*Forum Articles*

# Valuing Life-Detection Missions

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RECENT DISCOVERIES IMPLY that early Mars was habitable for life as we know it (Grotzinger *et al.,* 2014), that Enceladus might be habitable (Waite *et al.,* 2017), and that many stars have Earth-sized exoplanets whose insolation favors surface liquid water (Dressing and Charbonneau, 2013; Gaidos, 2013). These exciting discoveries make it more likely that spacecraft now under construction—Mars 2020, the ExoMars rover, the James Webb Space Telescope, Europa Clipper—will find habitable, or formerly habitable, environments. Did these environments see life? Given finite resources ($10 billion/decade for the United States[1]), how could we best test the hypothesis of a second origin of life? Here, we first state the case for and against flying life-detection missions soon. Next, we assume that life-detection missions will happen soon and propose a framework (Fig. 1) for comparing the value of different life-detection missions:

$$\text{Scientific value} = (\text{Reach} \times \text{grasp} \times \text{certainty} \times \text{payoff})/\$ \tag{1}$$

After discussing each term in this framework, we conclude that scientific value is maximized if life-detection missions are flown as hypothesis tests. With hypothesis testing, even a nondetection is scientifically valuable.

## 1. Should the United States Fly More Life-Detection Missions?

Once a habitable environment has been found and characterized, life-detection missions are a logical next step. Are we ready to do this?

*The case for emphasizing habitable environments, not life detection:* Our one attempt to detect life, Viking, is viewed in hindsight as premature or at best uncertain. In-space life-detection experiments are expensive. Other expensive experimental disciplines, such as US laser fusion and US particle physics, have histories that are cautionary tales about over-promising. Today, the search for life beyond Earth sustains congressional and public enthusiasm for planetary science. This enthusiasm could die down if life-detection missions

yield nondetections (even if they are false negatives). Perhaps the real payoff would be something so unexpected that it would be missed. To the extent that the science questions cannot be precisely defined in advance (Heng, 2016), a better motivation for planetary missions is pure exploration—to push the boundaries of what humans can do, visit, and know. This argues that the next generation of astrobiology missions should emphasize detecting and characterizing habitable environments, rather than the search for extinct or extant life.

*The case for flying more life-detection missions:* Life appears near the start of Earth's geological record and could be widespread in the Universe. A detection of a second origin of life has the potential to transform the science of biology. It would also provide guidance about our own future (Bostrom, 2008), including the human role in the Solar System. If we indefinitely defer decisive life-detection tests, then the search for life is simply PR for planetary science and astronomy. To optimally spend the $10 billion allotted to us over the next decade, astrobiologists should aim to test for life as quickly, as decisively, and as often as possible.

As the number of habitable extraterrestrial environments increases, the arguments for developing life-detection missions that target those locations becomes stronger. Recent developments, such as the publication of the report of the Science Definition Team for the Europa Lander (Hand *et al.,* 2017), show that life-detection missions are again being seriously considered. Therefore, we now need a framework for valuing different life-detection mission concepts (Neveu *et al.,* 2018).

Here, we emphasize science goals, not the specifics of mission implementation. A rigorous comparison of any two specific missions would have to consider many nuances to the design of the specific missions—for example, instrument capabilities, trajectory design, and risk versus cost—and we do not attempt to do that here.

## 2. Reach

One measure of value is a mission's *reach*—how many independent opportunities for finding life are there at the specific target to be investigated? This depends on (1) the size and diversity of the target environment and (2) the fraction of the target environment that is effectively sampled by the mission.

The odds that life as we know it will emerge and persist get better the greater the area of rock-water interfaces, the greater the dynamical cross-section for panspermia, and the

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[1]We pick $10 billion/decade as a rough estimate of current US spending on astrobiology. We note that international cooperation gathers talent, brings a reduced probability of cancellation, and is valuable in itself.

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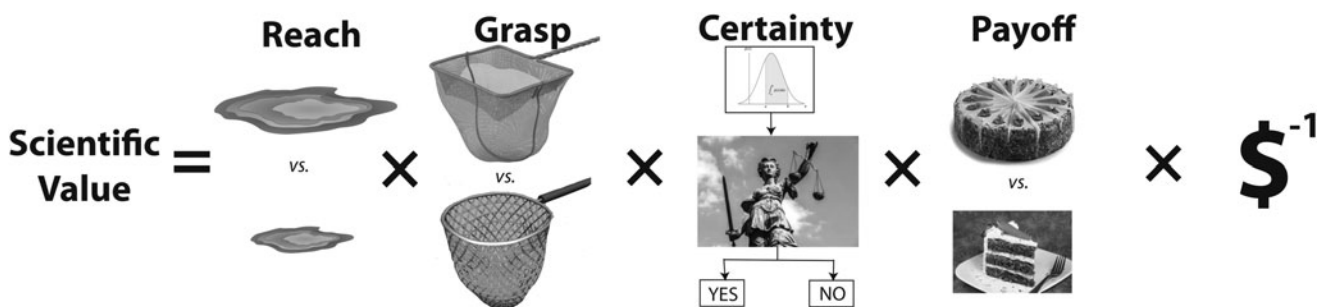[3]Princeton University, Princeton, New Jersey.

**FIG. 1.** Framework for valuing life-detection missions. **Reach**: The number of independent opportunities for finding life at the specific target to be investigated. **Grasp**: One minus the probability of a life-detection false negative (estimated prelaunch). **Certainty**: One if the postdetection probability of a false positive is low enough to permit scientific consensus, and zero otherwise. **Payoff**: Reward to science, culture, and the economy from a detection of life with certainty.

longer-lived the habitable conditions. More diverse environments are more likely to encompass the (unknown) conditions required for life to establish itself. By this logic, looking for microbes on a globally habitable early Mars may offer better odds than looking for life in now-frozen impact-generated lakes of liquid water on Titan (Artemieva and Lunine, 2003). On the other hand, Earth-sized planets can remain geologically active for so long as to erase ancient fossils (Earth and Venus are examples; Sleep and Bird, 2007). The potential reach of exoplanet missions is $\gtrsim 10^8$ habitable-zone worlds, although we do not yet know what fraction of habitable-zone planets are habitable and so do not yet know how the number of habitable targets depends on telescope specifications (*e.g.,* Postman *et al.,* 2010).

Reach is maximized when geological processes allow a single mission to sample for life that evolved in a voluminous environment. For this to happen, life or biosignatures must be conveyed to the probed location by groundwater flow, cryo-volcanism, winds, or ocean currents. Winds and currents swiftly mix planet-sized environments. Therefore, the reach of a mission that probes an atmosphere, the surface of a globally habitable planet, or an ocean is large. Exoplanet spectra also probe global environments. By contrast, groundwater flow can be slow and spatially restricted (petroleum can be trapped, and rarely flows >100 km from its source, for a similar reason; Hunt, 1995). The transport of living cells can be still more restricted than the transport of soluble biosignatures. Yet subsurface environments may be longer-lived than surface environments (*e.g.,* Mars; Grimm *et al.,* 2017) and can preserve life's signatures well (*e.g.,* petroleum again; Peters *et al.,* 2005). Nonetheless, a mission searching for a rock-hosted biosphere has a reach that is a small fraction of the planet's crustal habitable volume. Proving or falsifying the hypothesis that a rock-hosted biosphere exists deep beneath a hostile-to-life surface in any given planetary crust may simply be too expensive for the current budget.

The scale of an environment is a crude yardstick for its probability of hosting life. For example, Europa has $\sim 100 \times$ the seafloor area of Enceladus. Intuitively, Europa might have a higher probability for life (all other things equal), in part because larger environments are more likely to be stable and persist[2]—but not $100 \times$ more. This intuition can be captured by using a log

prior (or a log log prior; Lacki, 2016). These priors say that, if our ignorance about the likelihood of the origin of life spans very many orders of magnitude—which it does—then it is likely that either suitable planet-sized environments are almost all inhabited (habitability is all that is required, and life is inevitable) or almost all uninhabited (life as chemical accident). It is rather unlikely that (say) $\sim 50\%$ are inhabited, because there is no reason for the scale of the environment needed for origin of life to be equal to the scale of a planet, even to order of magnitude (Carter, 1983; Lacki, 2016). Priors that behave in this way moderate the importance of reach. With a log prior, the more ignorant we are, the more it makes sense to look for life in habitable environments that are small, short-lived, or both. Examples of such environments include asteroid parent bodies (Gaidos and Selsis, 2006) and rock bodies on Earth that were buried to uninhabitable depths but have since been exhumed (Onstott, 2016). For the same reason, a search for life on the hundred closest habitable planets is not much less valuable than a sweep of the entire Galaxy.

## 3. Grasp and Certainty

We define grasp as equal to one minus the probability of a life-detection false negative (estimated prelaunch). Both planetary processes and instrumental effects can degrade biosignatures, so both contribute to the false-negative probability. False-negative probability is defined relative to the best-available prelaunch understanding of the distribution of biosignatures in the specific materials to be actually investigated (based on Earth's geological record, Earth-analog environments, lab work on biomarker preservation, etc.). A mission that asks the wrong question (relative to our prelaunch model of what is there to be found) has low grasp regardless of whether it is ready to successfully answer that question.

We define certainty as equal to unity if the postdetection probability of a false positive is low enough to permit scientific consensus, and zero otherwise (Neveu *et al.*, 2018). Recent examples of scientific certainty include the discoveries of Neanderthal DNA (Krings *et al.,* 1997), gravitational waves (Abbott *et al.,* 2017), and the $^{16}O$ enrichment of the Sun relative to the planets (McKeegan *et al.,* 2011). Solid scientific progress demands certainty. Certainty is maximized by integrating multiple approaches (Schulze-Makuch *et al.,* 2015; National Academy of Sciences, 2017). For example, by combining molecular, isotopic, and textural clues, scientific certainty about ancient microbial life is possible (Wacey, 2009; Knoll *et al.,* 2016). By contrast,

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[2]But see Fuller *et al.* (2016) for a mechanism by which Enceladus' ocean could be as long-lived as Europa's ocean.

consensus is elusive for isotope-only claims and texture-only claims for Hadean life (Bell *et al.*, 2015; Nutman *et al.*, 2016). The histories of claims about Precambrian life and life in martian meteorites (McKay *et al.*, 1996) illustrate that reaching consensus involves a loop: analyze, interpret, critique, then analyze again. This loop takes time. Time may be in short supply on an *in situ* mission (Hand, 2008).

Returned-sample life detection, therefore, has better certainty and grasp than *in situ* life detection. Sample return allows thorough molecular (*e.g.*, Summons *et al.*, 2008), isotopic (*e.g.*, Stephan *et al.*, 2016), and textural inspection, but only up to the limits set by sample size and by contextual documentation at the sampled site. If the returned samples are too small or too few in number (*e.g.*, due to an overestimate of the in-space density of interesting samples; Westphal *et al.*, 2014), then a false negative can occur. Nevertheless, sample return to Earth maximizes grasp and certainty (Mustard *et al.*, 2013). This comes at a cost: for sample return from a habitable body, >\$1 billion. This cost is increased by rules that are set by NASA. Given constraints on spacecraft mass and cost, the coring and sample caching payload on a mission that is intended to be the first in a sample return campaign (*e.g.*, Mars 2020) might seem to maximize future certainty (from subsequent sample return) but at the expense of instruments that could increase grasp—unless the *in situ* analyses are able to complement the package by increasing grasp (Mustard *et al.*, 2013). Certainty for extant-life detection comparable to that of sample return might be achieved by combining multiple proxies for life, such as motility or perhaps consumption of redox gradients, that can be measured *in situ* (Weiss *et al.*, 2000; Nadeau *et al.*, 2016). Some of these *in situ* proxies may be unmeasurable in a returned sample. The grasp of *in situ* instruments will improve with further technology investments. However, relative to Earth laboratories, flight instruments have lower certainty for life detection and cannot be swapped out nor upgraded in response to initial results.

Despite great reach, exoplanet life detection using inner-Solar-System telescopes has low certainty (Seager and Bains, 2015). Spectroscopic detection of high levels of $O_2$ and/or chemical disequilibrium have been proposed as exoplanet biomarker candidates (Kasting *et al.*, 2014; Schwieterman *et al.*, 2017; Krissansen-Totton *et al.*, 2018). Both are really biohinters, because most detectable gas combinations can be produced without life. Exceptions, such as isoprene or CFCs, are too rare in Earth's atmosphere for detection at interstellar distances (Seager *et al.*, 2012). Unfortunately, the $CH_4 + O_2$ combination yielded by Earth's biosphere has been undetectable in long-range transmission spectroscopy throughout Earth's history: today because of low $CH_4$ and refraction (Misra *et al.*, 2014) and in the Precambrian due to low $CH_4$ (Reinhard *et al.*, 2017) or negligible $O_2$. If a large fraction of planets have both abundant $CH_4$ and abundant $O_2$, then this would be hard to explain in terms of abiotic transients (Catling and Kasting, 2017; Krissansen-Totton *et al.*, 2018). However, we have no reason to think that a large fraction of inhabited planets will be so cooperative. Non-gas biosignatures such as the vegetation red edge (Seager *et al.*, 2005) are intriguing, but for these, little effort has yet been spent on modeling to identify false positives.

These problems cannot be sidestepped by probabilistic approaches, because our prior uncertainty on life's abundance is so broad (Lacki, 2016) and rocky planets are diverse. If we want to do a Bayesian model comparison of with-life versus no-life models (given some exoplanet data), then we need to know the probability of the data given the no-life model (Catling *et al.*, 2018). This requires a forward model for atmospheric evolution on uninhabited yet habitable planets. Although it is easy to build such a model on a computer, our modest predictive power for Solar System atmosphere composition suggests humility about predictions for exoplanet atmospheres that are potentially much more diverse (Zahnle and Catling, 2017). Moreover, the models are in danger of being overfit to a few Solar System data points. It is risky to use uninhabitable rocky exoplanets as the no-life control set, because abiotic false-positives are correlated with some abiotic processes that promote habitability. Moreover, Earth may be too limited a template for an inhabited planet due to anthropocentric selection effects. Thus, although we might test the hypothesis that biospheres are "infrequent" (they might stand out with respect to other habitable but uninhabited planets), we cannot deal with the case that biospheres are "very uncommon" (sample size will always be insufficient to both detect the very-uncommon biosphere itself and also to rule out equally uncommon, but expected, abiotic false-positive scenarios) nor the "prolific" biosphere case (all or almost all habitable planets have life). Thus, we might detect a true biosignature but not know with certainty that life is the source. For example, suppose that 99% of $O_2$-rich atmospheres have $O_2$ as the result of life. Solar System telescopes could never approach 99% certainty that even one of those atmospheres roofs a biosphere, because abiotic $O_2$ production scenarios (Schwieterman *et al.*, 2017) cannot be ruled out to this confidence level[3]. This has implications for the use of the James Webb Space Telescope. Should we look for biosignatures around a few planets or instead probe for habitability in a larger sample of planets (Bean *et al.*, 2017)? If reaching certainty about exoplanet life detection requires observations of many *uninhabited* "control cases," then more planets are better.

## 4. Payoff

To find an independent origin of life would be a scientific breakthrough. The breakthrough would have a payoff that would depend on the nature of the evidence. Ancient-fossil evidence would be studied by using the same techniques used to study Precambrian fossils on Earth. Depending upon the preservation, these techniques constrain metabolism, composition, and cell size and structure but say little about genetics (Knoll *et al.*, 2016). Therefore, ancient-fossil life would have limited direct impact on sciences outside astrobiology unless the fossils preserved their molecular structure. Space-telescope detection of an exoplanet biosphere (Dalcanton *et al.*, 2015) would offer tantalizingly little information about the organisms themselves. This might stimulate interstellar flight if the biosphere orbits a nearby star (Lubin, 2016), or the construction of very large single-target space telescopes to study/monitor the biosphere and surface. Detection of extant life (or young fossils that retain DNA or equivalent) would offer the biggest payoff. For example, information about intact life might transform the biological sciences—which, via the health sector, underpin >10% of gross world product. Microbial

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[3]At least as long as high-resolution data to constrain abiotic-planet models are confined to the Solar System.

TABLE 1. A MATRIX FOR ASSESSING THE LIFE-DETECTION CASE FOR SELECTED POTENTIAL MISSION PROFILES

| Mission profile | Reach | Grasp | Certainty | Payoff | New technologies needed | Cost |
|---|---|---|---|---|---|---|
| *Space-based* | | | | | | |
| Return sample of ancient martian surface environments (*e.g.*, Mars 2020 to Jezero) | ✓✓ | ✓ or ✓✓ | ✓✓ | ✓ | | $$$ |
| Return sample of ancient martian subsurface environments | n.a. or ✓ | ✓ | ✓✓ | ✓ | | $$$ |
| Mars deep drill, *in situ* measurements only (assuming present-day aquifers exist) | n.a. or ✓ | ✓ | ✓ | ✓✓ | Compact high-output power source | $$$$ |
| Seek refugia on present martian surface, *in situ* measurements only | n.a. | ✓ | ✓ | ✓✓ | | $/$$ |
| Mars *in situ* paleontology (*e.g.*, ExoMars lander) | ✓ or ✓✓ | n.a. | n.a. or ✓ | ✓ | Enhanced by improved *in situ* instruments | $$ |
| Ocean world drill to ≤1 m (*e.g.*, Europa lander) | ✓✓ (*) | n.a. or ✓ (*) | ✓ | ✓✓ | | $$($$$?) |
| Ocean world, probe liquid water ocean *in situ* | ✓✓ | ✓✓ | ✓✓ | ✓✓ | Compact high-output power source | $$$$ |
| Ocean-sourced plume *in situ* (*e.g.*, Enceladus Life Finder) | ✓✓ | ✓ | ✓ | ✓✓ | Enhanced by improved *in situ* instruments | $$ |
| Ocean-sourced plume sample return | ✓✓ | ✓ | ✓✓ | ✓✓ | | $$ |
| Retrieve earliest Earth materials (from "Earth's attic," the Moon) | ✓ | n.a. | ✓✓ | n.a. or ✓ | | $/$$ |
| Exoplanet survey transit or direct imaging | ✓✓✓ | ✓ | n.a. | ✓ | | $$$/$$$$ |
| Investigate material from interstellar interloper (*e.g.*, 'Oumuamua) | n.a. | n.a. | ✓ | ✓ or ✓✓ | High $\Delta V$ to land or return samples | $$$ |
| Interstellar probe | ✓✓ | ✓ | ✓ | ✓ | Interstellar propulsion and communication | $10^{11}$ to $10^{12}$ |
| *Earth-based* | | | | | | |
| Laboratory origin-of-life experiments | n.a. | ✓✓ | ✓✓ | ✓ | | <$ |
| Probe natural origin-of-life experiments on Earth | n.a. | ✓ | ✓✓ | ✓✓ | | $ |
| SETI | ✓✓✓ | n.a. | ✓✓ | ✓✓✓ | | $ |

The values we have assigned are notional, and our goal is to encourage a broader discussion that draws on the community's collective expertise. n.a.=does not strengthen the life-detection case for a mission. ✓=ambivalent implications for the life-detection case for a mission. ✓✓=Bolsters the life-detection case for a mission. ✓✓✓=Offers strong support for the life-detection case for a mission. *=Depends on geological history of landing site. Notional costs: $ < 1 billion. $$ = 1–3 billion. $$$ = 3–10 billion. $$$$ > 10 billion.

life that shares a common ancestor with life as we know it might be easiest to exploit economically, but analysis of life that evolved completely independently could solve a wider range of scientific puzzles.

Even a low-payoff detection would supercharge space exploration and thus potentially speed up the discovery of high-payoff life elsewhere. This moderates the importance of payoff. Similarly, confirmed *in situ* detection of a living organism (with no characterization) is almost as good as retrieval, because a retrieval mission would then be launched by one or more countries with minimal delay.

## 5. Valuing Life-Detection Missions as Hypothesis Tests

Inevitably, our notional attempt to apply the criteria of reach, grasp, certainty, and payoff (Table 1) is mottled by our blind spots and prejudices as authors. Our intent is to encourage a broader discussion that draws on the community's collective expertise. Moreover, Table 1 could be reset by a scientific wild card, such as liquid water at <1 km depth on Europa, or by a technology development, such as fission reactors for deep-space missions (McNutt *et al.*, 2015). Nevertheless, two low-cost opportunities appear to have potential out of proportion to current funding. First, and perhaps the most compelling, is SETI. The other is study of natural origin-of-life experiments in Earth's subsurface—isolated water pockets that were first sterilized then exhumed to habitable depths (Holland *et al.*, 2013). These terrestrial environments are dwarfed by the crustal volume of Mars, but using a log prior this should not count against them too strongly. Natural intraterrestrial origin-of-life experiments can be investigated by sterile drilling, which is in any case a needed technology for ocean-world exploration. This argues for NSF-NASA or DoE-NASA cooperation.

Using origin-of-life research to drive target selection is risky. Because the geological setting (or settings) of abiogenesis is (are) unknown (*e.g.*, McCollom and Seewald, 2013), geologically diverse targets—and targets with the highest production rates of free energy able to drive chemosynthesis—are the best bets. (Titan's surface might be an example of a suitably diverse target, but only if life can establish itself in non-aqueous fluids [National Research Council, 2007; Shapiro and Schulze-Makuch, 2009].) However, prioritizing a mission because of any one origin-of-life hypothesis is questionable. For example, the environment targeted for life detection can be distant (physically and chemically) from the environment of abiogenesis: fragmentation during impacts early in Solar System history enables re-inoculation after giant impacts (Wells *et al.*, 2003). Nevertheless, prebiotic systems where life did not arise might inform origin-of-life research. Life might yet be created in the laboratory—perhaps tomorrow. While scientifically significant, would this inform the search for life on other worlds? Probably not: there may be many mechanisms for abiogenesis—many roads to life—and because of the timescale and chemical limitations of laboratory work, we should not expect the one that first works in the lab to be the same as the one that happened at planetary scale.

Once a habitable environment has been identified, refined constraints on fluxes of free energy and nutrients offer (limited) guidance for target selection. Energy and nutrient fluxes could scale with biomarker production/concentration,

which when elevated offers better sensitivity for life detection. However, life endures in nutrient-poor environments (Priscu *et al.*, 1999), many energy conservation strategies are possible (Schulze-Makuch and Irwin, 2002), and—if given an initial minimal nutrient budget and an energy source—a biosphere may self-sustain via heterotrophy, recycling, and adaptation.

Current reconnaissance missions, such as Mars Reconnaissance Orbiter and Europa Clipper, have a strong science return regardless of astrobiology outcome. However, life detection requires instruments that differ from those used to study habitable environments. Therefore, future Solar System astrobiology planners will have to weigh continued characterization of habitable environments against life detection.

A life-detection mission is a hypothesis test if the probability of life is greatly reduced by a nondetection (Platt, 1964). Missions that are not hypothesis tests—usually due to low grasp—have low value within the framework we propose here. Although it has been said that "exploration often cannot be hypothesis testing" (Chyba and Phillips, 2001; Hand *et al.*, 2017), hypothesis testing has served us well in the past (Mars Science Program Synthesis Group, 2004). Hypothesis testing also offers a clear basis for reallocating resources in response to negative results (Smolin, 2006). Hypothesis testing is necessary but insufficient for high science value: with post-1996 data in hand, we now see that the 1976 Viking landers had both low grasp and low reach.

Recent successful missions have uncovered apparently habitable environments. Each target offers unique tradeoffs. As the number of known habitable environments increases, it will be tempting to rebalance the US astrobiology portfolio away from continued exploration of habitable environments and toward testing the hypothesis of life. Proposed life-detection missions may be valued by sizing up their reach, grasp, certainty, and payoff (*e.g.*, Table 1). Missions that emphasize life detection should test astrobiology hypotheses. Framing good hypotheses requires precursor missions. Life-detection missions have low scientific value unless a negative result can guide future decisions and future missions—for example, whether or not to move on to more promising targets.

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