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STRATEGIC MAP FOR EXPLORING THE OCEAN-WORLD ENCELADUS

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Among the many "ocean worlds" of our solar system, Enceladus appears unique in its combination of astrobiologically relevant, exploration-worthy attributes: extensive liquid-water ocean with high-temperature hydrothermal activity, containing salts and organics expressed predictably into space. The Enceladus south polar plume allows direct access to telltale molecules, ions, isotopes, and potential cytofragments in space. Plume mass spectroscopy and sample return, in situ investigation of surface fallback deposits, direct vent exploration, and eventually oceanographic exploration can all be envisioned. However, building consensus to fund such ambitious exploration hinges on acquiring key new data. A roadmap is essential. It could start with cost-capped onramps: 1) flythrough analysis of the plume, following up on *Cassini* measurements with modern instruments; 2) sample return of plume material for analysis on Earth. A methodical mission sequence in which each step depends on emergent results from prior missions would push in situ oceanographic exploration into the second half of this century. Even for this scenario, prioritization by the next planetary Decadal Survey would be pivotal.

I. INTRODUCTION

Cassini discovered tidally-modulated jets emitting salty water from the interior of Saturn's small moon Enceladus.[1-3] It gravitationally inferred a water reservoir of regional extent underlying the jet source,[4] directly measured salts and organic species in the resulting plume,[5] determined that the plume is the source of the long-lived E-ring around Saturn,[6] determined that the nanosilica particles comprising the ring originated in a high-temperature hydrothermal system on the Enceladus seafloor,[7] and found that the ocean must be global.[8] Even though we do not yet know the longevity of this hydrothermal activity, Enceladus nonetheless meets today's textbook conditions for habitability.[9] And because its well-mapped plume expresses ocean material far into space where its composition can be sampled directly without landing, Enceladus is one of the most promising places in our solar system to search for evidence of alien life. Using Enceladus as the exemplar, this paper proposes a template for stepwise Ocean World scientific exploration that could determine in this century how singular Earth life is.

II. EPOCHAL MOTIVATION

Discovery of life elsewhere is one of the defining challenges of our time: an existential human question, now finally addressable. It shares with the mid-20th century's Project Apollo a kind of atavistic power to compel action – something humankind has fantasized about down through the ages, historically so impractical that it became mythologized, but which technological progress has now brought within our reach. Indeed, it is difficult to imagine how our 21st-century space-science enterprise could escape the grip of this quest: to finally know whether we are alone, and if so, why.

In an ironic turn that evinces humanity's readiness to tackle this Big Question, contemporary space flight capabilities have put us on the cusp of pursuing it in three utterly different ways.

The first approach is through exoplanet spectroscopy – using telescopes to collect spectra of the light reflected by and transmitted through the atmospheres of planets orbiting other stars. By doing this for hundreds or thousands of exoplanets, we can establish chemical fingerprints of these worlds' atmospheres, seeking evidence of nonequilibrium conditions best explained by a biosphere.[10]

The second approach is through increasingly sophisticated exploration of Mars. Via roving avatars, humankind has already determined that early Mars had clement conditions, including long-lived bodies of flowing and standing water[11] that were chemically compatible with habitability.[12] Today Mars is dry and frozen, but nonetheless taunts us with indications of subsurface environments that might still be habitable today.[13] The hope of finding extant, tenacious life on Mars is not unreasonable. But even evidence of extinct life would be an epochal discovery; we have "followed the water," and now are ready to follow clues of habitability including organics and other traces left by life.

The third approach was not even imagined before the startling discoveries of the *Voyager*, *Galileo*, *Cassini*, and *Dawn* projects revealed that our traditional definition of a stellar habitable zone was far too simplistic. Despite having only eight major planets, our own solar system has by some counts at least ten "ocean worlds" (Figure 1). Some, like Mars and Ceres, are now apparently frozen. Not enough is known yet about Venus to determine whether it ever had oceans, and

Fig. 1: ***Our solar system may contain ten Ocean Worlds****, some now frozen but many hosting enormous liquid seas. Compared to scale: three inner solar system bodies (top center and left), three moons of Jupiter (lower left), and four moons of Saturn (lower right).*

today only Earth has surface water. But the outer solar system is rich in moons of the giant planets, some of them quite large. Magnetometry, imaging of libration, plumes, and surface features, and gravity measurements combine to identify many of these places as worlds that could harbor oceans inside their ice shells: Europa, Ganymede, and perhaps Callisto at Jupiter;[14, 15] and Titan, Enceladus, and perhaps even Mimas and Dione at Saturn.[16, 17] Today's thinking subdivides the Ocean Worlds further: oceans now gone; oceans evidently in contact with silicate rock; oceans hypothesized to be geologically long-lived; ice shells that cycle oxidizing chemicals into the oceans below; oceans that express material into space via plumes.

Alas, the three approaches to the Big Question will compete for limited resources in the coming decades. Together they comprise a surfeit of potential opportunity that will exceed reasonable expectations for the space science budgets of NASA and its partner agencies in the coming decades. Each is aligned with a different sector of the science community, so its advocates will work hard to differentiate it from the others.

Among the three, the first has the advantage of statistical impact throughout the galaxy, due to the thousands of exoplanets discovered even already. But only the latter two, being within reach of direct, in situ exploration, are amenable to converting inference into confirmation through progressive investigation, and analysis of returned samples, in this century.

III. A PROGRAM WAITING TO HAPPEN

The newly appreciated diversity of potentially habitable environments "in our own backyard" suggests a programmatic challenge to shape a strategic Ocean Worlds Exploration Program akin to the traditional Mars Exploration Program, which would systematically explore and understand these places and, through them, learn the limits of life in our solar system.

A seemingly inevitable result of such a momentous discovery as the confirmation of life elsewhere would be to trigger a century of intensive in situ exploration to learn more: to characterize its biochemistry, extent, history and origin, and the nature of one or more alien ecologies. (Unlike the Apollo "Moonshot," discovery of life is not likely as susceptible to a "been there, done that" abandonment.)

But unlike the Mars Exploration Program, ocean-world exploration enjoys neither an integrated program yet nor the benefit of being an eventual target for

NASA's human exploration program. Mars is unique in this regard: it is both a destination of keen astrobiological interest and a place that humans could go someday – either to participate in the astrobiological investigation, or to follow it, settling and eventually terraforming the place by colonizing it with Earth life.

However, as with Mars, potential can precede proof as an effective programmatic motivator. After all, Mars has continued to hold the attention of scientists, bureaucracies, and the public – who sustain funding because of the hope of habitability – even though no evidence of life has yet been found. The Mars Exploration Program precedent demonstrates that an optimistic spirit of exploration can win a line-item program in the NASA budget.

This means that a prospective assumption that life does exist in liquid-water environments elsewhere in the solar system could be a valid planning tenet. Aiming for life would focus resources toward a multi-mission "grand mission" to find it, confirm it, and study it. Following this path, we would come to understand how common or rare the emergence of life actually is, given apparent habitability. Using this approach, as is already being done for Mars, we could conceive, gain commitment for, and execute a coherent Ocean Worlds Exploration Program.

IV. ROADMAP FOR EXPLORING AN OCEAN WORLD

The classic sequence of scientific enterprise starts with serendipitous discovery through observation of nature, and proceeds through logical investigation, including experimentation, to scientific understanding. This sequence has already been triggered for ocean-world exploration by discoveries made by multiple outer solar-system missions: *Galileo* imaged and made fields measurements at Jovian moons upon multiple encounters; *Cassini* has imaged and made fields, particles, and gas measurements at Saturnian moons, and *Dawn* has found unanticipated, extremely bright deposits in multiple locations on the dwarf planet Ceres. Thus the past 40 years of planetary exploration have opened several paths for an ocean-worlds program to pursue. As this landscape unfolds, critical thresholds of knowledge define key branch points for strategic planning, chief among them: 1) validation of habitability by the contemporary definition; 2) indications of biosignatures; and 3) confirmation of life.

Figure 2 shows a candidate roadmap for progressive exploration of the ocean-world Enceladus. Through the center from left to right runs the classic sequence that starts with the act of exploring and culminates with the state of scientific understanding. At a minimum, a new environment can be characterized and understood (as

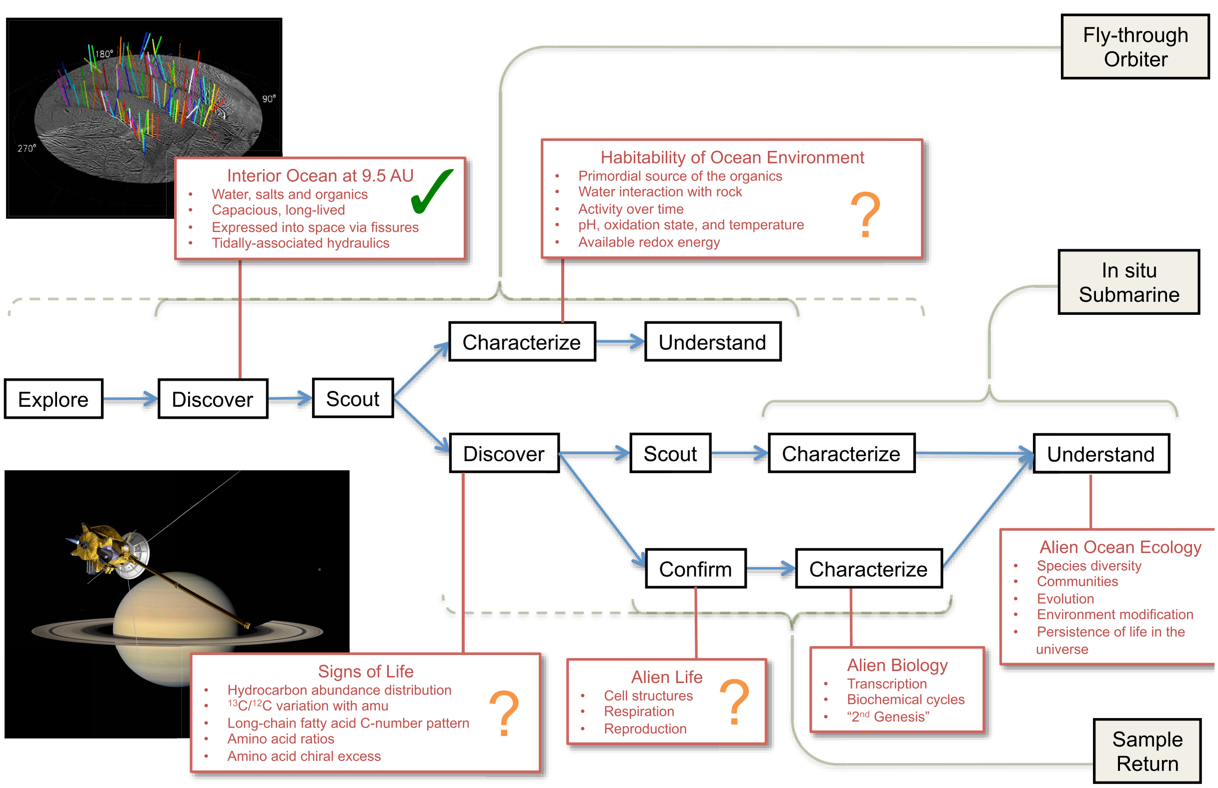

Fig. 2: ***Roadmap for exploring the ocean-world Enceladus*** *calls for increasingly sophisticated exploration techniques if and when promising interim results emerge.*

we have done with dozens of moons in the giant-planet systems) by building predictive models that reconcile observations with causal physics and chemistry.

Should scouting discover biosignatures, a new branch opens to pursue those findings especially. At a minimum, this branch leads to understanding how the discovery might be mimicking an actual biosignature, which would inform our understanding of the limits of life in theoretically habitable environments. But alternatively, it could trigger yet another branch: one to confirm the presence of life and pursue the understanding of an alien ecology.

Most of the basic sequence can be executed to a great degree by flyby and orbiter missions. Indeed, *Cassini* has exemplified this approach already by performing both remote-sensing and "flyby in situ" measurements throughout the Saturn system (and the Europa Multiple Flyby mission takes this approach for the next step of exploring that ocean world). We can say that *Cassini* discovered a habitable ocean at Enceladus: the interdisciplinary findings enabled by its flagship payload have confirmed all the attributes that make Enceladus so compelling an ocean-world destination.

Frustratingly though, the two key *Cassini* instruments that conduct direct chemical measurements in the Enceladus plume – the INMS (Ion and Neutral Mass Spectrometer) and CDA (Cosmic Dust Analyzer) – cannot themselves do more than they have done already. Designed in the early 1990s for a general exploratory mission, they lack the mass range, sensitivity, and molecular-fingerprint discrimination to follow up their own results with greater finesse. Their capabilities are now far outstripped by today's state of the art in space flight instruments. A flyby-orbiter mission following the path that *Cassini* has blazed, but with only two instruments – modern mass spectrometers for gases and particles – could answer the question marks on both main branches of the roadmap: quantifying chemical tracers of the habitability of the ocean environment; and directly conducting multiple, independent chemical tests for the presence of life.

Characterizing habitability would mean determining how hospitable the liquid-water environment inside Enceladus is today, and has been through time. In particular, the acidity of the water, its temperature, its oxidation state, and the presence of a redox "battery" that could power biological reactions can all be inferred via ratios of key chemical species in the plume. Fingerprinting high molecular-weight organics could determine whether the molecules *Cassini* has detected arose from abiotic synthesis, or instead evidence macromolecules expected in biological systems. The investigative thread would seek in the end to understand the geophysical conditions that prevent environments from being truly habitable.

Direct tests for life are also possible for a plume flythrough mission. Mass spectrometry of plume constituents could discriminate types of preferential chemistry that are a signature of all living systems we know. Are organic hydrocarbons including amino acids prevalent in ratios expected from thermodynamics, or indicative of selective synthesis and uptake? Does the abundance profile of carbon-atom number in long-chain fatty acids follow a Poisson distribution, or evince a pattern implying synthesis from common-unit building blocks? Is ^{12}C preferentially incorporated into the organics measured? Does the abundance distribution of amino acids match abiotic synthesis (e.g., with glycine dominating), or is there a family of amino acids both more common and more evenly represented? If such a family is discerned, is it different from the 22 amino acids used by terrestrial life? Finally, is there a significant chiral excess among biomolecules, and is it the same as on Earth (L-amino acids and D-sugars)? Results from a plume analysis mission could range from strongly negative (inconsistent with the presence of biological activity), to ambiguous, to strongly positive.

Should ambiguous, or circumstantially positive, or inarguably positive biosignature indications be found, programmatic focus would inevitably shunt onto the life-confirmation path of the roadmap, and thus to the retrieval of samples to Earth for detailed analysis in terrestrial laboratories. Plume samples could be returned either before, or along with, samples of fallback "snow" collected from Enceladus' surface, or even from within sulcus vents. Unequivocal confirmation of life would hinge on structural evidence (e.g., cytostructures like cell walls, organelles, and chromosomes), and finding intermediate products in biochemical cycles for respiration and reproduction. Understanding the genetic transcription process would follow, including the determination of whether Enceladan life shares the same chemical language as Earth life. Every step of this investigative chain would yield important results.

Beyond the headlines, confirmation of Enceladan life would open a final branch on the roadmap: to gain an understanding of the biogeochemical cycles that link multiple species in an ocean ecology, and of the diversity and tenacity of life that may occupy multiple ocean niches. This ultimate step of the roadmap would require in situ cave and ocean exploration: autonomous spelunking and submarining under the ice crust of a faraway moon.

V. BRIDGING FROM CASSINI TO THE FUTURE

Clearly this ambitious roadmap spans many decades of potential exploration, several key decision points, many classes of mission, and technologies ranging from today's state of practice to capabilities far beyond what we know how to do. The technologies and mission types best able to address each step of the roadmap matter greatly, because their readiness and cost will determine the feasibility and pace of those steps.

Cassini represents both the catalyst for a methodical Enceladus exploration roadmap and our only benchmark of flagship-class capability for a Saturn-class flyby orbiter (Figure 3). With its large payload, radioisotope power, and long life, *Cassini* has conducted five distinct investigation "missions" in the Saturn system (Rings, Magnetospheres, Icy Moons, Titan, and Saturn). Flight-system development alone cost about $1.5B in early-2000s dollars, more than twice the development budget of a New Frontiers mission today. Among the many breakthrough capabilities *Cassini* demonstrated were: 1) multi-encounter tour operations at Saturn, where the gravity of Titan is used to "crank" a Saturn-centric spacecraft orbit to achieve precise, targeted close encounters with moons; 2) the navigation required to safely perform 25-km altitude flybys of a 500-km moon at 9.5 AU; 3) INMS and CDA, 1995's best spaceflight ion and neutral mass spectrometer and cosmic dust analyzer (mass spectrometer), respectively. For that investment (and in the context of dozens of other unforeseen major discoveries), *Cassini* detected the Enceladus plume via remote sensing, then scouted it in situ by flying through it several times, yielding: gross characterization of the environmental constraints on plume production; a detailed description and model of plume attributes; and compositional measurements that point the way forward.

The key planning question for our generation is how to build a practical bridge – of investigations and inferences, of capabilities to enable them, and of advocacy to invest in those technologies and missions – from the discoveries of that 1990s flagship, across the unknowable future of the next quarter century to yield, sometime later in this century, ambitious results from the in situ exploration of a remote, potentially life-bearing ocean.

Fortunately, the roadmap has an easy onramp that does not require programmatic commitment by Administrations or acts of Congress. The next two investigations (Habitability of the Ocean Environment, and testing for Signs of Life) could be accomplished using only a Discovery-class (i.e., ~$0.5B) mission completely within NASA's current authority to commit (Figure 4). In this example, a solar-powered flight system would navigate as *Cassini* has done, through the well-characterized Enceladus plume. Equipped with best-in-class gas and particle mass spectrometers (essentially, modern versions of *Cassini*'s INMS and CDA instruments), an Enceladus Plume Investigation could determine the habitability of the ocean environment and source of its organics; and perform several direct, independent tests for the presence of life. All the necessary technologies are ready today for project development. One additional capability that could significantly augment such a mission is capillary micro-electrophoresis, a wet-chemistry technology currently nearing TRL 4 (technology readiness level 4) that could measure chirality of bio-relevant molecules in the plume.[18] The dollar-sign emblem in the figure qualitatively represents how the resources required for this mission contrast with the *Cassini* benchmark.[19]

Missions to retrieve Enceladus material for detailed analysis on Earth – certainly necessary for the confirmation branch of the roadmap – might use a range of approaches, depending on programmatic ambition and budget appetite, technological readiness, and the emergent state of knowledge about Enceladus.

At the less complex end of this range, an approach analogous to the *Stardust* mission that returned coma samples from comet Wild 2 in 2006 could be used to return samples directly from the plume. Figure 5 illustrates such a mission concept; this example is based on radioisotope power, as was *Cassini*. This mission would capture into Saturn orbit, then fly through the Enceladus plume multiple times at moderate velocity,

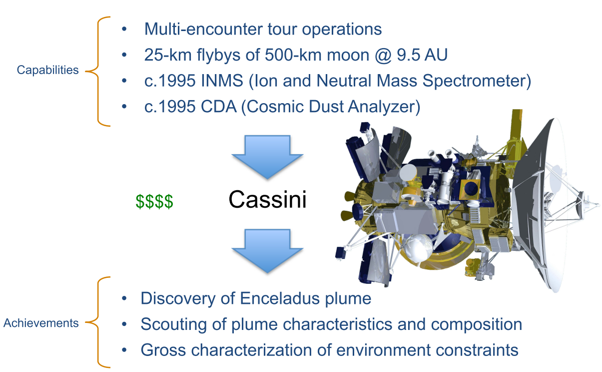

Fig. 3: ***Cassini* capabilities yielded the key achievements** *to trigger an ocean-world exploration roadmap for continued investigation of Enceladus.*

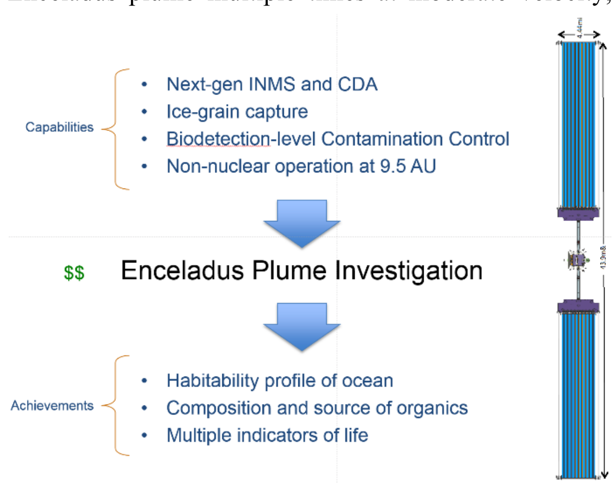

Fig. 4: ***Enceladus Plume Investigation****, a Discovery-class, multi-flyby orbiter, would use modern instruments to directly measure the habitability and biotic state of the ocean.*

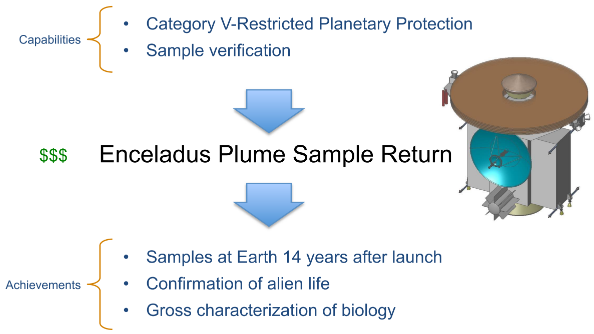

Fig. 5: ***Enceladus Plume Sample Return*** *mission concept embodies the lowest-cost approach for a first mission that might confirm the presence of Enceladan life.*

exposing an aerogel-equipped collector to capture dust and ice grains. The collector would be sealed for the return trip to Earth, the sample return capsule retrieved upon direct entry, and the collector opened in a purpose-designed bio-containment facility for initial evaluation. While conceptually straightforward, and built upon the *Stardust* and *Cassini* precedents, such an Enceladus Plume Sample Return investigation would depend on capabilities not yet developed. The most cost-uncertain of these are: 1) the means to verify – in flight – successful capture of sufficient sample to justify departing the Saturn system for home, the integrity of its containment, and the bio-cleanliness of the unsealed parts of the sample return capsule; and 2) satisfaction of international planetary-protection obligations for the design and operation of such a mission, classified as Category V-restricted. These two challenges alone elevate the concept to at least a New Frontiers-class development (i.e., ~$1B).

In return though, Enceladus Plume Sample Return would deliver ocean-material samples to Earth just 14 years after launch (even faster if launched on SLS, the Space Launch System). Such material, examined in the world's best laboratories with instruments built two decades from now, might yield irrefutable evidence, and initial characterization, of alien life.

Even though *Cassini* has directly measured the plume's particle density, the microbial biomass that might be collectible from within these grains is highly speculative. By the time a sample-return mission could be selected, scientific consensus might instead favor more elaborate types of sampling. Two such classes of mission might be: 1) landing on the snowy plains, where plume fallback material is deposited in great abundance, for collection and subsequent launch to Earth; and 2) landing near a sulcus, followed by roving to and rappelling into a vent to collect wall deposits, retrieval to an ascent vehicle, and launch to Earth. The contrast among the three types of sample-return missions described here – plume flythrough, surface snow collection, and vent descent – is evident in terms of implementation complexity, but not yet in terms of science value. Thus a useful inter-comparison of their value propositions warrants significant community discussion (which the Enceladus Plume Investigation mission described above could significantly inform).

The roadmap culminates in a vision easy to imagine but very challenging and costly to attain (Figure 6): interior exploration of a remote ocean world. What lies beneath the visible surface of Enceladus? Presumably, a complex and fascinating "plumbing system" of vents and chambers connects the surface to a vast, dark ocean trapped between a thick ice crust (presenting an inverted submarine landscape on its underside) and a sea floor spotted with hydrothermal vents where sea water reacts with silicate rock to create nanosilica grains that end up comprising the E ring enveloping Enceladus' orbit. Diverse types of autonomous machines, delivered through kilometers of icy crust, could study the vents, chambers, ocean-crust interface, open ocean, and sea floor of this potentially habitable world.

The technologies required to implement such a vision far exceed our resource capacity today. Some needs are obvious: efficient radioisotope power, and safe landing near potential access points. But in situ exploration of the ocean worlds, including Enceladus, would require five key capabilities as yet undeveloped: 1) assured access into an ocean through kilometers of low-temperature ice; 2) mobility and navigation inside an unmapped environment consisting of water, ice, and rock; 3) surveys and experiments appropriate for what might be found there; 4) fully autonomous investigation decision making without any option for consultation or intervention from Earth; and 5) extraction of findings for transmission back to Earth.

Taken out of context and compared to today's capabilities, such challenges may appear insurmountable and might remain untackled even despite their evident benefit for autonomous terrestrial oceanography and cryosphere science. Only the allure of epochal discovery – as triggered by compelling results from

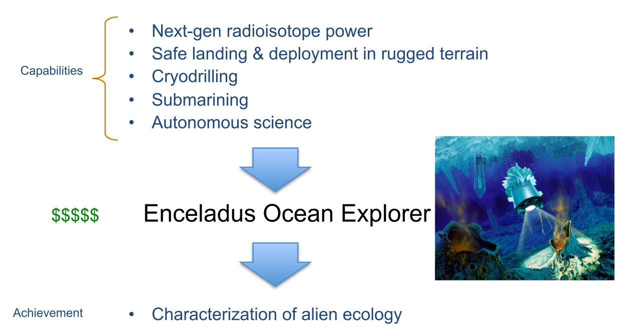

Fig. 6: ***Enceladus Ocean Explorer*** *concepts culminate the roadmap. Fully characterizing an alien ecology would require significant and successful investments in technologies for autonomous, remote science.*

precursors like those described above – could likely justify the commitment, investment, and persistence needed to bring them to flight readiness. Thus another takeaway from the roadmap is that it would be prudent for each precursor mission to prototype and progressively demonstrate advanced technologies to enable subsequent missions. For example, next-generation instrumentation for in situ analysis could be flown as technology demonstrations on sample-return missions; if successful, their science results could enhance contextual understanding of the sample sites while generating essential technical performance data needed to finalize designs for future missions.

VI. GETTING TO YES

Some enthusiasts might hope that *Cassini*'s tantalizing findings at Enceladus could lead directly to a programmatic commitment to invest in technologies and missions to explore its ocean in situ (Figure 7). However, the high but uncertain cost and unpredictable readiness schedule for such ambitious exploration render this path vanishingly likely. Instead, the fastest path could go through both of the aforementioned precursor investigations, in parallel. Since even the simplest type of sample-return mission requires a New Frontiers-class investment, but since the current (*Vision & Voyages*) Decadal Survey does not call for such a mission for either of the next two New Frontiers competitions, and since NASA has no other programmatic opportunities in this size class, advocates have but three options: 1) embark on an Enceladus Plume Investigation mission via the Discovery program to jumpstart the roadmap; 2) get Enceladus Plume Sample Return included in the New Frontiers framework by the next Decadal Survey (for the 2023-2032 decade); and/or 3) include sample-return and/or Enceladus oceanography in the flagship-class priorities of the next Decadal. There are no practical alternatives; non-US opportunities (e.g., Cosmic Vision in ESA (European Space Agency)) have an even lower probability of prioritizing such missions, or insufficient

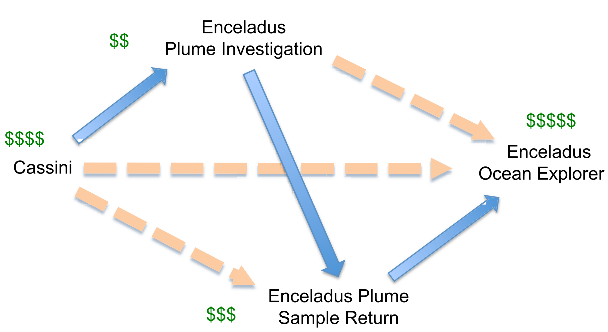

Fig. 7: ***Progressive investigation of the ocean-world Enceladus*** *could take multiple paths. "Indirect" paths through sequential missions may be the fastest in fact.*

resources, or both.

An important consequence of this result is the need for programmatic planning to balance strict scientific rationality (i.e., awaiting promising results from in situ plume investigation prior to even starting development of a sample-return mission, and then awaiting its findings back on Earth before investing in technologies that could enable in situ submarine exploration) against a sense of urgent scientific curiosity and adventure (e.g., creating an Ocean Worlds Exploration Program that simultaneously opens multiple parallel paths on a systematic roadmap). The former basis is most easily defensible – indeed it is the approach being used with Europa – but would take most of this century to execute fully; the latter might take only half as long but would depend on a willingness – as Mars exploration has enjoyed – to "aim for the fences" and presume that Big Question answers await us inside the ocean worlds. A complete science cycle for Mars (from prior-mission results, to next-mission development, to operations, to new results) takes about a decade; since a similar cycle takes at least twice that long for the outer solar system, planners might find that the balance between rationality and adventure needs to be different for an Ocean Worlds Program.

An optimistic path through Figure 7, should the 2023–2032 Decadal prioritize Enceladus Plume Sample Return for New Frontiers, and if it were competitively selected, still would return samples to Earth no earlier than 2038 if SLS launch were allowed, or 2045 if not (Figure 8).

The figure overlays extrinsic events (the cadence of US presidential Administrations and the aging of today's scientific leadership), governing events (planetary Decadal Surveys), and NASA planetary mission new-starts (flagship, New Frontiers, and Discovery, assuming reasonable cadences). With the exception of an Enceladus Plume Investigation mission, the Discovery program is powerless to effect progress on the roadmap. The New Frontiers program could perform a minimal (plume) sample-return mission, but only if the next Decadal includes it in the New Frontiers target set. The current Decadal prioritized Mars Sample Return (to be implemented stepwise, starting with the Mars 2020 caching rover that will gather samples that could be returned by a future mission) and the Europa Multiple Flyby mission as the planetary flagships for this decade. The next available opportunity, therefore, would result from the next Decadal's flagship and New Frontiers mission priorities. Concepts submitted into the Decadal cycle before 2020 will be pivotal: concepts that are thoroughly traded, "socialized" throughout the community, and vetted by independent review will fare best in the Decadal deliberations.

If on the other hand, NASA and the 2023–2032 Decadal Survey decide to await promising new findings

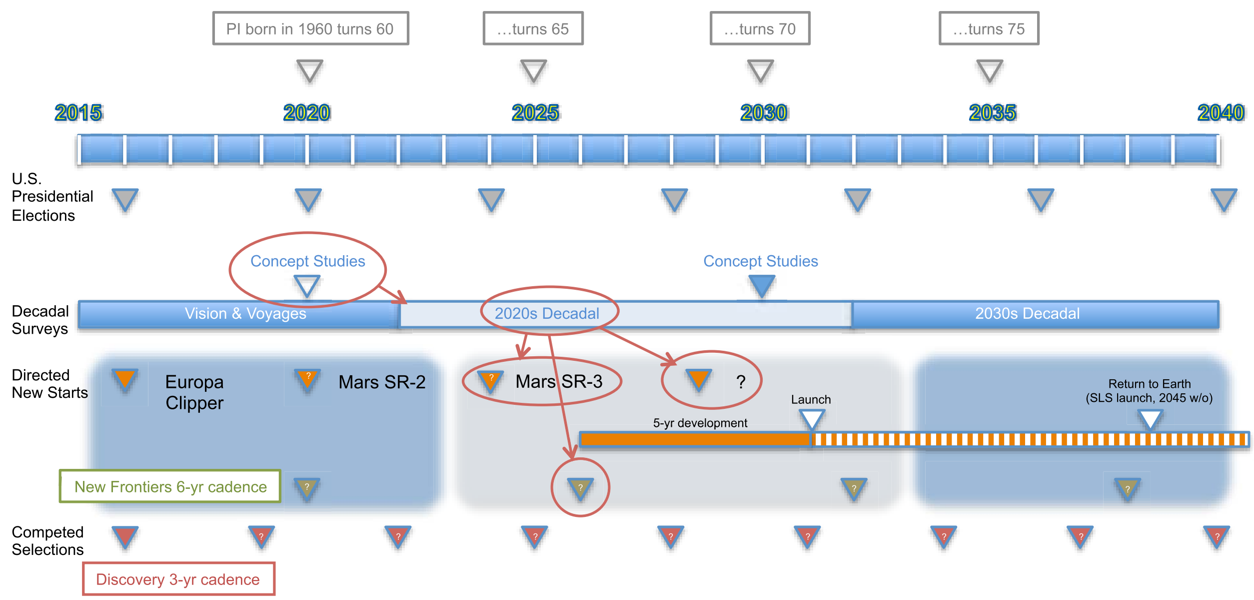

Fig. 8: ***Approval of planetary-science missions in the US*** *is governed by Decadal Surveys and constrained by programmatic categories. Non-science NASA directorates use more flexible mission-selection processes. Question marks on mission-start milestones indicate decisions not yet made.*

(e.g., from an Enceladus Plume Investigation, whose earliest conceivable fruition is 2032) in lieu of adopting a proactive program strategy, sample return or landed in situ exploration missions would be pushed out deep into the 2030s, and their results into the 2050s or beyond.

Faster progress would hinge on gaining advocacy for an integrated Ocean Worlds Exploration Program, reflecting Decadal priorities that balanced programmatic urgency with the strictly systematic approach of awaiting results from each in a series of missions.

The epochal goal of pursuing understanding of an alien ocean ecosystem shares important characteristics with the epochal goal of landing humans on Mars: a multi-decade schedule that cannot be predicted with precision; a requirement to commit significant resources far in advance of certitude that the investment will be rewarded; dependence for advocacy upon optimistic interpretation of incremental results from interim steps; and of course, the potential to put a human mark upon an ages-old dream.

Enabling elements for a successful, sustainable roadmap that culminates in oceanographic exploration of Enceladus include: a coherent sequence of science questions; technologies that mature at the right time to enable approval of relevant missions; integrated concepts that survive cost-estimating scrutiny; lessons learned from analogue exploration (e.g., terrestrial oceanography and extremophile research), and community momentum and international partnerships.

Analysis of these roadmap elements in the context of how US missions are selected directly implies a very short list of decision pressure points for the next two decades: 1) use of the Discovery program for a plume fly-through mission that takes the next two scientific steps; 2) development and vetting by 2019 of mission concepts for a range of sample-return and advanced in situ investigations, for infusion into the next Decadal Survey; and 3) cultivation of mutually beneficial partnerships with international space flight agencies who intend to participate in this grand adventure. Failing these pivots, undirected progress toward an age of comparative ocean exploration will be slow.

The windows for significant advance are sparse, and much remains to be done to prepare to take advantage of them. By leveraging the key planning opportunities, the astrobiology community may find and explore an alien ecosystem within the working lifetime of today's graduate students.

VII. ACKNOWLEDGMENTS

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VIII. ACRONYMS

AU	Astronomical Unit
CDA	Cosmic Dust Analyzer
ESA	European Space Agency

INMS	Ion and Neutral Mass Spectrometer
NASA	National Aeronautics and Space Administration
SLS	Space Launch System
TRL	Technology Readiness Level

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