~~IROUV~~
Habitable Worlds Observatory Decadal Context

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SPEAKING FOR OURSELVES

Context and things to remember

- The people on the Decadal Committees and panels are smart and knowledgeable (though not infallible)
- NASA is smart and knowledgeable
- The broad astronomical community is smart and knowledgeable
- It's impossible to write a document that will cover ever eventuality

- Consequently: we know things will evolve, and we didn't attempt to design missions
- The survey represents guidance and priorities, not dictatorial fiat
- No number should be considered accurate to more than 20-30%

Factors influencing the study

- In 2020-2021, the shadow of JWST loomed over budgets and programmatic planning needs
- WFIRST/Roman evolved in complicated ways and is still on the ground
- Detailed and compelling large-mission studies
- Much reduced uncertainty about astrophysics (zodi, eta_earth) and technology (coronagraphs, optics)
- Extensive work on new project approaches for missions by many groups
  - More substantial early technology maturation
  - Larger investments to produce more accurate cost estimation before committing
  - This led to the Great Observatories Mission and Technology Maturation Program

The official language

Conclusion: A high-contrast direct imaging mission with a target off-axis inscribed diameter of approximately 6 meters provides an appropriate balance between scale and feasibility. Such a mission will provide a robust sample of ~25 atmospheric spectra of potentially habitable exoplanets, will be a transformative observatory for general astrophysics, and given optimal budget profiles it could launch by the first half of the 2040 decade.

Recommendation: After a successful mission and technology maturation program, NASA should embark on a program to realize a mission to search for biosignatures from a robust number of about ~25 habitable zone planets and to be a transformative facility for general astrophysics. If mission and technology maturation are successful, as determined by an independent review, implementation should start in the latter part of the decade, with a target launch in the first half of the 2040s.

Tensions

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The goal of launching 2041-2045 is critical to program balance and the progress of the field, and is as important as any science yield

Mission costs

“To assess the budget scale and profile requirements for the recommended direct imaging mission, the survey committee performed an analysis assuming the cost profile and schedule from the LUVOIR-B TRACE analysis, normalized to a total integrated cost equivalent to JWST inflated to current year dollars”

$11 billion FY20

Even in optimistic budget scenarios, a $11B-class mission is challenging; growth beyond that would make it very challenging to launch by 2045 and implement the next mission

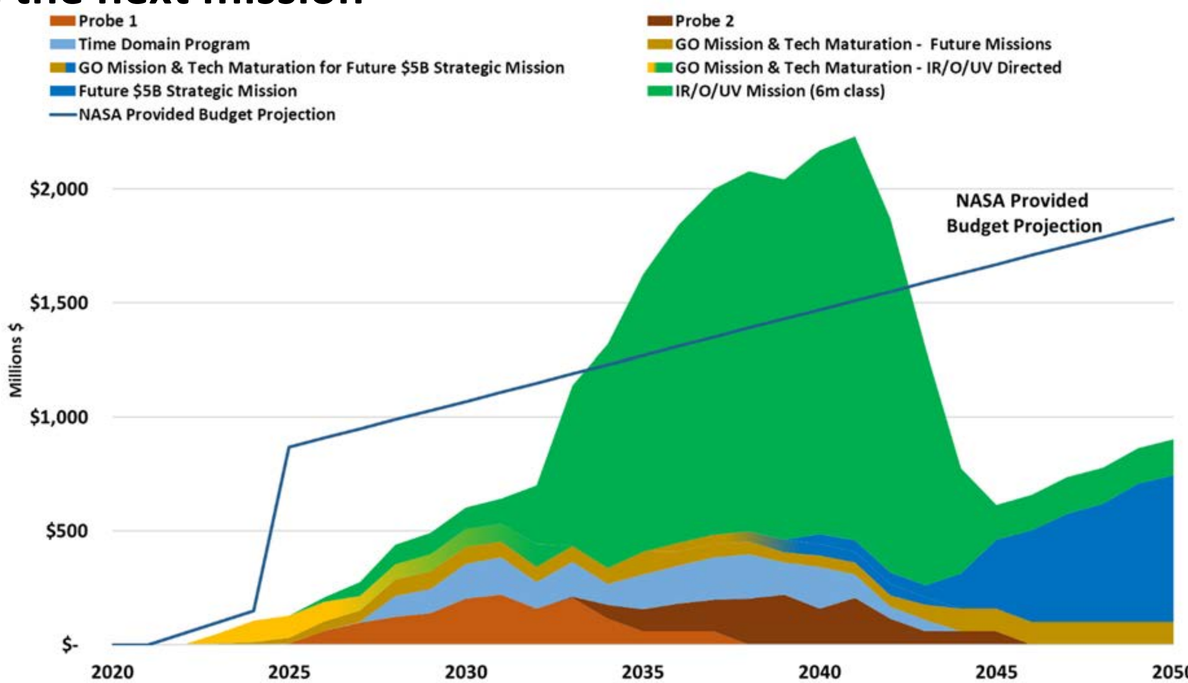

FIGURE 7.10 Astro2020 recommended program for NASA. This budget profile shows approximate funding requirements associated with construction and operation of all space-based medium and large recommendations. For the IR/O/UV mission, operations are assumed to extend beyond 2050. The ultimate project/program profiles and budget requirements will depend on the actual implementation and on NASA's budgeting process. The chart shows a program whose costs integrated through FY2043 are approximately equal to the budget available over the same period. The solid line indicates the optimistic budget projection that NASA provided to the survey. The jump in NASA's available astrophysics budget around 2025 reflects completion of Roman and reduction in other current commitments.

Mission costs

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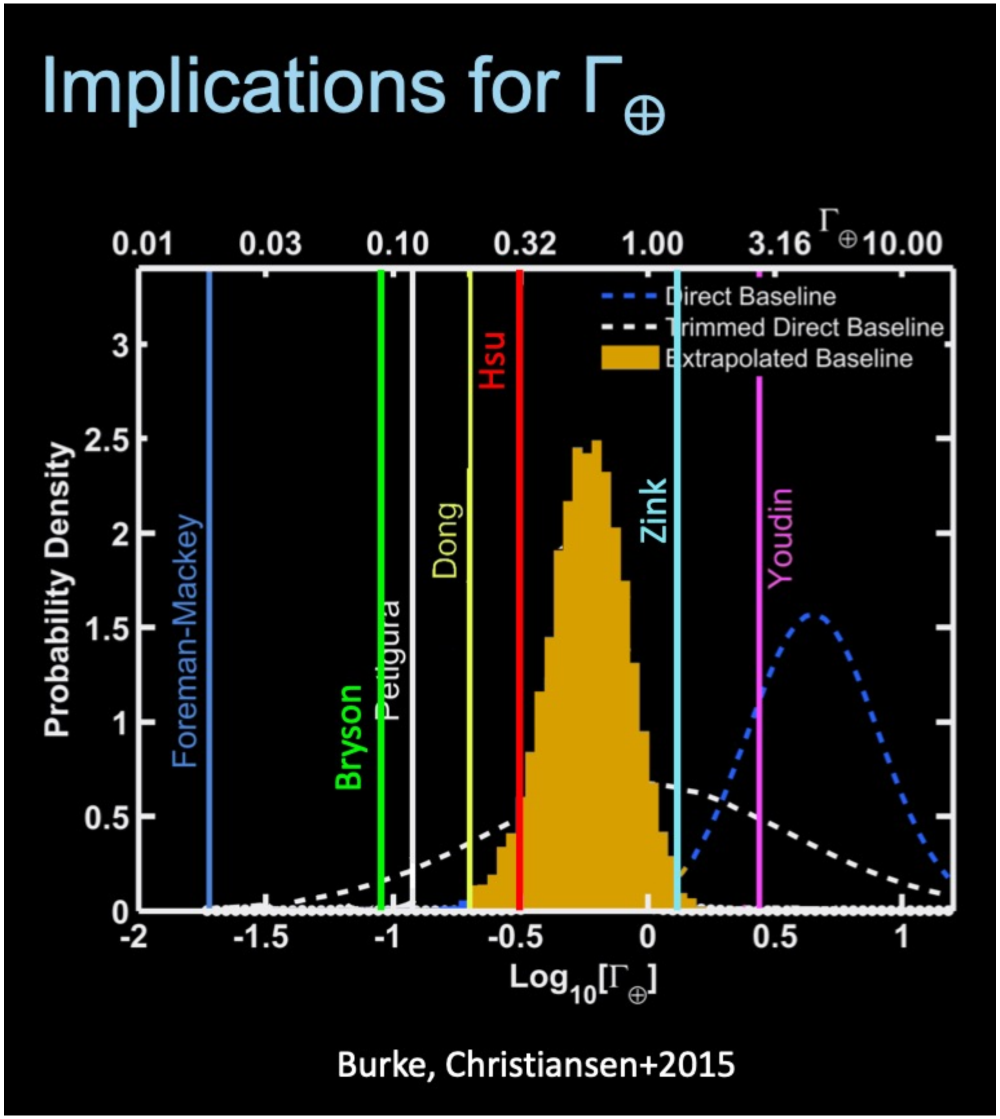
- We recognize that mission costs are significantly uncertain
- Refining, iterating and estimating the cost and making the science <-> cost trade is explicitly the goal of the GOMAP program
- The goal of a 2041-2045 launch produces a plausible upper bound on cost; even achieving $11B will require require an inspiring vision and the support of the whole community

Eta_earth

- The Survey and Panels (Exoplanet science, EOS1) had briefings and updates on occurrence rates of planets (eg Christiansen, EOS1 panel, January 2020) and remained engaged with the literature (Bryson et al. 2021)
- Helpful discussions with Chris Stark and others on implications for mission yield

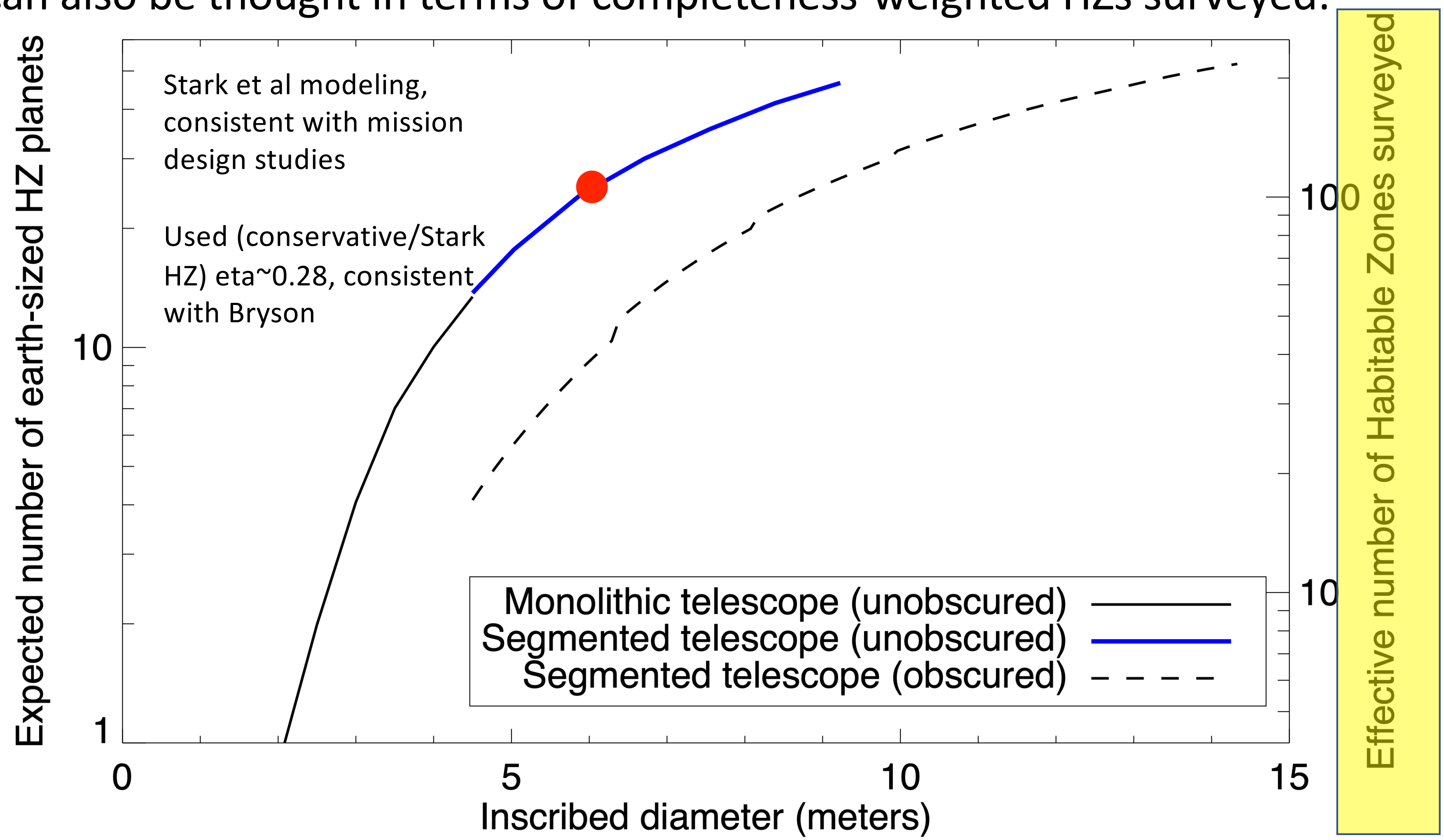


25?

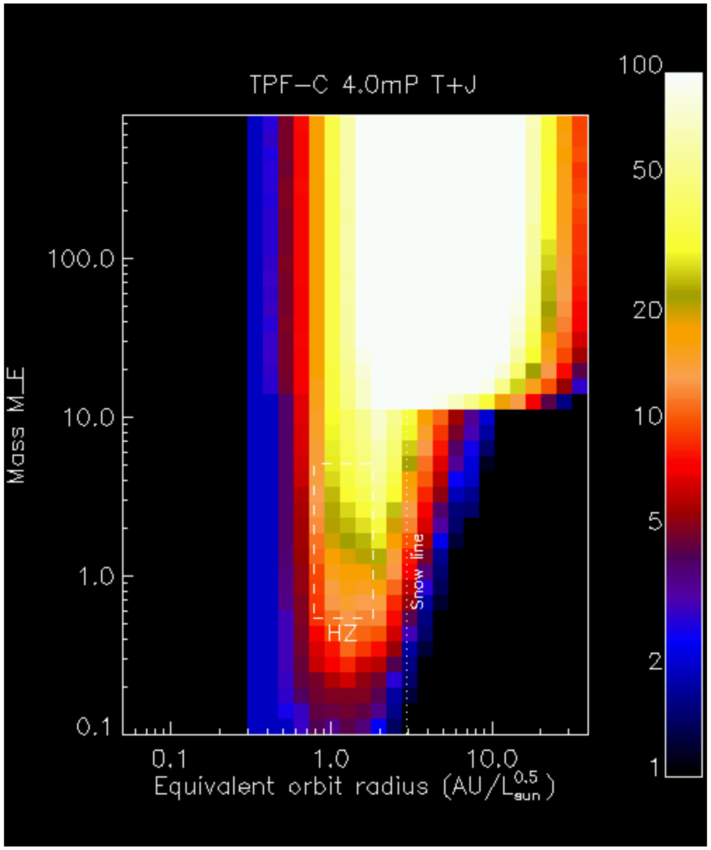
"A sample this size provides robustness against the uncertainties in the occurrence rate of Earth-sized worlds, and against the vagaries associated with the particular systems near Earth"

- Eta_earth uncertainties are built into the requirement for target number of systems
  - Bryson et al 2021 uncertainty: -50%
  - Eta_earth is not the dominant uncertainty in probability of finding life
- We expect that the mission will characterize a large number of additional planets, providing context for HZ planets and illuminating evolution
- Final trade for mission yield will require a much more detailed DRM and be assessed in the GOMAP process

Yield can also be thought in terms of completeness-weighted HZs surveyed:



Effective number of HZ surveyed



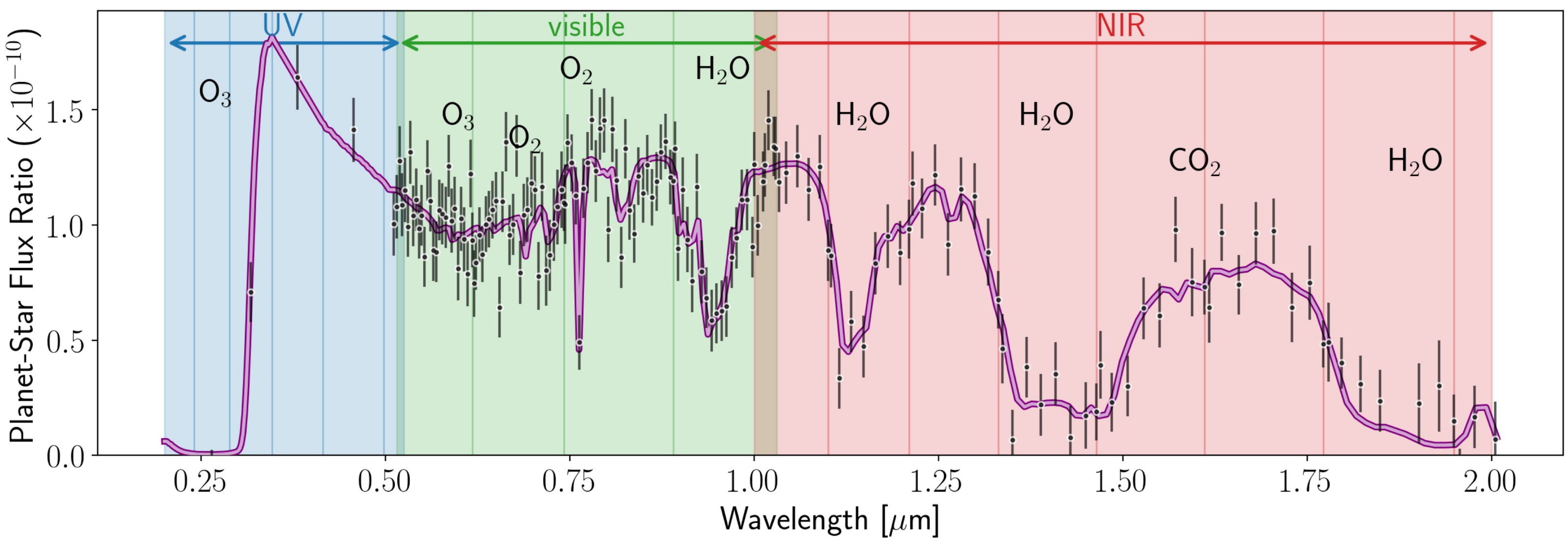
Total completeness example from Lunine et al 2008 ExoPTF

- This can be thought of as the sum of the completeness in the HZ (weighted by planet occurrence) over a whole survey
- For moderate eta_earth, this is equal to the number of planets characterized in a simulated survey divided by eta_earth
- Weakly dependent on eta_earth in full mission simulations (Stark et al 2019) but generally robust against planet properties
- Possibly a better high-level requirement

What sort of spectra?

"Then for the most exciting ~25 planets, astronomers will use spectroscopy at ultraviolet, visible, and near- infrared wavelengths to identify multiple atmospheric components that could serve as biomarkers"

Optimizing this is also a GOMAP iterative cycle; requires very careful DRM development



Conclusions and key takeaways

- **Eta_earth margin was built into the original science goal**
  - **Requirement could also be expressed as ~100 HZ-equivalents**
  - Mission requirements have to be astrophysics-independent
- (Roughly) 6m, (roughly) 11 billion dollars, (roughly) 100 HZ-equivalent surveyed was a good balance of Decadal priorities
  - Any mission on this scale will be transformative even if eta_earth is low
- An upscoped massively-capable mission that explodes out of its budget envelope and is cancelled is not, in fact, a success
  - The goal is to actually fly a mission
- **The cost and schedule envelope is as critical a recommendation as the science yield**
- This will evolve under GOMAP and these will always be under tension