Astro2020 Science White Paper

The Search for Life Elsewhere as a Compelling Science Theme for Astro2020

Thematic Areas: ☒ **Planetary Systems** ☐ Star and Planet Formation
☐Formation and Evolution of Compact Objects ☐ Cosmology and Fundamental Physics
☐Stars and Stellar Evolution ☐Resolved Stellar Populations and their Environments
☐Galaxy Evolution ☐Multi-Messenger Astronomy and Astrophysics

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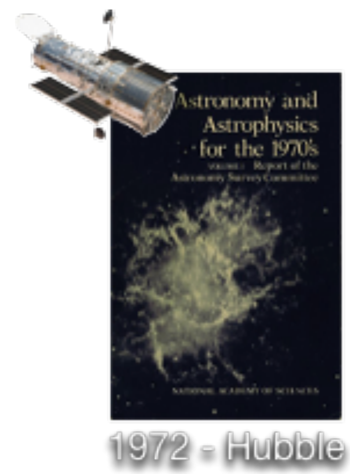
Abstract: (optional)
In this white paper, we review the evolving science drivers underpinning the search for life elsewhere, as articulated in our own decadal surveys in years past. We also review other documents, and provide a brief digression that considers a philosophy of addressing cost. Our hope is that this material helps today's astrophysics community set the stage for finally concluding the initial phase of the search for life. We believe the time is right for the search for life elsewhere to serve as the primary scientifically-compelling topic for this decadal survey.

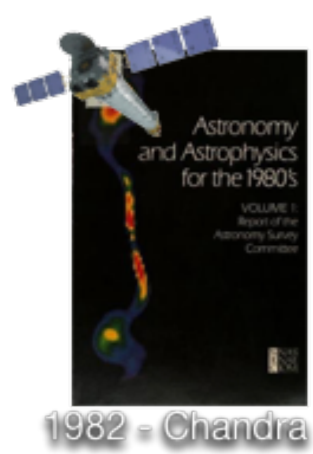
Introduction

Search for life as a compelling scientific program is not new to astrophysics; it has been explored within each of our community's decadal studies for over 50 years. In this white paper, we review the evolving science drivers underpinning the search for life elsewhere, as articulated in decadal surveys in years past. Our hope is that this helps today's astrophysics community set the stage for finally concluding the initial phase of the search for life. We believe the time is right for the search for life elsewhere to serve as the primary scientifically-compelling topic for this decadal survey.

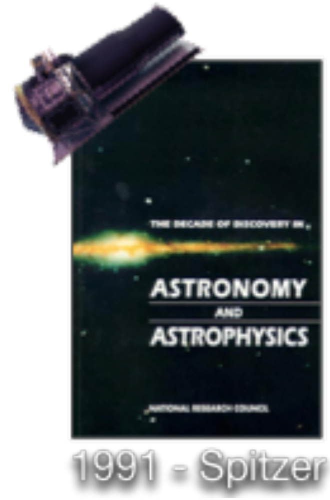
Life Elsewhere: A Deep Heritage in Astrophysics' Foundational Documents



The 1970s. *"Astronomy and Astrophysics for the 1970s"*, the decadal led by Jesse Greenstein, is notable as the visionary document that gave us the Hubble Space Telescope. The astrophysicists of that day were also thinking about life in space: *"…we are at a frontier of knowledge at which the techniques of optical, radio, infrared, and space astronomy, combined with the laboratory and theoretical skills of chemists and physicists as well as astronomers, are leading toward a better understanding of not only our ultimate beginnings but those of life everywhere in the universe. …each passing year has seen our estimates of the probability of life in space increase, along with our capabilities for detecting it… The promise is now too great, either to tum away from it or to wait much longer before devoting major resources to a search for other intelligent beings."* In that decadal, astrophysicists were thinking about listening via radio – detecting signals from other civilizations. The idea of exoplanets was mere speculation, and the idea of characterizing their atmospheres was not yet a topic of serious discussion, as we had barely begun to characterize the atmospheres of planets within our own Solar System.



The 1980s. *"Astronomy and Astrophysics for the 1980s"* brought us a new window on the Universe with a call for an x-ray telescope, eventually dubbed Chandra. At the same time, the astrophysicists of that decade realized that it might be possible to detect planets around nearby stars, in addition to signals from intelligent life: *"we are entering an era when it is technically possible both to detect planets around nearby stars and to detect signals from intelligent life on planets immensely farther away, even if we cannot detect the more distant planets themselves… Have condensations to planets and the origin of life occurred elsewhere as well? And has that life evolved into communicative intelligence, with which we human beings might be able to enter a conversation about life in the Universe?"*



The 1990s. An infrared space telescope emerged from *"The Decade of Discovery in Astronomy and Astrophysics"* as the next Great Observatory after Chandra, eventually named the Spitzer Space Telescope. Yet prescient passages in that document provide remarkable echoes of today's debate of truly large facilities in space: *"A large-diameter telescope (16-m class) operating at infrared, optical, and ultraviolet wavelengths could have enormous scientific potential. As discussed in Space Science in the 21st Century (NRC, 1988), such an instrument could detect earth-like planets around nearby stars and perhaps detect O_3 at 10 µm or O_2 at 1 µm in their atmospheres."* Being astrophysicists, of course, they realized that such a telescope in space would also have enormous transformational potential for astronomy: *"This very large telescope could also study the formation and evolution of galaxies by taking images and spectra of galaxies at large redshifts."* Interestingly, the question was not just whether we should search for (or listen for) life, but where to put the telescope that would do the search: *"The question of what is the best location…will depend on technological developments in the next decade and on the infrastructure that will become available to support orbiting and lunar observatories."*

The 2000s. By the 2000s, the existence of exoplanets had moved from speculation to fact. The community hadn't quite gotten to the point of detecting exoplanet atmospheres with Hubble (which had been operational for a decade), but the decadal committee recognized this was right around the corner: *"Astronomy and space science have reached a stage where astronomers can make important contributions to answering fundamental questions related to the origin and distribution of life in the universe….Within a decade, astronomers may be able to search for the spectroscopic signatures of biogenic gases, which provide evidence for life on such extrasolar planets."* This was also just a few years after the 1996 kerfuffle surrounding the martian meteorite ALH84001. Even though most scientists eventually concurred that the signatures in ALH84001 did not require life, the drama was a turning point of sorts; NASA began to bring together diverse fields to foster the field of astrobiology, and astrophysicists took note in the decadal: *"It is difficult to predict the potential for advances at this new interface between the physical and life sciences—other than to note the extraordinary potential of bringing diverse scientific cultures together at the right moment in time…astrobiology has the potential to link the seemingly abstract world of research at the frontiers of knowledge to questions that have excited the human imagination since people first gazed at the heavens."*

The 2010s. This brings us finally to "New Worlds, New Horizons," the first decadal to mention in its title something other than astronomy and astrophysics. The new worlds beyond had—after decades of being mere speculation—finally come into their own as a robust field of astrophysical study: *"Astronomers are now ready to embark on the next stage in the quest for life beyond the solar system—to search for nearby, habitable, rocky or terrestrial planets with liquid water and oxygen. … The observational challenge is great, but armed with new technologies and advances in understanding of the architectures of nearby planetary systems, astronomers are poised to rise to it. We have only the most rudimentary ideas for what conditions are necessary and conducive to the formation of life. Even here modern astronomy has a key role to play, by finding and characterizing planets with the features that allow for life around stars other than the Sun. It will require study of individual planets by directly sensing their light to find the molecular signposts of habitability in the atmospheres and surfaces of these distant bodies."*

Where We Stand Today

Strategies. Our decadal surveys are not the only documents articulating the importance of the search for life as an astrophysics field. The National Academies recently produced two important community documents: an Exoplanet Science Strategy and a document describing the role of strategic missions in NASA's portfolio, as well as a study about the importance of large strategic missions within the NASA portfolio (see select quotes in sidebar).

HDST. A few years ago AURA convened a blue-ribbon committee of scientists and technologists to explore the community's desires for a strategic mission capable of detecting Earth-like planets around Sun-like stars. Their final report: "From Cosmic Birth to Living Earths" merges the search for life with all of astrophysics, and developed a consensus desire for a large UV/optical/infrared space telescope they named the High-Definition Space Telescope (HDST; for more information, see www.hdstvision.org).

"NASA should lead a large strategic direct imaging mission capable of measuring the reflected-light spectra of temperate terrestrial planets orbiting Sun-like stars."

"NASA should launch WFIRST to conduct its microlensing survey of distant planets and to demonstrate the technique of coronagraphic spectroscopy on exoplanet targets."

"NASA should continue to plan for large strategic missions as a primary component for all science disciplines as part of a balanced program that also includes smaller missions."

"It is not possible for NASA to abandon large strategic missions simply because they can be challenging and still maintain world leadership in the space sciences."

LUVOIR and HabEx. The Science and Technology Definition Team flagship study reports commissioned by NASA will have much to say about the search for life. We note that the two concepts HabEx and LUVOIR are often seen as competing. We think instead that these concepts are unified in science: there was an extensive collaboration between the two STDTs, with the designs studied spanning a continuum of possible mission implementations. The two teams landed on two different implementation methods, but what we really need is the capability. We assert that the next decadal survey should not decide on a particular implementation approach, but rather endorse the idea of finally accomplishing the longstanding decadal and scientifically compelling goal of finding the signature of life on another world, and recognizing that to do so requires high-resolution, high-sensitivity UV/optical capability in the post-Hubble era.

Bottom line. From the perspective of the search for life, the decadal that lies ahead of us is the culmination of all of the decadals that came before. We can ACTUALLY ANSWER the question "are we alone?" to a statistically significant degree, if we build a telescope with sufficient capability. Technologically, we know how to do this. Scientifically, we know how to do this. We just need to assess the best way from a cost, effort, and schedule standpoint. A strong endorsement of this goal from the decadal committee would finally put us on the path to doing so.

A Digression about Cost

Let us speak briefly about cost. Attempts to "bound the problem" or "fit in the box" drive a certain kind of thinking. The 1970s decadal is sometimes hailed as the most visionary of all decadals – as it ultimately led to Hubble. How did that happen? That decadal committee said: here are ALL the things we would like to do. We understand the dollars are limited, but we think that all these things are SO IMPORTANT that we will write them down, and if NASA can figure out a way to do them, it should. We sometimes forget that Hubble was #9 on their list, ranked so low because the decadal committee itself did not know how NASA would afford it. But NASA found a way, and the resulting transformation of astronomy was ultimately profound. Looking back to that decadal in the 1970s: no one at that time knew that Hubble would take decades to complete, require many billions of dollars, and rely on a synergistic relationship with the human spaceflight program to succeed. Regardless, the science was clear, and the vision was not limited by an assumption of fitting into that year's NASA budget.

We want to offer a different way of thinking, akin to that of the 1970s. No one knows today with certainty what astrophysics budget scenario will play out in the future. We believe the decadal steering committee understands that it might not know which scenario is likely for many years. There are too many variables. And if we do not know, we should not bound our thinking by assuming today's number. The top line always rises when compelling flagships are required. Flagships do not "eat into the rest of astrophysics" though they might delay future flagships. The National Academies has studied this, concurs with this, and has emphasized balance in the NASA portfolio, including the express need for flagship class missions (see the "Powering Science" National Academy study on large strategic missions referenced above).

We encourage the current decadal committee to adopt this same strategy. We acknowledge it is difficult to cost large strategic missions, and we know the next one will be no exception. Rather than throwing up our hands, perhaps we can challenge NASA and industry and academia to find a way to help solve this problem. Perhaps set criteria (see box) and adjure our stakeholders to find the solution space that addresses the criteria, including cost. What is needed to make this possible? What kinds of (new) approaches can we take? What innovative thinking is needed? We personally do not have all the answers, but we believe these are questions worth asking.

Criteria for a future flagship

- Detect dozens of earth-like planets around sun-like stars
- US cost not to exceed $10B in FY'20 dollars
- Multi-center approach
- Industry and academia involvement
- Launch in 2035
- Partnerships (ESA, CSA, perhaps new partner)

Summary

Whatever the decadal survey selects, it should build on everything that came before. A sophisticated mission to search for life elsewhere will not be an isolated one-off. It will be a part of the grand continuum of astrophysical exploration that our community has established over multiple decades and multiple generations. Advocating for a future flagship is a heavy lift. It will be worth it.

We close with a thought from Michelangelo di Lodovico Buonarroti Simoni, an Italian sculptor, painter, architect and poet of the High Renaissance who exerted an unparalleled influence on the development of Western art.

Michelangelo said: "The greatest danger for most of us is not that our aim is too high and we miss it, but that it is too low and we reach it."

We strongly encourage the Astro2020 Decadal Steering Committee to aim high. Ad astra per aspera