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Science Proposes, Technology Disposes or Why We Need an Infrared Interferometer

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Abstract. The science goals of detecting extrasolar earths and searching for evidence of their environment and signs of life require observations at both infrared and visible wavelengths. It is argued that both the character of the information received from these studies, and the state of technological readiness show that there should be an early infrared interferometer.

1. The Science of TPF

The study of biomarkers preceded the development of techniques for observing terrestrial planets around other stars, and gave impetus to their development. Owen (1980) suggested the possibility of looking for the oxygen A band (760nm) in the atmospheres of terrestrial planets as a sign of the presence of photosynthetic life. Angel, Cheng and Woolf (1986) proposed the 9.7 μm band of ozone. Leger, Pirre, and Marceau (1993) and Leger et al. (1999) investigated the danger of false positive identifications of life and found it encouragingly small. Following the suggestion of techniques for observing both wavelength regions there was a study of biomarkers by Des Marais et al. (2002). The key conclusions included: "Detection of O_2 or its photolytic product O_3 merits highest priority..." Planetary size and mass are very important indicators of habitability and can be estimated in the mid-IR and potentially also in the visible to near-IR.

NASA has supported the development of infrared techniques for estimating sizes and masses of planetary-sized bodies, and there is reason for expecting good answers from the process. Estimates of surface temperature will yield results if the planet's atmosphere is tolerably transparent at key wavelengths. Thus for the solar system the process yields sizes and masses for all terrestrial planets, and surface temperatures for all except Venus. Likewise use of the 9.7 μm ozone band is preferred to other ozone and oxygen bands for initial study because its high optical depth makes it the first indicator of photosynthetic life. We need better models both of earth evolution and associated planetary atmosphere spectra for a firm answer, but it is plausible that infrared ozone is visible as an indicator $\sim$1 billion years before the A band. These reasons make an IR first reconnaissance a natural step in the study of extrasolar terrestrial planets. Visible observations show weaker bands that with higher signal-to-noise ratio are very useful for more detailed analysis of a planet whose main properties have been established. JPL's summary states: "Observations in both the MIR and VIS-NIR ranges can give far more definitive results about conditions and composition than can a single range by itself..." Both wavelength ranges should be examined prior to launching a Life-Finder mission. "The choice of wavelength range for TPF will...be driven by the technological readiness of a given technique." It is the purpose of the remainder of this paper to give a personal view of technological readiness, and to ask whether one large mission is the best approach.

2. A Historical View of the Technology

At Steward Observatory we have been examining potential devices to observe extrasolar planets, and especially earth-like planets for many years, e.g., Angel, Cheng and Woolf (1986). In 1990, Angel modified the concept and suggested a 4-mirror interferometer in the "Angel Cross" configuration as a potential 16m class NGST. That configuration became the basis of the European "Darwin" mission concept, though with much smaller mirrors, and an original goal of orbiting the device at $\sim$3 AU. The ESA Darwin 2-dimensional interferometer continues this concept with separate free-flying telescopes. A 4-telescope linear interferometer concept for the task was developed as part of the Ex-NPS study (Beichman 1995; Angel and Woolf 1997).

At visible wavelengths, the studies of the adaptive optics requirements for detecting an extrasolar planet preceded Ex-NPS (Malbet, Shao and Yu 1994; Angel 1994; Stahl and Sandler 1996). At this meeting Roger Angel has shown that the optimum configuration for the adaptive optics sensing is in the focal plane, and that it would allow detection of earth-like planets either from a sufficiently large ground telescope or space telescope (though that has additional needs).

3. Ex-NPS

Serious interest in a terrestrial planet seeking mission began in 1995 when the NASA budget was declining 10% per year. Former director Dan Goldin described how he thought that "Earth in Space" is a great picture that has elicited great public interest and support for NASA. So he suggested offering the public an "Extrasolar Earth picture." At JPL, three industry/NASA/ University teams were set up to discuss the issues, and quickly saw that extrasolar Earth is likely to be 10^9 times further away than the moon, and require a camera 10^9times larger to get the same detailed picture. So if "Earth in Space" from the Moon needs a 1cm lens, to see $\sim$ 20 km detail, then an "Extrasolar Earth picture" needs an Earth-sized lens or primary mirror! And the fill-factor cannot be too small, or exposures will be blurred by the rotation of the extrasolar planet. It is an option we can best leave until later.

The next best image is the Copernican picture set "Planets going around a star" and two of the three ExNPS groups suggested the linear interferometer for this task. The device could also automatically obtain the spectra of each planet separately, and by looking for the 9.7 μm band of ozone (with long exposure times) one could determine whether a terrestrial planet was inhabited by organisms that photosynthesized oxygen.

The Ex-NPS study noted the possibility of two main options for direct detection of planets, a space interferometer in the infrared, or a visible coronagraphic adaptive optics system in space or on the ground (or possibly tried out first on a balloon). However, in the further development it was considered by

NASA HQ or JPL (I do not know which) that limited resources would require only one device to be developed and created soon, and the easiest of these, the interferometer, was chosen for further study.

The Terrestrial Planet Finder interferometer concept, and its planned follow up with more advanced missions met with immediate good support, both in the federal government and the general public. But some scientists saw differently, and thought it more as a potential drain on resources that might better go to their own work. So JPL suggested that the device should also be made useful for astrophysical observations.

After the concept (Beichman, Lindensmith and Woolf 1999) was "blessed" by the decadal review, there was concern that the analysis that led to the selection of the interferometer might have excluded some better option. A request for exploration of all other options in space resulted in rediscovery of the space coronagraph option (but not, by terms of reference, the ground coronagraph option). Also found were other, more complex, larger devices that did not perform any better. So the effect of the search for new options was to redirect attention to a concept which had already been rejected. However, one new element entered the picture. By the year 2000, astronomers were already concerned about the planned demise of HST. And it was thought that the excuse of TPF might be a way to get a second HST. Thus although the path to a visible wavelength TPF seems more technically challenging, some are willing to pursue the path in the hopes that it also leads to a Hubble successor. NASA offers no route to a ground coronagraph, and that is currently up to private foundations.

The "science first" approach of making the TPF goal independent of available technology has resulted in three current concepts, none of which have great credibility. We have a coronagraph with a 10m monolithic mirror, whereas NGST is currently struggling to work with 2-3m segments. We have a 40m interferometer truss squeezed and folded into a rocket shroud which recalls the words "even with a turkey that you know will fold, you may get stranded out in the cold." And we have a concept for an interferometer with free-flying telescopes despite the fact that NASA has grounded a proposal for a space test with two tiny free-flying telescopes because it is too expensive. The reality as I see it is that free flying interferometry is for the future. A 4m aperture in a coronagraph is the largest plausibly possible, if the vibration issue is resolved. And a 20m truss interferometer with just two mirrors could be tested and flown with current technology. Either of these could look for an earth around $\sim 20-30$ stars. And if we assume that the likelihood of new devices working in space is 50%, then those two missions would give us a 75% chance of starting the scientific program of seeking and studying extrasolar earths, and a 25% chance of getting both kinds of observation started. If instead we crank up the level of technology to a higher level for a larger scale first mission, then regardless of what we do, the probability of success is likely to go down from 50%, not up, and we have zero chance of getting both kinds of observation started.

I have worked on designs of both kinds of mission, and have conducted the only study to date of the device beyond TPF, Life Finder. I have had to consider both interferometers and coronagraphs for this type of work. Further, I have obtained a visible spectrum of the earth from studying earthshine (Woolf et al. 2002). Life Finder is only reasonable for the forseeable part of this century if we find an earth in the nearest group of stars. Although a TPF to explore more than 100 stars may be needed if terrestrial planets are very rare, it does not make sense for a program where we hope for Life Finder within the lifetime of even the youngest taxpayers. The operative words are *faster* and *cheaper*.

4. Interferometer Progress

Considerable progress has been made in design of a mission since the first paper on the linear interferometer: first there have been positive outcomes of nulling demonstrations both in the lab and at the telescope that show it to be a viable technique (Hinz et al. 2001; Serabyn 2000). The Double Bracewell nulling scheme has been invented (Woolf et al. 1998). Some of the key lessons learned are as follows:

1. Missions far from the sun are more helpful for cooling the telescope than for reducing the background. Likely exozodiacal dust signals may become a significant source of photon noise even for telescopes at 1 AU.

2. The mirrors in the OASES (ExNPS) configuration are not used effectively to optimize SNR. The outer mirrors broaden the null rather than add photons. The "Double Bracewell" scheme uses photons much more effectively.

3. The Double Bracewell configuration can be used to give two interferometer spacings a factor 2.5 difference in length, and so alleviate photon noise from star leak, when the interferometer baseline starts to resolve the stellar disk. Similarly a 3-mirror interferometer with only 2 used at one time gives the same result.

4. With a 2 mirror Bracewell interferometer, chopping can eliminate signals associated with thermal drifts of the equipment. Chopping can be used with a Double Bracewell interferometer to make it insensitive to symmetrical zodiacal clouds, and to reject phantom planets reflected across the star position for most interferometers.

5. The reduction in scope of a mission can allow much smaller telescopes to be used. Thus the detection of ozone may require a SNR of ~ 20 and a spectral resolution of ~ 20. In contrast, detection of a planet, determination of its size, approximate mass, and surface temperature can be performed at a spectral resolution of 5 and SNR ~ 5. The mirror diameter for the reduced task is 4 times smaller for the second task than the first. Thus a full mission perhaps needs four ~ 4m telescopes, a reduced scope mission searching for earths needs four ~ 1m telescopes or two ~ 1.4m telescopes. Ozone detection takes 64 times longer than that first step.

6. The length of an OASES configured interferometer is unnecessarily long. Thus a 75m baseline is needed to give an angular resolution of 0.035 arc seconds at the 9.7 μm ozone band. The same resolution can be obtained with a Double Bracewell interferometer of 40m baseline with easier packaging. Further, since substantial progress could be made with an early mission that looked to earths whose separations are > 0.07 arc seconds,

that could be performed with an interferometer with a $\sim$ 20m baseline, and the packaging of that into an EELV rocket is believable.

7. The projected cost of an infrared interferometer may perhaps not be the cost of the parts, but in addition might include major engineering costs. There is as yet no TPF-cryogenic-truss-nulling test bed, and until one is demonstrated, there is a wide discrepancy between costs estimated on the basis of the components expected to be adequate, and costs based on assumptions that it will need very many engineer-years of work to achieve an operative system. Only test bed experience can tell which is correct, and unless we get a simple test bed soon, the interferometer concept will be blowing in the wind without a solid hitch to real money.

8. The phase E cost can easily get out of hand. Putting everybody and "his sisters and his cousins and his aunts" [HMS Pinafore] into an inherently small project raises the cost so high that there is concern whether the technological certainty matches the cost. Then there get to be committees of overseers, division of the work between different companies and groups, and because the work has been divided, someone's schedule slips and it is not picked up, and we get into standing-army costs. Very soon faster-cheaper-better has become slower-costlier and worse. That division seems to come somewhere between keeping the cost roughly to a Discovery class mission, or pushing requirements beyond that range. That transition for a truss interferometer is likely between three stages of beam combining and one stage.

5. Current Status and the State of the Art

A review of the results of industrial studies and the current state of the art is available on line at:
http://planetquest.jpl.nasa.gov/TPF/TPFrevue/FinlReps/JPL/tpfrpt1a.pdf.

The executive summary notes that for the interferometric TPF "no single component seems to be unusually challenging, this architectural class presents significant challenges at the system level, including passive cooling, nulling, beam transport and formation flying or large precision deployable structures... The TPF-SWG also felt strongly that there were important scientific questions than could be addressed by fully capable TPF. For example a mission of lesser capability would be able to detect earths around a few tens of stars within $\sim$ 8 pc should any exist, and study the composition and physical properties of gas giant planets around stars as far away as 50 pc."

Our more recent study for a small interferometer found that there is a progression of technological needs that allow smaller scale missions to be accomplished with minimal development. That sequence is:

a. Construct and demonstrate a cryogenic truss nulling test-bed.

b. Take advantage of larger relatively inexpensive EELV rockets and shrouds.

Then for larger missions:

c. Learn how to null four telescopes together, operating three nulling stages together.

Telescopes	2 x 0.6 m	2 x 0.85m	2 x 1.7 m	4 x∼2.5m	4 x 2.5m	4 x 3.5m	4 x 3.5m
Truss	9m Truss Demo or hot Jupiters	9m truss Earths around $\sim$ 5 stars, Radius,Temp	18m truss Earths around $\sim$ 25 stars Radius, Temp, Ozone??	30m truss Earths around $\sim$ 100 stars, Radius, Temp, Ozone, 7.5μm H_2O	30m truss Earths around $\sim$ 100 stars, Radius, Temp, Ozone, CO_2, H_2O	40m truss Earths around $\sim$ 300 stars, Radius, Temp, Ozone, CO_2, H_2O	Free flyer Earths around $>$ 300 stars
Ground system demo	X	X	X	X	X	X	X
EELV			X	X	X	X	X
4 telsc., 3 beam combos				X	X	X	X
Sunshield/knuckle/orbit					X	X	X
Complex deploy						X	-
Free flyers and beam pjn.						-	X
Active cryo						-	X

Table 1. Possible TPFs with varying capabilities and requirements.

d. Either develop the extra complexity and handle the packaging problems needed for 40K operation at 1 AU, or swallow the rocketry cost of a 1x3 AU orbit.

These developments give options of a somewhat smaller and more believable TPF mission, capable of observing $\sim$ 100 stars, or a distinctly cheaper simpler mission capable at looking at between 20 and 30 stars. The detection of ozone for these mission options needs careful re-analysis, including a new study of the data reduction process. The JPL report suggests that the technical tasks needed to build a nulling interferometer are **nulling, detectors** and **cryocoolers**, **optics - especially 3.5-4m telescopes**, **precision deployable structures** and **precision formation flying.**

The fundamental technology of **nulling** has already been demonstrated cryogenically in the laboratory to the necessary level of performance for two-telescope missions. Nulling of two telescopes is simple, but nulling is quite complex for three or more telescopes. Missions that require more than two telescopes are ones where more stars need to be examined, and higher signal-to-noise ratio is sought to allow detection of ozone absorption.

Detectors performing at the level of SIRTF detectors are just adequate for a TPF mission. Improvement would be welcome, but is not necessary.

Likewise **cryocoolers** are a useful addition to the repertoire, but the existing performance of dewars carrying cryogens is enough to accomplish a currently proposed TPF mission.

Telescopes of 3.5 to 4m diameter would be needed to have a mission look at some hundreds of stars for planets and be able to take spectra under the assumption that many spectra would be needed. But reasonable TPF missions with 20-100 stars searched, and spectra obtained for some, could be accomplished without the larger mirrors.

Deployable structures are indeed needed for structurally connected interferometers, and we see them as the easy way to get started. For trusses up to $\sim$ 30m length, a simple hinged, lockable pair of rigid sections is adequate. Complex precision deployments enter if longer trusses are needed. Fortunately the Double Bracewell scheme accomplishes what had formerly seemed to need a 75m truss with a 40m truss etc. If we use trusses, **formation flying** is not needed.

We have found an additional difficulty with passive cooling. Passive cooling to $\sim$ 60K is fairly simple for these rotating trusses, but to get to 40K requires a more complex deployment that takes shroud space which otherwise might be used for a longer truss. A 60K device cannot look for the CO_2 band at 15 μm, though all other tasks are possible. Also, a device with a fixed length has lower resolution at the CO_2 band, and so only 1/3 of the stars that can be studied for planet temperature, size and ozone could be studied for CO_2 anyway. We need to further explore the performance and technology of missions with two telescopes and 60K cooling versus ones with 4 telescopes and more cooling.

The most significant item is that a cryogenic truss nulling test bed is needed for any mission, and we need to be able to null two telescopes before we try to null more than two. I believe that we have the knowledge to implement a test bed now. Thus the next step for development of a flight interferometer is to test the performance of a cryogenic nuller with a truss, and to show that the level

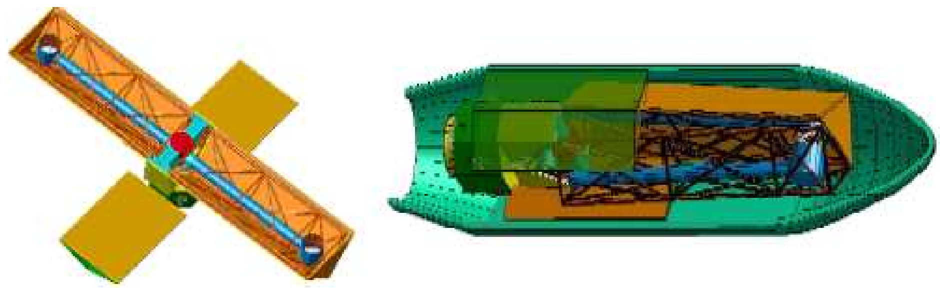

Figure 1. Concept for a simple 2-mirror space interferometer (left), and folded in a Delta 2 rocket shroud (right).

of control of the nulling is sufficient for a flight. A share of the use of BLINC, together with a small truss (which we hope to get in phase two of our study) would allow us to start learning immediately.

The smallest, simplest space nullers are ones in which a small truss is fitted into a Delta 2 rocket shroud and launched into a SIRTF-like orbit. Such devices could either be a pure test bed, or be a device for observing jupiter sized planets, particularly young jupiter sized planets at distances of 30-100 pc. Larger trusses and mirrors in an EELV cost about $100M more. The problem with any interferometer is that the task of stabilizing and pointing a rotating truss to the required accuracy while making observations has not been demonstrated. If it is easy, the costs for a two mirror system are within range of Discovery class missions, but if major difficulties are found, then as described, the cost of even a simple mission moves into in the $ billion class.

6. Comparison of Interferometer and Coronagraph Technology

While the interferometer mainly needs a test bed, there are substantial technological issues to be resolved with coronagraphs. The particular most crucial issue is how one deals with spurious planets. Both the interferometer and the coronagraph can produce spurious planets, as a result of temporal phase fluctuations (interferometer) or spatial phase errors (coronagraph). The critical temporal frequencies are $\sim 10^{-3}$ Hz for the interferometer and the spatial frequencies a few meters for the coronagraph. The amplitudes that cause difficulty are given by the formula:

$$(2\pi\sigma/\lambda)^2 = (planet\ intensity/star\ intensity).$$

For the coronagraph the concern is about amplitudes of ~ 1 picometer, and for the interferometer it is ~ 500 picometers. The starlight does allow control at the resonant frequency ~ 10 Hz to a level roughly of that required for the interferometer, but for the space coronagraph it takes many seconds to get the wavefront information. For the ground coronagraph, measurements are made at frequencies $\sim 10^3$ Hz, and a long time averaging process is expected to bring spurious planet signals to an adequately low level. Ground testing of a space mission with vibration control at 1 picometer seems a daunting task. If there is not to be a ground test, then there needs to be some appropriate substitute.

7. Conclusions

In summary, both coronagraphic and interferometeric work are needed. They give different information, that together tells a great deal about a planet, and with one alone tells far less. However, I do believe that the fastest and cheapest we can learn about the "local" presence and general character of terrestrial planets is through constructing, testing and launching an infrared interferometer as soon as possile. I recommend that this be a smaller device than has currently been thought of as satisfying TPF goals, and that it should be "a mission of lesser capability [that] would be able to detect earths around a few tens of stars within ~ 8 pc should any exist, and study the composition and physical properties of gas giant planets around stars as far away as 50 pc."

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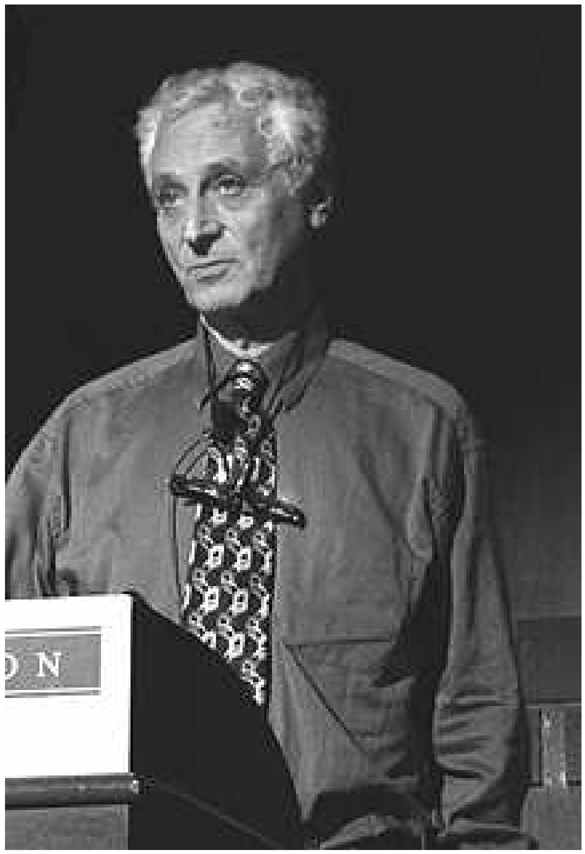

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