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AIAA
SHAPING THE FUTURE OF AEROSPACE

CHILLING EFFECT?

Development of the James Webb Space Telescope has dominated NASA's astrophysics budget for a full decade longer than planned. In fiscal 2013, for instance, the agency spent more on Webb than the rest of its astrophysics programs combined, and it nearly did the same in 2014. The total development cost has soared to $8.8 billion, shattering a 2008 estimate of $5 billion.

We wondered what impact the Webb saga has had on the zest for innovation in astronomy.

The question is a timely one, not only because of Webb's launch possibly in November on an Ariane 5, but because the latest U.S. Decadal Survey of astronomy priorities is due for release shortly following a peer review that began in June. These reports by the National Academies of Sciences, Engineering and Medicine have historically guided NASA's astronomy spending, including the decision to build Webb. The 2020 Decadal Survey committee (whose work was bumped to this year by the pandemic) deliberated over four Webb-class telescope concepts submitted to it by NASA.

To find out if Webb should have a chilling effect on the technical scope of these future ambitions, we posed the same question to four of those who know the Webb program and its impacts the best:

- Former NASA Administrator Dan Goldin, whose administration conceived of the concept that would become the Webb telescope.
- Astronomer Alan Dressler, who advocated a smaller telescope.
- NASA astrophysicist John Mather, who has been with the program since the beginning and remains its top scientist.
- Astrophysicist Martin Elvis, an outspoken critic of NASA's current strategy.

On the following pages are their answers. — *Cat Hofacker and Ben Iannotta*

2017: Webb in a clean room at NASA's Goddard Space Flight Center in Maryland.

"Should the Webb telescope experience whet the appetite for technological advancements among those charting the future of space-based astronomy, or should the experience be viewed as a cautionary tale?"

The first boss looks back: Regrets? "Get outta here."

Dan Goldin

NASA Administrator, Washington, D.C., April 1, 1992-Nov. 21, 2001

During discussions in the mid-1990s over what should come after the Hubble Space Telescope, Goldin became personally engaged and pushed astronomers to think big in both a literal and figurative sense.

Breaking down the technical barriers and opening up the space frontier is really hard. People love to sit in the galleries and watch space developments unfold as though they were watching a soccer game, and in real time criticize those who are in the arena, pouring their guts out trying to make it happen. So, to answer your question, I got up about a half hour ago, and I just typed out some words. Here goes:

Evolution and destiny of the universe, life therein, and the laws of nature are essential to understanding who we are, providing knowledge to improve the quality of our lives here on Earth and ultimately giving us access to the stars. Building Webb was not an easy task. It was really hard. The audacity of attempting to see the first stars that ignited after the Big Bang and to see primordial solar systems deep in the heavens is outrageous. It took courage, hard work, dealing with failures along the way, and the self-confidence of those who followed me at NASA. I salute the courageous NASA and industrial team that persevered while addressing head-hurting issues. The process was messy. Could it have been done more efficiently? Absolutely. However, we are here on the threshold of launch, and I wish them Godspeed on their most important mission: to lift the collective eyes of humanity.

Astrophysics is one of the loves of my life. At TRW **[later purchased by Northrop Grumman]** before I came to NASA, I oversaw the Compton Gamma Ray Observatory and the Chandrasekhar **[later shortened to Chandra]** X-ray space telescope. In fact, I oversaw the grinding of the developmental lens for Chandrasekhar. So, about six months after Hubble got its contact lenses, maybe in '94, a group of cosmologists visited me. They said, "Dan we gotta replace the Hubble." I said, "For God's sake it's now working for the first time." And they said, "No, we got to start thinking about a replacement now." Their original idea was a 4-meter visible and ultraviolet telescope. I asked them what scientific question they'd like to answer. They said they'd like to see the first stars that ignited after the Big Bang but that you couldn't do it because that would require a 6- to 7-meter infrared telescope that has to be cooled, and it wouldn't fit in the biggest rocket shroud. I said we're not going to build another telescope unless we're answering a fundamental scientific question that's going to have an impact on the lives of the people on this planet. Astronomers battled me for a year when I challenged them to consider a 6-to-7-meter infrared telescope. I called them Hubble huggers, but it was kind of said in friendship. Now, I did not interfere with the process. NASA took the concept to the National Academy of Sciences, to the Space Studies Board.

That is how the Webb space telescope began. There were some early cost studies, but during my tenure there wasn't a mature design yet to start doing really strong cost studies. Do I regret that Webb turned out to be hard? Hell no. Get outta here! [he laughs] There had to be work done after me. But I believe we can't walk away from hard things. If we walk away, we are not worthy of the resources the American public gives us to explore the unknown. ★

2012: Webb's first two mirror segments arrive at NASA's Goddard Space Flight Center in Maryland.

From the astronomer whose report was overruled: Reasons for caution...

Alan Dressler

Staff astronomer, now emeritus, Carnegie Observatories in California, 1981-present

Dressler chaired a committee that in 1995 recommended a telescope with a 4-meter-diameter primary mirror as a successor to Hubble and its 2.4-meter mirror. Then-NASA Administrator Dan Goldin viewed this recommendation as timid and dismissed proponents as "Hubble huggers."

When the Hubble Space Telescope and Beyond committee began our work in 1994, the fixing of Hubble was still in doubt. But once it was clear the Hubble was restored and it became a huge success, our committee was anxious to build something even more ambitious. NASA suggested a target budget of $500 million, so we recommended a 4-meter telescope with a more conventional design — not a segmented mirror and unfolding sunshield and all the things that the Webb will be.

A few months after the report came out, NASA Administrator Dan Goldin addressed the 1996 American Astronomical Society meeting in San Antonio. I was in the front row, and I just remember him leaning over the lectern, looking straight at me. He called our recommendation too cautious, too timid. He wanted an 8-meter telescope, which increased both the development time and cost. **[NASA eventually settled on a 6.5-meter diameter primary mirror.]** Webb became the perfect storm: The more expensive it got, the more critical it was that it not fail, and that made it even more expensive.

2017: Webb's optical portion in front of the door to Chamber A, a thermal vacuum chamber at NASA's Johnson Space Center in Houston.

A similar decision point over complexity will come once the 2020 Decadal Survey is released, with the 8-meter to 15-meter LUVOIR design and the 4-meter HabEx that would both look for planets around other stars. I'm torn, just like I was when Dan Goldin looked down at me at the AAS meeting and urged us to go bigger with Webb. Your first reaction is, "Ah, that's fantastic!" and the second is just, "I'm terrified at this thought."

I think we better take Webb's cautionary tale very seriously. So if I were in NASA and the Decadal Committee said, "If there's enough money we want to do LUVOIR, but if not we want to do HabEx," we ought to have a technical study that goes much further into how much it will cost to build each telescope. We need to do the engineering, whatever it costs, so we can say with certainty what we're buying. ★

Lessons, yes. Chilling effect, no.

John Mather
Astrophysicist, NASA's Goddard Space Flight Center in Maryland, 1974-present
Mather led the group that defined Webb's science objectives and chose its instruments. He began studying possible objectives in 1995, seven years before NASA named the future telescope after James Webb, NASA's second confirmed administrator. Mather is now senior project scientist, the top scientist on the program.

Your provocative question is about advancements versus a cautionary tale, and I'd say Webb has been both. We can't make progress in astronomy, most of the time, without inventing something, and that's always harder than people think it will be. Every single time. Success is a matter of people, as well as ideas. We find extraordinary talent out there in the aerospace industry, but mistakes set you back. On Webb, we had to invent a refrigerator, so NASA ran a competition, and in demonstrations the refrigerator worked fine, but when it came time to build the one that would fly, it didn't work so well. We got Northrop Grumman to give us a new manager. And within weeks of his coming in, progress increased very rapidly.

So, astronomers know there is gold out there, but you need the right people and tools to find it. As for that next tool, within a month or two or three you should have a big story about what's in the Decadal Survey report. There were four Webb-telescope-class observatories that were to be evaluated by this giant committee of the National Academy of Sciences, and they all are extremely ambitious. At least three of the four build directly on the technology that the Webb developed, with better detectors, with things that unfold in space, with focusing the telescope after launch or with something very cold. One of those telescopes would actually run at about 4 or 5 degrees Kelvin, so that's a whole lot colder even than the Webb telescope.

On Webb, we learned some things we will want to repeat. The mirrors were obviously a big challenge, so we had an external committee, a team, that came in to tell us whether we were doing the right thing. They kept us out of trouble. A more general lesson was: If you haven't got a complete plan, you shouldn't be promising the price. Everyone was surprised at how difficult it was to finish defining the test program. We actually had to change that after we had chosen Northrop Grumman. Nobody can really tell you how hard things are going to be when you start into the forest.

We made that start under the very ambitious NASA administrator, Dan Goldin, who was very creative and very pushy toward rapid progress. In '96, he went to the American Astronomical Society, and he said, "Why does Alan Dressler's committee ask for such a small telescope? We're going to build you a bigger one." He got a standing ovation, and we said, "Well, OK, we better do this." That's sort of our first peer review. He urged us to do faster, better and cheaper. He said we know that the Spitzer Space Telescope is going to cost whatever the number was at the time, so we want you to build this bigger, better one for less. People didn't really believe that it was possible, but we said, "OK, boss, we'll try." No one should be surprised that if you start out with that kind of instruction you're not going to get the answer you wanted. Wishful thinking is not the same as truth. Never has been, and never will be. But the boss could see right away that Webb's segmented mirror technology was an investment in the future, because this was the only way we could break the boundary of telescopes bigger than the rocket. ★

2019: Webb's five-layer sunshield fully deployed at a Northrop Grumman facility in California.

“Should the Webb telescope experience whet the appetite for technological advancements among those charting the future of space-based astronomy, or should the experience be viewed as a cautionary tale?”

Abandon the “flagship” obsession.

Martin Elvis

Astrophysicist, now senior, Harvard-Smithsonian Center for Astrophysics in Massachusetts, 1980-present

Elvis has been an outspoken critic of NASA’s approach of focusing on one flagship astronomy mission at a time, most recently in his book “Asteroids: How Love, Fear, and Greed Will Determine Our Future in Space,” published in June.

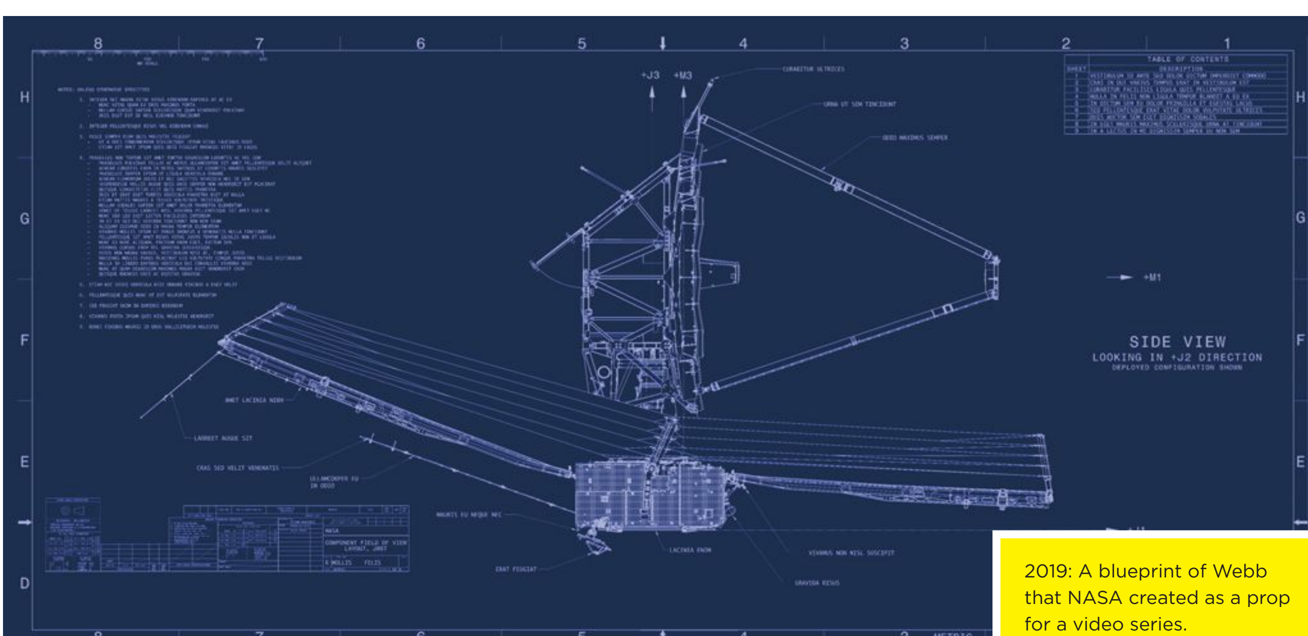
We will find things that will amaze us with Webb, and I hope those discoveries inspire us to pursue technological advances in different areas of astronomy to complement those findings. But the cautionary tale is: If we follow the Webb procedure of choosing one flagship mission that gets all the attention, gets the budget, then, when it runs into a problem, what can NASA do except throw more money at it?

If Webb fails, if it doesn’t deploy perfectly, then we’ll have spent $10 billion for a turkey, and that’s going to hamper NASA astrophysics for sure, and maybe a broader part of NASA’s science program, because who’s going to risk giving NASA $10 billion again for a single thing?

A smarter strategy would be to have multiple missions in development simultaneously, each with a fixed budget. You say, “OK, you get $3 billion or $5 billion even, but if you go over that you’re dead.” But NASA still has a flagship program. Having multiple missions and cost caps imposes what they call in the U.K. “tensioning,” or discipline, that keeps costs realistic, keeps people focused.

The 2030 Decadal Survey should also consider emerging technologies that weren’t mature enough in time for the 2020 report. We’re now getting to the point where we could service telescopes to low-Earth orbit quite cheaply, I would think, using the SpaceX Dragon capsule, for instance. When you serviced Hubble, it cost you a billion dollars to launch the shuttle plus the instruments you were taking up. That cost comes down by more than an order of magnitude if you could put something like a new instrument or replacement computers in the Dragon trunk and take it up, where either astronauts or robots could make the repair. Another avenue is the bigger fairings on today’s in-development launch vehicles, which eliminates the need to fold up telescopes and then deploy them as Webb was designed to do. And their much bigger mass to orbit will spark a change in philosophy of the engineering for space that should be well-established by the time we have a new decadal.

We need to start thinking about these problems now before all the great observatories are gone. Hubble recently got a software fix; we’ve lost the Spitzer telescope and the Chandra X-ray telescope is clearly not as powerful as it used to be. You can’t rely on any of them being there in five years’ time, certainly not in 10. We need a new approach for the next generation of great observatories. ★



2019: A blueprint of Webb that NASA created as a prop for a video series.