Are Flagships the Best Way to Advance Astrophysics?

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One of the questions that the SIG on the "Future of UV-Visible Space-based Astronomy" is evaluating is whether "the pursuit of a flagship for the UV-visible is in the best interests of the field." I believe the answer to that question does not carry any informational content. To answer in the negative would mean that we know everything about the Universe that can be known by looking at UV-Vis wavelengths. This will never be true. So, the only possible answer can be "Yes".

And this is obviously the answer that will be provided for the other flagships (the Habitable-Exoplanet Imager, the X-ray Surveyor, the Far IR surveyor). Every group will likely submit a white paper similar to the one being discussed by AURA ("From Cosmic Birth to Living Earths: The future of UVOIR Space Astronomy"), which advocates for a large aperture successor to JWST. NASA will have to decide between the alternatives based on non-scientific criteria: Which satisfies the larger number of astronomers? What is the most interesting field, according to Congress? Which advances technology the most?

I believe that a better question would be: "Is another flagship, after JWST, in the best interests of astrophysics?" Here the answer is not so obvious, and I would like to propose it is "No."

The Problem with Flagships

According to the decadal review, a flagship is a mission that is not cost-constrained. Astronomers usually understand flagships to be general-purpose observatories, although maybe this is because they are so expensive that they need to satisfy the widest constituency possible.

Flagships are intended to be "High Performance" missions (**Figure 1**), and share this place in the cost-performance relationship with certain military missions. They advance the state of the art, but at a great cost. They take a long time to develop, because they are complex and expensive. The result is the so-called "cost spiral", where the desire for low risk leads to long development times, which lead to very high cost, which increases the desire for low risks (Wertz et al. 2011).

However, in the real world, costs are always constrained, and funding is close to a zero-sum game. By the time it is launched, the $8.5B JWST will have consumed between 20% and 50% of the astrophysics budget, *every year*, for *15 years* (**Figure 1**, right). While it is unfair to completely blame JWST for the restrictive budget

[1] The views expressed here are not necessarily those of The Aerospace Corporation. This white paper does not contain any proprietary information.

climate we find ourselves in, we clearly have had fewer space missions of other sizes because of the existence of JWST (Astro2010).

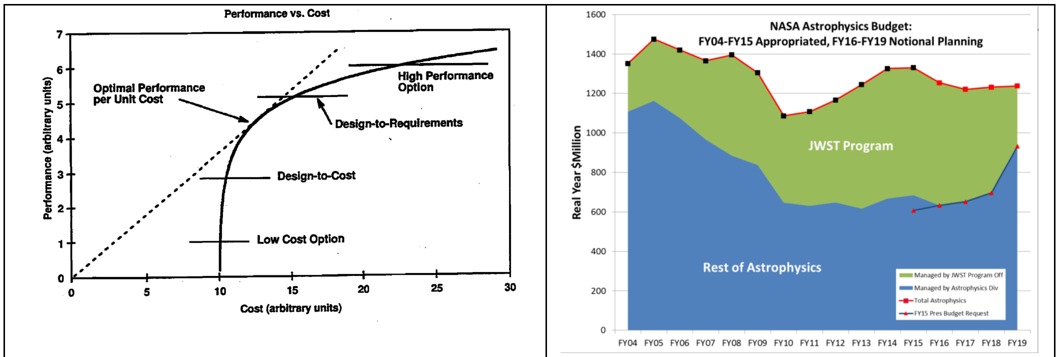

Figure 1. Left: Notional cost-performance curve showing different conceptual models for missions (Wertz 1996, pg. 4). Right: NASA Astrophysics budget (Hertz 2015).

And the price of a flagship buys a lot of things. As quoted in the Astro2010 decadal review, "according to NASA the *combined development cost* (not including operations) for WMAP, Swift, and WISE was $590 million (real year), *about 50 percent the cost of a single past NASA Great Observatory*." (Astro 2010, pg. 17, my emphasis. See **figure 2**.)

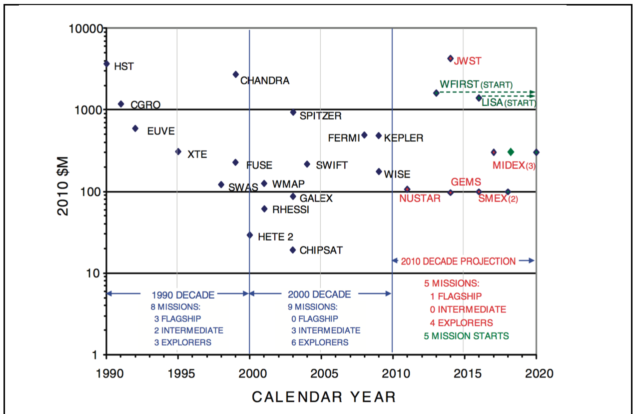

Figure 2. From Astro2010: "NASA SMD Mission cost over time, including future projections, 1990 to 2020. Flagship missions are those that are not cost constrained at selection. Intermediate and explorer-class missions are so designated by their cost."

Some of my colleagues argue that there are science questions that: (A) Can only be answered by certain observations, and (B) those observations require a multibillion dollar, multipurpose observatory to be performed. They arrive to (A) by posing a science question, deriving an observable that would address that question, and then calculating the instrument/aperture/wavelength necessary to obtain the observable. However, this procedure does not imply that measuring a particular observable is the only way to address a science question. As a matter of fact, astronomy is full of examples of repeated "discoveries," done at different wavelengths and with different telescope sizes (Harwit 1984). Experience shows that it is incorrect to say that unless a particular observation is done, we will not be able to advance a certain astronomy subfield.

Even if we were to accept (A), we would have to demonstrate (B). One would have to show that a set of less capable, smaller, more focused, cheaper missions cannot be used to obtain those observables. That we cannot trade, for example, aperture by instrument throughput to obtain the same total sensitivity, by waiting a few years to perform the launch. These kind of trades are not usually done, or publicized, so it is uncertain how feasible they really are. However, experience suggests that they are possible: the Spitzer Space Telescope went through substantial redesigns during development, trading aperture with lifetime and mass. The result was a mission that was only marginally less capable than originally intended (Rieke 2006).

The long development times mean that by the time the flagship is launched, it is carrying $\gtrsim$10 year-old technology (~15 in the case of JWST). By the time the mission is over, it may carry ~20 year-old technology. It is likely that a large fraction of the people involved in the original proposal, the living memory of the project, would have moved on. It is also likely that the original science questions would have lost their relevance, unless they were so vague (i.e. "How are stars formed?") that they were not relevant to begin with.

The long development times and big expense have an impact beyond the community waiting for the facility. Astrophysics and cosmology seem to progress better when observations are obtained across the entire landscape of signals: not only the entire electromagnetic spectrum, but neutrinos, gravitational waves, and cosmic rays as well (Harwit 2013). The starving of certain areas in favor of a single one will result in their contraction and weakening. While NASA should not keep supporting antiquated areas, neither should we risk losing a healthy field because it cannot be kept sufficiently funded, due to cost concentrated in other areas, particularly when some of that cost is due to unanticipated overruns.

Another argument for reducing development time is the programmatic risk of cancelation. To describe this, Hurley et al. (2010) coined the concept of “designing a system to a reliability of zero,” referring to missions in which the quest for zero risk results in longer and more expensive development periods, eventually resulting in the cancelation of the mission. For space astronomy, the analogous concept is “system design to a capability of zero:” A mission so powerful, taking so many years to develop that is eventually canceled. This has happened at least once with JWST, which was canceled on the House’s version of the FY2012 budget. Funding for JWST was eventually restored, but we all know of other missions that never came back.

A Proposal

I suggest the elimination of the concept of flagship. This should be replaced by a concept in which cost is capped to a percentage of the astrophysics budget over certain period. I suggest ~20% of the astrophysics budget over ~5 years.

A model like this would result in series of ~$1B missions (current dollars), that would respond to interests of the current community (not the community from 15 years ago), would carry the latest technology, and would be affordable in the long-term. Single-purpose missions in this price range are sometimes called 'probes', although the missions I suggest may be multi-purpose observatories.

In this way, I believe we can obtain the same value that we get from a flagship, but at a much lower price. Clearly these missions would be simpler and have fewer capabilities than flagships. However, their fewer capabilities should be weighted against the fact that we would be able to launch many of them, often, each making use of new, original concepts. Their lower price means that other activities could be funded more robustly: the mission itself can be funded for a longer period of time, with a healthy data analysis program. Complementary ground-based facilities can be built or upgraded to support the mission. A program of risk reduction suborbitals can be developed. And all that would STILL be cheaper than a flagship!

Launching a mission such as this with Class A risk characteristics[2] is challenging but not impossible. Reducing the risk to appropriate levels in 5 years would require some creative engineering and management, as it would likely involve risk reduction precursor missions, more reliance on Commercial-Off-The-Shelf (COTS) components, and original testing procedures.

Achieving ambitious science goals requires that we get a lot out of the 20% budget wedge. This can be done by implementing robust cost reduction methods throughout the mission. The AURA report mentioned before suggests investing on instrument and science simulators, as well as new detector technology. In their discussion of cost reduction methods across multiple missions, Wertz et al. (2011) find that while there is not a unique way of reducing costs, some ideas are crucial: the recognition that lower costs do not mean lower reliability, the adoption of cost reduction as an explicit goal, the idea that requirements should be traded to control cost, the provision of stable funding, the minimization of the impact of failures, etc.

Conclusion

In my opinion, flagship missions like JWST are very worthwhile, but not at any price. The prospect of living another ~15 years with a project of this relative price should be something that gives every astronomer pause.

I believe that a healthy space astrophysics program is crucial to advance our understanding of the Universe. I also believe that imposing some level of fiscal restraint does not need to limit our ambitions or our dreams, and will result in a better long-term outcome for our community. To thrive, we do not need to break the bank, by paying for another flagship.

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[2] NASA missions are classified as A, B, C, or D, depending on their risk. Class A is high priority, high national significance, high complexity, high cost, long lifetime, critical launch constraints, and no alternative flight opportunities (NASA procedural Requirements 8705.4).

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