

The Challenge of NGST

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NGST is a great endeavor. It is distinguished from many other endeavors of similar size and cost in that it seeks to alter our view of the universe, to push back the observable frontier to a time before there were stars and galaxies, when the only light was the faint echo of the recombination era we call the cosmic background radiation. If successful, we will be able to show our benefactors that we have "seen the edge", and I am confident that the justification for our efforts will be self-evident to those who have little to do with science in their daily lives. Although it may be difficult to imagine now, we will help give meaning to the ordinary struggle of existence by enlarging people's sphere of awareness to encompass all that can be seen by ordinary extensions of the human eye.

The scientific promise rests strongly on the central theme of the two books in support of NGST by Alan Dressler and Peter Stockman: observing the first stars and galaxies. That goal is the principal driver for the telescope design and from that one driver most of the key features may be derived: the 8m size, the infrared wavelength coverage, the low temperature, hence the orbit at L2, and two basic instruments, a camera with a large field of view limited only by the zodiacal background, and a simple spectrometer to demonstrate that the faint objects picked up by the camera are, in fact, at the edge of the observable universe and not some nearby interlopers masquerading as young galaxies. That is all that we really need to address the central theme.

But a large space telescope is a powerful device. It is so powerful, that most current problems in astronomy can be studied with its instruments. And with a few more instruments, we can tackle all of our favorite areas, regardless of whether they are of wide appeal or niche capabilities, regardless of whether they are actually designed to answer questions or simply provide more data in the hope that we can figure out the patterns of nature and eventually arrive at a theory.

So we see the natural tendency of scientists to expand the potential. NGST will not only be great for distant galaxies, it could be great for studying star formation. One of my favorites is the possibility of direct observations of extrasolar planets with a coronagraph - and I think this problem is compelling enough to merit a strong emphasis in NGST. It is another example of a grand endeavor that would immediately captivate the minds of ordinary taxpayers as well as providing tremendous scientific benefit. And there are a host of other areas within reach, from the interstellar medium to peculiar stars to low redshift galaxies to the Milky Way and the study of the outer planets and Kuiper Belt objects. We can study dust in the intergalactic, interstellar, and interplanetary regions. We can take spectra of very distant stars just to make sure they look like nearby ones. Who knows, we might discover something that way?

Each of these desires has a slightly different set of requirements for the "optimal observing facility". The contractors can see the normal phenomenon of requirements creep. Scientists are like that. We slowly increase the capabilities of the telescope until it will do everything for everybody. We extend the scope of our cameras with differing fields of view, we add spectrometers that can observe many objects simultaneously and then insist that we need to observe many parts of any one object simultaneously, too. We increase spectral resolution. We extend the wavelength range.

We do it all with the hope - and sometimes the promise - that our methods to build instruments will advance, giving us more bang for the buck and allowing a great capability at no added cost or risk to the project. We do it with our eyes wide open, knowing we need the political constituency to support an expensive big project, but also knowing the enormous cost and risk associated with trying to be all things to all people.

Astronomers, like many people, are greedy. And as Ivan Bosky told us, greed is good, because it forces competition and ultimately advances in productivity.

Engineers like this approach, too. Our new telescope will need new technology to make it work. NGST will be neater than all previous space telescopes. It will have an articulated mirror and telescope assembly that is stuffed into a small rocket and blooms like a flower in space. I do not believe that if we were given the money and support to launch a monolithic telescope on a huge rocket that we would choose to do so. It is a little too brute force. It is not really elegant, not technically sweet.

Engineering, like science, is an intensely intellectual activity. When done right, it is as creative a process as anything in the arts or the sciences, and the very best engineers need that challenge to their creative powers to keep them interested. Technology, like science, can also be compelling and has all the power to change our view of the world as scientific discovery. Steven Jobs described the attitude of the team that built the first user-friendly Macintosh computer as "wanting to make a dent on the universe". We, too, can make a dent in the universe with a project that extends the capabilities of the human eye by more than 10 orders of magnitude, creating an extension of our senses that was unimaginable only a few decades ago.

I hope the symbiotic nature of this enterprise is not lost on the rugged individuals among the audience. To achieve the scientific goals, we need the technological advances, too. And the technology will be a waste if it does not produce some mind-boggling discoveries. None of these grand visions will be accomplished without large, diverse teams drawing on scientific, technical, political, management, and even entertainment talents to make it reality. We are all in this together.

But why do we do NGST now? What makes this time special?

The obvious answer is because we have already built telescopes that get us close to the edge of the universe, and these telescopes have found favor not just among scientists but also among the public. The premier example is the Hubble Space Telescope, the

predecessor to the NGST in many ways. Hubble has been operating for almost 10 years now, and it continues to produce a wealth of data, discoveries, and interesting insights into the universe that are readily popularized and brought to the attention of millions of people. The construction of the large ground-based telescopes has been equally important to the search for the edge of the universe. The two Kecks, the VLT, and soon the twin Gemini and LBT telescopes have suddenly appeared on the landscape like giant mushrooms created by a thundershower of money and technical advances. Our technical prowess continues to increase almost exponentially, meaning that we have now made possible what was earlier just a dream. NGST is the logical extension of our current progress.

A less obvious answer is because in the modern economy, the cost of an NGST is not that large. The former senator Daniel Patrick Moynahan has been quoted as saying, "A billion here, a billion there, and pretty soon you are talking about real money." Private wealth in this country alone has made many individuals capable of funding an entire NGST without even taking a hit to their net wealth; they could probably use the tax deduction.

Two years ago I gave a public lecture at the Wheeler Opera house in Aspen, Colorado as part of a workshop at the local Aspen Center for Physics. Prior to the lecture, I was invited to dinner at the home of one of the wealthy benefactors of the Center for Physics, a couple from Texas who had evidently made their fortune in the oil boom. I was seated next to the hostess at an elegant and large dinner table in a sumptuous house located in the heart of Aspen. Over dessert, she asked me to describe the most exciting new project astronomers wanted to do. I described the NGST and while waxing about the great things it could do, mentioned that the major impediment was probably the high cost. She asked me what the project would cost, and I replied that the official cost to build it was \$500 M, but most of us knew it would cost more than a billion dollars, when all the various pieces were put together. I will never forget her next statement or the expression on her face. Her jaw dropped very slightly and she looked at me with a slightly quizzical expression, then she said in a deep Texas drawl, "Well, Professor Beckwith I don't mean to hurt your feelings, but that just isn't a lot of money." If her intent was to impress me, it worked.

The scale of our efforts is matched to the scale of what is possible. Make no mistake, this is big science on a big scale. NGST is not cheap. It is not given that the public should fund this telescope. But it is also not unreasonable for us to think of a 1 billion dollar project, if the vision is sufficiently compelling that the average taxpayer could see value in the results we produce. I believe we can produce results that will be worth 1 billion dollars to humanity, and that humanity will see the value of future scientific projects because of the discoveries we make.

Now, even though the cost of the project is not enormous by modern standards, it is large enough to merit the attention of the NASA administrator, who has imposed on us a strict cost-cap of half a billion dollars for phase C/D. The big question is: Can we do it? Can we actually build this thing with the blossoming primary mirror, the articulated telescope assembly, and all the great instruments for a mere half a billion dollars?

We are currently in the highly optimistic stage of the project and possibly in denial about the task ahead of us. It is easy to be upbeat about a viewgraph telescope, where adding a bit more capability incurs only incremental cost, since it does not produce any additional problems, at least not on paper. We have yet to see the problems, to confront the failures, the mistakes, and the inevitable oversights that can be corrected only after they pop up at a far greater cost than would have occurred had they been designed out from the start.

Cost overruns are common in large projects, especially those for which the funding is tight to begin with, and the projects' proponents are overly ambitious in their goals. There is a common dance between the project managers and the contractors, whereby the managers plan for and retain contingency funding, and the contractors inevitably manage to spend the contingency. But having a contingency does not guarantee that we will get what we want, and Mr. Goldin has been crystal clear about his intent to hold us to a \$500 M cost cap.

Recall the problems of the Hubble Space Telescope, the predecessor to NGST. It had an enormous number of problems before it achieved what its supporters had promised. The 1973 design, a pre-Phase A concept, called for a 3m telescope with 7 scientific instruments. By 1977, it was 2.4m with 5 instruments. The very early estimates from Grumman and Marshall Space Flight Center indicated a lifetime cost of \$700M (1972) equivalent to \$2 billion (1987). We currently estimate a program cost of order \$4 billion (1998). The buy-in cost estimate in the mid-70s was \$300 M imposed by James Fletcher, the NASA Administrator at the time. Fletcher, like Goldin, insisted that more than \$300 M would be impossible to sell to Congress. By today's standards, the \$300 M is much more than our cost-capped Phase C/D.

Many problems arose in this program that were not obviously high-tech items. A prominent example was the construction of the latches to hold the scientific instruments in the telescope bay. These low-tech items were largely ignored until late in the program when it was discovered that they were frighteningly difficult to build. They had to hold the instruments to incredible tolerance and acted as the thermal links with the telescope. The eventual solution, to coat the latches with silicon carbide, was simple and inexpensive but was discovered only after much money had been spent figuring out what was wrong.

Hubble now has the strong support of the agency, of Congress, of the public, and of the scientists, and it looks like the enormous cost has produced a correspondingly enormous benefit. It is tempting to think that you can buy more by investing in many small projects than in a mixture of small, medium, and very large ones, but the Hubble experience demonstrates the opposite. Sometimes, the largest projects produce more per dollar spent than the small ones. NASA currently touts Hubble as being its most cost-effective mission when normalized to the number of discoveries.

So, can we do it? Can we really build NGST with all its new technology for less than the amount that would get Senator Moynahan's (Dirksen's) attention?

I think we have reason to be optimistic about the progress on the telescope itself. Bernie (Seery) and his team have made enormous progress in identifying and funding the different companies exploring lightweight optics technologies. The project plan calls for considerable investment to develop these technologies before the start of Phase C/D. In fact, the rules state that Phase C/D does not start until the technologies have been demonstrated to work, and up to 40% of the budget will be spent in this development phase unlike the 1% that was spent on Hubble. The project team at Goddard appears to understand well the challenge of building this telescope, and their plan to confront the challenge is realistic, although by no means risk free. It is comforting that the cost of ground-based telescopes per square meter has come down sharply since the last generation of 4m-class observatories was built.

Unlike Hubble, there is a single NASA center in charge of NGST. Intercenter rivalries almost crippled the Hubble program in the early days, when Marshall and Goddard duelled for control of key decisions. NGST is firmly in Goddard's domain, and there should be no question of priority from the center management, should NGST run into problems. The science community expects to see a demonstration of Goddard's commitment to the project, since it has many other irons in the fire, but there is no doubt about the responsibility.

And the scientists, represented by the Institute, are coming on board early in the game. We have good working relations with the project management, and we are large enough to get their attention. To give the scientists even more influence and responsibility for the outcome, we hope to enter into a cooperative agreement with NASA to operate NGST. A cooperative agreement is fundamentally different from a contract, in that it vests the responsibility and authority with the Institute for key decisions affecting the program. This kind of cooperation never happened with Hubble, and it is essential to the success of NGST, if we operate in a cost-capped environment.

It is not yet obvious that we can build instruments of the sort discussed at this conference to the cost-cap in the program. Typical instruments for Hubble cost \$100 M or more. The simplest instruments you can imagine, single channel cameras with a few mirrors, a filter complement, and a single detector array, would already push the cost envelope of NGST. To cite two examples, the Advanced Camera for Surveys was supposed to be a straightforward instrument using detectors that were commercially available at a cost of about \$40 M. The current cost is between \$75 and \$80 M, and we are not done yet. More relevant to NGST is the Wide Field Camera 3. Last year, we proposed to add an infrared channel to this camera, a channel that consists of half a dozen mirrors, only two with power, a filter wheel, and a 1024x1024 pixel detector that had been in use at ground-based observatories for over 3 years. This addition will cost \$40 M.

I was personally very skeptical of these costs, so Jim Crocker and two engineers from Ball Aerospace came to the Institute and explained how the costs of a space program add up. I was introduced for the first time to the need for \$50 screws, where each piece is inspected, tested, and documented in excruciating detail, including the secretarial and management overhead associated with this documentation, to ensure product reliability in

space. These screws cost less than 50 cents at your local hardware store, but the space-qualified screws will buy dinner at a good restaurant.

After a long lecture on the cost, it was clear that we could not expect to have cheaper instruments, unless we could get the cost of screws down to something more reasonable, say the cost of a meal at Burger King. I do not see that happening at the moment. Jim, if you know how to do it now, let's talk again about the Wide Field Camera 3.

Both of these instruments are being built by teams including Ball Aerospace, Goddard Space Flight Center, and astronomers, the same kind of group that will do NGST. If a simple infrared channel to an existing instrument costs \$40 M, how are we going to build cameras with 8k x 8k mosaics, let alone multi-object spectrometers? As far as I can tell, we have not demonstrated a technology or management method that will buy us the instrumentation proposed at this conference for anywhere near the instrumentation budget, in which typical instruments should be much less than \$100 M.

And so, I address the scientists, many of whom are vying for the right to build sophisticated instruments for NGST. How do you plan to achieve these ambitious plans by working to the same model that we always have: several PI instruments, different contractors, different interfaces, and project management of your own choosing? This model is well known, and the evidence to date is that it produces sophisticated instruments for much more money than will be available in the NGST budget.

SIRTF faced similar problems recently, when it had to come to terms with its own cost caps. Marcia Rieke can describe to you the draconian measures needed to meet SIRTF's cost constraint. Among other cooperative measures, two of the three instruments share the same electronics. All partners are working cooperatively such that no one profits unless everyone does, and the pain of cutbacks is shared. If we want NGST to be a grand mission, we will have to adopt a new approach to building instruments.

Since I am an optimist, I believe we can do it. I think we can make the compromises and tradeoffs necessary to fit the package within the budget and by working together manufacture a facility that costs far less than it would have under the Hubble model. The challenge of NGST is a challenge to us. It is we who will have to make this facility capable of revealing the edge of the universe for what to some people is not a lot of money.

We have seen the problem. The problem is us.

Our grand vision is a worthy vision, one that we can be proud to bring before Congress and ask to be funded. It is a vision that should sustain us as problems with the construction arise. But I am sure we can meet this challenge, and when we reach the destination, it will be that much sweeter because we took the journey.