Strategic implications of a small U.S. investment in Euclid

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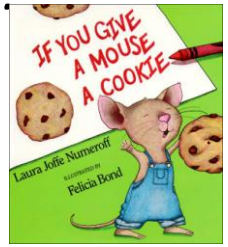
Why this is a big decision

- WFIRST is the first NWNH space priority and last big space mission standing; highly optimized to address 2 major science goals + survey science
- Community is counting on it and the future of US space astrophysics this decade (and next) turns on it
- Small investment in Euclid *will* have a negative impact on WFIRST
  - Experts know that Euclid does not achieve the science goals of WFIRST, but that is a subtle fact in a time of very constrained budgets. Non-experts might conclude Euclid > WFIRST. *Theorem: Euclid will not improve the chances for WFIRST to go forward -- and could de-rail it.*
  - Small investment can become larger, cf, Planck and yesterday's discussion, and slow WFIRST
  - The Big Question: For the limited benefits to a few (and good world science citizenship), how big a risk are we willing to take?
  - NB: The US has the largest Dark Energy community, and a small investment in Euclid will benefit only a few



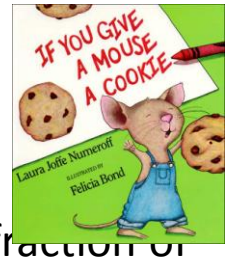
WFIRST vs. Euclid on dark energy

High points to add to what Paul Schechter said

- No SNe on Euclid is a big deal
  - hi-z SNe can *only* be done from space
  - SNe are a proven DE probe; BAO can probe DE, but ultimate limitations are unknown; WL no track record and greatest risk. "FOM scenarios"
    - If all three probes work well: 1200 - Euclid vs. 1000 for WFIRST
    - If WL isn't effective: 800 for WFIRST vs. 400 for Euclid
    - If WL, BAO both ineffective: 400 for WFIRST vs. 0 for Euclid
- The WFIRST design is better than Euclid (it ought to be, we have been at this since 1998!); Euclid suffers from
  - Under sampled IR pixels
  - Undersized aperture
  - Inferior PSF (obstructed FOV)
  - Not to mention possible future de-scopes to get into budget box

Lessons from (W)MAP/Planck

- In the wake of COBE, two CMB satellite missions were selected in 1996
  - (W)MAP: lo-f detectors and l = 2 to 900 (limited polarization)
  - Planck: hi-f and lo-f detectors, l = 2 to 2500, polarization
- Despite significant differences in capability, selection of MAP almost derailed the selection of Planck (science panel couldn't recognize the technical differences – Efstathiou still speaks of a key e-mail/fax from Spergel saved the day at the ESA shoot out
- NASA & Planck
  - Planned investment: $10M; actual ~$200M(?)
  - CIT/JPL spider bolometer technology was critical to mission success
  - For all that, US visibility is small (European mission) and only a tiny fraction of the US CMB community (which dominates the world) benefited
- Science strategy
  - Because WMAP and Planck were so different in capability and design, WMAP followed by Planck was an excellent global strategy and Planck results following WMAP by 10+ years are worth waiting
  - But Euclid and WFIRST are very similar in design and capability (for dark energy) and even 3 years might not be worth waiting for



Nightmare scenario

- Small investment ($40M) + modest escalation (to $100M) + constrained budgetary environment (esp at NASA) + inability to justify participation in Euclid and WFIRST (at 100,000 ft, Euclid checks the box!) = NO or very postponed WFIRST & disaster for US Astronomy
- How much risk are we willing to take for the sake of a few + good world citizenship? 10% 20% 50%

What is wrong with this picture?

US scientists predict[1] dark energy, discover[2] cosmic acceleration and win[2] a Nobel Prize, conceive[3] a space dark energy mission, have the required technology and dominate dark energy research -- and ESA leads the only space dark energy mission with small but essential US participation. [All this is the wake of the LHC, E-ELT, pull out of IXO, LISA…]

[1]Krauss/Turner, GRG 27, 1137 (1995) and Ostriker/Steinhardt, Nature 377, 600 (1995); Turner/White, PRD 56, R4439 (1997)

[2]Perlmutter/Riess/Schmidt, 1998

[3]SNAP, NRC Quarks to Cosmos

Big strategic issues

- US Leadership in Space Astrophysics
- Risk to WFIRST from Euclid investment
- Limiting financial exposure and ensuring Euclid mission success (if we participate)
- Using LSST data access strategically (Kahn)

My take (but you will study this in more detail and more carefully)

- Euclid/WFIRST should have been a joint ESA/NASA mission (world astronomy community can't afford two missions where one would suffice)
- "Go big" in Euclid or not at all – the downside risk for significant harm to WFIRST (and US astronomy) is unacceptably large
- Given all the needs of Euclid (technical, ground resources, people, money), there might be an opportunity for a significant negotiation. Possible positive outcomes:
  - Larger U.S. participation (leadership level)
  - Agreement on joint science mission with 2 complementary satellites
  - At the very least, much better deal than that on the table

NB: We have a stronger negotiating position than some think -- ESA needs our money, our technology, our scientists & our ground resources (esp. LSST) -- and we should use that leverage.

Final thoughts

You have an important but very hard job. In no small measure, the future of US astronomy this decade (and of NWNH) is in your hands.

You don't have easy options, and for better or worse, you will get the credit or blame for the outcome

Good luck!