The Regolith Biters

a divide-and-conquer architecture for sample-return missions

NASA Innovative Advanced Concepts Phase-I Investigation Final Report

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Executive Summary

In this document we detail the findings of the NIAC Phase-I investigation for the concept *The Regolith Biters---A Divide-And-Conquer Architecture for Sample-Return Missions*.

The concept investigated consists of a space mission architecture for collecting multiple, distributed samples from small, primitive celestial bodies (like asteroids and comets) and bringing them back to Earth for their study; it is fundamentally different from existing alternatives because it is based on the premise that separating the *navigation problem* from the *sample collection problem* will lead to a more robust and flexible overall system.

The current architectural paradigm for sample-return missions is centered around a design where spacecraft and sampling device are merged into a single, complex system; we argue that this monolithic approach couples the navigation and sample-collection problems, making both more difficult. We diverge from this vision, and propose a decoupled system based on the coordinated interaction between a spacecraft and a collective of small, simple devices which we have called the *Regolith Biters* ($\mathbb{RB}$s).

A spacecraft carrying a number of $\mathbb{RB}$s would travel to the vicinity of a small body. From a favorable vantage point, and while remaining at a safe distance on a non-colliding trajectory, it would release an approach stage capable of delivering the $\mathbb{RB}$s towards the target body. Upon encountering the body, the $\mathbb{RB}$s would *bite* the regolith (thus retaining a sample), and eject back to a heliocentric orbit. The spacecraft, being endowed with appropriate propulsion, navigation and tracking capabilities, would rendezvous with and collect those $\mathbb{RB}$s within its reach, and bring them back to Earth.

Separating the navigation and sampling concerns could remove the need for proximity operations with the small body---the stage in current architectures that carries the most challenges and risks. Eliminating the need for small body proximity operations brings back to the discussion the exploration of exciting prospects like highly active comets, fast-rotating bodies, and binary systems.

In addition, distributing the sampling problem among a collective of agents could provide the opportunity to sample multiple regions---on one or multiple bodies within a system---in a single mission. It may also provide robustness to various environmental conditions, and enable the distributed, in situ characterization of the small body. These technical distinctions separate our concept from existing art.

The Phase-I investigation detailed in this report focused on assessing the astrodynamic feasibility of the concept. In particular, we set out to test three basic hypotheses:

Availability There are sufficient small-body candidates and launch opportunities in the next decades to ensure that a target would be available if the mission were executed.

Reachability The propulsion requirements to travel to the small body, chase and capture the $\mathbb{RB}$s, and return to Earth are within reach of current or near-term technology.

Visibility The capabilities required to track the $\mathbb{RB}$s after their ejection from the small body are within reach of current or near-term technology.

Given availability, reachability, and visibility, our core technical analyses then focused on obtaining an estimate of the number of $\mathbb{RB}$s that could be captured for a given Δv budget.

Based on the results of our analyses, we observe that:

- A wide variety of small bodies exist that could be targets for a sample-return mission based on the $\mathbb{RB}$ architecture.
- The Δv and thrust requirements are such that a dual-mode propulsion system would be required: a high-efficiency, low-thrust propulsion mechanism for the transfer between Earth and small body (for both legs of the trip), and a high-thrust, chemical propulsion system for the capture of the $\mathbb{RB}$s after their ejection from the small body.
- Astrodynamically plausible $\mathbb{RB}$ capture tours were found for a variety of Solar System small bodies, under a wide range of perturbing conditions; for most of these tours, several $\mathbb{RB}$s could be captured at a reasonable Δv budget, and for smaller bodies the number increases considerably.
- The optical tracking of the $\mathbb{RB}$s is feasible with technologies which are either readily available or close to maturing within the next decade.

During the investigation, we also developed basic, preliminary computer-aided designs of the physical $\mathbb{RB}$s to serve as proof-of-concept and visualization aid. The investigation also enabled us to to identify key technologies and drivers critical for further development.

In conclusion, our findings suggest that the physical foundation of the $\mathbb{RB}$ architecture is astrodynamically sound, and exhibits robustness to variations in size and rotational state of the target body.

1 Introduction

The collective interaction of simple systems can be leveraged to attain complex goals. Based on this principle, we envision space systems where the core functional components are decoupled, autonomous, and cooperative. In particular, we propose a space mission architecture for collecting multiple, distributed samples from small, primitive celestial bodies (like asteroids and comets) and bringing them back to Earth for their study.

The architecture is fundamentally different from existing alternatives because it is based on the premise that separating the *navigation problem* from the *sample collection problem* would lead to a more robust and flexible overall system.

We have focused our architectural development in the context of small-body sample-return missions for three main reasons:

1. We believe that no experimental study sheds more light on our understanding of the origin and evolution of the Solar System than the analysis of samples from asteroids and comets [16].

2. We are convinced that their study is important from a strategic perspective: meteorite impacts pose a direct and credible threat to life on Earth [23], and the development of contingency small-body deflection missions presupposes some knowledge of the target body.

3. Our concept came to life at a historic time when private industry decided to pursue asteroid mining as a commercial venture: our architecture could prove to be a valuable framework for prospecting potential mining candidates.

While meteorites have proved invaluable in our understanding of the origin and evolution of the Solar System, linking them to the original small body from which they originated currently eludes our capabilities [14]. In addition, the violence of atmospheric entry and surface collision leads to irreparable alterations in their material integrity and composition. The most direct route to furthering our knowledge about primitive bodies is to sample asteroids and comets directly, and bring the samples in pristine integrity back to Earth for detailed study.

Indeed, the analysis of microscopic samples (1 to 300 μm in size) of comet Wild 2 brought by the Stardust mission challenged Solar System evolution theories, and provided deep insight in the physical chemistry of the comet's nucleus [8]. One can only imagine what discoveries may be enabled by significantly larger sample sizes.

The scientific community has long recognized the value of studying asteroids and comets. An unprecedented and inspiring fact speaks for itself: between 1985 and 1986, five international spacecraft were dedicated to the study of comets Halley and Giacobini-Zinner, while nearly 5,000 professional and amateur astronomers monitored both comets from the ground [30].

In addition to the relentless scientific interest in small bodies, recent announcements have been made [19] by private ventures expressing their interest in mining asteroids as a commercial venture. Such rare alignment of interests promises to energize the community to develop new technological advances addressing the difficulties associated with discovering, prospecting, sampling, and working in general with asteroids and comets.

The current architectural paradigm for sample-return missions is centered around a design where spacecraft and sampling device are merged into a single, complex system. We argue that this monolithic approach couples the navigation and sample-collection problems, making both more difficult.

In contrast, we propose a decoupled system based on the coordinated interaction between a spacecraft and a collective of small, simple devices---the *Regolith Biters* ($\mathbb{RB}$s): a spacecraft carrying a number of $\mathbb{RB}$s would travel to the vicinity of a small body. From a favorable vantage point, and while remaining at a safe distance on a non-colliding trajectory, it would release an approach stage capable of delivering the $\mathbb{RB}$s towards the target body. Upon encountering the body, the $\mathbb{RB}$s would *bite* the regolith (thus retaining a sample), and eject back to a heliocentric orbit. The spacecraft, being endowed with appropriate propulsion, navigation and tracking capabilities, would rendezvous with and collect those $\mathbb{RB}$s within its reach, and bring them back to Earth.

Separating the navigation and sampling concerns could remove the need for proximity operations with the small body---the stage in current architectures that carries the most challenges and risks. Eliminating the need for small body proximity operations may bring back to the discussion the exploration of exciting prospects like highly active comets, fast-rotating bodies, and binary systems.

In addition, distributing the sampling problem among a collective of agents could provide the opportunity to sample multiple regions---on one or multiple bodies within a system---in a single mission. It may also provide robustness to various environmental conditions, and enable the distributed, in situ characterization of the small body. These technical distinctions separate our concept from existing art.

From the point of view of its generality, our concept can be aggregated at three different levels:

The *paradigm* A common technological paradigm for the attainment of complex goals is to increase the complexity of the individual components, forming a *specific*, *centralized* system. Our ambition is the opposite: we conceive of simpler, *generic*, *distributed* components, and shift the complexity to their *common interactions*.

Whereas centralized systems search for *deterministic success* (one device reaches the desired outcome in one attempt), we aim to explore *stochastic success*---the interaction of a collective achieving overall success, even in the presence of individual failures.

The *platform* The distribution of numerous, relatively simple, cooperative, autonomous devices across the surface of a body could enable the development of new scientific techniques. Just like scores of GPS sensors are deployed across Earth to study plate tectonics, one can visualize the deployment of simple sensors across other bodies for their extended characterization.

Even the most modest prospects are appealing, like placing simple devices furnished with a thermocouple and an accelerometer in ten different places of a comet for measuring thermal properties and rotational state. Miniaturization of individual components would enable the deployment of numerous sensors, making this prospect all the more exciting.

The *architecture* At this most specific level, we believe our concept could provide a wider flexibility in the selection of candidate small body targets than alternative architectures.

For example: some bodies rotate so fast that proximity operations become too problematic; some comets are more interesting when they are highly active, but their ejecta may damage the spacecraft; many asteroids belong to binary systems, and their common gravity field defies our current navigation capabilities.

In contrast, we aim to maintain a safe distance from the target in a manner such that mission risk would no longer be dominated by small body proximity operations. This would open the architecture to previously unreachable bodies.

1.1 Alignment with Current Priorities

From a scientific perspective, our vision is directly aligned with some of the highest priority goals of the Space Program, which recognizes that returning a sample from primitive asteroids and comets is scientifically compelling and would have a major impact on our understanding of the origin and evolution of the Solar System [16].

Our concept is also consistent with technical challenges identified in NASA's Space Technology Roadmaps and Priorities, and the goals for specific Technology Areas [25]. In particular, our concept is most aligned with the following *top challenges* identified for Technology Area 4---Robotics, Telerobotics, and Autonomous Systems [1]:

Rendezvous Develop the capability for highly reliable, autonomous rendezvous, proximity operations, and capture/attachment to (cooperative and non-cooperative) free-flying space objects.

The development of *highly reliable, autonomous* rendezvous and *capture* capabilities is central to our architecture, and it is precisely in the context of cooperative (the mother ship) and non-cooperative (the $\mathbb{RB}$s) space objects.

Maneuvering Enable robotic systems to maneuver in a wide range of NASA-relevant environmental, gravitational, and surface and subsurface conditions.

While $\mathbb{RB}$s would not be capable of maneuvering, we do believe that the architecture directly provides means for a *system to maneuver in a wide variety of environmental, gravitational, and surface/subsurface conditions*. In the proposed architecture the mother spacecraft is not required to undergo proximity operations with the small body, and $\mathbb{RB}$s travel on ballistic trajectories. This separation of concerns addresses the *maneuvering* problem by eliminating it.

In Situ Analysis and Sample Return Develop subsurface sampling and analysis exploration technologies to support *in situ* and sample return science missions.

The concept itself constitutes a potential technology supporting sample-return science missions, and its potential as a platform for distributed characterization of small bodies provides a unique approach to *in situ* exploration.

Relative Guidance Algorithms Anticipate applicable environmental effects, the nature of the trajectory change/attitude control effectors in use, and the inertial and relative navigation state data available to the guidance algorithms.

The development of guidance and navigation algorithms required for the deployment of the delivery stage, and the tracking, chase, and capture of the $\mathbb{RB}$s after their ejection from the small body are concerns directly addressing this technical challenge.

We are inspired by one bold challenge issued in the Roadmap for Technology Area 4 [1], which summarizes the *desired* outcome of technological development for a comet nucleus sample-return mission:

> Return up to several kilograms of samples from multiple sites on the nucleus, with stratigraphy and all ices intact, and no cross contamination of collected samples.

We believe that the distributed aspect of our architecture represents a step in the right direction: $\mathbb{RB}$s can conceivably be designed to collect a few tens or hundreds of grams from different regions of a comet, and the fact that they are independent agents provides a natural barrier against cross-contamination of samples.

While preserving the stratigraphy and ices intact would require furthering our concept to consider controlled $\mathbb{RB}$ landings, advanced core sampling mechanisms, and furnishing the spacecraft with cryogenic storage capabilities (and such advanced capabilities are not the central focus of our current development), the architecture itself does not restrict future adaptations to address these concerns.

2 Concept Description

We created the concept of Regolith Biters as a way to remove the small body proximity operations problem inherent to current sample-return paradigms. The concept is based on decoupling the navigation and sample collection concerns, thus transforming the core difficulty: from navigating a spacecraft to---and extracting a sample from---a poorly characterized, possibly active (in the case of a comet), and likely spinning small body; into acquiring, chasing and capturing the $\mathbb{RB}$s. We believe the later alternative is not only safer from a mission success standpoint, but that it is an approach for which reliable, generic technology can be more easily developed.

The narrative of a small-body sample-return mission based on our architecture can be divided into six stages, and would read as follows:

Transfer A spacecraft would be launched from Earth on a trajectory en route to the target body; the trajectory would reach a closest approach of at minimum a few hundred kilometers, and with the relative velocity between body and spacecraft maintained below a maximum threshold.

Deployment Upon arriving to a favorable vantage point in the *vicinity* of the body, the spacecraft would deploy a delivery stage loaded with a number of $\mathbb{RB}$s.

Sampling The delivery stage would release the $\mathbb{RB}$s upon close proximity to the body; they would encounter the surface with a given arrival dispersion pattern and velocity. Immediately upon contact, the $\mathbb{RB}$s would collect a regolith or core sample; throughout this process, the spacecraft would have been cross-referencing each $\mathbb{RB}$ to its landing site via active or passive tracking.

Ejection Either as a reaction to the landing impact (bouncing), or as an action initiated by an autonomous mechanism, the $\mathbb{RB}$s would eject from the surface of the body; they escape the body and enter some heliocentric trajectory with random orbital parameters.

Capture The spacecraft---continuing the active or passive tracking of the $\mathbb{RB}$s---would algorithmically determine the optimal path to rendezvous with and capture some of the ejected $\mathbb{RB}$s.

Transport The captured $\mathbb{RB}$s would be stored in a container designed to maintain sample integrity during their transport, and return to Earth with the collected samples.

2.1 Limitations of Related Approaches

In our review of current sample-return technologies, we noticed that the overarching conventional approach relies on the *touch-and-go* (TAG) concept, where a spacecraft endowed with a sampling mechanism undergoes proximity operations with a small body, touches the surface with said mechanism, and leaves[1].

We contend that such a monolithic system effectively couples the navigation and sample collection problems making both more difficult: navigation errors directly affect the performance of the sampling device, and the sampling process itself introduces torques which complicate navigation. It is also a concern that the exhaust jets from the propulsion and attitude control systems could contaminate the surface of the small body. The TAG architecture tends to demand high Δv to capture into the sampling orbit, and once the sampling program has been initiated, it is difficult to modify the inertial trajectory.

Proximity operations with asteroids and comets are difficult in general. The gravity field of these small bodies tends to be highly irregular, and while it is strong enough to significantly perturb the spacecraft trajectory, it is too weak to serve as an *anchor* for a stable orbit. For some of the most interesting bodies the complications are even greater: their high rotation rates can induce unmanageable torques upon contact, and unpredictable streams of debris and ejecta (especially in the vicinity of comets) are capable of impinging on and damaging the spacecraft.

2.2 Risks of this Approach

We acknowledge formidable challenges that must be overcome before materializing our architecture. Below we enumerate those risks which we have identified during the development of this proposal. In addition to the risks associated with any new technology, we compiled the following non-exhaustive inventory, where we aim to capture some of the unique risks and trades which need to be addressed more fully during the development of our concept.

System Design The community has less expertise characterizing non-deterministic systems, and the performance guarantees required for flight qualification are difficult to measure using conventional methodologies. On the other hand, our efforts in addressing this concern are aligned with a top priority of the Technology Area 4 [1]: the development of novel validation and verification techniques for autonomous, stochastic systems.

[1] The Hayabusa mission is an exemplary and daring example of the TAG architecture. Unfortunately, problems related to the spacecraft led to an overall unsuccessful sampling program. While microscopic samples did make it into the canister and back to Earth [14], the mission---an admirable venture---underscores the motivation to consider separating navigation from sample collection.

Spacecraft Failure The architecture's single-point-of-failure is the spacecraft. It must deploy the $\mathbb{RB}$s with acutely precise pointing, perform reliable tracking, successfully rendezvous with and capture a number of $\mathbb{RB}$s, and undergo atmospheric reentry. While any one capability is within reach of current technology, their combination into a single spacecraft represents---to the extent of our experience---a previously unattempted venture.

There is a risk that the spacecraft could be damaged when trying to rendezvous with an $\mathbb{RB}$. However, this type of risk is inherent to any sample-return mission, and we consider it more pronounced in the coupled TAG architecture.

Fortunately, there is a large amount of prior art when it comes to managing this type of risk. We aim to collect and interpret the lessons learned from other related missions (like Dawn, Hayabusa, Deep Impact, Stardust, DART [5], and PRISMA, to name a few) and incorporate their risk mitigation strategies into our concept development.

Economical A clear trade-off exists between $\mathbb{RB}$ performance and development cost. Another trade-off exists between $\mathbb{RB}$ performance and the minimum number of $\mathbb{RB}$s required to achieve a given probability of mission success. For a given measure of $\mathbb{RB}$ performance, probability of mission success increases with the number of $\mathbb{RB}$s. On the other hand, additional performance implies additional development cost.

It is possible that in balancing the technical $\mathbb{RB}$ requirements, a design is reached that falls outside of economical viability.

Common-Mode Failure As we envision them, $\mathbb{RB}$s would be similar amongst each other; their similarity is inherent to the architecture, and could reduce development cost. However, it also introduces the risk of common-mode failure: if $\mathbb{RB}$s are afflicted by a common design flaw, the concept may collapse regardless of the number of $\mathbb{RB}$s.

A given small body could have a harder surface such that the $\mathbb{RB}$s bounce-off before successfully collecting the sample. It could also have a softer surface, burying the $\mathbb{RB}$s and keeping them from successful ejection. These and other potential common-mode failures need to be investigated further.

Sample Collection, Preservation, and Handling While *any* sample from a small body would be a valuable treasure, it is a priority that the mission architecture is able to cross-reference the different samples to their corresponding body surface locations; this would require the development of reliable $\mathbb{RB}$ tagging mechanism (for example, an $\mathbb{RB}$ could leave an RFID tag on its landing location).

For more advanced applications, the $\mathbb{RB}$ collection process should not destroy the morphological or stratigraphical integrity of the sample; the preservation of volatiles and ices---a top priority---would likely require a cryogenic return vehicle, which in itself is a difficult venture.

Finally, it is possible that the samples would need to be contained and subject to special handling requirements for some asteroids and comets [27] before being admitted into Earth.

2.3 Potential Impact

If successful, our approach would fundamentally change the conversation for sample collection, scouting, and prospecting of small bodies because it could enable the sampling of bodies currently out of scope for the TAG architecture, like fast-rotating bodies, active comets, and binary systems.

We also believe it could be made proportional in its complexity to the desired outcome; for example, a less reliable system would be made to take a quick sample of a potential mining candidate, while a more reliable system would be made to gain understanding about the composition of an incoming threat before attempting a deflection mission.

The architecture would be more immediately suitable for sampling the surface of small bodies, a central scientific component to understanding the mechanisms of *space weathering*, i.e., how does exposure to space alter the structure, optical properties, chemical composition, and mineralogy of the material [17]. A pristine sample from the surface of a small body would provide an anchor for spectral interpretation of the compositions and interrelationships of numerous asteroids and establishing links between meteorites and their parent bodies [16].

The deployment of autonomous agents is not limited to sampling devices that would eject back to a heliocentric orbit. In fact, we envision the deployment of other sensing devices, transponders, or reflectors to be left on the surface for later distributed characterization of the small body. There is nothing intrinsic to the architecture limiting this vision.

Some concepts for Mars sample-return missions also rely on the development of systems for autonomous tracking and rendezvous with small devices. Our investigation on this front would both leverage existing expertise, and extend current capabilities strengthening both ventures.

On a more philosophical level, we believe that our approach changes the paradigm from *deterministic* to *stochastic* success, and can teach us that victory does not need to come from success in all the parts; we can embrace the failure of individual components as long as their overall interaction is successful. We hope that it will serve as a catalyst for new exploration paradigms, where the cooperation of simple systems is leveraged to attain complex goals.

3 Technical Analyses

The investigation focused on assessing the astrodynamic feasibility of the concept. In particular, we set out to test three basic hypotheses:

Availability There are sufficient small body candidates and launch opportunities in the next decades to ensure that a target would be available if the mission were executed.

Reachability The propulsion requirements to travel to the small body, chase and capture the $\mathbb{RB}$s, and return to Earth are within reach of current or near-term technology.

Visibility The capabilities required to track the $\mathbb{RB}$s after their ejection from the small body are within reach of current or near-term technology.

Given availability, reachability, and visibility, our core technical analyses then focused on obtaining an estimate of the number of $\mathbb{RB}$s that could be captured for a given Δv budget. In addition, we developed basic, preliminary computer-aided designs of the physical $\mathbb{RB}$s to serve as proof-of-concept and visualization aids.

3.1 Modeling

The preliminary trajectory calculations between Earth and small bodies assume conic motion of the celestial bodies; the motion of the mother spacecraft is described by a simple dynamic model describing a point mass moving under the influence of the Sun's gravity and a low-thrust engine:

$$\begin{aligned} \frac{\mathrm{d}^2\mathbf{r}}{\mathrm{d}t^2} &= -\mu_{\odot}\frac{\mathbf{r}}{r^3} + \mathbf{a}_T, \quad \text{where} \\ \mathbf{a}_T &= \tau\frac{T}{m}\mathbf{u} \quad \text{and} \\ \frac{\mathrm{d}m}{\mathrm{d}t} &= -\tau\frac{T}{g_0 I_{sp}}. \end{aligned} \tag{3.1}$$

A given propulsion system can be characterized in terms of a maximum thrust level, T, and a specific impulse, I_{sp}; the throttle parameter, τ, and directional cosines of the thrust direction, **u**, are determined during the trajectory calculations. In the case of chemical propulsion, the trajectory calculations were modeled using impulsive velocity changes; targeting problems (reaching a given position in a given time) were modeled as Lambert arcs, and solved using standard algorithms [10].

3.2 Target Population

The population of asteroids and comets in the Solar System has been documented by the Solar System Dynamics Group at the Jet Propulsion Laboratory, and made available via the on-line *Horizons* system [9]. As of this writing, the database contains 3,216 comets and 654,603 asteroids, and it constituted our main source for small body information. Our concept aims to sample small bodies, which have different rotational properties than

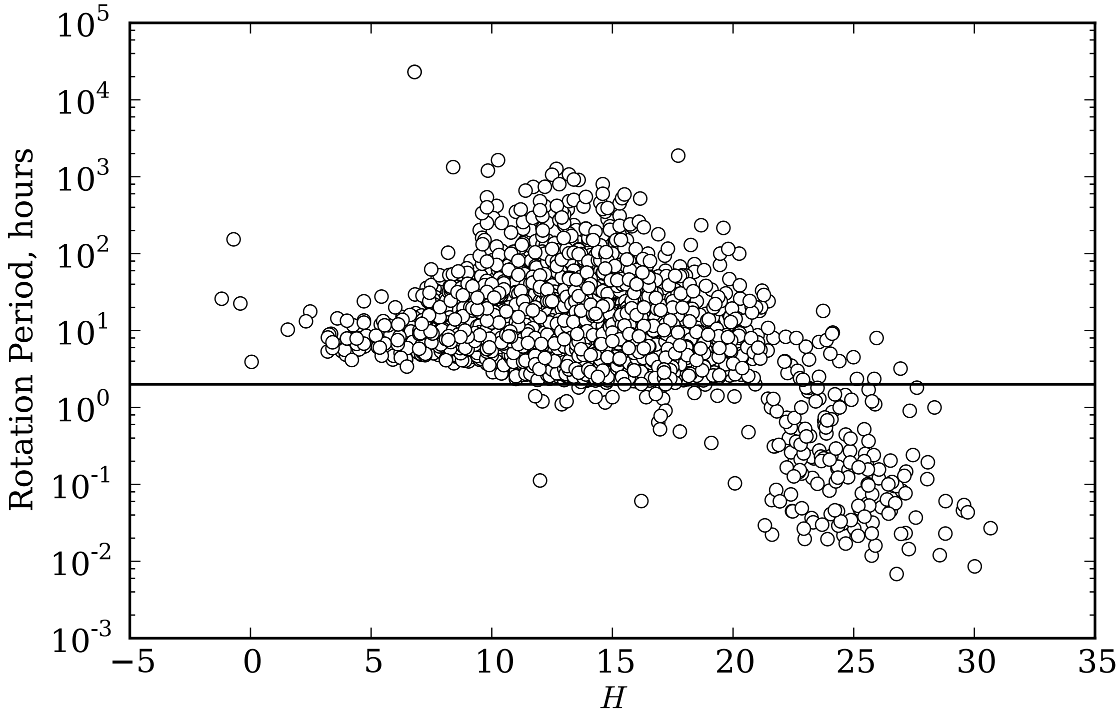


Figure 3.1: Smaller asteroids (those associated with larger absolute magnitude, H) tend to rotate faster. The solid line denotes the rotational limit which divides gravity- from strength-bound asteroids (cf. [4]).

larger bodies. In particular, it has been determined [12] that small asteroids tend to be rapid rotators; Figure 3.1 depicts the rotation period of asteroids in the Horizons database as a function of their absolute magnitude[1].

As it can be seen in Figure 3.2, smaller asteroids are associated with larger absolute magnitudes (in particular, those bodies with $\varnothing < 2$ km are associated with $H > 17$). Notice

[1]For database entries which contain both of these parameters.

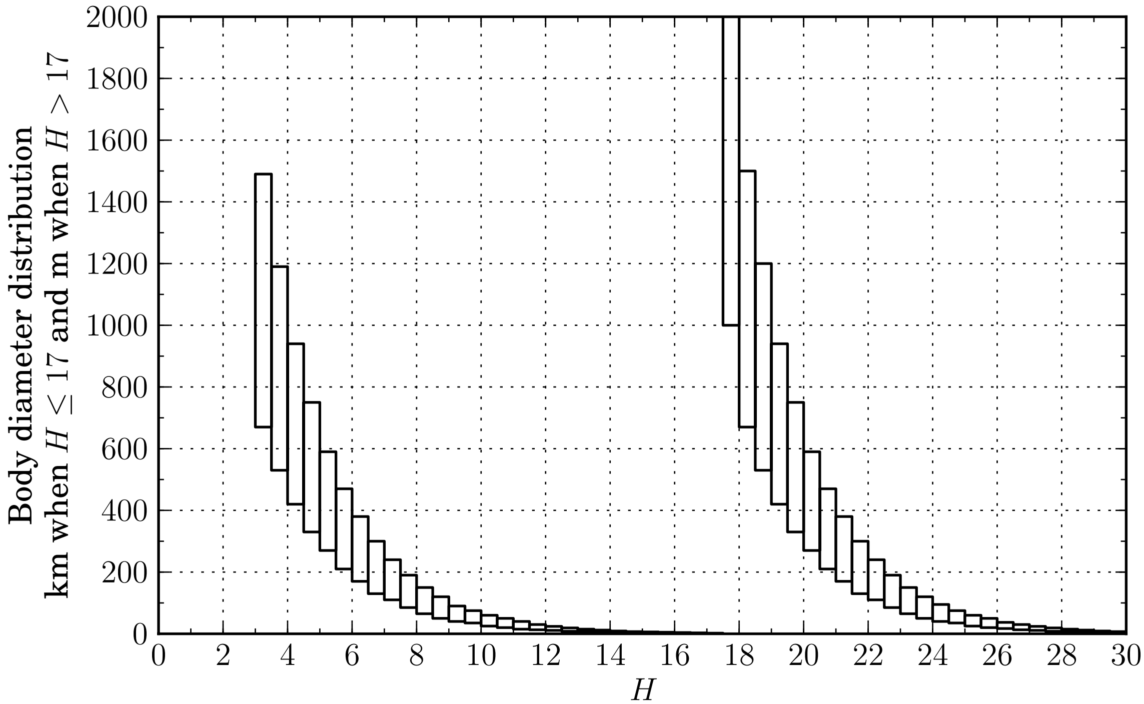


Figure 3.2: The absolute magnitude (H) of an asteroid is inversely proportional to its size.

also the abrupt change between strength-bound and gravity-bound bodies (the change is perhaps simpler to visualize in Figure 3.3). Investigating these very fast rotators can provide important scientific insight regarding the collisional processes which formed the Solar System [4].

In conclusion: while the majority of asteroids rotate between one and ten times per day (in the case of comets, the effective radii fall between 200 m and 37 km, and the range of rotational periods extends from 5 to 70 h [15]), a small but interesting fraction can exhibit hundreds of rotations per day (cf. Figure 3.3); such rotational characteristics have important consequences in the the ejection stage (cf. §3.5).

The low gravitational potential of small bodies leads to low escape velocities. In order to characterize the expected escape velocities we relied on available small body density [13] and porosity [7] values to derive the mass of a sphere of a given radius. The general rule used in this investigation is that the escape velocity measured in m/s at the surface of an asteroid is roughly equal to its radius measured in km (cf. Figure 3.4).

Such heuristic is equivalent to assuming a default density of 2 g/cm^3; this approximation will tend to underestimate the escape velocity for metallic (M-type) and silicaceous (S-type) bodies, and overestimate the escape velocity for carbonaceous bodies (C-type); the escape velocity is then given by

$$\sqrt{\frac{2Gm}{r}},$$

where G is the gravitational constant.

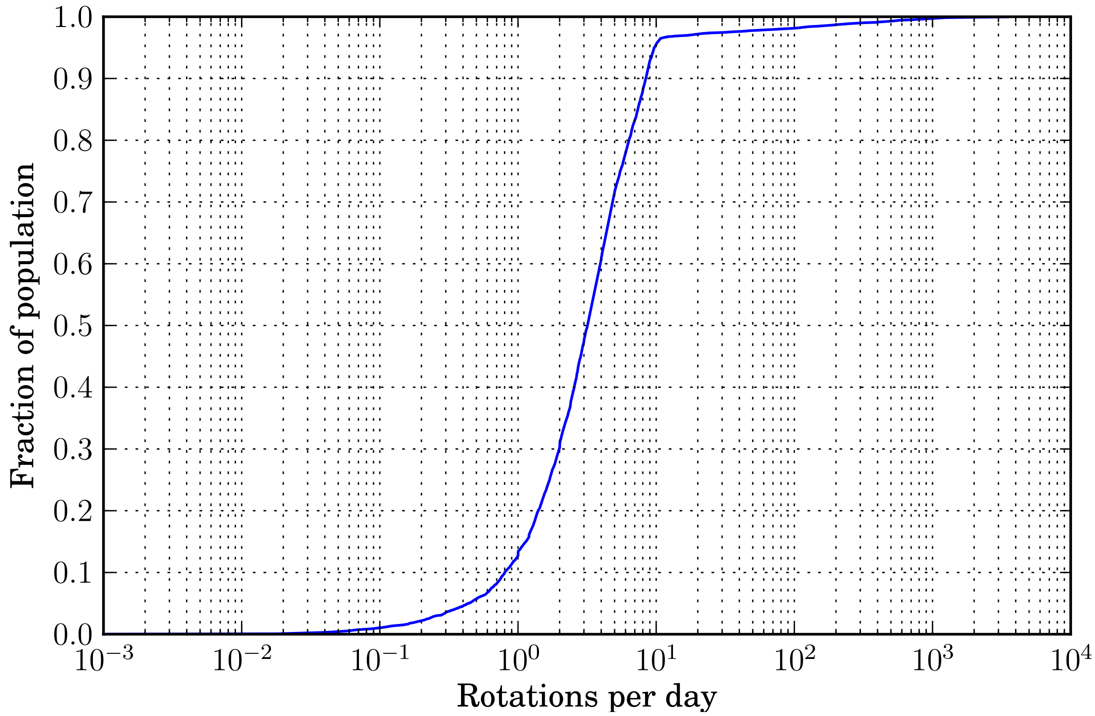


Figure 3.3: While the majority of asteroids rotate between one and ten times per day, a small but interesting fraction can exhibit hundreds of rotations per day.

3.3 Transfers

The identification of spacecraft trajectories between Earth and small, primitive bodies has been subject to a considerable amount of research, and there are numerous general-purpose methodologies to calculate feasible and optimal transfers [3, 6].

While finding valid spacecraft trajectories was not the core concern of the investigation, we did find it necessary to develop a custom, simple optimizer for low-thrust trajectories in order to investigate the required propulsion requirements. And our findings are consistent with previous work: in general, low-thrust propulsion is an alternative superior to chemical propulsion for both legs in a small body sample-return trajectory. Missions using chemical propulsion alone require gravity assists and many years to rendezvous with a comet in order to deliver a reasonable mass using an affordable launch vehicle [24, 11]. In addition, low thrust propulsion reduces implementation risk by enabling longer launch windows and robustness to launch-date slips [26].

Our preliminary transfer analysis relied on the performance figures for the NEXT thruster, which is the current state-of-the-art low-thrust propulsion system, and will be readily available in the coming years as an off-the-shelf solution [18]. Table 3.1 depicts a comparison between NEXT and the previous-generation NSTAR [2] thruster. As expected, our trajectory analysis confirms that it is possible to reach a wide variety of targets in the coming decades, and currently available low-thrust propulsion systems are suitable for delivering

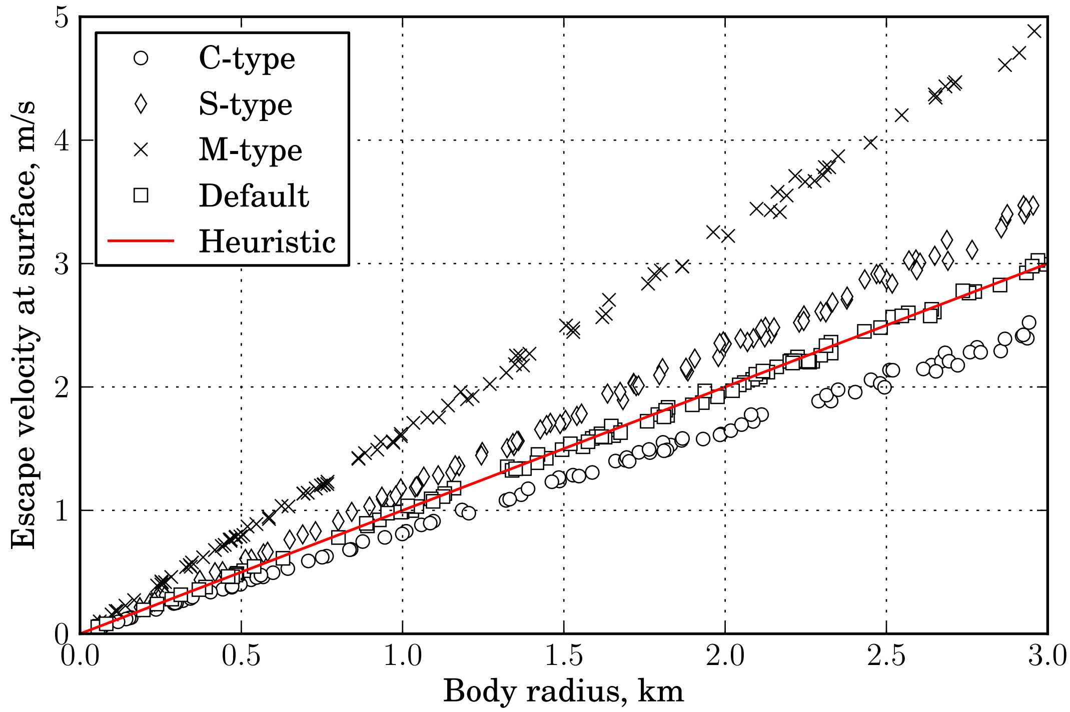


Figure 3.4: Small bodies have low escape velocity. As a basis for comparison, jumping half a meter on Earth requires about 3.1 m/s; the National Basketball Association record for vertical leap (about 1.16 m) requires about 4.8 m/s.

Parameter	**NSTAR**	**NEXT**
Maximum power, kW	2.3	6.9
Maximum thrust, mN	91	236
Throttling range, (T_{max}/T_{min}),	4.9	13.8
Maximum I_{sp} sec	3,120	4,190
Total impulse 10^6N-sec	4.6	> 18
Propellant throughput, kg	150	450

Table 3.1: The state-of-the-art NEXT thruster offers significant propulsion advantages over the previous generation NSTAR thruster.

the mother spacecraft to the vicinity of the small body at various relative velocities. Figure 3.5 presents a typical comet sample-return trajectory[2]; the transfer parameters are presented in Table 3.2.

[2]Sims determined in previous work that *a typical trajectory using low-thrust propulsion to rendezvous with a comet completes more than one revolution around the Sun and rendezvous is shortly after the comet's perihelion passage; launch from Earth occurs when the Earth crosses the longitude of the perihelion of the comet's orbit* [24]. We leveraged such finding to assess the suitability of our simplified dynamic model; our trajectories exhibit such behavior.

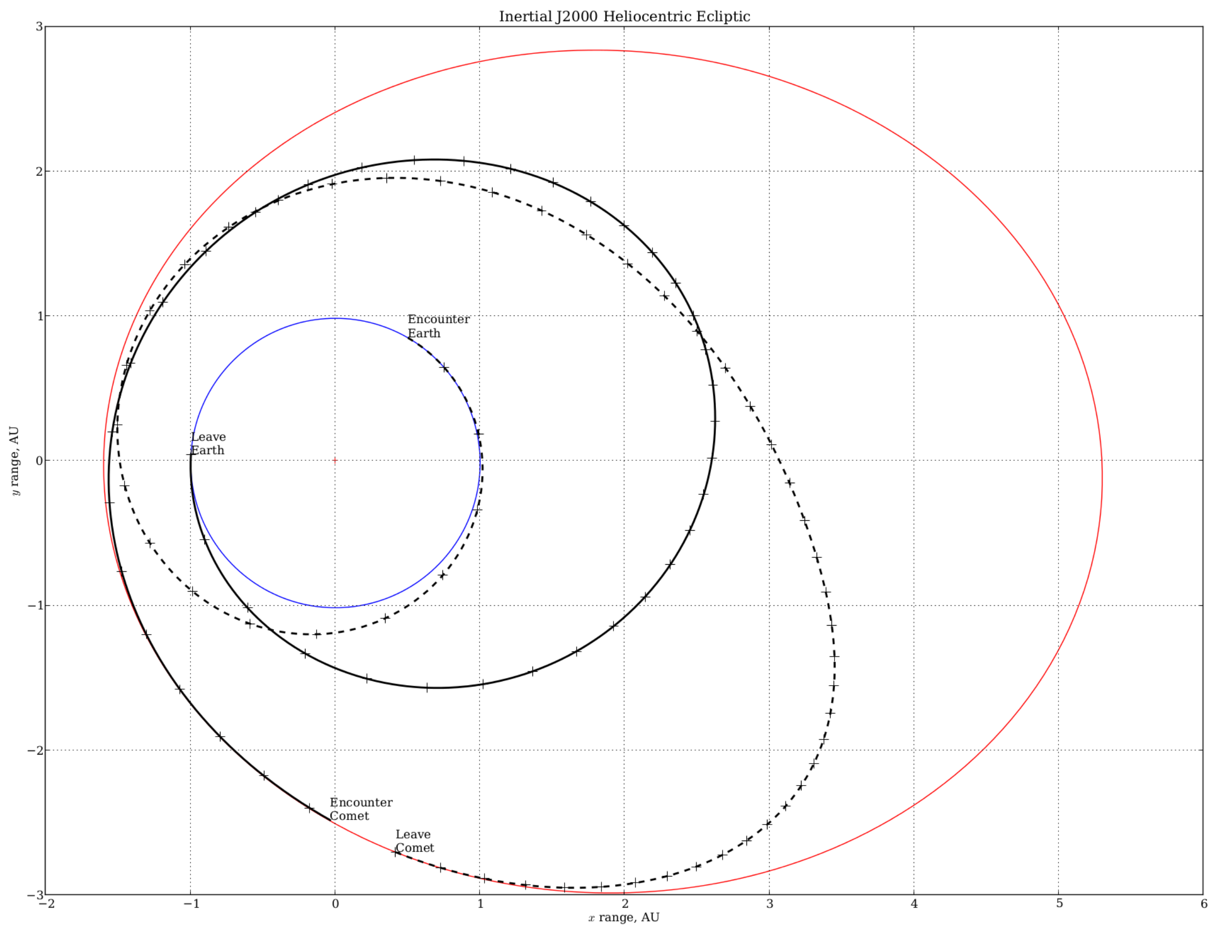


Figure 3.5: An example transfer from Earth to comet Wild-2 using the NEXT thruster. The Earth-to-comet trajectory is denoted by a thick line, and the return trajectory by a dashed line; the cross markers are spaced every thirty days.

3.4 Deployment of $\mathbb{RB}$s

The second stage in the architecture is the deployment of the Regolith Biters ($\mathbb{RB}s$) from the spacecraft to the small body. The original assumption was that the $\mathbb{RB}s$ would be

Launch date	2020, March 18
Arrival to Comet	2023, July 14
Time to Capture 10 $\mathbb{RB}$s	43 days
Chase Δv	1.0 km/s
Start Return Trajectory	2023, August 26
Arrive to Earth	2027, November 21
Total xenon mass	902 kg

Table 3.2: Details for a preliminary Wild-2 sample-return transfer using the NEXT low-thrust engine.

deployed independently (as projectiles that would be launched from the spacecraft targeting the small body). However, after simplified Monte Carlo analyses it was determined that such strategy introduced complications. In particular, it resulted in the delivery of the $\mathbb{RB}$s exhibiting a large variation band, and thus requiring a closer distance between the mother spacecraft and the small body.

Instead, we opted for an architectural element that would consist of a delivery stage similar in concept to the one used by Deep Impact to deploy the impactor from the spacecraft to comet Tempel 1. This vehicle is capable of maintaining attitude control, and maintain a communication relay with the spacecraft. In addition, its propulsion capabilities would enable the reduction of the approach velocity relative to the small body[3]. Figure 3.6 presents the outcome of the Impactor Targeting Maneuvers (ITMs) executed by Deep Impact's delivery stage. We consider that our concept can rely on such technology for the deployment of an autonomous delivery vehicle carrying the $\mathbb{RB}$s.

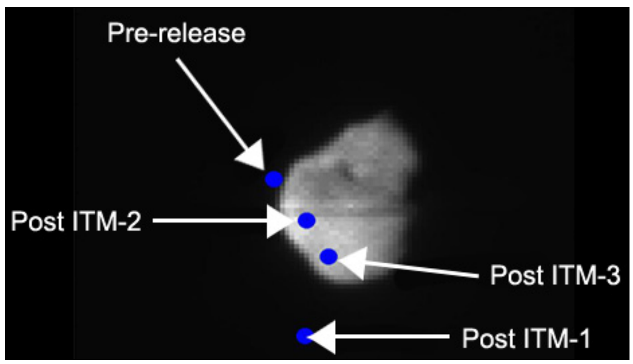


Figure 3.6: Impactor Targeting Maneuvers (ITMs) performed by Deep Impact's delivery stage as it made the final approach to its target, comet Tempel 1. The autonomous navigation system on the delivery stage was designed to make as many as three ITMs to correct its course to the comet (image credit: NASA/JPL-Caltech/UMD).

3.5 Post-Ejection Stage

Given the availability of target bodies and spacecraft transfers to reach them, the core technical analysis developed during Phase-I investigated the challenges of acquiring, chasing, and capturing the $\mathbb{RB}$s after their ejection from the small body surface.

For each independent $\mathbb{RB}$ capture, the mother spacecraft would have to cycle through the following steps:

Acquisition & Tour Design After the $\mathbb{RB}$s have ejected from the small body surface, the mother spacecraft would perform an initial acquisition and orbit estimation to support

[3] Simple simulation scenarios were designed to consider the deployment of this delivery vehicle: in principle, the output of the simulations in the previous sections was used as the initial condition of the delivery stage, and variations in the size of the *release fan* were investigated. The motion of the delivery stage in the vicinity of the small body was approximated by the restricted three-body problem subject to solar radiation pressure [21] as opposed to the dynamic model described by the system (3.1).

the design of an optimized chase tour[4].

Chase Once the initial tour has been designed, the mother spacecraft would begin a chase and capture sequence with the first $\mathbb{RB}$. The spacecraft would likely take intermittent observations as it closes in on the target, reducing orbit uncertainty and allowing it to safely approach to capture distance.

Capture & Re-Acquisition The spacecraft would zero-out the relative velocity with the $\mathbb{RB}$ at a very close distance, deploy the capture mechanism and physically grab the $\mathbb{RB}$. Once the $\mathbb{RB}$ has been successfully stowed, the mother spacecraft would acquire the next target and repeat the sequence. This may involve re-acquiring all targets and re-optimizing the entire chase tour.

The terminal parts of the chase and capture segments would need to be handled by an on-board autonomous navigation system (it may be desirable for such a system to handle other segments as well). Such a system would be responsible for taking observations of the target, converting them to tracking data, updating the relative navigation solution, and making it available to other on-board subsystems---all in real-time.

AutoNAV---a system with many of these capabilities---has been developed at JPL and flown on several missions, most notably on Deep Impact. Although as of this writing AutoNAV does not have all the abilities desirable for our architecture, no fundamental technical concerns have been identified that would prevent a future version of this system from delivering these capabilities.

The analyses presented below focus on the acquisition and chase segments; they are intended as a preliminary demonstration that chase tours can be designed that visit multiple $\mathbb{RB}$s for a reasonable Δv budget, and that at typical ranges the $\mathbb{RB}$s can indeed be acquired by the spacecraft.

3.6 Chase

A simulation was designed to assess the overall astrodynamic feasibility of a chase tour capable of capturing several $\mathbb{RB}$s from a typical solar system small body.

The conceptual scenario for the simulations is stated as follows: the mother spacecraft is off-set from a rotating small body by some initial distance. A swarm of $\mathbb{RB}$s has just finished collecting samples, and are ejecting off the small body with the lowest possible relative speed (nominally, the escape velocity of the small body). The spacecraft performs an initial acquisition of the $\mathbb{RB}$s as they eject, designs an optimized chase tour, and starts a chase sequence (cf. Figure 3.7).

We considered the following simplifying assumptions in the development of the simulation model:

Spacecraft An early finding of this study was that the Δv budget of any chase tour was dominated by a large relative velocity between the mother spacecraft and the small

[4]This tour is structurally similar to a *traveling salesman* problem, with the important distinction that it's nodes are dynamic.

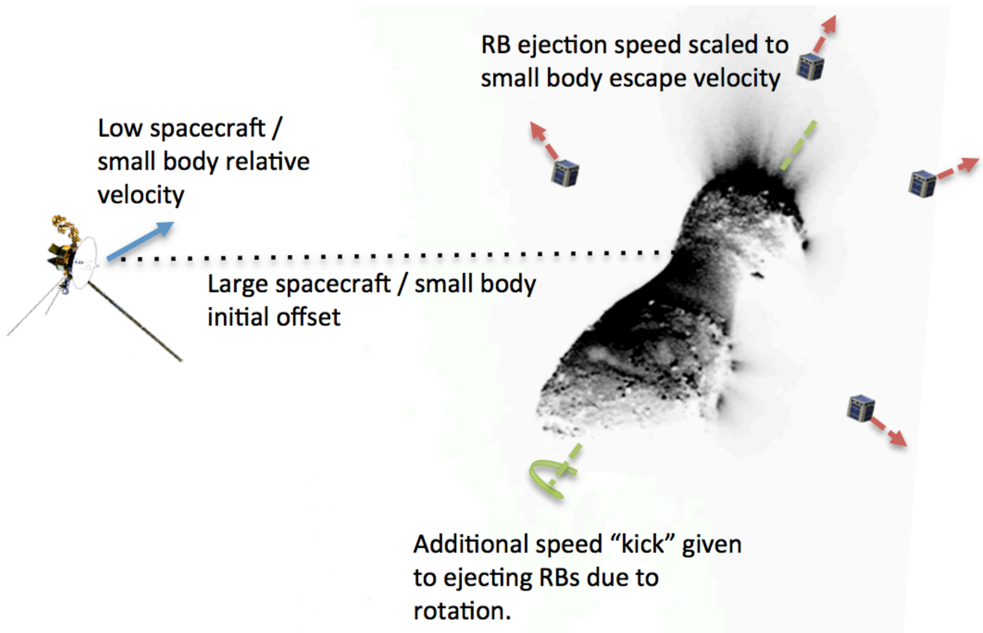


Figure 3.7: Chase Analysis Conceptual Scenario.

body. In order to remove this important source of variability, we assume a zero relative velocity as a starting condition, and a stand-off range of 10,000 km. The implication of this simplifying assumption is that the Δv budget for the transfer stage increases (cf. Table 3.2).

The simulation relied on a Lambert arc to estimate the Δv required to chase an individual $\mathbb{RB}$, which requires the selection of a time-of-flight (TOF) between the mother spacecraft and the $\mathbb{RB}$. The selection of the TOF represents a challenge: characteristic rendezvous Δv vs. TOF curves approached a minimum near thirty day transfer times, increasing exponentially as the TOF decreased. However, a notion of *reasonable tour length* needed to be developed to prevent multi-month chase tours from being the norm. For the current results, a compromise was reached by selecting a baseline acceleration value, and iterating the TOF so that the Δv / TOF converged to this acceleration. This value was developed by assuming a 600 kg spacecraft equipped with a continuous thrusting engine capable of generating a thrust of 0.15 N.

Small Body The small bodies explored in this study ranged in radius between 50m and 30km. Special attention was payed to the $<$ 5km radius bodies, as many bodies in this size range are particularly interesting. Typical rotation rates were randomly assigned to these small bodies, scaled according to their radius [20]. As described in §3.2, we assumed that the escape velocity of a small body measured in m/s was equal to the its radius in km.

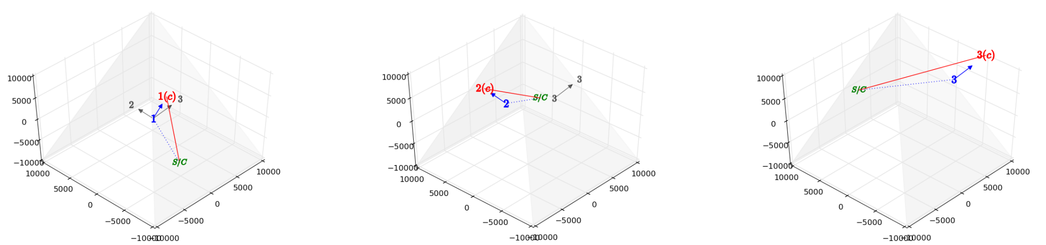
$\mathbb{RB}$s The small body rotation rate and escape velocity parameters previously discussed were key in determining reasonable ejection velocities for the $\mathbb{RB}$s. For the simulation, $\mathbb{RB}$s were allowed to randomly eject in any direction from the surface of the small body, with their relative velocity vectors directed radially and with a baseline magnitude equal to the escape velocity, and and additional Δv was added to account for the rotational effect.

A typical run of the simulation is depicted in Figure 3.8. The chase tour begins in Subfigure (a), with the mother spacecraft at an initial offset from the small body, and the $\mathbb{RB}$s (numbered 1 through 3) ejecting in random directions with random (but bounded) escape velocities.

The spacecraft acquires $\mathbb{RB}$-1 as it ejects from the small body; the location of $\mathbb{RB}$-1 at acquisition is depicted in the figure as the blue numeral **1**; the distance between the spacecraft and $\mathbb{RB}$ at this time---depicted by the blue dashed line---is termed the *detection distance*.

After acquisition, the spacecraft follows the chase trajectory depicted by the red solid line, and eventually captures $\mathbb{RB}$-1 at the capture location, depicted as the red **1(c)**.

In Subfigure (b), the spacecraft is now positioned at the capture location of $\mathbb{RB}$-1, and goes on to acquire, chase and capture $\mathbb{RB}$-2 in a similar manner. Subfigure (c) completes the tour with the capture of $\mathbb{RB}$-3.



(a) Ejection; capture of $\mathbb{RB}$-1. (b) Capture of $\mathbb{RB}$-2. (c) Capture of $\mathbb{RB}$-3.

Figure 3.8: Typical simulation rendezvous tour.

The results of running a Monte Carlo analysis on this simulation are summarized in Table 3.3, which presents the estimated number of $\mathbb{RB}$ captures possible for different small body and Δv budget combinations. For each small body class and Δv budget, a hundred independent tours were generated by randomly sampling a small body and swarm of $\mathbb{RB}$s as described above, and then following an non-optimal rendezvous tour until the Δv limit was reached. The number of $\mathbb{RB}$s visited, the maximum detection distance, and the length of the resulting tours were averaged, and those are the values reported in the table. The maximum detection distance generated here serves as the basis for the following discussion on $\mathbb{RB}$ acquisition.

The class of small bodies ranging in radius from 50 m to 5 km are particularly interesting. To this end, a modification of the simulation was created to investigate these in more detail. Figure 3.9 presents the simulation results for small bodies in this size range. As can be seen, the lower escape velocities of these small bodies allows for a sizeable increase in the number of possible $\mathbb{RB}$ captures for a given Δv budget.

Radius range, km	Δv **budget** m/s	$\mathbb{RB}$**s Captured**	**Max Detection Distance**, 10^3 km	**Tour Length** days
1--10	100	1	11	4
	200	3	11	9
	300	4	14	13
	400	5	19	17
	500	6	24	22
	600	7	29	26
	700	8	36	30
	800	9	41	35
	900	9	44	39
	1,000	10	52	43
10--20	100	1	11	4
	200	2	19	8
	300	2	30	12
	400	3	41	16
	500	4	50	20
	600	4	64	24
	700	5	76	28
	800	5	86	32
	900	6	97	36
	1,000	6	108	40
20--30	100	1	15	4
	200	1	28	7
	300	2	39	10
	400	2	61	15
	500	3	75	19
	600	4	94	23
	700	4	109	27
	800	4	126	31
	900	5	144	35
	1,000	5	161	39

Table 3.3: $\mathbb{RB}$ chase tour profiles for different classes of small bodies.

3.7 Acquisition

There are several methods which may be used to acquire and subsequently track the $\mathbb{RB}$s after they eject from the small body surface. *Active* detection schemes, which use transponders or other mechanisms mounted on the $\mathbb{RB}$s, sit at one end of the spectrum; they enable compelling possibilities, such as the ability detect $\mathbb{RB}$s regardless of lighting conditions and cross-reference specific $\mathbb{RB}$s to their sampling location impact sites. However, this capability increases the overall complexity of the $\mathbb{RB}$ design, thus driving up both mass and development costs. In contrast, for this study we adopted a *passive* tracking

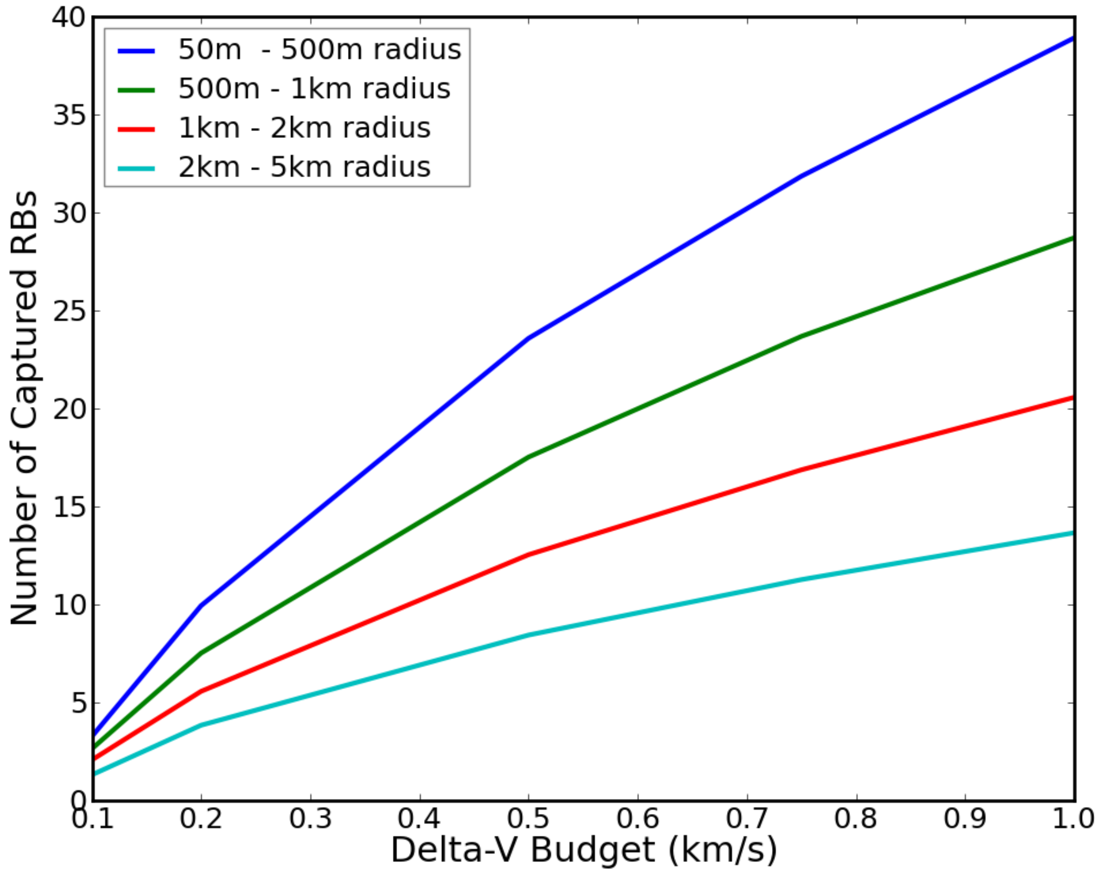


Figure 3.9: $\mathbb{RB}$s captured vs. Δv for small bodies with radii in the range 50 m to 5 km.

method; it places the burden for acquiring and tracking the $\mathbb{RB}$s on the mother spacecraft and its instrumentation.

The JPL OpNav Camera (ONC)---which has been flight-proven on missions such as the Mars Reconnaissance Orbiter---was used as the baseline optical navigation system. We performed a first order analysis to determine whether the post-ejection $\mathbb{RB}$s would be visible to the ONC at the maximum detection distances calculated in the previous section (from 10,000 km to about 160,000 km).

Admittedly, *detecting* or *acquiring* the $\mathbb{RB}$s in this context is not synonymous with being able to accurately track them. However, visibility is a first requirement to passive tracking, and we chose such criteria for our preliminary analyses. For the simulation we assumed a heliocentric distance of 2 AU and favorable lighting conditions.

Figure 3.10 shows the resulting $\mathbb{RB}$ signal-to-noise ratio (SNR) for the ONC as a function of the distance between spacecraft and $\mathbb{RB}$. The minimum threshold for a positive detection using the ONC happens at a SNR value of 3, which occurs at around 42,000 km for this baseline scenario.

We explored different alternatives to increase the SNR, like varying the detector aper-

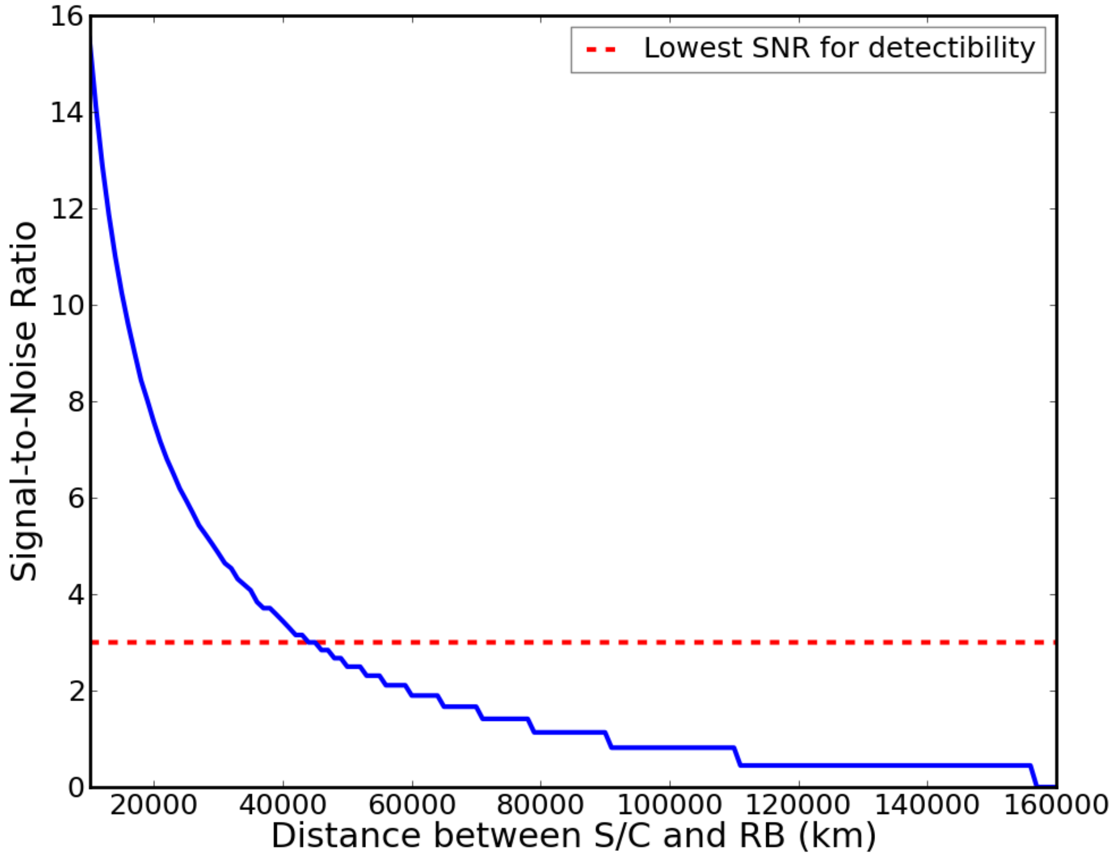


Figure 3.10: $\mathbb{RB}$ signal-to-noise ratio vs distance from spacecraft.

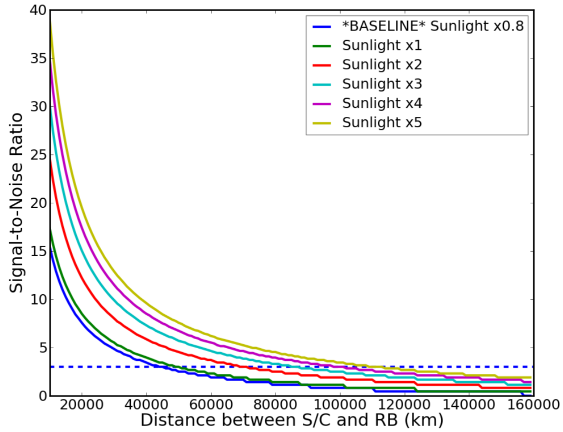
ture and efficiency, or increasing the $\mathbb{RB}$ brightness and exposure time. Our findings (cf. Figure 3.11) suggest that a relatively inexpensive mechanism to boost SNR would be to increase the brightness of the physical $\mathbb{RB}$, perhaps by mounting a low-power LED beacon signal.

3.8 Return

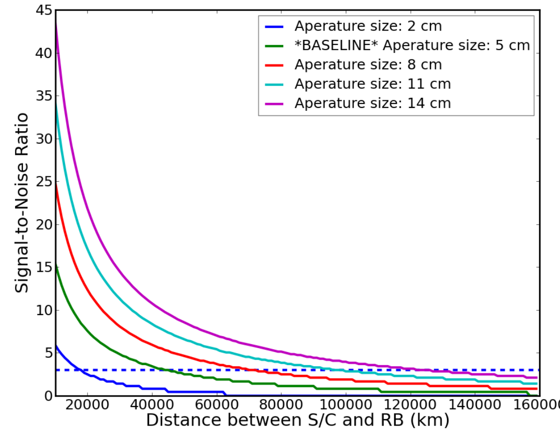
After a number of $\mathbb{RB}s$ have been collected, it is time to calculate a return trajectory to Earth.

This is a challenge fundamentally different from the classical *round-trip trajectory optimization problem* because the rendezvousing process introduces uncertainty on the initial conditions for the return leg. For this reason, it is important to consider---yet again---the statistical nature of the post-capture trajectory, and evaluate the possibilities for a mixed low- and high-thrust trajectory for Earth return.

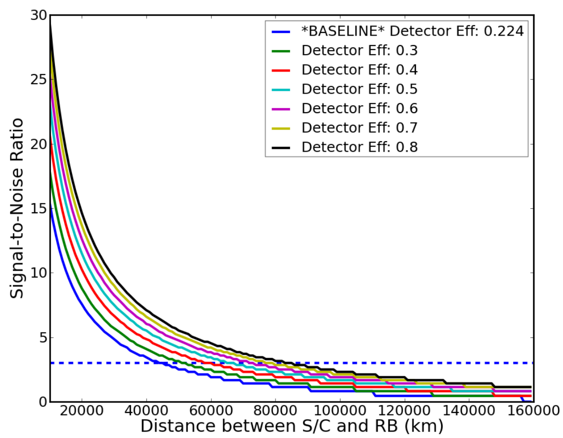
In addition, the return transfer has an important component: its duration will determine the nature of space vehicle required to ensure the thermal and radiation insulation (longer



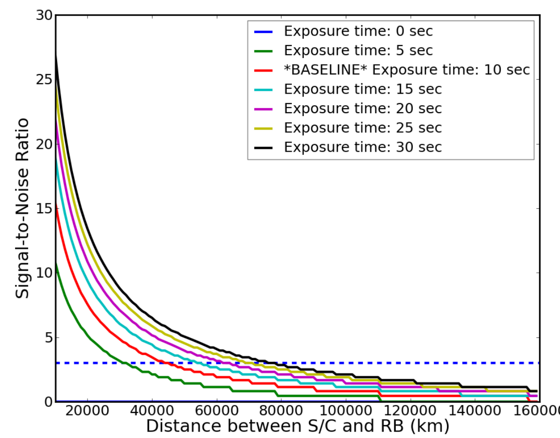
(a) Response to $\mathbb{RB}$ brightness.



(b) Response to detector aperture.



(c) Response to detector efficiency.



(d) Response to exposure time.

Figure 3.11: Response of signal-to-noise ratio to various optical system performance parameters.

transfers which fly close to the Sun will require more thermal insulation, or entire cryogenic subsystems)

We believe that the distributed nature of the $\mathbb{RB}$ architecture implicitly provides a mechanism for sample protection: the $\mathbb{RB}$s themselves can be visualized as protective containers. However, further considerations must be taken into account for sample transport, like cryogenic capabilities in the mother spacecraft to conserve volatiles and ice.

In addition, it is possible that the samples would need to be contained and subject to special handling requirements for some asteroids and comets [27] before being admitted into Earth.

3.9 ℝ𝔹 Preliminary Designs

We investigated several conceptual, preliminary designs for the physical ℝ𝔹s. More than developing a physical ℝ𝔹, our investigation in this area focused on determining the *criteria* to evaluate different concepts. We determined that four independent traits can serve as the basis for preliminary comparisons of different designs:

Compactness Given that the physical size of an ℝ𝔹 will restrict the number of devices which can be carried, the compactness criteria serves to eliminate designs whose bulk size or form factor renders them difficult to pack in a given delivery stage.

Simplicity Our concept is based on carrying a relatively large number of ℝ𝔹s. If their design is not *simple* (in terms of manufacturing, testing, stowing, deployment, and capture), mission reliability may decrease or costs may increase beyond manageable levels.

Capacity The physical amount of sample which can be collected into a single ℝ𝔹 will be a factor in determining the number of units needed to attain a given science objective.

Versatility The ability of a nominal design to undergo slight modifications to function in different environments, like porous vs. hard surfaces; ices vs. rocks; cores vs. regolith.

Figure 3.12: The *Squid*---a preliminary ℝ𝔹 proof-of-concept.

We have just begun exploring the nature of the ℝ𝔹 mechanical devices. Their physical design has become the core focus of subsequent concept development. At this point,

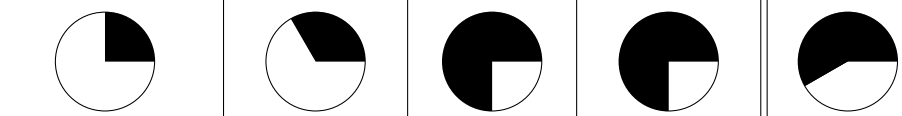
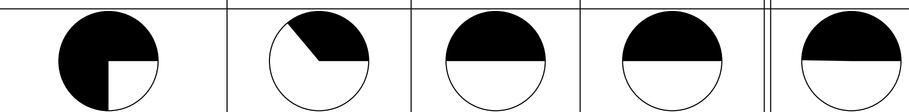
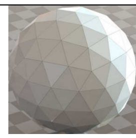
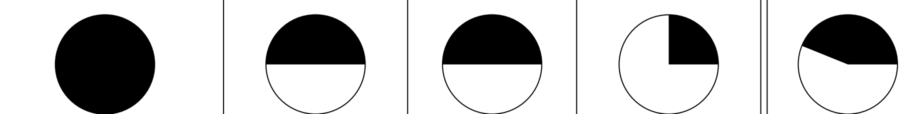
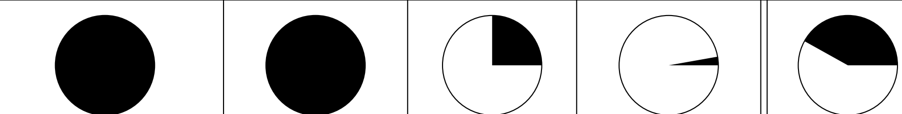
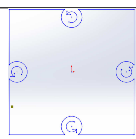
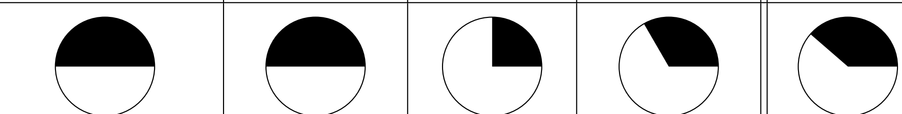
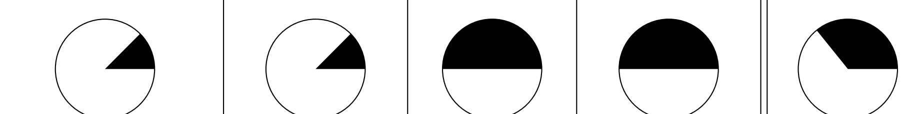
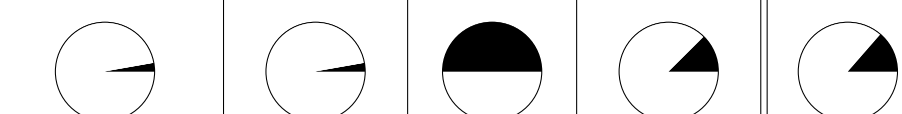
Name	Concept	Compactness	Simplicity	Capacity	Versatility	Overall
Squid						
Puck						
Geodome						
Pac-Man						
Scoop						
Pen						
Sawblade						

Table 3.4: Preliminary qualitative evaluation of various $\mathbb{RB}$ proofs-of-concept. The degree of fill denotes how well the concept satisfies the criteria (least; mid-way; most). The overall score weights the criteria as: **compactness**, $1/8$; **simplicity**, $1/4$; **capacity**, $1/8$, and **versatility**, $1/2$. The *Squid* design has been selected by our team to undergo further refinements.

we have selected the *Squid* design (presented in Figures 3.12 and 3.13) to undergo further refinements. In Table 3.4 we present an evaluation of different $\mathbb{RB}$ proofs-of-concept developed for this investigation, which highlights the qualitative rationale in selecting the Squid concept.

In addition to the basic concepts presented in this report, we are investigating different mechanical sampling methods designed for the varying possible small body terrain types. For instance, a crush-bite mechanism, with extremely high bite-pressures, could be used to crush and collect samples from hard rock surfaces; a core-sampling device may be more appropriate for surfaces of moderate hardness, such as loose collections of rock or water ice; a sticky-pad could be used to sample extremely soft surfaces; a magnetic pad for ferrous surfaces; and other equally exciting prospects. There is no fundamental reason to limit the baseline $\mathbb{RB}$ design to a single kind of sampler.

An important aspect of the $\mathbb{RB}$ design which has not been assessed in this investigation is the ejection mechanism. Somewhat ironically, the simplest conceivable ejection mechanism is *no mechanism at all*: depending on the encounter velocity and material composition of the target, the $\mathbb{RB}$s may spontaneously bounce off the body with enough velocity to escape its gravitational pull. While this fact is often encountered as a difficulty for small-body lander concepts, in our architecture it is welcomed as an advantage: we *expect* the $\mathbb{RB}$ to escape from the body.

On the other hand, it may be desirable to soft-land the $\mathbb{RB}$s on the surface, and some time later eject them into heliocentric orbit. The dynamic deployment of an airbag, for instance, could be used to launch the $\mathbb{RB}$ off the surface. If the airbag were made of a highly reflective material, it could be later useful for passive tracking purposes (as detailed in Figure 3.11).

Soft-landing the $\mathbb{RB}$s on the small body surface would enable a staggered ejection strategy, in which $\mathbb{RB}$ ejection may be triggered either by a timer or a signal from the mother spacecraft. Staggered ejection would allow more $\mathbb{RB}$s to be collected for a given Δv budget, as it eliminates the drift problem of simultaneous ejection. Also, a soft-landing of many $\mathbb{RB}$s may enable the distributed, in-situ characterization of the small body. Equipped with the right sensors, the $\mathbb{RB}$s could spend their time on the surface collecting simultaneous measurements from many different locations, enabling exciting new scientific possibilities.

Mature concepts collectively known as *surface hoppers* have been developed; they exploit the low-gravity environment of small bodies for mobility purposes. We aim to leverage the wealth of information available for such concepts [22, 28, 29] during the analysis of possible ejection mechanisms.

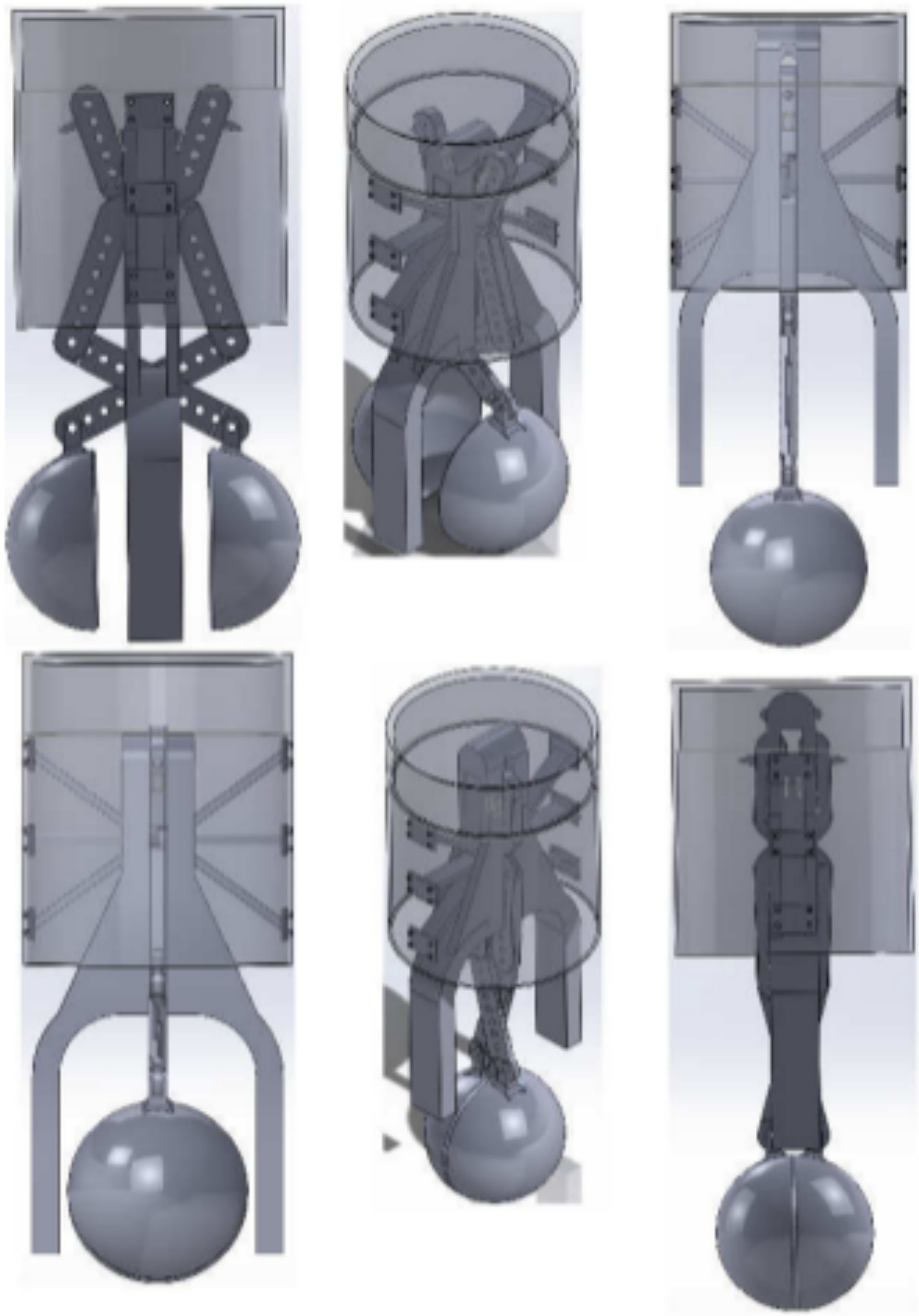


Figure 3.13: Different views of the *Squid*, a preliminary $\mathbb{RB}$ proof-of-concept.

4 Conclusions and Future Work

Our Phase-I investigation focused on assessing the astrodynamic feasibility of the Regolith Biter architecture. Briefly, we set out to confirm that there are a large number of interesting, accessible small bodies, and the cost of capturing the $\mathbb{RB}$s after sample collection was not prohibitive. In the course of pursuing these analyses, we also identified key mission critical technologies and drivers. In addition we developed preliminary $\mathbb{RB}$ proofs-of-concept. Based on the results of our analyses, we observe that:

- A wide variety of small bodies exist that could be targets for a sample-return mission based on the $\mathbb{RB}$ architecture.
- The Δv and thrust requirements are such that a dual-mode propulsion system would be required: a high-efficiency, low-thrust propulsion mechanism for the transfer between Earth and small body (for both legs of the trip), and a high-thrust, chemical propulsion system for the capture of the $\mathbb{RB}$s after their ejection from the small body.
- Astrodynamically plausible $\mathbb{RB}$ capture tours were found for a variety of Solar System small bodies, under a wide range of perturbing conditions; for most of these tours, several $\mathbb{RB}$s could be captured at a reasonable Δv budget, and for smaller bodies the number increases considerably.
- The optical tracking of the $\mathbb{RB}$s is feasible with technologies which are either readily available or close to maturing within the next decade.

During the investigation, we also developed basic, preliminary computer-aided designs of the physical $\mathbb{RB}$s to serve as proof-of-concept and visualization aid. The investigation also enabled us to to identify key technologies and drivers critical for further development.

In conclusion, our findings suggest that the physical foundation of the $\mathbb{RB}$ architecture is astrodynamically sound, and exhibits robustness to variations in size and rotational state of the target body.

4.1 Future development

We consider it a great privilege to have been awarded funds to develop a highly innovative concept with a very low level of technological readiness. At the forefront of our minds throughout this study was to favorably position these idea for future development.

And while the Phase-I investigation established the baseline feasibility of the Regolith Biters concept (it is viable from an astrodynamic perspective and is devoid of unattainable physical or technological leaps), it also raised a number of new questions to be answered; this avenue is actively being pursued, and a proposal has been submitted to continue the development of this concept during the NIAC Phase-II Program; the investigation would focus on three priorities:

$\mathbb{RB}$ **Development** Provide a mechanical $\mathbb{RB}$ design around which to pivot the overall concept. This design would enable us to provide a specific, quantitative measurement of expected scientific return, like amount and nature of the sample (e.g., its depth, maximum grain size, containment of volatiles, and others), and narrow the technological requirements for the spacecraft, propulsion subsystem, delivery vehicle, and autonomous tracking and navigation subsystems.

$\mathbb{RB}$ **Capture** Address the specific $\mathbb{RB}$ tracking, chase, and capture concern. Determine a specific *flight envelope* relating the maximum sensing distance to the available Δv and control authority of a specific autonomous navigation solution.

Mission Scenario Develop a baseline mission scenario with specific requirements on launch period, target small body, and expected scientific return. Such reference mission would enable us to compare our concept to related technologies on a specific basis, and solidify our arguments for further development.

In addition, there is a highly correlated trade-space of critical technologies that are necessary to further the development of our concept. These technologies can be divided into two broad categories (cf. Table 4.1): those with a *generic scope* (i.e., broad-based general application across many domains), and those with an $\mathbb{RB}$ *scope* (i.e., specific to the $\mathbb{RB}$ architecture).

Generic Scope	$\mathbb{RB}$ **Scope**
Propulsion system efficiency	Design of delivery stage
Autonomous chase & capture algorithms	*Biting* or sampling mechanism
Optical instrument sensitivity	Surface ejection mechanism
Miniaturization of space hardware	Sample handling and transport
etc.	

Table 4.1: Critical concept technologies divided by scope.

In tackling these outstanding technological challenges, we would follow a two-pronged approach: for those with technologies with generic scope, we would limit our effort to articulating the dependencies of the $\mathbb{RB}$ architectures on such technologies, and describe what advances we are expecting to obtain from industry through a detailed technological roadmap. For those technologies with $\mathbb{RB}$ scope, we would focus our efforts into advancing the technology readiness level. By focusing our efforts on the $\mathbb{RB}$ scope technologies while leveraging the work of others on the generic scope technologies, we hope to rapidly advance the concept *as a whole*.

We believe that our concept fulfills the core requirements of the NIAC Program: an exciting, unexplored, and credible aerospace architecture. We aim to compound the investment made by the NIAC Program into our architecture by focusing the scope and maturing the technology to an extent that it becomes a compelling architecture for subsequent development and industry partnership. Reflecting on the capability of our concept and our team to attain this goal, we can enumerate five main reasons why we believe we stand on a unique opportunity:

Capability Based on our preliminary investigations, we believe that no other architecture for sample-return and distributed characterization of small bodies offers the ability to investigate the most interesting prospects: highly active comets, fast-rotating bodies, and binary systems.

Viability Our Phase-I investigation suggests that the concept is viable from an astrodynamic perspective, and we did not identify fundamentally unattainable physical or technological leaps required for overall implementation.

Compatibility The technological advances required to implement our concept are aligned with the current technological roadmaps, and our architecture would capitalize on those advances without a need to directly fund them; it does not require a different prioritization or investment level for its general technical components.

Symbiosis Our concept came to life at the historical moment when NASA's ambition to investigate small bodies in general---and return samples in particular---aligned with commercial ventures aiming to mine such bodies. Such rare simultaneous compatibility promises to spark effective partnerships.

Timeliness We believe the $\mathbb{RB}$s are advanced enough to be novel and exciting, while maintaining a foundation on technologies which can enable an imminent realization. While we are proposing an advanced concept at least 10 years away from possible implementation, we directly aimed to strike a balance between *vision* and *pragmatism* in the interest of demonstrating a radically new technology in a comparatively short period of time.

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