

This is a challenging engineering project

Notre Dame – 1160-1345

Great Wall of China – 5th Century to 16 Century BCE

TPF-K – ?

(Great Pyramid in 20 years and Machu Picchu in 10 years,
but they had slaves)

Incrementalism seems to be our future.

Characterizing earthlike planets is hard.

Solutions are expensive.

Rather than TPF-C, let's talk about an exoplanet program.

In that context, we heard a great deal of interesting science and mission ideas.

What about a “flagship” mission?

Design to Cost

Vs.

Cost the Design

It appears that we are limited to \$600M for the foreseeable future.

What can we accomplish if we collaborate with Europe? Japan?

What is resulting science? Is it compelling? Will it garner public support? At what point do we say not good enough?

Existing technology and experimental evidence must support the design and cost credibility is essential

So, how do we spend the taxpayer money?

- Support *analysis* and *basic technology* research with emphasis on laboratory validation
- Robust program of small scale science (ground observations, sounding rockets, balloons)
- Incremental evolution of larger missions (e.g., smex/midex/discovery, ST's, internal coronagraph followed by occulter)

This will most likely involve smaller scale science and postponing terrestrial planet characterization.

We need a mix of healthy competition and cooperation.
Delicate balance.

Purpose of Workshop

- This is a community workshop; we will:
 - Survey all types of exoplanet missions
 - Estimate detection and characterization yield for each
 - Focus on low-cost missions with dazzling science
- We may need to revise our view of desired science:
 - More Jupiters & zodis?
 - Fewer Earths?
- **Our goal is to identify a scientifically exciting mission that makes dramatic advances in exoplanet science and paves the way for still more exciting projects to come.**

Is there an opportunity here outside smex/midex/discovery?

