ASTROBIOLOGY
Volume 8, Number 5, 2008

DOI: 10.1089/ast.2008.0276

News & Views

Worlds Beyond: A Strategy for the Detection and Characterization of Exoplanets Executive Summary of a Report of the ExoPlanet Task Force Astronomy and Astrophysics Advisory Committee Washington, DC June 23, 2008

The ExoPlanet Task Force:

Jonathan I. Lunine (Chair),[1] Debra Fischer,[2] H.B. Hammel,[3] Thomas Henning (ESA Liaison),[4] Lynne Hillenbrand,[5] James Kasting,[6] Greg Laughlin,[7] Bruce Macintosh,[8] Mark Marley,[9] Gary Melnick,[10] David Monet,[11] Charley Noecker,[12] Stan Peale,[13] Andreas Quirrenbach,[14] Sara Seager,[15] and Joshua N. Winn[15]

1. Executive Summary

1.1. Introduction

We stand on a great divide in the detection and study of exoplanets. On one side of this divide are the hundreds of known massive exoplanets, with measured densities and atmospheric temperatures for a handful of the hottest exoplanets. On the other side of the divide lies the possibility, as yet unrealized, of detecting and characterizing a true Earth analogue—an Earth-like planet (a planet of one Earth mass or Earth radius orbiting a Sun-like star at a distance of roughly one astronomical unit). This ExoPlanet Task Force Report describes how to bridge the divide. The recommendations emphasize immediate investment in technology and space mission development that will lead to discovering and characterizing Earth analogues.

We recognize that setting a goal of detecting planets like Earth sets the bar high. It is important that the program target such objects if we are to determine whether the conditions we find on our own world are a common outcome of planetary evolution. The only example of a habitable world we have is our own one-Earth-mass planet; and indeed our nearest neighbor, Venus, is nearly the same mass but uninhabitable by virtue of closer proximity to the Sun. Searching for planets, for example, five times the mass of Earth is easier. But should they turn out to lack habitable atmospheres, we would not know whether this is by chance or whether it is a systematic effect of the higher mass. The connection of

[1]The University of Arizona, Tucson, Arizona.
[2]San Francisco State University, San Francisco, California.
[3]Space Science Institute, Boulder, Colorado.
[4]Max Planck Institute, Heidelberg, Germany.
[5]California Institute of Technology, Pasadena, California.
[6]Penn State University, University Park, Pennsylvania.
[7]University of California, Santa Cruz, California.
[8]Lawrence Livermore Laboratories, Livermore, California.
[9]NASA Ames Research Center, Moffett Field, California.
[10]Harvard-Smithsonian Center for Astrophysics, Cambridge, Massachusetts.
[11]United States Naval Observatory, Flagstaff, Arizona.
[12]Ball Aerospace and Technologies Corporation, Boulder, Colorado.
[13]University of California, Santa Barbara, California.
[14]Landessternwarte Heidelberg, Heidelberg, Germany.
[15]Massachusetts Institute of Technology, Cambridge, Massachusetts.

the strategy described here to the big questions that motivate astronomical endeavors lies in understanding whether our home world is a common or rare outcome of cosmic evolution.

The discovery of Earth analogues will change the way we humans view our place in the cosmos. Just 500 years ago the standard Western view of the cosmos was that Earth stood at the center with all the planets and the Sun moving around it. Copernicus's bold treatise of 1543 brought about a reluctant paradigm change that Earth and the other planets orbited the Sun. Later, our Sun was recognized to be but one of 100 billion stars in the Milky Way Galaxy, and today astronomers believe the Milky Way is but one of hundreds of billions of galaxies in the Universe. Earth remains special as the only planet we know of with life. Finding Earth analogues that show signs of habitability or atmospheric indicators of life will bring about a new paradigm shift—one that completes the Copernican Revolution.

Discovering an Earth analogue is one of the most challenging feats for any planet-finding technique. While an Earth analogue only 30 light years distant is not fainter than the faintest objects (galaxies) ever observed by the Hubble Space Telescope, the adjacent massive, huge, and overwhelmingly bright parent Sun-like star makes planet detection extremely challenging. The Sun is 100 times larger, 300,000 times more massive, and 10 million to 10 billion times brighter than Earth. Detecting Earth in reflected light is like searching for a firefly 6 feet from a searchlight that is 2400 miles distant. Each of the different exoplanet-finding techniques is most sensitive to planet-star combinations that are very different from the Earth-Sun system. At the same time—for the first time in human history—several different exoplanet discovery techniques are close to finding Earth analogues. The ExoPlanet Task Force Report recommends using the radial velocity technique to push to find Earth-mass planets around nearby Sun-like stars for the handful of stars that are bright enough for monitoring. The only technique appropriate to survey the nearest hundred or so bright Sun-like stars in the mid-term is space-based astrometry, and this is one cornerstone of the Task Force recommendations. To study a planet's atmosphere for signs of habitability or life, direct imaging is required, and the Task Force Report recommends investment in direct-imaging technology development across different wavelengths and techniques. Once Earth-mass planets are known to orbit nearby Sun-like stars, the Task Force recommends launching a direct-imaging space mission for habitability characterization.

The hierarchy of pressing questions in the search for Earth analogues are "Do Earth-like planets exist?" "Are they common?" and "Do they show signs of habitability or biosignatures?" NASA's Kepler mission aims to answer the first two questions by monitoring a large number of faint stars to find the frequency of Earth analogues. We call this frequency $\eta_{\oplus}$ ("eta sub Earth"), the fraction of Sun-like stars that have at least one planet in the habitable zone (where the habitable zone is the region around the star with temperatures suitable for surface liquid water). In order to assess whether an individual Earth analogue has signs of habitability we must find Earth analogues around the nearest and brightest stars. Only the bright stars host planets bright enough to take spectra—that is, to "fingerprint" the planet's atmosphere in order to identify molecular features and biosignatures. The Task Force strategy focuses on the Earth analogues around the bright, nearby Sun-like stars (called F, G, and K stars), provided that $\eta_{\oplus}$ is high enough.

There is an exciting possibility of a fast track to finding and characterizing habitable exoplanets. This is the search for big Earths (super Earths) orbiting small, cool stars (called M-dwarf stars or M-dwarfs). M-dwarfs are much less luminous than the Sun so that the locations amenable to surface liquid water to support life as we know it are very close to the star. The relative size and mass of the planet and star are more favorable for planet detection than the Earth-Sun analogue. Two different yet complementary planet-searching techniques (transits and radial velocities) are very sensitive to super Earths orbiting with close separations to M-dwarfs—and indeed a handful of super Earths orbiting small stars have already been discovered. A set of transiting planets (those that go in front of their stars as seen from Earth) will enable average density measurements and hence identification of terrestrial-type planets. The Task Force report recommends bolstering support for both radial velocity and transit discovery and characterization of super Earths orbiting in the habitable zones of M-dwarfs. Suitable transiting planets can have their atmospheres characterized by the James Webb Space Telescope, now under development, building on the current Spitzer Space Telescope studies of hot Jupiters.

Beyond the search for habitable exoplanets, the Task Force has identified the observation of "planetary architectures" (orbital arrangements and types of planets around a star) as key to determining the diversity of planetary systems (and hence how common or uncommon is our own Solar System's geography) and the long-term habitability of inner terrestrial planets as affected by overall system architecture. A third major area of planetary formation completes the picture for understanding the origin and evolution of exoplanetary systems.

1.2. Background to the Recommendations

The Task Force developed a 15-year strategy for the detection and characterization of extrasolar planets ("exoplanets") and planetary systems, as requested by NASA and the National Science Foundation (NSF) to the Astronomy and Astrophysics Advisory Committee. The strategy is an outgrowth of the efforts underway for 2 decades to detect and characterize extrasolar planets—from which over 260 planets and dozens of multiple planet systems have been found and studied. It is informed by a variety of technological studies within the astronomical community, industry, NASA centers, and NSF-funded facilities that point the way toward techniques and approaches for detection and characterization of Earth-sized (0.5–2 times Earth's radius) and Earth-mass (0.1–10 times the mass of Earth) planets in the solar neighborhood. The raw material for the strategy was provided in the form of invited briefings and 85 white papers received from the community.

The strategy the Task Force developed is intended to address the following questions, given in order of priority:

1. **What are the physical characteristics of planets in the habitable zones around bright, nearby stars?**
2. **What is the architecture of planetary systems?**
3. **When, how, and in what environments are planets formed?**

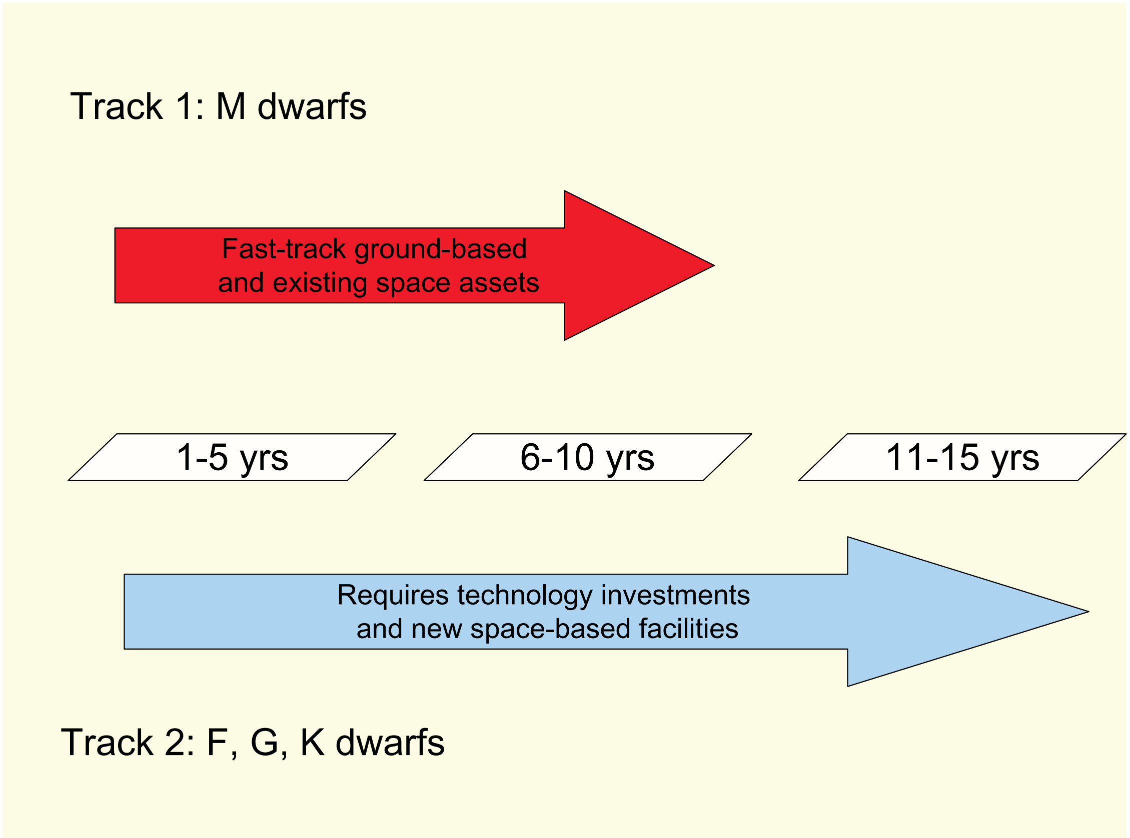

FIG. 1. The recommended strategy is shown conceptually as consisting of parallel tracks. The top track for M-dwarfs relies on ground-based and space assets that exist or are under development. The bottom track, for Sun-like stars, requires technological investments and new space-based facilities but is flexible and builds on previous findings.

The time horizon for the strategy is divided into three epochs, 1–5, 6–10, and 11–15 years. A two-pronged effort toward answering the first question is recommended, the first track focused on ultimately detecting and characterizing Earth-size/Earth-mass planets around M-dwarfs ("M-dwarf planets") using ground-based and space assets in place or under development today, and the second with the ultimate goal of detecting and characterizing Earth-size/Earth-mass planets around stars like our Sun with new capabilities whose technologies are under development today (Fig. 1). The first track uses optical and near-infrared Doppler spectroscopic (radial velocity) and transit surveys from the ground, possibly supplemented by a space-based transit survey, if needed, to find M-dwarf planets approaching the size/mass of Earth that can be characterized with Spitzer (even after its cryogen is depleted) and/or the James Webb Space Telescope. This track is expected to be completed within the second time epoch (6–10 years).

The second track is focused on stars more similar to the Sun, what are called F, G, and K dwarfs. Its key feature is deployment in the second epoch of an astrometric facility in space with the sensitivity to survey for one-Earth-mass planets around up to 100 solar-type stars in our cosmic neighborhood, providing a target list for a direct-detection mission to be deployed in the final epoch. We envision this to be an astrometry mission that heavily emphasizes sub-microarcsecond planet-finding science, thereby limiting its scope and hence cost when compared with mission architectures such as the Space Interferometry Mission (SIM) that would have broader goals across astrophysics. Following this, in the third epoch, a direct-detection mission is deployed capable of doing a spectroscopic examination of Earth-mass planets identified by the astrometric mission. The scope of the direct-detection mission depends on the findings of the astrometry mission. If, for some reason, an astrometry mission is not executed, then an assessment of whether a direct-detection program should proceed will depend upon measured values of $\eta_\oplus$ and on the dust in the system (the "exozodiacal environment") around target stars, among other factors. The COROT and Kepler space-based transit searches will constrain $\eta_\oplus$ early enough (within the first 5 years of this strategy) to scope the size of the direct-detection mission and, together with results of technology studies, determine whether it should be based on coronagraphic/occulter or nulling interferometric technologies. The astrometric mission itself does not require transit survey results since it will provide high-value data on planetary system architectures even if specifically Earth-mass planets turn out to be nonexistent or so rare that no candidates are found within a volume accessible to direct detection.

This approach is adaptable to new discoveries and surprises along the way. It provides the potential for early results on Earth-sized planets around M-dwarfs. The strategy moves beyond M-dwarf Earths, on which life may be limited due to potential habitability issues, to find Earth analogues around stars similar to our own. Because the astrometric survey approach is insensitive to confusion from background objects and zodiacal dust clouds around candidate stars, its use in the second epoch ensures significant results on planetary system architectures and the existence of Earth-mass planets even in the unhappy circumstance that most systems are much dustier than our own. By using the astrometric mission to survey for targets for the direct-detection mission, the strategy allows maximally efficient design of direct-detection approaches unburdened by the need to conduct surveys. Decision points contingent on the exozodiacal and $\eta_{\oplus}$ surveys occur early enough in the strategy to permit a shift in focus in the later part of the strategy.

Throughout the 15-year period of the strategy, surveys of the architectures of planetary systems with ground- and space-based capabilities and work toward ground-based direct detection and examination of larger (Neptune-class) planets would be pursued to complement the search for planets like our own. A sensitive survey of the distribution of extrasolar (exo-) zodiacal emission in solar-type stars should be conducted in the first time epoch to enable key decisions on the direct-detection mission.

Although the Task Force believes this strategy to be reasonably paced, it is also flexible in the sense that individual elements can be delayed or stretched out, while the overall program continues to provide exciting discoveries.

Complementary to the planet search and characterization strategy is the use of existing and anticipated ground- and space-based resources to study circumstellar disks in their planet-forming and post-formation phases, with the goal of understanding planet formation and inferring the presence of planets in young systems.

Theoretical investigations are critical for interpreting and guiding key observations. Because theory results span all time epochs, the recommendations are described in Section 1.6 covering all years. This placement does not reflect the priority of theory compared to the observational and technological recommendations in Sections 1.3–1.5. The interplay between theory and observation is an ongoing activity that is essential to returning the full value of our scientific missions.

Exoplanet science and technology is a rapidly changing field. There is potential for new, transformational technologies that may significantly impact planet discovery. One such example, that of a new radial velocity technique, arose just as this report was being completed. The new radial velocity technique enables precise wavelength calibration of high-resolution spectrographs using laser frequency combs. Demonstrated in the lab, the frequency comb spectrograph still needs to be tested on a telescope. If the significant challenges of long-term instrument stability and intrinsic stellar variability can be met, the precise calibration could enable discovery of Earth analogues around hundreds of Sun-like stars. This development serves as a reminder that transformational techniques will arise, and it may call for reevaluation of the relevant decision points in the strategy.

Recommendations of the Task Force to implement the strategy are below, with the detailed programmatic elements depicted in Fig. 2. The recommendations are divided into different time epochs, the 1–5 year, 6–10 year, and 11–15 year time frames. While we do not strictly prioritize the recommendations, we do order them—within each time epoch—according to the priority order of the compelling scientific questions introduced above. Where appropriate, we further organize the recommendations into the F, G, K (*i.e.*, Sun-like) strategy and the M-dwarf strategy.

We expect the report and our recommendations, based as they are on input from 85 *white papers* from the community and a year-long intensive study process, to be a key part of the community input to the panel(s) dealing with exoplanets and to the survey committee during the upcoming 2010 Decadal Survey of Astronomy and Astrophysics. Our recommendations for detailed assessment of critical technological developments should be implemented as soon as possible, ideally well within the time frame of the Decadal Survey process, to enable the strategy proposed here to move forward in a timely fashion.

1.3. A. Recommendations for 1–5 Years

A. I. a. What are the physical characteristics of planets in the habitable zones around bright, nearby F, G, K *stars?*

Recommendation A. I. a. 1. *Sufficient investment should be made in ground-based telescope time for radial velocity measurements to enable the discovery of low-mass exoplanets down to Earth-mass planets orbiting bright stars. The required precision for the detection of Earth analogues is substantially better than 1 m/s. In the first time period, we recommend feasibility studies for extreme Doppler precision (down to several cm/s) for bright-star targets. It is also critical to continue surveys for planets of all detectable masses with a target list well in excess of 1000 stars, with 3000 observations per year total.*

The investment could be NASA time on the Keck telescope, including programs such as the NASA/Keck $\eta_{\oplus}$ program, as well as the dedication of underutilized and existing 3–4 m class telescopes with high-precision and high-throughput spectrographs. A focus on bright stars including the brightest M-dwarfs is the fast track to finding a potentially habitable exoplanet.

In order to implement 2 key spaceborne capabilities—astrometry and direct detection—within the 15 year time frame of the strategy, technological studies and mission development are required early on. NASA has already invested substantially in technologies for both, and particularly for spaceborne astrometry, so that there is a strong foundation already for completing the additional technological developments needed to conduct astrometric and direct-detection missions in space.

Recommendation A. I. a. 2. *Preparations should begin for a spaceborne astrometric mission capable of surveying between 60 and 100 nearby main sequence stars with the goal of finding planets down to the mass of Earth orbiting their parent star within the habitable zone—that is, approximately 0.8–1.6 AU, scaled appropriately for stellar luminosity. Achieving this goal will require the capability to measure convincingly wobble semi-amplitudes down to 0.2 microarcseconds ("μas") integrated over the mission lifetime. Space-borne astrometry is currently the only technique that can distinguish masses in the range of 1–10 Earth mass. To this end, a rigorous technical feasibility assessment should be undertaken immediately, and any additional required technological development beyond what has been accomplished to date should be completed promptly, leading to an implementation phase start in the intermediate 6–10 year time period. The feasibility assessment should include a critical analysis of stellar systematic effects, such as starspots.*

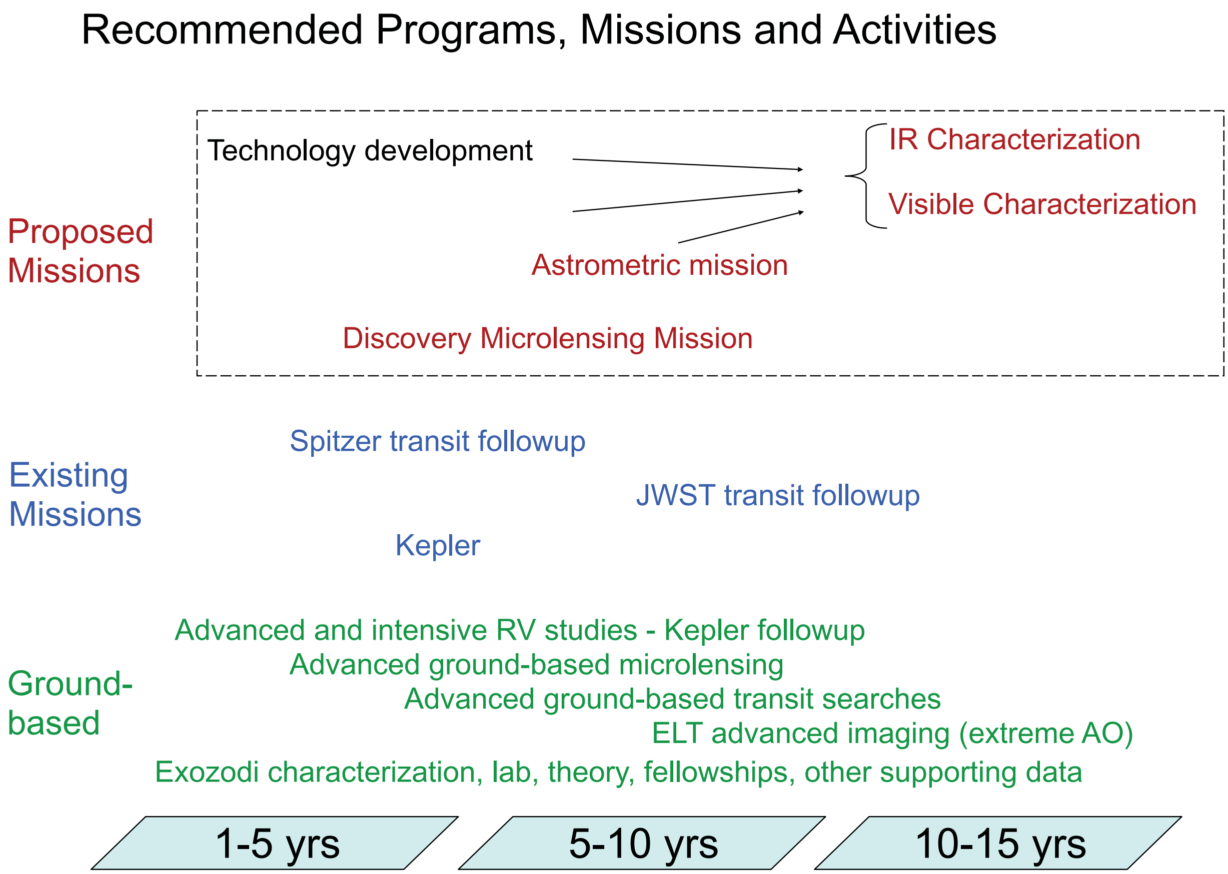

FIG. 2. Graphical depiction of the recommended strategy parsed in time periods and according to ground-based, space-based or existing assets.

Recommendation A. I. a. 3. *Technological development of spaceborne direct-detection capabilities to ultimately find and characterize Earth-sized planets should be undertaken at the start of the strategy. This includes visible wavelength internal and external coronagraphs and infrared nulling interferometers. A key enabling technology for internal coronagraphs is advanced wavefront sensing and control; support must be sufficient to assess the viability of internal coronagraphs operating at an inner working angle IWA* $< 3.5\lambda/D$. *(The IWA is the minimum angular separation from the star at which the observatory can detect a planet.) Additional technologies that need attention include, but are not limited to, next-generation deformable mirrors, low-noise detectors, coronagraphic masks, and ultraprecise optical surfaces. A key enabling technology for external occulters is validated diffraction modeling. Support must be sufficient to complete demonstration scalable to flight dimensions. Additional technologies that need attention include, but are not limited to, alignment sensors, deployment methods, high-specific-impulse thrusters, and studies of plume effects. Also needed are mission models assessing the science harvest as it depends on system size.*

Because both the astrometry and direct-detection technologies are so crucial to the strategy, ongoing technological development should be supplemented with in-depth reviews by experts in the various fields.

Recommendation A. I. a. 4. *NASA should establish a blue-ribbon panel, consisting largely of physicists or optical scientists with expertise in wave optics, to evaluate various coronagraph and wavefront control concepts and ensure that no fundamental physical effect has been overlooked in planning for an optical wavelength, direct-detection mission. An equivalent panel should be established for direct detection by interferometry.*

Sizing of direct-detection systems, indeed, their feasibility, for studying planets around Sun-like stars depends on how typical our Solar System's dust emission is—that is, what is the distribution of zodiacal emission around other stars.

Recommendation A. I. a. 5. *Invest in a census of exozodi systems around solar-type stars that might be targets for exoplanet searches.*

A. I. b. What are the physical characteristics of planets in the habitable zones around bright, nearby M stars?

This parallels Recommendation A. I. a. 1., but for the smaller M-dwarfs.

Recommendation A. I. b. 1. *In view of the fact that M-dwarfs might harbor the most detectable Earth-sized planets, search the nearest thousand M-dwarfs* ($J \leq 10$) *for transiting low-mass exoplanets with radial velocity measured masses.*

Near-infrared spectrographs are potentially the best way to find terrestrial-mass planets in the habitable zones of main sequence stars later than M4 (a relatively warm or "early" M-dwarf); optical spectroscopy is competitive and already available for earlier spectral types. Develop infrared spectrographs with a target precision of 1 m/s for radial velocity surveys of late M-dwarfs. A near-term demonstration of 10 m/s is critical to validate this technique.

Recommendation A. I. b. 2. *Develop near-infrared spectrographs with 1 m/s precision for radial velocity planet surveys of late M-dwarfs, once feasibility at 10 m/s precision has been demonstrated.*

Given the possibility that some M-dwarfs might harbor planets whose properties could be studied with warm-Spitzer and/or the James Webb Space Telescope (JWST), these space assets form an important part of the strategy.

Recommendation A. I. b. 3. *Continue to operate Spitzer as a warm observatory for characterizing low-mass transiting planets around main sequence stars, particularly around M-dwarfs.*

A. II. What is the architecture of planetary systems?

In this near-term period, progress toward a complete understanding of planetary systems requires further development of two ground-based techniques: microlensing, which will provide a census of planetary mass and orbital separation as a function of stellar type and galactic environment; and extreme adaptive optics (AO) systems, which will enable the direct detection of young giant planets. Ground-based extreme-AO coronagraphy on 8 m class telescopes represents the next major scientific and technical step beyond the current generation of Hubble Space Telescope (HST) and ground instruments. Similar instruments on future Extremely Large Telescopes (ELTs) will further advance the state of the art and provide a proving ground for technology that may ultimately be part of an optical wavelength, direct-detection approach. Maintenance of US involvement in facilities that utilize or advance these technologies is key to addressing all 3 key scientific questions and to obtaining full benefit from the data.

Recommendation A. II. 1. *Increase dramatically the efficiency of a ground-based microlensing network by adding a single 2 m telescope.*

Recommendation A. II. 2. *Continue technological developments and implementation of key ground-based capabilities such as extreme AO in the laboratory and on 8 m class telescopes. Future ELTs should be designed with the capability of extreme-AO coronagraphy in mind. In the very near term, establish a blue-ribbon panel to determine in detail the requirements, costs, and opportunities associated with ground-based work on exoplanets.*

A. III. When, how, and in what environments are planets formed?

Recommendation A. III. 1. *Maintain US involvement in key facilities including Herschel and ALMA for studies of disks, and continue support for archival analysis of relevant Spitzer, Chandra, Hubble, and ground-based data.*

Recommendation A. III. 2. *Sustain a healthy level of support for ground-based, space-based, and theoretical (see Section D) investigations of star and planet formation.*

1.4. B. Recommendations for 6–10 Years

The recommendations here follow from the accomplishments of the first time period and the logic of the strategy summarized above.

B. I. a. What are the physical characteristics of planets in the habitable zones around bright, nearby F, G, K *stars?*

Recommendation B. I. a. 1. *Launch and operate a space-based astrometric mission capable of detecting planets down to the mass of Earth around 60–100 nearby stars, with due consideration to minimizing the width of any blind spot associated with Earth's parallax motion. (This requires a mission precision, over many visits to a given star, as small as 0.2 microarcseconds.)*

Recommendation B. I. a. 2. *Contingent on the latest knowledge of $\eta_\oplus$ and exozodi brightness for potential target stars, move spaceborne direct detection into the advanced formulation phase to enable a mission launch in the 11–15 year time frame.*

Note: if Kepler suffered a sufficiently serious mission failure, development of the direct-detection mission should proceed forward based on COROT and ground-based results if they indicate a likelihood of high $\eta_\oplus$. Likewise, if the astrometric mission fails or turns out to be infeasible, pursuit of space-based direct detection in the final time period would require a sufficiently large $\eta_\oplus$ based on COROT and Kepler to give a reasonable probability of mission success.

B. I. b. What are the physical characteristics of planets in the habitable zones around bright, nearby M *stars?*

Given the possibility that some M-dwarfs might harbor planets whose properties could be studied with warm-Spitzer and/or JWST, these space assets form an important part of the strategy.

Recommendation B. I. b. 1. *Use JWST to characterize Earth-sized transiting planets around M-dwarfs.*

B. II. What is the architecture of planetary systems?

Recommendation B. II. 1. *Move planetary system architecture studies and multiple-planet statistics beyond the 3–5 AU "ice-line" (also called the "snow-line") boundary for G-type stars by continuing long-time-baseline Doppler spectroscopic studies.*

A space-based microlensing mission has significant advantages over a ground-based network in being able to collect complete statistics on planetary masses and separations, including free-floating planets, as a function of stellar type and location in the Galaxy.

Recommendation B. II. 2. *Without impacting the launch schedule of the astrometric mission cited above, launch a Discovery-class space-based microlensing mission to determine the statistics of planetary mass and the separation of planets from their host stars as a function of stellar type and location in the galaxy and to derive $\eta_\oplus$ over a very large sample.*

Recommendation B. II. 3. *Begin construction of a 30 m telescope to do optical direct detection of giant planets to understand planetary system architecture and planet formation, and invest in appropriate instrumentation for planet detection, characterization, and disk studies.*

B. III. When, how and in what environments are planets formed?

Recommendation B. III. 1. *Implement next-generation high spatial resolution imaging techniques on ground-based telescopes (AO for direct detection of young low-mass companions and interferometry for disk science).*

1.5. C. Recommendations for 11–15 Years

C. I. What are the physical characteristics of planets in the habitable zones around bright, nearby F, G, K *stars?*

Recommendation C. I. 1. *Provided $\eta_\oplus$ is high and typical exozodi emission sufficiently low, conduct space-based direct detection and characterization for Earth-mass planets found by astrometry in the habitable zone of nearby solar-type stars.*

Given the current state of development of direct-detection approaches, the coronagraph/occulter appears to be more mature and less costly than interferometric techniques. However, before a choice of which direct-detection approach to pursue is made, additional technological studies as well as better constraints on $\eta_\oplus$ and exozodi emission should be obtained.

Should any of the following occur, namely $\eta_\oplus$ low, exozodi emission high, or no astrometric candidates are found which are suitable for direct-detection missions, then technology studies of facilities capable of direct detection of Earth-sized planets around more distant or more difficult targets are warranted.

Recommendation C. I. 2. *Begin development of a more ambitious space-based direct-detection system, with international collaboration where appropriate, to be launched beyond the report's time horizon. Such a mission would either follow up on the successful direct-detection work begun in this period of the strategy or be used to overcome technological or observational* (e.g., *low $\eta_\oplus$) impediments that prevented such detection and characterization in the report's 15-year time horizon.*

C. II. What is the architecture of planetary systems?

Recommendation C. II. 1. *Should any of the following occur, namely $\eta_\oplus$ low, exozodi emission high, or no astrometric candidates are found which are suitable for direct-detection missions, then pursue with stronger emphasis studies of larger planets via ground-based direct detection and studies of the architecture of planetary systems with ground- and space-based tools including microlensing.*

C. III. When, how, and in what environments are planets formed?

Recommendation C. III. 1. *Invest in technology for the next-generation observations of planet-forming disks and disks where young planets reside. These capabilities should include, for example, sensitive (equivalent to Spitzer and Herschel) far-infrared interferometric observations from space to achieve the resolution of the Atacoma Large Millimeter Array (ALMA) closer to the peak of the dust spectral energy distribution. A particular need for the far-infrared is technology investment to increase detector performance.*

1.6. D. Ongoing All Years

Theoretical work is the tool to interpret the findings resulting from the observational and technological recommendations in Sections 1.3 through 1.5 above and to put them into a broader context.

Recommendation D. 1. *A strong theory program is essential to address all three compelling questions. Theory programs include planet atmosphere and interior studies; laboratory astrophysics; n–body and hydrodynamic codes with large computational demands to study planet formation, evolution, and dynamical evolution; and stellar astrophysics* (e.g., *nearby young star samples, stellar ages).*

Recommendation D. 2. *NASA and NSF should provide support for activities that maximize the knowledge return from the data and train new scientists in the field, including theoretical studies (See D. 1.), stellar properties surveys, and competitive fellowships for young researchers.*

The implementation of this strategy—resulting potentially in the detection and preliminary characterization of an Earth-like planet in the habitable zone of another star—will be of profound scientific and philosophical significance. We assert here that the goals are attainable within decades, but its accomplishment will require a sustained commitment of fiscal resources. We expect, given the universal nature of the questions being addressed, that this strategy will be pursued as a collaboration among international partners. But the United States' pivotal role in large space scientific endeavors over the history of space exploration suggests that the nation must lead the effort if it is to be completed in a timely fashion.

Abbreviations

ALMA, the Atacama Large Millimeter Array; AO, adaptive optics; ELTs, Extremely Large Telescopes; HST, the Hubble Space Telescope; JWST, the James Webb Space Telescope; NSF, the National Science Foundation; SIM, the Space Interferometry Mission.

Government Disclaimer

Portions of this work were performed under the auspices of the US Department of Energy by Lawrence Livermore National Laboratory in part under Contract W-7405-Eng-48 and in part under Contract DE-AC52-07NA27344.

Address reprint requests to:
Dr. Jonathan Lunine
Lunar and Planetary Lab
1629 E. University Blvd.
Tucson, AZ 85721-0092

E-mail: jlunine@lpl.arizona.edu