

Decadal Survey Science Goals and the HWO flagship

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SPEAKING FOR OURSELVES

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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 every 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 melodramatically 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

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.

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

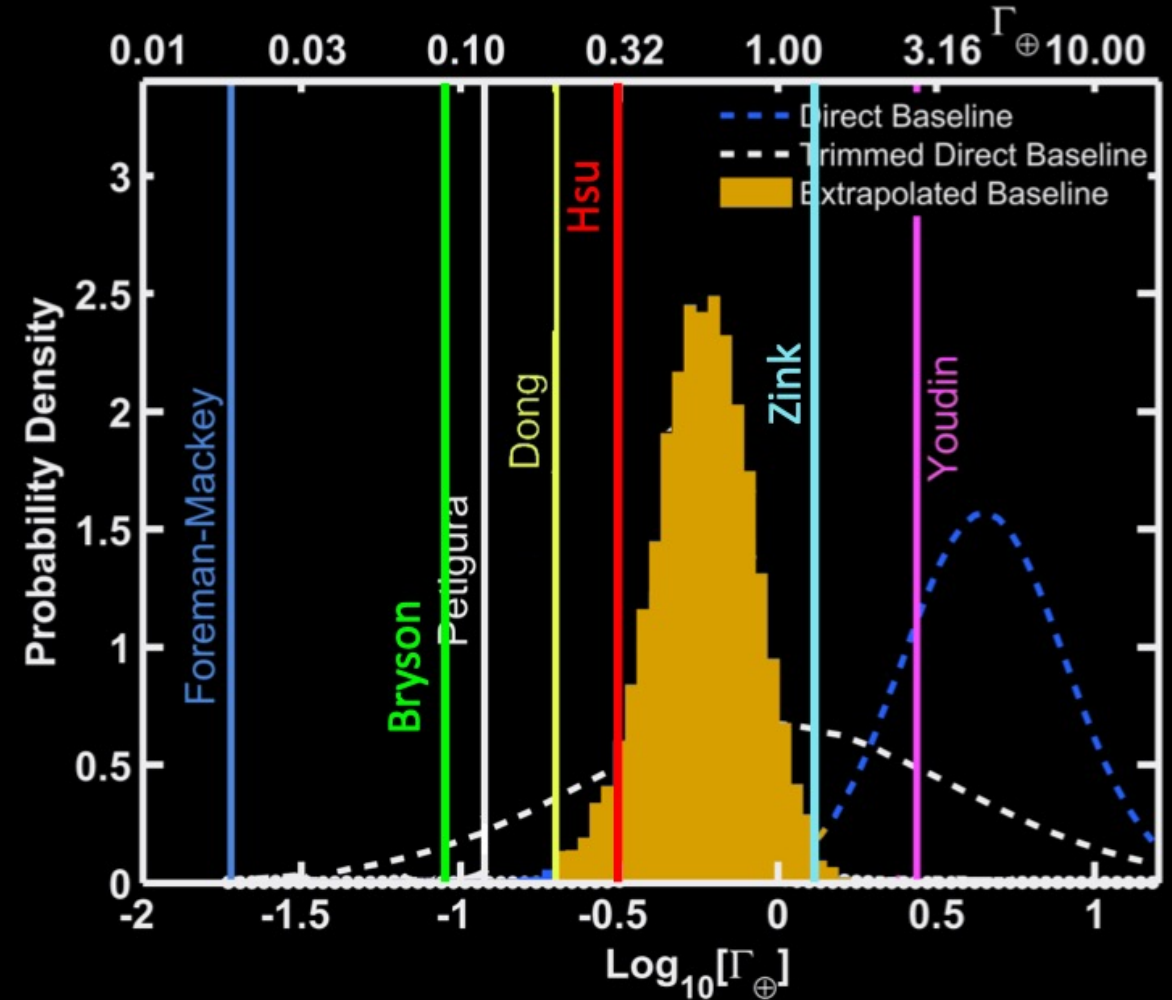
\$11 billion FY20

- 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 HZ definitions, weighting, implications for mission yield

Implications for Γ_{\oplus}



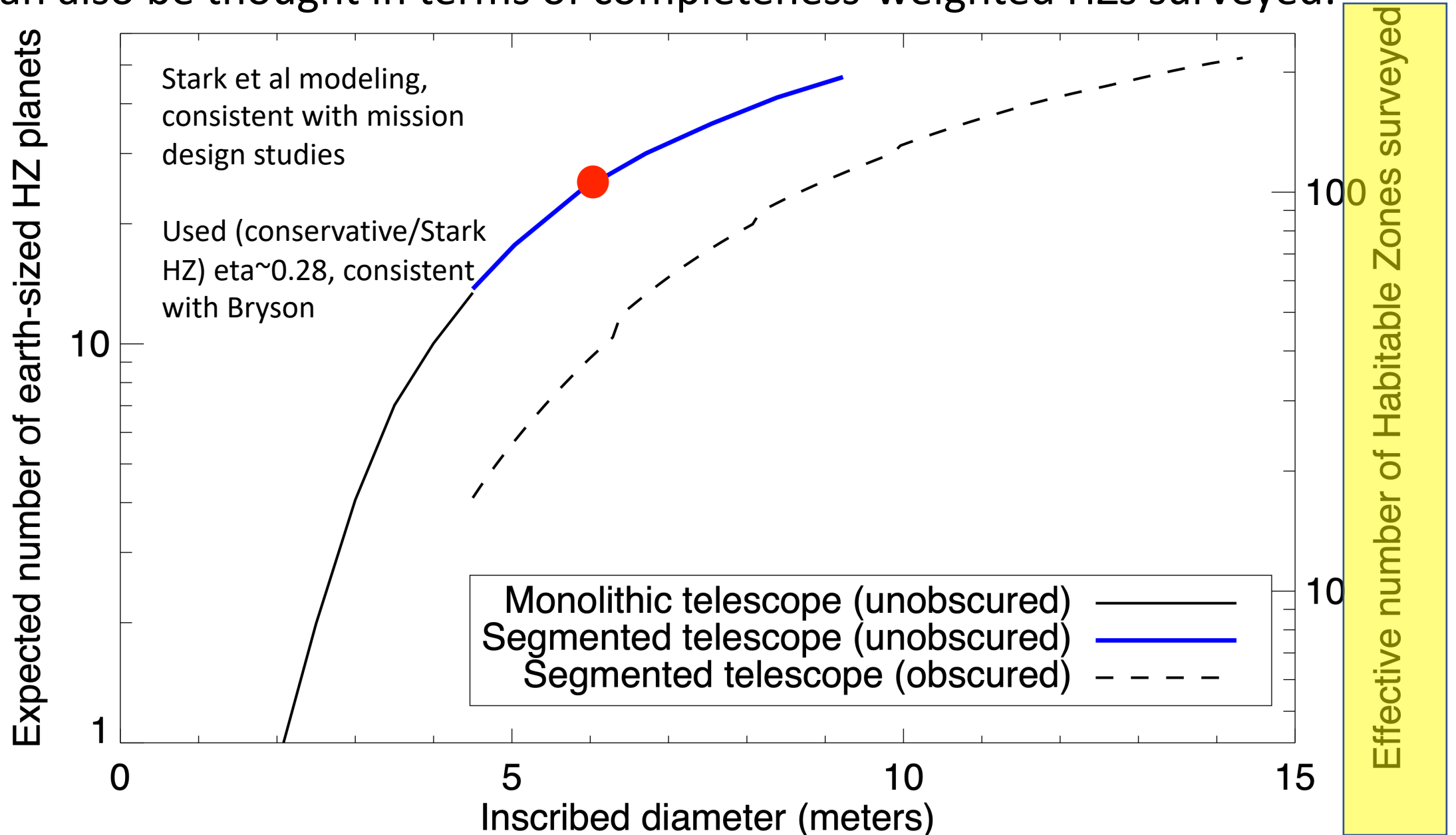
Burke, Christiansen+2015

25?

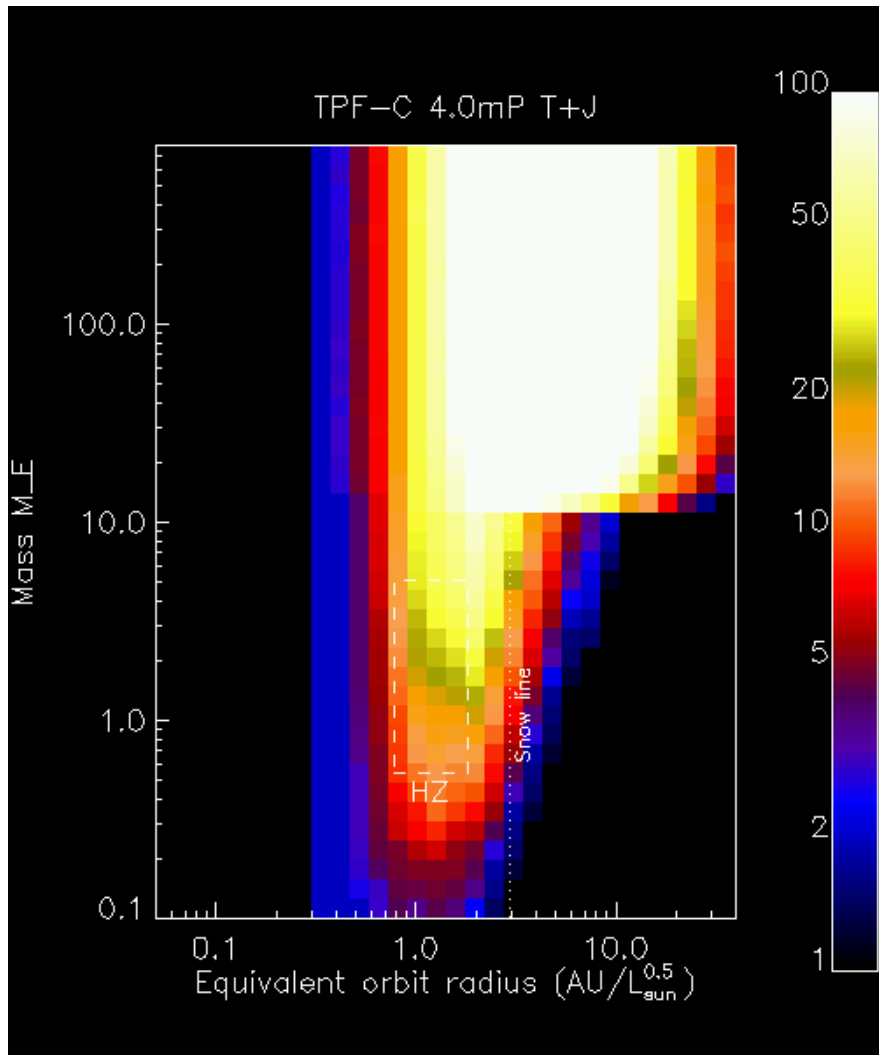
“A sample this size provides robustness against the uncertainties in the occurrence rate of Earth-sized worlds, and against the vagaries associated with the particular systems near Earth”

- 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 knew this would evolve and likely downward
- We expect that the mission will characterize a large number of additional planets in many systems, 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

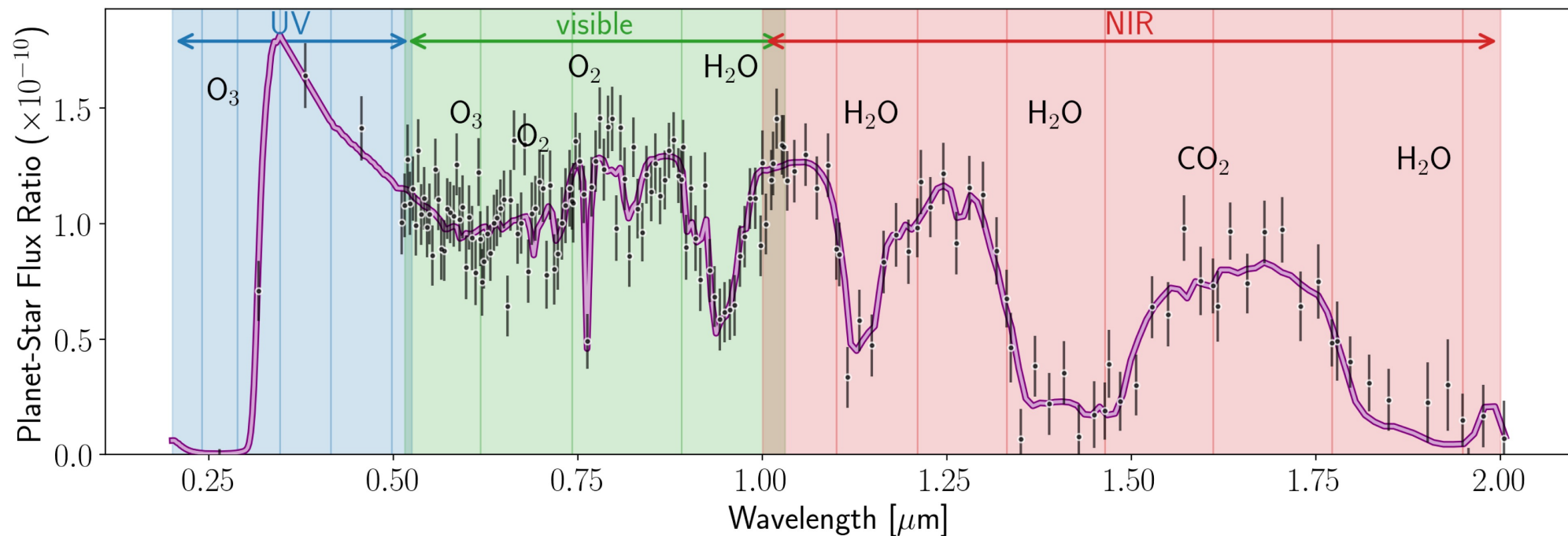


- This can be thought of as the sum of the completeness in the HZ (weighted by planet occurrence) for spectrally-characterizable planets over a whole survey
- For moderate η_{earth} , this is equal to the number of planets characterized in a simulated survey divided by η_{earth}
- Weakly dependent on η_{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?

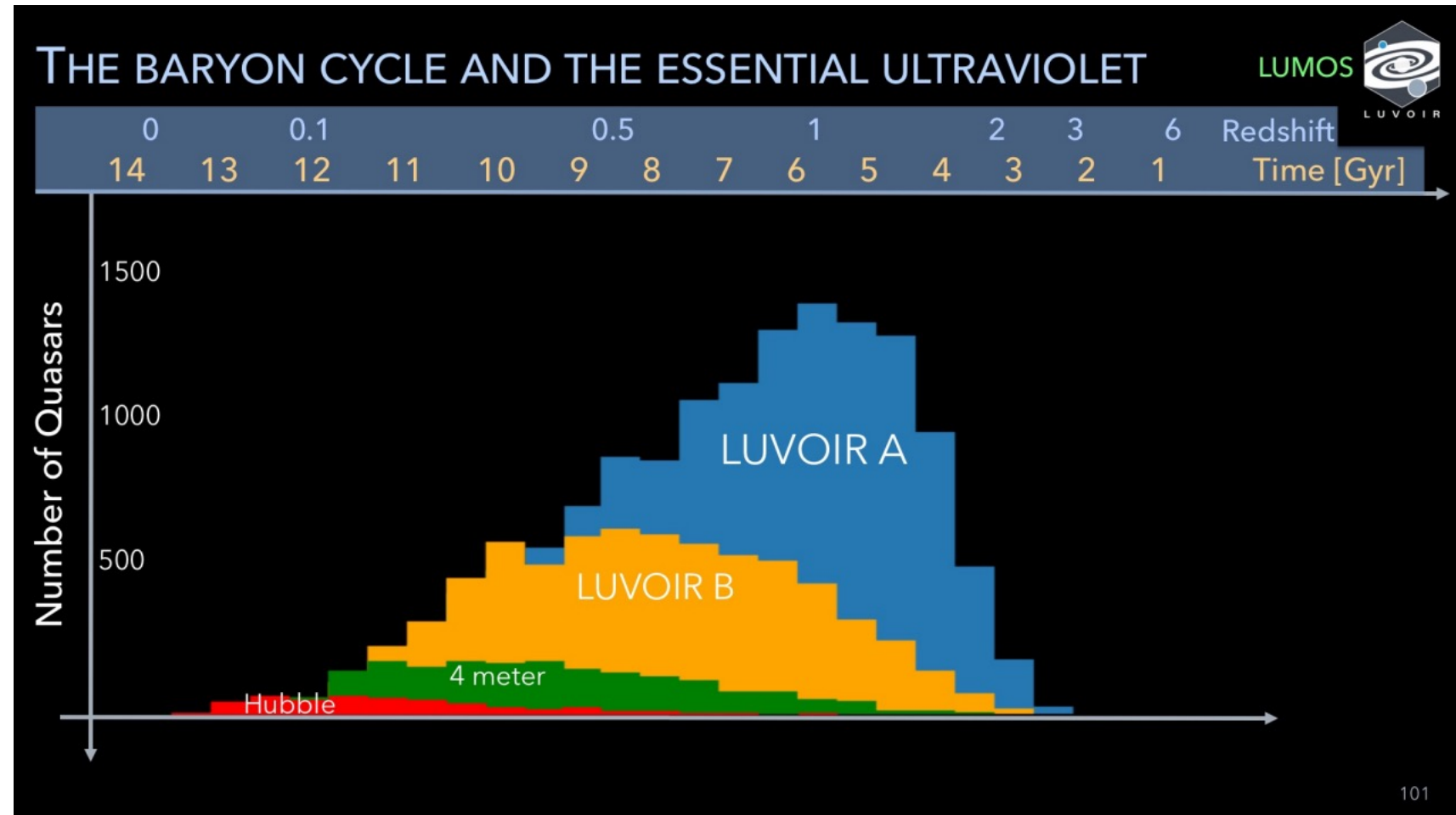
“Then for the most exciting ~25 planets, astronomers will use spectroscopy at ultraviolet, visible, and near-infrared wavelengths to identify **multiple atmospheric components** that could serve as biomarkers”

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



“A transformative facility for general astrophysics”

- Key decadal non-exoplanet goals (eg galactic ecosystems) push towards UV capability and 6-m-class aperture (see EOS1)
- JWST-class-diameter is a compelling hook



Risks and concerns

Coronagraph

- Lack of laboratory demonstrations at full bandwidth and contrast level
- Physical scale of BMM DMs and lack of laboratory demos
- UV and near-IR performance
- Needs to be at a TRL high enough to do mission trades as GOMAP stars

Telescope

- Optical stability of telescope, especially for mid-frequency errors (EOS-1 Section D)
 - CTE variations
 - Segment motions
 - Composite creep
 - JWST experience worrying...
- Coronagraph should be robust and amenable to continuous wavefront control

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, \$11 billion, (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
- The cost and schedule envelope is as critical a recommendation as the science yield
- Telescope and coronagraph and wavefront sensing and wavefront control have to be thought of as a system to enable trades
 - Telescope stability is a high-risk area – can we help?
 - Coronagraph margin can buy down telescope risks
- This will evolve under GOMAP and there will always be under tension