initial investment. Closer to Earth, the use of small satellites appears more sensible, as long as a high frequency of regular routine flights can be expected in the long run. If at least occasional maintenance by direct human intervention is an option designed into a system, the scientific advantages of bundling a large variety of instrument payloads in one platform addressing one object of interest can also be realized cost-efficiently, since the efficiency in the mitigation of failures can be increased to near the level of a permanently crewed mission.

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[a] For example, Mars has a synodic period of 780 days = 2.135 years, but a synodic cycle of favourable oppositions of approximately 15 to 17 years, and a synodic cycle of the most favourable oppositions of approximately 32 years. Similary, an Earth-crossing small body may have a synodic period of a few years, of alternating oppositions, lower conjunctions, and weak and close encounters with Earth. Its synodic cycles may apply to favourable telescopic viewing in the oppositions, or good radar opportunities around the encounters. These may occur in one or two groups of a few successive occasions at intervals of one synodic period interspersed by one or two pauses of several synodic periods, the sum of which forming the period of the synodic cycle.

[b] The Blind Zone is the range of distances in which an object about to hit the Earth or pass by at an extremely low distance becomes invisible to motion detection. Its outer limit is passed when the object's apparent motion relative to the background of stars due to the relative curvature of the orbits falls below the detection threshold. Its inner limit is passed when the object's apparent motion relative to the background of stars due to the parallax of the observatory moving with the rotation of the Earth or due to the angular projection of the miss distance rises above the motion detection threshold. Its depth can be reduced by increased angular resolution and increased intervals between the exposures taken.

[c] A Tunguska-like event was in [48] defined by the level and extent of the damage on the ground actually caused by the event on June 30th, 1908. It is therefore not tied to the impact frequency estimates tied to the average estimated yield as of 2002/2006, ~15 Mt TNT-equivalent in a range of 4...40 Mt TNT, e.g. [6,24,38]. A yield of 15 ±5 Mt TNT was still baselined in [48] with the caveat that fundamental properties of non-nuclear explosions hinted at a lower yield perhaps within the single-digit Mt range (Appendix B to [48], B.1.2., B.1.4.1.), supporting eyewitness accounts reported in [6] and on the English-language wikipedia Tunguska page, and multiple-epicenter models and the 'spindle model' of energy release along the bolide trajectory at Tunguska proposed by a number of Russian authors to explain the tree fall pattern and some of its peculiarities. Yield estimates at or below the very lowest end of those previously reported have since been strongly supported by computer simulations that include momentum preservation of the impactor, see [15].

[d] Such events smaller than Tunguska as defined above include, for example, airbursts in the several kt to few Mt TNT-equivalent range occurring at ground-affecting altitudes, airburst meteorite falls of the type of the Madrid 1896 event which toppled walls, and low fragmentation altitude stony or iron 'shotgun' impacts like the falls of the Pultusk Peas in 1868, of Sikhote-Alin in 1947, and of Jilin in 1976.

[e] In [47], a spacecraft was defined as a flying vehicle that is, before the beginning of any process related to a specific flight experiment, intended to move in a space of increasingly rarified gases substantially beyond the spaces and velocities attained by contemporary technologies of transport accessible to everyone, and in places such that it is in the direct and temporal context remote from the external, comprehensive, and direct perception and intervention of human beings. (Internal human beings, i.e. astronauts, are thereby defined as a spacecraft component or subsystem. The outer edge of the envelope of 'everyone' at the time of completion of this study was roughly defined by people purchasing a ticket to fly on Concord, or pursuing a career that led them to fly high-performance military aircraft such as the SR-71 or MiG-25 and -31, or flying stratospheric round-the-world balloons as a form of extreme sports or advertisement. As of today, a reformulation of this definition is not yet thought necessary, since space tourism has not taken place in manners other than buying into pre-existing government programmes.)

[f] In [47], success is defined as follows: Success is seen as the normal state of spacecraft which does not require further specific documentation and is not evaluated for the study of reliability; the incompleteness of any extracted information on the collected failure incidents to be analyzed is given on the base of principle.

[g] A failure is comprised of one or more elementary failure events. These can occur sequentially as a chain of dominoes toppling one another, in converging or diverging root or tree patterns, or individually as a chain breaking at the weakest link, for example. The analysis has in [47] focused on failure events first, and then their typical modes of propagation and hidden hibernation from cause to trigger event.

[h] In [47], a system is defined as the set of objects that includes the flying vehicle and its related ground installations, including all their respective technical and human functional components and all flows of material and information, which are related to the specific vehicle and the specific intended flight experiment.

[x] available in German, only.