A Brief History of NASA's ExoPAG
2009-2012

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NASA's ExoPlanet Exploration Program Analysis Group (ExoPAG) was commissioned in Fall, 2009, by the Astrophysics Subcommittee of the NASA Advisory Committee, to which the ExoPAG Chair reports. Jim Kasting was chosen as the initial ExoPAG Chair. The first ExoPAG meeting (ExoPAG1) was held in January, 2010, in conjunction with the AAS meeting in Washington, DC. The room was full with at least 80 attendees for 2 days of this pre-conference meeting. The atmosphere was electric. People were excited, partly because they recognized that exoplanet science was a young and rapidly growing field, and partly because the Astro2010 Decadal Survey was in full swing. Big decisions would be made within the next several months, and everyone was eager to find out who would be the winners and losers.

ExoPAG1 had a number of positive outcomes. Five Study Analysis Groups (SAGs) were formed to study various aspects of exoplanet science. Steering Committee member Aki Roberge led a discussion of the exozodi dust problem—one of the two key issues that would be nice to resolve before a direct imaging flagship mission is flown. (The other key issue is η_{Earth}—the fraction of Sun-like stars that have at least one rocky planet within their habitable zone.) Alycia Weinberger led a discussion of the potential for exoplanet science from future extremely large ground-based telescopes. Wes Traub, Chief Scientist for the Exoplanet Exploration Program (ExEP) Office at JPL, gave an initial presentation regarding double-blind studies of occulters and coronagraphs—two different mission architectures that might one day be used to do a direct imaging flagship mission to look for Earth-like planets. This mission, or set of missions, was called "Terrestrial Planet Finder" (TPF) in the literature up to 2010. The Astro2010 Survey renamed this mission "New Worlds Observer" (NWO), after Web Cash's proposed space-based occulter.

ExoPAG2 was held 6 months later in Pasadena (June, 2010). We were now getting close to the time when the Astro2010 Survey was supposed to release their report. NASA's Kepler Mission was starting to get results, and we heard an update on this from Nick Gautier. We also heard lots more discussion of exozodis from the exozodi SAG. Phil Hinz gave a progress report on the Large Binary Telescope Interferometer (LBTI) being installed at the Mt. Graham Observatory. LBTI represents our best chance of determining exozodi brightnesses from the ground. We had talks on external occulters from Charley Noecker, Stuart Shaklan, and Remi Soummer. Wes Traub gave a more detailed presentation on his proposed instrument performance study to compare the relative merits of internal coronagraphs versus external occulters. Several speakers talked about the synergy, or lack thereof, between direct imaging and astrometry. SIM Lite, the downscaled Space Interferometry Mission, was still considered viable at this time, and much of this discussion centered on whether SIM Lite was a useful precursor for TPF/NWO. For the record, this discussion never reached a definitive conclusion. Astrometric information about a planet's existence and orbit is quite useful for some variants of TPF and less useful for others. If the TPF telescope is a relatively small (~4 m aperture) coronagraphic type, then the target star list is restricted and multiple revisits are possible, and so it doesn't matter too much whether one

has prior astrometric information. But if the telescope is large, and particularly if it employs a slow-skewing occulter, then information from a precursor mission like SIM Lite might contribute significantly to the number of Earth-like planets found.

This brings us to ExoPAG3, held in January, 2011, in Seattle. This meeting was also electric, but for a different reason. The Astro2010 Survey had been released several months earlier. SIM Lite was gone for good, and WFIRST was selected as the next flagship (well, small flagship) mission. WFIRST contained a microlensing component that some exoplanet scientists were excited about, *e.g.*, the current ExoPAG Chair, Scott Gaudi, and former Steering Committee member, Dave Bennett, but that had less appeal to the broader exoplanet community. The elimination of SIM Lite meant that we needed to know more about the potential for ground-based radial velocity (RV) measurements for discovering Earth-sized exoplanets. Andrew Howard gave a presentation on the state of the art for RV. Current measurement precision is ~1 m/s for most stars, which is a factor of 10 less than the 10 cm/s needed to find the Earth. Furthermore, it is limited more by stellar noise than by measurement precision. Thus, RV may or may not be able to make up for the absence of SIM Lite. (But stay tuned—this field continues to evolve, and perhaps the RV practitioners will indeed end up identifying nearby ExO-Earths.)

The good news from the Decadal Survey was that NWO technology development was listed as the highest medium cost priority for NASA. Hundreds of millions of dollars of technology money were to be spent on this project in the latter half of this decade to allow NWO to be ready to be selected in 2020. This galvanized us all into action. We collectively decided that, if large amounts of money were shortly to be made available, we needed to figure out how best to spend it. That meant that we needed to make a downselect between competing NWO technologies—coronagraph or occulter—by 2015. A plan was formed for how to do this. We issued "Points of Scientific Agreement" that directly articulated the need for a >4 m aperture, optical/maybe-UV telescope. (UV capability is considered optional for exoplanet science, but is thought by some to be essential in obtaining broader community support for such a telescope.) We also issued "resolutions" in which we recommended that NASA not spend further money pursuing a thermal-IR interferometry mission as the next flagship and that we needed to do a mid-decade downselect on an optical/UV direct imaging telescope. The full list of points of agreement and resolutions can be found in the Chairman's report to the Astrophysics Subcommittee under the "Meeting Summary" tab for ExoPAG3.

One important caveat should be added to these recommendations: The fact that we recommended not pursuing a thermal-IR interferometry mission, *e.g.*, TPF-I or ESA's Darwin mission, does *not* mean that we considered it to be scientifically less valuable than an optical mission. Indeed, at one point several years earlier than this, many of us had strongly encouraged NASA to fly *both* missions, one after the other, as their science is complementary. But most of the participants in the ExoPAG who have worked with NASA in planning for these projects over many years believe that TPF-I is more expensive and difficult than an optical TPF. So, we think that the IR-interferometry mission should be done second, rather than first.

As part of the outcome of ExoPAG3, Exoplanet Program Doug Hudgins (from NASA Headquarters) outlined a plan to create two funded Interim Study Working Groups (ISWGS) to study coronagraphs and occulters, respectively. These ISWGs were to begin their work in

January, 2013, and turn in their reports by January, 2014. We were on a roll, big things were anticipated, and everyone left the meeting feeling energized.

Sometime after ExoPAG3, all of these big plans started to unravel. The federal budget was getting harder and harder to forecast, partly as a consequence of the recession and partly because of political differences in Congress. More importantly, within NASA, the true magnitude of the budget problems for JWST, the James Webb Space Telescope, became apparent. JWST is an ambitious, 6.5-m aperture telescope destined to be flown at Earth-Sun L2. For reasons that need not concern us here, its projected cost had grown to something exceeding $8 billion. This created a huge fiscal problem within NASA—one that has ended up delaying, or indefinitely postponing, a number of large projects. By the time of ExoPAG4 (June, 2011), some of the implications of this problem were beginning to sink in to the participants. At that point, we were still talking about NWO/TPF. But NASA Astrophysics Division Director Jon Morse had told us that it might not be important to do the mission architecture downselect by 2015, as the money for NWO technology development might be slow to materialize. The writing was already on the wall: no mission of that magnitude was going to happen anytime in the near future.

By the time of ExoPAG5, held in January, 2012, in Austin, the flavor of the ExoPAG meeting was quite different. Michael Devirian, the ExEP manager at JPL, gave a presentation at the end of the first day in which he pointed out the need to start thinking about smaller, probe-class exoplanet missions. He proposed two general cost categories, $350-650M and $1-2B. Devirian had always been a strong proponent of TPF, so to hear him back off from this goal affected us all. We heard talks at this meeting from the PIs of three Small Explorer-class missions, TESS, FINESSE, and EXCEDE. The good news, as this history is being written, is that TESS (the Transiting Exoplanet Survey Satellite) has been selected by NASA as a future mission. This is really quite exciting, as this mission should identify transiting Earth-sized planets around a number of nearby stars, some of which might eventually be characterized using transit spectroscopy from JWST. But it is a far cry from TPF, and the chances of finding nearby habitable planets are correspondingly smaller.

This brings us up to the current ExoPAG era, and I will turn the history lesson over to current ExoPAG Steering Committee Chair, Scott Gaudi. Before I do, though, let me say what I think is the appropriate long-term path for exoplanet science. I'm an astrobiologist by training, and so my own personal goal has been the search for other Earth-like planets and, ultimately, for evidence of extraterrestrial life. This goal, I might add, is one that has been shared by many astronomers, most famously the late Carl Sagan. I am skeptical that this search can be carried out successfully from the ground, despite the expected completion of large (30-40 m aperture) telescopes within the next 10-15 years. Even if these telescopes succeed in finding nearby Earths, they will have difficulty searching for biogenic gases, because they will be looking through the same biogenic gases that are present in Earth's atmosphere. Hence, I think we need to eventually move forward with a space-based, optical/UV/near-IR version of TPF/NWO. But, because we have lots of time in which to design this mission, and because we desperately need the support of the larger astronomical community for this project, we should opt for something larger, perhaps an 8-m aperture, rather than something smaller. And, most importantly, we should design this telescope to be *serviceable*, like the Hubble Space Telescope (HST). HST was in low-Earth orbit, and so it could be serviced by the Space Shuttle. TPF will be at Earth-Sun L2, and so

servicing it will require enhanced launch capability, along with a new crew transport vehicle. One option would be to fly the telescope to Earth-Moon L1 and then service it there, but even this would require increased lift capability and a new crew vehicle. JWST could not be designed to be serviceable because there was no manned servicing capability envisioned for its projected time frame. We should therefore work with the manned space side of NASA to ensure that future large telescopes such as TPF/NWO can be serviced and can remain on duty for 20 years or more, like the Hubble.