Title: From a sounding rocket per year to an observatory per lifetime

Author: Martin C. Weisskopf [1]

Date: March 15, 2013

Abstract

I attempt to summarize the excitement of my role primarily in the early years of X-ray Astronomy. As a "second generation" X-ray astronomer, I was privileged to participate in the enormous advance of the field, both technically and astrophysically, that took place in the late 1960's and 1970's. The remainder of my career has concentrated on the design, construction, calibration, operation, and scientific maintenance of the "cathedral" that is the Chandra X-Ray Observatory. I contrast my early experiences with the current environment for the design and development of instrumentation, especially X-ray optics (which are absolutely essential for the development of the discipline). I express my concerns for the future of X-Ray astronomy and offer specific suggestions that I am hopeful will advance the discipline at a more effective and rapid pace.

The Columbia Years

I spent my early post-graduate years from 1969 until the fall of 1977 as part of Robert (Bob) Novick's group at the Columbia Astrophysics Laboratory (CAL), Columbia University in the city of New York. To say that these were exciting and interesting times would be an understatement. For example, a few days after I arrived, I found myself in the entrance hall of the Pupin building sitting with T.D. Lee and a number of other faculty members holding long discussions with (mostly) student demonstrators that were attempting to take over this physics building in protest over Columbia scientists' participation in a military think tank.

Even before I accepted the position at CAL, Bob took me to a meeting in Cambridge Massachusetts at the company American Science and Engineering. The discussion centered about something called a "Super Explorer" and the "Principal Investigator Group" (acronyms withheld by popular request). It was at this meeting that I first met a number of people who were to play significant roles in my career, especially Leon Van Speybroeck and Harvey Tananbaum. The most profound impression was made on me by Riccardo Giacconi, who was emphasizing the importance of X-ray imaging for advancing the field, an insight that he began within a year after the sounding rocket experiment that led to the discovery of the brightest non-solar source in the sky, Scorpius X-1, in 1962. Little wonder he was eventually awarded the Nobel in 2002.

It puzzles me even to this day that, despite the leaps and bounds made by ever improving angular resolution, there are many proponents of seeking added photons (large area) at the price of angular resolution. I don't mean to imply that such experiments haven't proven and won't prove

[1] Space Sciences Office, NASA/Marshall Space Flight Center, Huntsville AL 35812

fruitful, but frankly I feel that they pale in comparison to what has been, could be, and should be, accomplished.

Just after I arrived at the CAL a rocket that Bob Novick and those already there had put together blew up on launch at the White Sands Missile Range. The payload featured the first X-ray concentrator made of dozens and dozens of gold-coated microscope slides mounted to approximate a parabolic shape. I mention this only because the circumstances taught me (and the rest of us) an important lesson that influenced my approach to interacting with NASA and especially my approach to Chandra. The reason that the rocket blew up was that the liquid fuel Aerobee never ignited. The liquid fuel stage stood on a metallic "milk stool" above the solid rocket Nike missile booster that started the journey into space. As the Nike's acceleration built up it passed through the milk stool and entered the Aerobee's liquid fuel tanks. At least the event was spectacular. The firing of the liquid fuel was to have been triggered by means of a lanyard connecting the ignition system to the launch tower – but someone forgot to connect the lanyard. Of course there were investigations and blame and this was not the scientist's responsibility, but to us the message was clear --- we hadn't paid enough attention to everything involved in the entire system. When the rocket failed, ultimately we scientists pay the biggest price.

During these early times, there were a number of excellent groups forming at various institutions around the country (Columbia, MIT, AS&E, CIT, Wisconsin, NRL, LBL, GSFC, Lockheed Palo Alto, Stanford, etc.). All were funded at a more or less adequate level to develop and demonstrate instrumentation, including X-ray optics, capable of obtaining scientific results. In the early 70's these demonstrations were mostly through the mechanism of sounding rocket flights. At Columbia we made use of sounding rockets with a passion. With four young Assistant Professors (myself, Paul VandenBout, Roger Angel, and Richard Wolff) supplementing Bob's fertile imagination and abilities as a hands-on experimentalist, we developed X-ray spectrometers, X-ray concentrators, X-ray telescopes, and X-ray polarimeters. We were designing, building and flying new instruments at a cadence of approximately once per year. Like most of the other groups we were all, to a greater or lesser degree, participating in every experimental approach to doing the science of X-ray astronomy, not necessarily specializing in one arena or another. The competition and rivalry (for the most part friendly) was an essential ingredient in the development of the field. The best technical approaches were often the result of a merging of ideas, techniques, and approaches from different organizations.

During the 1970's at CAL we also began to participate in the satellite era, successfully proposing, building, testing and flying both an X-ray spectrometer (solar and stellar) and an X-ray polarimeter (stellar) on the OSO-8 satellite. Our pioneering efforts to establish the field of X-ray polarimetry were, sadly, the precursor to a frustrating future. Based on the early successes, Bob was able to lead the development of a polarimeter for the first Spectrum-X Russian satellite mission. Unfortunately, this mission was eventually cancelled. More recently, a Small Explorer Mission dedicated to low-energy X-ray polarimetry and led by the GSFC was cancelled by NASA for excessive cost and schedule overrun.

Simultaneously, we at CAL partnered with a number of institutions to develop instrumentation for the HEAO series of satellites. Our team at Columbia had a major role in three of the four HEAO satellites at the time including a one-arc minute, 1000-cm^2 Kirkpatrick-Baez telescope in collaboration primarily with AS&E/SAO (Bob Novick and Paul Gorenstein running the show). This experiment would have performed an all sky survey on the first HEAO mission. During that same time period the HEAO program hit a major obstacle and I (along with the rest of the community) experienced the first of a series of political decisions that seemed to place science second in lieu of an assumed expediency. Frankly, the cancellation and resurrection of the HEAO program came as quite a shock to this naïve young researcher. Clearly the HEAO Program was excellent and at the cutting edge of much of the science and technology --- why should it be cancelled? Obviously I had a lot to learn about NASA politics, but the experience was, in its own way, invaluable. Long story short, the HEAO program with its 4 satellites was cancelled. A new and reduced program was resurrected from its ashes, which, sadly, did not completely encompass the best science. Historians will tell us that that political necessity formed the decisions that were made. Perhaps, but one can never be sure. Can you imagine the progression of X-Ray astronomy had the arc-minute-resolution all-sky survey that was the first of the original HEAO series been performed in the 1970’s?

Figure 1. Picture taken in 1970 at Wallops Island of sounding rocket 17.09 featuring two types of X-ray polarimeters. This experiment unambiguously measured the polarization of the

integrated emission from the Crab Nebula. From left to right: Robert Novick, directory of the Columbia Astrophysics Laboratory, my graduate student Gabriel Epstein, me, my office mate Richard Wolff and his graduate student Richard Linke

The Marshall Years

In 1977 I received an offer from NASA to go to the Marshall Space Flight Center (MSFC) in Huntsville, Alabama to become the Project Scientist for what was then known as the Advanced X-Ray Astrophysics Facility (AXAF) and which was eventually renamed the Chandra X-Ray Observatory. I still hold this position. MSFC, in partnership with Riccardo Giacconi and scientists at SAO, had won the management of this potential mission in a competition with JPl/CIT and GSFC. The community wanted to avoid many of the difficulties encountered in accomplishing the HEAO-B (Einstein Observatory) Mission and a critical element was to have on-site Project Science (as opposed to long-distance) as was the case for the *Einstein* Observatory. Despite appearing immodest, I am firmly convinced that this decision was a major, if not the key, factor in the ultimate programmatic, technical, and scientific success of Chandra. A second critical element was that Project Science function was not to be implemented by a single person but by a team of scientists some of whom were to be part of the team at the MSFC and some of whom were provided by SAO --- which brought all of the *Einstein* experience to bear on this challenging project. Chandra was to be the mission that, amongst many other objectives, would address the questions raised by the diffuse glow of X-rays also seen during that first sounding rocket flight in 1962. To accomplish this task (resolving the "diffuse X-ray background"), the angular resolution would have to be arcsecond or better and the effective area be several hundreds of square centimeters. No one had ever built such an X-ray telescope but the scientific requirements were clear. Moreover, the community banded together to try to hold the line on requirements, placing the science, and not programmatic considerations, at the forefront. We were reasonably successful in this primarily for the telescope. (As noted below, this is not the time or the place to tell my version of the Chandra saga.) In 1977, the projected launch of Chandra was 1985. For a number of reasons, both technical and programmatic (mostly financial), the projected launch date was to move out at the rate of a year per year until 1992. The launch took place in 1999.

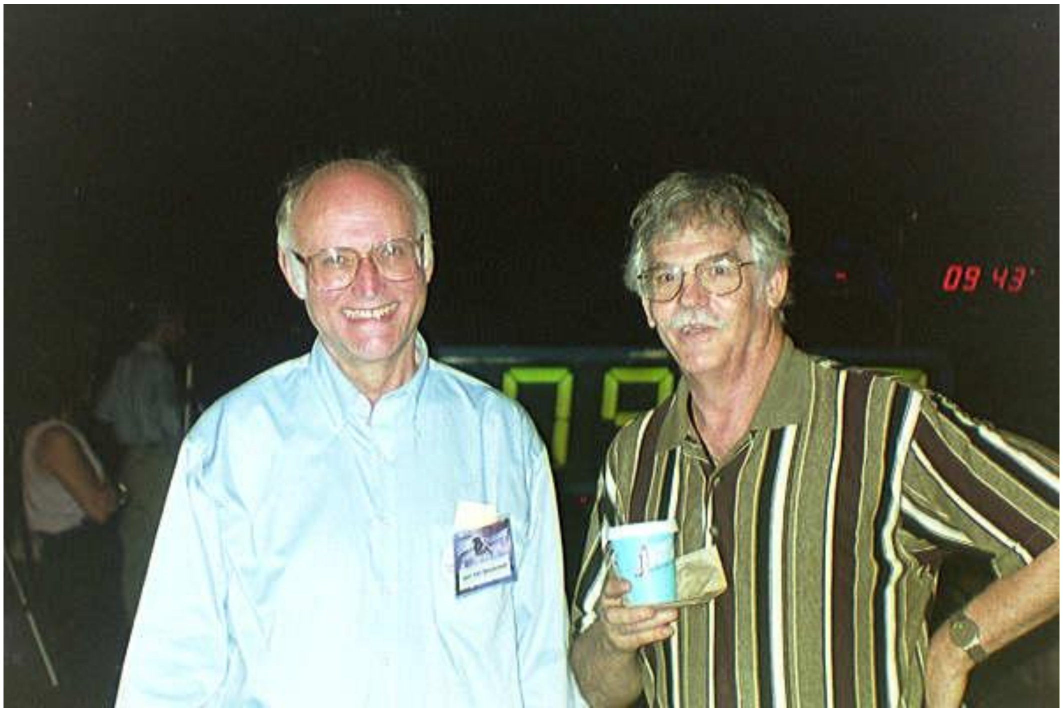

Figure 2. Leon Van Speybroeck, the Chandra Telescope Scientist, and myself during a launch hold 9 minutes and 43 seconds before the first attempt in July 1999. The third attempt on July 23 was successful.

The technical insights and experience that Chandra Project Science (not just me but also Ron Elsner, Allyn Tennant, Brian Ramsey, Steve O'Dell, Marshall Joy, Jeff Kolodziejczak, Doug Swartz, and many others who were part of the Project Science Team at MSFC at various times over the years), together with the scientists and engineers at SAO, provided, and the responsibility to the rest of the community that we all felt, served the project in numerous ways. Perhaps the most important was the control of requirements which had no major change over the 22 years between 1977 and the 1999 launch. Our ability to accomplish Chandra with such success was founded on the experiences and capabilities we developed in the "early days". We had all built instruments, sometimes making last minute repairs while the rocket was mounted in the launch tower; we had all participated in satellite missions, sometimes going through the agonies of cancellation, etc. In other words, by and large, we knew what we were doing!

This is not the place or the time for me to write my version of Chandra's history and accomplishments if for no other reason than that the Observatory, originally designed with a formal lifetime requirement of 3 years and a goal of 5, is still in its orbit, obtaining outstanding scientific results. Indeed, at this writing (March 5, 2013), a most famous supernova remnant, the Crab Nebula, is flaring in gamma-rays and Chandra will be pointed at this object this evening, participating in the important hunt for the specific location within the nebula where the gamma-rays are produced.

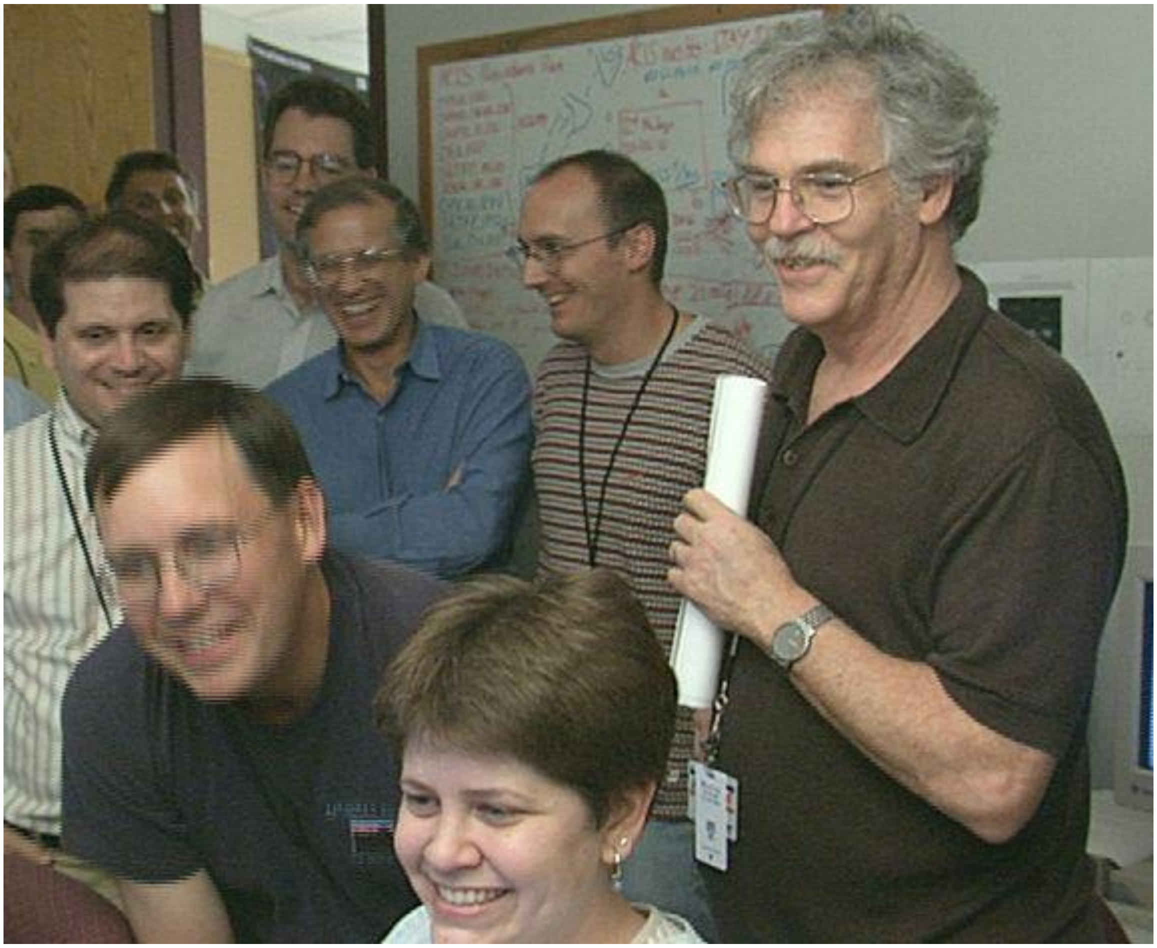

Figure 3: *From right to left: me, Tom Aldcroft, Catherine Grant, Harvey Tananbaum, Roger Brissenden, Mark Bautz, Mark Freeman, Fred Baganoff, and Ken. Gage at the Chandra control center sharing the excitement of the official first light observation in August of 1999..*

I want to conclude this brief narrative with some comments and personal insights for the future of X-ray astronomy. First of all, I am (and many of my colleagues are) very concerned over the ever shrinking number of organizations, primarily academic, that are currently directly involved in building scientific hardware. If the current trend continues there will possibly be only one institution in this country that has the infrastructure and resources to accomplish such tasks and that institution will most likely be at one of our NASA centers. Having fewer and fewer institutions responsible for,technology development is not only worrisome, but potentially wasteful, and can be (should be) seen as a hindrance to efficient progress. The oversimplified reasons are: 1) no one has all the answers; (2) competition breeds innovation.

How have we gotten into this situation? There are many answers and I would not presume to be able to accurately cite them all. One major factor is that we have moved X-ray astronomy into the observatory era and Chandra is a prime (and highly successful) example. Chandra has spawned the growth of a large general observer community. These observers are vital to the discipline, not only for the scientific acumen they bring to the table, but also because they form a large (and vocal) advocacy community. Without their widespread support, it becomes harder to raise money, etc. At the same time however, these great observatories have taken decades to build and cannot adequately serve as training grounds and development programs for optics and instrumentation that will meet the future needs of the discipline.

I believe that we pay another, more dreadful price for the lack of young active experimentalists. As our scientific requirements grow in scope, projected costs are no longer based on accomplishments, or even partial accomplishments, i.e. we must predict years, even decades, in advance how much a new mission will cost. Since the technology hasn't been demonstrated, the cost estimates become outrageously high. This of course delays the missions, and often leads to programmatic rather than science-driven technical decisions because one is trying to cost to a schedule that is totally unrealistic. We, as a community, must shoulder much of the blame. We cannot seem to stand fast and support a more rational approach for developing missions. Even in those cases where the future requirements are (somewhat oversimplifying) crystal clear (at least to me) and currently unobtainable, the Chandra example is an excellent one to follow. Thus, to be able to detect fluxes from galaxies at the dawn of the early universe will require sub-arcsecond optics of collecting power easily a factor of 10 or more over Chandra. Thus, we already know what we want, even must, do to advance the field. To me this situation is analogous to the situation at the conception of Chandra. The telescope then was beyond the current state of the art and *competing* technologies were investigated both by the NASA Project and its research partner at SAO, but also with industry. We had not the chutzpah to seriously put forth the mission for consideration until we had built and X-ray tested an X-ray optic that met all of the detailed Chandra requirements. (This latter in part because NASA and the Congress insisted!) How successful was this approach? The cost overrun at launch was measured at less than a few percent and the final cost was, accounting for inflation, the same as had been estimated by the Project and provided as input to the National Academy of Sciences Decadal Survey for Astronomy and Astrophysics for the 1980's. --- i.e. approximately 20 years before it was built! Once again, I maintain that one of the principal reasons for this success was the heavy involvement of experimental X-ray astronomers in all phases of the program. The question then becomes, where are these experimentalists to come from now?

The answer to the question raised at the end of the previous paragraph is of course expanded technical research primarily in the arena of X-ray optics and a much expanded balloon and sounding rocket program. This is nothing new, the Decadal Surveys and advisory committees have been advocating these ideas for years. The missing ingredient is money. I am not so naïve to suppose that NASA can significantly expand the balloon and sounding rocket programs through an increase to any current NASA budget. The budgetary derivative, if anything, is in the opposite direction. My suggestion is to set aside a non-trivial amount $25M-$40M per year from the Explorer budget to add to the existing funds already budgeted for these methods of providing rapid access to space. I realize that there may be, at some level, legal difficulties associated with the Congressional language for the Explorer program, but I am confident that, if this is what the community demands, it can be made to happen.