Annu. Rev. Astron. Astrophys. 2005. 43:1–30
doi: 10.1146/annurev.astro.43.090303.091253
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First published online as a Review in Advance on April 22, 2005

AN EDUCATION IN ASTRONOMY

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Key Words cosmic X-ray sources, Sco X-1, X-ray telescope, Nobel Prize

■ **Abstract** The scientific career of Riccardo Giacconi is summarized from an autobiographical point of view. The narrative moves from the discovery of Sco X-1, to the observations with *Uhuru*, *Skylab*, *Einstein*, *Rosat*, and the *Chandra Observatory*. His direction of the Space Telescope Science Institute (STScI) and of the European Southern Observatory are described. His recent involvement in the Atacama Large Millimeter Array (ALMA) project is briefly summarized.

INTRODUCTION

I was quite flattered by the invitation to write the Prefatory Chapter for this volume of the *Annual Review of Astronomy and Astrophysics* and I had no hesitation in accepting.

I have always felt a bit of an outsider in Astrophysics. My thesis was in elementary particle physics at the University of Milano, and I continued to work in that field until 1959. I thus did not have the classical training of an astronomer; I knew no astronomer at the beginning of my work and lacked some of the most elementary knowledge in astronomy. Although this was in many ways a handicap, I think it also presented some advantages in entering a new field.

Over the years I have become convinced that my training as an experimental physicist gave me a perspective different from that of traditional astronomers when I turned to observational astronomy. In the first half of the century, most astronomers had been trained in utilizing existing optical telescopes to carry out their investigations. Only in radio astronomy was there a great deal of new telescope building, stimulated in part by the rapid development of technology during World War II. The advent of spaceborne instrumentation necessitated a new approach in which new equipment had to be designed to solve specific quantifiable observational problems, much as one designed experiments in physics. Serendipity was always hoped for but did not set the experiment parameters. Furthermore, the complexity and ever changing nature of the equipment required ad hoc calibration and data analysis techniques resulting in what we now call an end-to-end approach.

The initial astrophysical problem sets the requirement for the instrument specifications. The instrumentation is designed with its operation in mind including actual operational conditions, calibration, data reduction and data analysis techniques to guarantee the achievement of the desired sensitivity. This approach, which my collaborators and I followed from the very first X-ray experiments by rocket to *Uhuru* and *Einstein*, was then introduced by me and by the Space Telescope Science Institute (STScI) staff to the *Hubble Space Telescope* (HST), and later at the European Sourthern Observatory (ESO) to the *Very Large Telescope* (VLT) and the Atacama Large Millimeter Array (ALMA) programs.

The end-to-end approach mentioned above is crucial in making it possible for astronomers in any subdiscipline of astronomy, from radio to γ rays, to utilize data obtained by instruments in which they have no special expertise and has paved the way for the virtual observatories of today. It enables an astronomer to examine the physical processes going on in the celestial object in all the wavelengths of the electromagnetic spectrum. This ability to use data by nonspecialists is, of course, a mixed blessing; it allows the best use and reuse of archival data from first-class facilities, but it also leads to the creation of a class of astronomers who never have had to conceive, design, build and operate their own instruments.

Another theme that I would like to illustrate in this review of my own research is the intimate relationship between pioneering scientific work and technological advances and the diversity of institutional arrangements, which in different phases of research appear best suited to ensure scientific advances.

I hope this review will provide a contribution to the understanding of the profound changes that have occurred in observational astronomy over the past few decades.

BIOGRAPHICAL NOTES

I was born in Genoa, Italy, on October 6, 1931. My father and mother were separated when I was 5 years old and I lived with my mother in Milano until my departure for the United States in 1956. My mother, Elsa Giacconi Canni, was a High School teacher of Mathematics and Physics and the author of many text books in algebraic and projective geometry. She believed that geometry was God's entertainment and I think I absorbed some of her love for geometry. I saw my father, Antonio Giacconi, from time to time. He had been an artillery spotter in World War I, hated war, was an antifascist, and was intrigued by technology. He had an uncanny ability to spot the nakedness of kings and transmitted that tendency to me.

I was a gifted child, much disturbed, however, by the family break-up and by the horrors of the Second World War in the 1939–1945 period. I typically could achieve the highest marks, but was often in trouble for not studying at all or for disciplinary problems. I completed high school a year earlier than normal and chose physics at the University. I would have preferred architecture, which I loved and still do , but I did not feel creative enough. At the University I met one amateur astronomer and I remember seeing the Horsehead Nebula through his home-built telescope. I must confess I was unimpressed and thought at the time that astronomers were more like botanists, rather than real physical scientists. The University lectures were wonderful in analysis and geometry and rather awful in chemistry and in theoretical and experimental physics. By becoming involved since the first year in the research activities of the department, I learned to work on experimental projects as an unpaid assistant and started to carry out research programs in cosmic ray with Antonio Mura, a very good scientist and a very nice person who unfortunately died quite young. On the instrumentation side, I worked with Carlo Succi, a gifted instrumentalist.

But the most profound influence in my training came from Giuseppe Occhialini. He was a far cry from the usual teacher in the rather provincial physics department of Milano. He had been, and remained, a world-class physicist who had discovered, with P. M. S. Blackett, the phenomenon of pair production and, with Cecil Powell, the pion. Blackett and Powell were each awarded the Nobel Prize for this work. In each case they employed new techniques to obtain these outstanding results. To my recollection Occhialini never gave a lecture, but his profound physical intuition, search for excellence and integrity in research, as well as his technical ability could almost be learned by osmosis. He took a liking to me and encouraged me to apply for a Fulbright Scholarship and told me, "go west young man," a phrase from his favorite cowboy movies.

After having done my thesis work in cloud chamber research on proton-produced interactions and having built a new very large multiplate cloud chamber, I left Milano and went west in 1956 to join Robert W. Thompson at the University of Indiana. Both Occhialini and I considered him one of the best cosmic ray experimentalists in the world. I was fascinated by the work he reported at the International Cosmic Ray Conference at Bagneres-de-Bigorre, France, in 1953. His approach by building powerful instruments to achieve crucial results, with certainty, remained a model for me. His extremely careful and thoughtful approach to data analysis, which apparently was stimulated by a youthful error of overinterpretation, has remained with me throughout my life. Mirella, my wife, and I worked for many months, while I was at the University of Indiana (1956–1958) on a Fulbright Fellowship, to measure and analyze data obtained by Thompson. Thompson was the first to accurately measure the mass of the θ_o particles. He had not only built the cloud chamber and operated it for years but had also prepared forms as guides to data analysis procedures that we followed faithfully using Marchant mechanical calculators in the vain hope to find an anti Λ_o particle. We were defeated by statistics. Cosmic ray research was ending, though supplanted by accelerator experiments.

After a year at Princeton I joined American Science and Engineering (ASE). I was in search of a place where I could stay for a few years with some prospect of stability for the family that I had now formed and some opportunity to learn a trade and get off the fellowship mill. It is fair to say that up to this point I had learned a lot of physics and techniques but I had not been very productive in research, nor had I found my real research interest. The years at ASE became the most scientifically productive years of my life.

ASE: A START IN X-RAY ASTRONOMY

In September 1959 I joined the staff of American Science and Engineering, a small research company in Cambridge, Massachusetts, which had been founded a year earlier by Martin Annis, a physicist who had worked in the cosmic ray group at MIT with Bruno Rossi, Stan Olbert, Herb Bridge, and George Clark. The purpose of this company was to provide support to the U.S. government in defense, education, and medicine. I joined when it had a staff of 28 people and my task was to initiate a program in space science with both defense and basic research applications. I started studying problems having to do with γ-ray transmission through the atmosphere and the detection of artificial γ-ray bursts in space. I also investigated, with George Clark, the feasibility of measuring ratios of α particles to protons in the Van Allen Belts. I apparently did well and was introduced to the Chairman of the ASE Board, Bruno Rossi, who suggested that I should look at the possibility of carrying out X-ray observations of celestial objects from space.

I was unaware of the work by H. Friedman and his colleagues of the Naval Research Laboratories (NRL) on X-ray emission from the sun, which had been proceeding for more than a decade using captured V-2 rockets. This work prompted discussions that had taken place at the Space Science Board (of which Bruno Rossi was Chairman) in 1958 and 1959. The discussions, which included contributions by John A. Simpson, Lawrence H. Aller, and Leo Goldberg, had come to the conclusion that X-ray observations appeared to be an interesting way to study high-energy radiation from stars. Furthermore, Malcolm Savedoff at Rochester and Philip Fisher at Lockheed had responded to the solicitation of James Kupperman of NASA to carry out experiments in X-ray astronomy as early as June 1959 (Savedoff) and August 1960 (Fisher). [All of the above is reported in great detail in the thesis, "Science, Technology and Public Policy: The case of X-ray Astronomy, 1959 to 1972," by Richard Frederic Hirsh, University of Wisconsin, Madison, 1979 (Hirsch 1979).]

I have always felt gratitude to Bruno Rossi for his suggestion to me. It was as if I had been preparing all my life for solving this particular problem. Ignorance of the competition was a blessing in disguise because I had to rethink both the theoretical and experimental approaches to the field. This rather informal conversation with Rossi was the first time I had ever heard of this field and, having been trained as a physicist, I tried immediately to assess the problem of detection of celestial X-ray sources in order to derive the instrumentation requirement.

I read with great interest the article by S.L. Mandelshtam and A.I. Efremov entitled "Research on Shortwave Solar Ultraviolet Radiation" (Mandelshtam & Efrmov 1958). The authors reviewed the state of solar X-ray astronomy up to 1958 from theoretical and experimental points of view, giving great relevance to Friedman's results and discussing their own plans for future work.

Considering the sun to be a typical X-ray emitter, it became immediately obvious that the expected flux at Earth from celestial sources would be below the threshold of current detector systems by many orders of magnitude. This remained true even when considering the possible emission from hotter stars or supernovaes. I must admit that this finding was so discouraging that I was on the point of abandoning the field. I had a taste of low statistic results when I had labored for two years at the Laboratorio della Testa Grigia (3500 m) to collect the 80 proton events that formed the basis for my thesis in cosmic rays.

I had dreamed then of being able to concentrate the cosmic ray flux on my cloud chamber. Now faced with a similar problem of very low photon rates I started looking for a way to concentrate the radiation. While reading the Encyclopedia of Physics (Flugge 1959) I came across a description of total external reflection by X rays. Projective geometry had been my early love: hence a reflecting grazing incidence paraboloid was the immediate result.

I was able to show that, using such paraboloids and current detectors, I could improve the sensitivity of the instrumentation by factors of 10^5, thus making it possible to essentially observe stars in their X-ray light.

Martin Annis, the President of ASE, immediately called Bruno Rossi to tell him of my idea. I had not seen Bruno Rossi since our first discussion, but he soon visited ASE and suggested the refinement of nesting many paraboloids.

Thus encouraged by this early success, I continued to study the field and in the period between September 1959 and January 15, 1960, I prepared a technical note, "A Brief Review of Experimental and Theoretical Progress in X-ray Astronomy," (Giacconi, Clark, & Rossi 1960) which already contained an assessment of the likely fluxes from extra solar celestial emitters as well as the basis for future experimental progress. This note included a reference to the paper "A Telescope for Soft X-ray Astronomy," which had been submitted by myself and Bruno Rossi to the *Journal of Geophysical Research* on December 7, 1959 (Giacconi & Rossi 1960).

I want to emphasize here the great contribution given by George Clark in writing this document. He provided most of the astronomical knowledge and some of the English. We have remained good friends over the years.

Also contained in the January 15, 1960 note were approaches to improve in the immediate future the performance of more conventional detectors, such as better collimators and anticoincidence rejection of cosmic rays. We tried to interest NASA in funding the development of a prototype of the telescope as well as exploratory flights of rockets with conventional equipment.

We were successful in interesting Dr. John Lindsay of the Goddard Space Flight Center (GSFC) in funding a small program to develop grazing incidence telescopes but not in interesting NASA Headquarters in funding rocket instrumentation to search the night sky for X-ray stars. We therefore turned to the Air Force Cambridge Research Laboratories that had funded previous work by ASE in the classified domain. The Air Force was receptive to providing support to place a small aperture (1 cm^2) Geiger counter aboard a Nike-Asp rocket. The flight attempted in 1960 failed because of rocket misfiring. In January 1961 we received a new contract to fly four Aerobee 150 rockets for our experiment to search for X-ray stars, as well as lunar X rays. The larger rocket permitted the design of a much more sensitive instrument.

We included three Geiger counters, each with an area of 10 cm^2. Thin mica windows were used, which permitted detection of X rays between 2 and 8 Å. Anticoincidence scintillator counters were used to reduce the cosmic ray background. An important feature of the experiment was the use of a large field of view, which increased the probability of both observing a source anywhere in the sky and receiving a sufficient number of X-ray photons to make detection statistically significant. Rather than the 3° field used by Friedman of NRL for his extreme ultraviolet (EUV) and X-ray searches, we used a field of view of 120°. The probability that a spinning rocket with a field of view of 3° passes over a source is 1%. With 120° it is 60%. Furthermore, a detector with such a small field dwells on a source (during a scan) for a correspondingly small time and thus collects a small number of photons, insufficient to distinguish a source from background noise.

In effect, the payload we designed and flew was 100 times more sensitive than any flown until then. The entire payload was the work of the scientists at ASE. The group now included Frank Paolini and Herbert Gursky in addition to myself. Paolini was responsible for much of the electronics and aspect system; Gursky was responsible for the detectors.

After another rocket system failure in October 1961 we had a successful flight on June 18, 1962, when we discovered the first extrasolar X-ray source (Sco X-1) as well as the extragalactic X-ray background.

The significance of the discovery was beyond all we had hoped to find. Here was an object that emitted a thousand times more X rays than the sun at all wavelengths and a thousand times more energy in X rays than in visible light! Clearly we were dealing with a new class of stellar objects in which physical processes not known in the laboratory were taking place.

If the hope of being able to develop the X-ray telescope sustained us in the beginning, the certainty of being able to do great astrophysics with relatively simple instruments provided an enormous stimulus to continue X-ray astronomy.

Soon after the announcement, and in response to our discovery, the NRL group, led by Herbert Friedman, confirmed our discovery of Sco X-1 and also observed the Crab Nebula. The new payload was designed and built by Stuart Bowyer (who had joined NRL in late 1962) and flown in April 1963. Their detector had a larger area than the ASE detector and better angular resolution. The NRL work provided an important confirmation of the ASE discovery.

The Formation of the Space Research System Division and Plans for X-Ray Astronomy

Beginning in the fall of 1961 the U.S. government initiated plans to carry out in 1962 nuclear weapons tests in the atmosphere, presumably as a response to tests by the U.S.S.R. and as a prelude to a nuclear test ban treaty. ASE was asked to participate in these tests by providing monitoring equipment that would be flown on rockets and satellites to measure electrons, X rays, γ rays, and debris produced by the nuclear explosions. My group at ASE expanded from a few people to more than seventy and designed, built, tested, integrated into vehicles, and supported the launch of twenty-four rocket payloads in an eight-month period. We also provided payloads for six satellites. The effort required to achieve these results was truly exceptional. We had a scheduled workweek of 72 hours over several months. Our payloads were highly successful (95%). The team showed great technical competence and dedication and I learned how to lead and motivate a highly skilled technical group.

This experience placed us in a strong position from which to continue the pursuit of X-ray astronomy. After 1963 the Air Force Cambridge Research Laboratories was no longer prepared to support what had become clearly an astronomy program.

We were successful in a proposal to NASA to place an X-ray detector on the rotating wheel section of the *Orbiting Solar Observatory* (OSO)-D but we yearned for support on a much broader program of research. Herbert Gursky and I submitted to NASA on September 25, 1963, a proposal for "An Experimental Program of Extra Solar X-ray Astronomy." This proposal was in essence a plan for the development of X-ray astronomy from rocket experiments to a dedicated satellite (which became *Uhuru*), to imaging X-ray telescopes on the Orbiting Astronomical Observatory, and finally to a 1.2 meter diameter X-ray telescope. This plan was based on a sober consideration of scientific requirements, though quite bold in approach. For instance, the requirement for a 1.2 meter telescope was derived from the sensitivity required to image the extragalactic background (assumed isotropic) with an angular resolution of 1 arc minute.

This program was to be completed in times that now look hopelessly optimistic: *Uhuru*, the scanning all-sky satellite was to be launched in December 1966 and the 1.2 meter X-ray telescope in 1968. Eventually *Uhuru* was launched in 1970 and the program for the 1.2 meter telescope was started in 1970. It took an intermediate step with the *Einstein Observatory*, launched in 1979, to finally achieve a 1.2 meter telescope orbit in 1999, the now famous *Chandra*.

Even so, I consider this strategic plan one of the most important documents I ever wrote as it provided my own view of how the field should develop and went well beyond the vision of anybody else at the time of what X-ray astronomy could become.

From Sco X-1 to *Uhuru*

The findings of the 1962 rocket flight stimulated an enormous amount of interest in the astronomical community. This was expressed in renewed observational efforts (at NRL and Lockheed) as well as theoretical efforts to analyze the possible sources of cosmic X rays and the production mechanisms that may operate in galactic and extragalactic space.

Geoff Burbidge was quick to take on this new field of astronomy and, by his enthusiasm and interaction with many astrophysicists worldwide, greatly helped to establish the interest of a broad community in the field. Because of the nearness of Sco X-1 to the galactic center, Burbidge tried to explain the emission of this source as coming from proton-electron interactions at the galactic core at relatively high densities. This prediction turned out to be wrong due to the fact that Sco X-1 was shown to be not coincidental with the galactic center and also because of the inefficiencies of the process. (Thermal bremsstrahlung from a hot, optically thin plasma was a much more efficient process as pointed out by Bruno Rossi). Burbidge was instead quite right in suggesting that the general extragalactic background was due to the unresolved cores of distant galaxies.

One of the most interesting consequences of the early finding was that, as pointed out by Fred Hoyle and Burbidge, the background intensity observed in the X rays was so greatly below that expected for the hot universe model of the steady state universe as to make it untenable. This was the first real input of X-ray astronomy to cosmological questions.

A number of rocket flights were carried out from 1963 to 1970 in order to discover additional sources and to answer the fundamental question of the class of objects corresponding to X-ray sources such as Sco X-1. It was clear that the Crab Nebula was a source of X rays presumably emitted through bremsstrahlung radiation. But the nature of the other galactic X-ray sources remained a puzzle. The group I was leading at ASE decided that we should try to identify Sco X-1. Using a modulation collimator proposed by Minoru Oda, we determined that the angular size of the X-ray source corresponding to Sco X-1 was less than 7 arc minutes. (This was the first collaboration effort with Minoru and the beginning of a life-long friendship). We also measured the X-ray spectrum of Sco X-1 and determined that it was either a power law or a thermal bremsstrahlung spectrum, thus we could compute the minimum optical brightness to be expected, which turned out to be thirteenth magnitude. A star-like object of thirteenth magnitude should be easily detectable. In 1966, a joint effort between ASE and MIT led by Herbert Gursky succeeded in locating with unprecedented accuracy the X-ray source that led to the identification (in collaboration with Allan Sandage) of Sco X-1 with a thirteenth-magnitude object whose spectrum resembled an old nova.

I had met Allan Sandage at the Fermi International Summer School in Astrophysics in Varenna, Italy, which took place in 1965. He was the first real astronomer I had ever met, and it was natural for me to turn to him for help in this work. I was more fortunate than I knew.

I also had the fortune to come to know during this period of rapid discoveries and publications the editor *par excellence* of the *Astrophysical Journal*, Prof. Subrahmanyan Chandrasekhar, who was a wonderful and most helpful person.

In 1966 Bruno Rossi attended the International Astronomical Union Symposium in Nordwijk, where he reported (with our permission) the identification of Sco X-1 with a star with an old nova-like spectrum. A group of interested astrophysicists including Burbidge, V.L. Ginzburg, and I. Shklovsky participated in the meeting at which a number of hypotheses were discussed. They focused on the possibility of a binary system. Robert Kraft had shown in 1962 that novae are members of double star systems. Although it was clear that novae, as such, were not X-ray sources, binary system models were quite attractive. S. Hayakawa suggested such a possibility in 1963. In 1965 Y.B. Zeldovich had discussed accretion on a neutron star as a means of producing X rays. Soon after this conference Burbidge, Al Cameron, and Shklovsky came up with binary star models. The 1967 paper by Shklovsky has been most frequently quoted but claims of priority by others have not been put to rest.

However, the data did not show any periodicity that would confirm this model. Other novas did not emit X rays, and the very survival of a binary system in the supernova explosion required to produce a neutron star was not clear. The situation was further complicated by the discovery of the pulsars by A. Hewish and Jocelyn Bell in 1967. Did the Crab Nebula pulsar also emit X-rays? NRL rushed to find out and in May 1969 they detected pulsed emissions from the Crab pulsar, a result confirmed weeks later by an MIT group led by Hale Bradt.

The discovery of pulsars proved the existence of neutron stars and revealed the importance of rotational kinetic energy to provide the source for the powerful radio and X-ray emission. Because of the success of the neutron star model for Crab, it became fashionable to try to explain Sco X-1 by similar models, but without much success.

The 1960s had been an incredibly exciting period in astronomy: The 3° K radiation, the X-ray background, the X-ray stars, the pulsars, and the quasars had been discovered. New observational capabilities were on the horizon. This included the launch of the X-ray astronomy satellite *Uhuru*, which occurred on December 12, 1970, and the development of X-ray imaging telescopes, which will be discussed later.

Uhuru

The launch of *Uhuru* in 1970 came some seven years after the initial proposal. It was an ambitious proposal when presented to NASA officials in 1963. Dr. Nancy Roman of NASA actually fell asleep during my presentation and I was quite surprised at the end of it when she expressed an interest in receiving a full-blown proposal to be peer reviewed. After approval by peer review there followed a long period of negotiations. NASA did not want to let ASE manage the program and build the satellite. Finally after creating the Small Astronomy Satellite Program, GSFC agreed to manage it and the Applied Physics Laboratory to build the spacecraft while ASE was responsible for all scientific instrumentation. Had it not been for the constant help and support of Dr. John Naugle of NASA during these years *Uhuru* would never have been built. Finally in December 1966 we got started. The experiment was to be built under my control at ASE for a fixed amount of money ($5,131,000); the contract was signed on November 1967. All aspects of the *Uhuru* science instrumentation were designed at ASE, with subcontractor support during fabrication.

I conceived of an extremely simple and reliable experiment whose data should be easy to interpret. The detection system consisted of two proportional counters with beryllium windows of about 800 cm^2 area. Mechanical collimators defined the fields of view of the detector $.5 \times 5$ and 5×5 degrees. The satellite was designed to rotate slowly on its axis, sweeping a 5° band of the sky every 12 minutes. The speed of rotation, as well as the spin axis orientation, could be controlled from the ground. Star sensors permitted us to locate the source of X-ray emission in the sky. We arranged with NASA to send us a portion of the data through a telephone line each day. The satellite was launched from the Italian S. Marco platform in Kenya on the Kenyan freedom day and was named *Uhuru*, which means freedom in Swahili.

As the satellite scanned the sky on different days the same sources were detected at different sky inclinations and thus a map could be constructed of the distribution of sources in the sky. The *Uhuru* catalogue resulted in more than 300 sources. Roughly one half were clustered along the plane of the Milky Way, and therefore originated in our galaxy, whereas the remainder was distributed uniformly over the sky.

It is difficult to describe the intensity of commitment of our small team, which at that point included Harvey Tananbaum, John Waters, and Ed Kellogg, to every aspect of the satellite. I remember waking up at night thinking about possible problems and traveling in my mind in the inside of the satellite to uncover any electrical or mechanical weakness. It was as if I were traveling in space and feeling the effect of vacuum, changing sunlight exposure, and particle bombardment. After launch it became an all-absorbing involvement to scan each day the data received and design the next day's observing plan. It was an unusual arrangement for NASA to transmit data to us on a daily basis. It was, however, essential to the success of the mission, which depended on our ability to change our observing program in response to actual discoveries. Steve Murray, Ethan Schreier, Paul Gorenstein, and Herbert Gursky, together with William Tucker, our in-house theoretician, became deeply involved in the operations of the satellite data analysis and scientific interpretation. The meetings at which the results were debated and the decisions taken were denoted by ruthless intellectual honesty made possible by mutual respect and a shared sense of privilege in seeing for the first time what had not been seen before. The experience became for us the "*Uhuru* spirit" never to be forgotten by those that took part in this intellectual adventure.

The description of the scientific findings is contained in books (Giacconi & Gursky 1974) and many other reviews, articles, and lectures, including my Nobel Lecture of 2002.[1] Here I would like to focus only on what were for me the most exhilarating moments. After the discovery of pulsars in 1967 we tried very hard to convince NASA that we should improve the planned time resolution of 0.1 seconds to 0.001 seconds, which would have been possible at a cost of 5% of the experiment. Although NASA did not provide the funds for this improvement, we were very aware of the potential significance of the study of time variability of the X-ray sources.

[1]See the Editor's postscript at the end of this review for more information regarding Giacconi's Nobel Prize.

Gursky and I asked Minoru Oda, who was visiting us just after the *Uhuru* launch, to scan the data with a view of finding objects in which variability could be observed during one pass of 10 seconds. We were immediately rewarded in finding time variability in Cygnus X-1. This led to the decision to slow down the spin rate of the satellite so that Cygnus X-1 could be observed for 100 seconds and it led to the discovery of Cen X-3 and Her X-1 by Ethan Schreier and Harvey Tananbaum, respectively. The timing data on the last two sources was of a quality never previously achieved in X-ray astronomy. We discovered pulsations and could determine the period with a precision of one part in a million and demonstrate their Doppler shift as the compact X-ray source travels its two-day orbit. Studying the decrease of the period over years led to the conclusion that these two energy sources had to be accreting magnetized neutron stars in binaries and that the energy source for the X-rays came from the gravitational infall of matter from the companion. A nucleon falling onto a neutron star could acquire and dissipate ten times more energy than one could obtain in nuclear fusion. Remo Ruffini and John Wheeler had shown that a nucleon orbiting around a black hole could be even more efficient: It could acquire and dissipate up to 40 times more energy than nuclear fusion! This was the source of energy for most of the galactic X-ray sources with luminosities up to 10^4 times larger than the solar one.

The rapid and large time variability of Cygnus X-1, which was erratic rather than periodic, set a constraint on the size of the compact star and focused the attention of astronomers on this object. The absence of regular pulsation, the energy, and the compactness of Cygnus X-1 could be explained either by a black hole or a nonmagnetized neutron star. The crucial difference between these two possible outcomes was the determination of the mass of the compact star: Rhoades and Ruffini had in fact determined that the absolute upper limit to the neutron star masses must be 3.2 solar masses. The enigma was resolved by A. Webster and Paul Murdin, who measured for the compact object a mass of 6 solar masses, much greater than permissible for a neutron star as shown by Ruffini. Therefore Cygnus X-1 had to be a black hole.

Equally exciting was the discovery that clusters of galaxies contained, in what had been believed to be empty space between galaxies, a low-density plasma at a temperature of millions of degrees containing more mass than all galaxies combined. This gas could never be observed except in X-rays. Its existence and study has opened new areas of cosmological research at early epochs. Alfonso Cavaliere, who was visiting us at the time, became deeply involved in this field.

Finally I had always been interested in the nature of the extragalactic X-ray background and whether it was truly diffused or made up of unresolved discrete sources. Burbidge had suggested the latter in 1963 and on that basis I had proposed the 1.2 meter telescope. The *Uhuru* data showed the background to be very smooth requiring a very high density of sources, of approximately 1 per square arc minute or $\sim 10^8$ sources over the entire sky. The Log N versus Log S plotted by S. Matilsky for high latitude sources showed a very rapid increase in the number of sources with decreasing sensitivity down to the limit of 10^{-11} erg cm^{-2} sec^{-1}.

(At 5×10^{-17} erg cm^{-2} s^{-1} *Chandra*, in fact, finds ∼1 source per square arc minute).

By 1973 the results of *Uhuru* had received much attention by the astronomical community. Thus a number of scientists in other disciplines in astronomy and several theorists visited ASE and provided useful models and debates on the nature of the objects we were observing. Apart from Martin Rees, Jim Pringle, Don Lamb, and John Bahcall, several Russian scientists visited us, including R. Sagdeev and Shklovsky. The collaboration with Remo Ruffini started a friendship that continued over the years. In 1975 the sixty-fifth Enrico Fermi Summer School "Physics and Astrophysics of Neutron Stars and Black Holes," directed by Giacconi and Ruffini, with the participation of S. Chandrasekhar, J. Taylor, and R. Sunyaev, among others, further evidenced the profound impact of *Uhuru* on the basic understanding of the process of gravitational collapse and its possible outcomes.

A fundamental lesson *Uhuru* taught me was the importance of establishing, in advance of flight, a sophisticated data analysis system capable of reducing large quantities of data for almost immediate examination. In this we were far ahead of other groups in X-ray astronomy and in astronomy in general. This lesson was applied by our group to the High Energy Astronomy Observatory (HEAO)-1 experiments and in its full extent to the *Einstein Observatory*. It has become a standard for the HST, VLT, *Chandra*, and ALMA.

The Development of the X-Ray Telescope

The development of X-ray telescopes occurred under NASA sponsorship through a contract from the GSFC. Dr. John C. Lindsay was the head of the Solar Physics Group there and had previously worked with Friedman at NRL. He was most interested in developing X-ray telescopes for solar studies. This was an entirely acceptable first step in developing X-ray telescopes for stellar research. Due to the characteristics of X-rays, effective reflection from mirrors can only be obtained at grazing incidence. An imaging X-ray telescope requires two reflections, and a typical system consists of a paraboloid and a hyperboloid (as first studied by H. Wolter for X-ray microscopes in 1952). Apart from the difficulty of polishing the inside surfaces of the tubular like mirrors, the main problem in achieving high efficiency at kev X-ray energy was the requirement to achieve surface polish smooth to a few Angstroms. This was not a problem for solar research because the very large fluxes from the sun still yielded usable results even with relatively rough surfaces. After more than three years of laboratory work with N.F. Harmon, R.F. Lacey, and Z. Szilagy at ASE, we were able to construct a rocket payload which in 1963 and 1965 obtained the first pictures of the sun with grazing incidence optics. This work continued until 1968 when a much better telescope was constructed and flown by Giuseppe Vaiana and our colleagues at ASE. The telescope achieved 2 to 5 arc second resolution and obtained a stunning photograph of a solar flare. The high-resolution picture revealed complex structures in the corona never previously observed and clearly showed the role of the solar magnetic fields in flare activity.

We soon began work on a more ambitious mirror with two nested surfaces of 22.5 cm and 30 cm in diameter to be flown ultimately on the *Skylab* Mission, a mini space station orbited in 1973. There were some interesting features to this solar experiment. John Lindsay, who was a great colleague and a wonderful person, died of a heart attack while we were preparing a joint proposal for the *Skylab*. His group at GSFC did not wish to be scientifically associated with a corporation (ASE) and thus the plan was to submit two independent proposals that would be essentially identical. We at ASE were rather desperate because it was obvious that for equally meritorious proposals, the peer review committees would favor government or academic institutions. We tried to improve our chances to be funded by improving our experiment with the introduction of a transmission grating acting as a slit-less spectrometer. This system was invented and demonstrated to work by H. Gursky and Ted Zempfering within a few weeks and was, I believe, important to allow us to proceed with our program. The prototype of the *Skylab* mission was used to demonstrate the feasibility of nesting many surfaces, an arrangement necessary for stellar astronomy. In order to test the solar mirrors in the laboratory we developed photoelectric image detectors (although the *Skylab* experiment utilized film). These detectors became the basis of the *Einstein* Stellar Telescope detector's design.

I have told this story to underline the unique opportunities we had to set up laboratories and carry out technology developments in a corporate setting and also some of the difficulty of having to compete as scientists against academics or government laboratory staff.

Giuseppe Vaiana, Leon VanSpeybroeck and their colleagues turned the X-ray telescopes into a major tool for solar research. It was more difficult to convince the astronomical community that X-ray telescopes had a major role to play in extrasolar X-ray research. Theoreticians (Philip Morrison, for one) stated that high angular resolution was not required because there would be no phenomena to be observed on scales less than an arc minute. Experimental groups at NRL and GSFC set their aims at larger and larger area detectors essentially like those used on *Uhuru*. This approach has diminishing returns as the sensitivity for nonimaging detectors only improves as the square root of the area, whereas the use of telescopes yields signal-limited detections whose sensitivity increases linearly with area. (To achieve *Chandra* sensitivity an *Uhuru*-type detector would have had to have a surface of 1 square mile!). However, I was successful in persuading my colleagues to include an X-ray telescope in the planned series of HEAO missions that was recommended to NASA by the Woods Hole Summer Study Group of the National Academy of Sciences (NAS) in 1967.

We created a consortium of research institutions including ASE, Columbia University, GSFC, and MIT, to propose a large X-ray orbiting telescope, LOXT, to be flown in the second half of the 1970s. The instrumentation consisted of two X-ray telescopes, one of the Wolter type (1.2 m diameter) and one of the Kirkpatrick-Baez type (1×1 m) to provide imaging, spectroscopy, and polarimetry. The observatory then proposed was in fact larger than the *Chandra Observatory*. In a twist of scientific sociology I was the designated Principal Investigator, thus having a corporation lead the scientific effort. To the delight and amazement of all of us NASA approved the program in September 1970. If one considers that *Uhuru* had not yet flown and that little was known of the X-ray sky, this was really a bold move by NASA.

We started work on this great project in 1970 and were stopped in 1973 because of the financial difficulties NASA was facing due to over-runs in the Viking program. We succeeded in continuing as part of a reduced HEAO program with three rather than four spacecrafts and only one X-ray telescope 0.60 meter in diameter, which became the *Einstein Observatory*.

There was a dramatic meeting at NASA Headquarters in which Dr. Jesse Mitchel explained to us that we were in the situation of a sinking ship, the lifeboats could not take all passengers and could those left behind please help to push the boats on!

FROM ASE TO HARVARD

Soon after the *Uhuru* launch Giuseppe Vaiana, who had become my best friend over the years, and I had innumerable and long discussions (typically over cognac) in which we tried to understand where the field of X-ray astronomy was going and what would be the best institutional setting to carry it out.

It was clear to both of us that we cared more about the scientific research we were carrying out than making profits for the corporation. My own Space Research and System division had grown to 500 people mainly involved in the engineering work of LOXT and then *Einstein*. I had become Executive Vice President of the Corporation and had both an Educational and a Medical Research Division reporting to me. The company itself seemed more bent on diversifying its activities and developing commercial products (like the airport scanners pioneered by ASE) than on carrying out basic research. The scope and complexity of the planned projects (such as LOXT and/or *Einstein*) were somewhat incongruous with being run by a corporation. Some of the X-ray astronomers, including me, had already discussed a National X-ray Observatory to be run as an independent institute (financed by NASA) to design, develop, construct, and operate the next generation of X-ray observatories. These discussions took place while considering how to proceed with institutional arrangements for Space Telescope.

The approach of independent institutes, which has been adopted by NSF and the National Institutes of Health (NIH) to provide research facilities in physics, astronomy, medicine, and biology, has not been followed by NASA, which always insists on retaining management control on both the operational and scientific aspects of the work they sponsor. At the beginning of the 1960s much of the technological expertise resided in NASA, whereas by the 1970s it had become obvious that the expertise now resided in industry and the universities. As for research direction, the community, when asked through the NAS, clearly preferred to directly manage the research (Hornig 1976) rather than have it managed by a NASA center. The direct involvement of the astronomical community was deemed essential by the Hornig's Committee in inspiring the best scientific utilization of *Hubble*.

It seemed to me that an academic institution would provide a more appropriate setting for such an X-ray astronomy enterprise than a private corporation. I also thought that X-ray astronomy observations, which could now extend to all known celestial objects, should be integrated with the rest of astronomy and that this would be much easier if we were surrounded by other astronomers and have the benefit of discussions with them. I had become quite conscious of this need and when helping to organize the International Astronomical Union (IAU) Symposium No. 55, held in Madrid in 1972 on "X-ray and γ-ray Astronomy," I insisted on the participation of optical, infrared, and radio astronomers.

The need to make X-ray astronomy relevant to all disciplines in astronomy was also clear if we were to have the broad community support needed to convince NASA to proceed in carrying out ambitious enterprises such as *Chandra*.

At this point in time, the Harvard Observatory and the Smithsonian Astrophysical Observatory (SAO) were being reorganized in what ultimately became the Center for Astrophysics (CFA) under the direction of George Field. I was offered a Professorship at Harvard and the ability to transfer some dozen of my staff at the Observatory to continue there my role of Principal Investigator on *Einstein*. The scientific group included Ethan Schreier, Herbert Gursky, Ed Kellogg, Steve Murray, Paul Gorenstein, Leon VanSpeybroeck, Harvey Tananbaum, William Forman, and Christine Jones. Giuseppe Vaiana joined the CFA a year later. The development of the *Einstein* telescope and instrumentation hardware remained the responsibility of ASE and the other institutions involved. The spacecraft was built at TRW Corporation and the overall program management was under the HEAO Program at Marshall Space Flight Center (MSFC). Again, this transition of my group benefited greatly from the understanding and support of Dr. John Naugle of NASA.

The *Einstein Observatory*

Given the background described above, some of the motivation for the overall approach we took to the implementation of the *Einstein* Observatory should be clear.

We, first of all, considered *Einstein* a national observatory rather than a principal investigator experiment, as indeed it was. The main difference was that we proposed to NASA at the outset of the program that 30% of the time should be assigned to astronomers not part of the initial consortium. Such time allocation would be administered by the Observatory Director (the Principal Investigator) on the basis of peer review.

We recognized that the responsibility for calibration of the instruments on the ground and in orbit should be the responsibility of the instrument builders but that the work should be integrated and managed by the Harvard-SAO team. Thus the *Einstein* team took on the responsibility not just to provide a facility for the community but to provide data of guaranteed quality to all observers. This was a radical step with respect to traditional approaches at ground-based observatories. In order to provide the data as described, a great deal of software had to be prepared and tested. Ethan Schreier and his staff did a fantastic job prior to and during the mission.

The Harvard-SAO team took on the science operation responsibility in attempting to maximize the lifetime of the *Einstein* mission. We had suggested to NASA that the slewing of the Spacecraft could be achieved by magnetic torquing (as in *Uhuru*) rather than gas jets because we were aware that the gas reservoir would soon be depleted. NASA refused. As a result we decided to operate the mission choosing targets and slew sequences that would minimize gas consumption.

The *Einstein Observatory* was launched in low Earth Orbit so that the shuttle could reach it from time to time to prevent its re-entry owing to the drag of the residual atmosphere. Unfortunately Shuttle was not completed prior to *Einstein* reentry. We tried to point *Einstein* in such a way as to minimize drag. Furthermore, the gyros that were needed for holding the pointing direction to arc seconds had a high rate of failure. We devised schemes to work with 4, 3, and 2 gyros.

Interestingly enough, NASA was not really supportive of these attempts to prolong *Einstein*'s life. A representative of NASA Headquarters (Dr. Al Shart) frankly told me that they considered the cost of continued operations of the spacecraft ($\sim$3% of capital costs) an undue burden. The net result was a 20 year hiatus in the operation of comparable observing X-ray capabilities by the United States from the demise of *Einstein* in 1981 to the launch of *Chandra* in 2001.

The *Einstein Observatory* obtained for the first time high angular resolution (5 arc seconds) X-ray pictures of individual objects and regions of the sky. It detected all known types of astrophysical objects including planets, main sequence stars, binary X-ray systems, supernovas, individual galaxies of all types, active galaxies, clusters of galaxies, and sources of the X-ray background.

The high increase in sensitivity of *Einstein* with respect to *Uhuru* (a factor of more than 100) completely transformed the impact of X-ray astronomy observations on astronomy in general. Astronomers in all disciplines found they needed and could receive and use X-ray data relevant to their investigations.

Thus X-ray astronomy changed from a tool used by specialists interested in neutron stars, black holes, and high temperature plasmas to a new view of the universe in which high-energy phenomena played a key role in the formation, evolution, and dynamics of celestial objects from stars to the largest structures in the universe.

Each scientist in the *Einstein* Harvard-SAO team tried to specialize in some particular subject of astronomy so that we would not be totally ignorant when confronted with the data. The typical approach was to give lectures on subjects we knew nothing about.

I broadened my outlook on high-energy astronomy during a visit to the Soviet Union in 1978 when I met again for prolonged discussions with Ginzburg, Shklovsky, Zeldovich, Sunayev and their collaborators. It is difficult to overestimate the contributions of Russian theoretical astrophysicists to the development of astronomy in general and X-ray astronomy in particular.

I concentrated on the problem of resolving the sources of the X-ray background. With a 1 million second exposure I was able to see 25% of the background (BKG). I knew that we needed a 1.2 meter telescope to solve the problem! Together with

Harvey Tananbaum I wrote a new proposal for the *Advanced X-Ray Astronomy Facility* (AXAF) Project, which was renamed *Chandra* after launch, in a letter to NASA in 1976. I also tried to convince Harvard and SAO to support a proposal to create a National X-ray Observatory within the CFA. Finally I tried to convince NASA to support the creation of such a center. NASA, at the time, was considering the creation of STScI and the reaction of Dr. James C. Fletcher (then the Head of NASA) was that NASA already had an Institute too many.

Eventually the AXAF was started by NASA in the early 1980s. I competitively won a role as interdisciplinary scientist while STScI Director in 1986 and continued to participate in the program for the next 19 years.

Harvey Tananbaum did a magnificent job of leading the AXAF efforts, through a very ambitious technology development period and the shoals of NASA politics. As it turned out, CFA was designated as the host of the *Chandra* Science Operation Center and Harvey the Director of *Chandra*.

In the long interval between *Einstein* and *Chandra*, X-ray astronomy was advanced mainly through German and Japanese missions. Among them a collaboration was established between the Max Planck Institute for Extraterrestrial Physics and NASA in the execution of the *Rosat* satellite, a scaled-up version of *Einstein*. In fact an identical copy of the *Einstein* high-resolution imaging instrument was provided by the Harvard-SAO group, under the lead of Steve Murray, for flight on *Rosat*. The high positional accuracy and high angular resolution that had been shown to be essential in the optical identification of the X-ray sources in *Einstein* deep surveys were made available to the entire community without any proprietary data period for the scientists responsible. I am grateful that I was allowed to continue my studies of the background through a scientific collaboration with Joachim Trümper, Günther Hasinger, and Maarten Schmidt that continued for more than 15 years. With *Rosat* we were able to resolve some 80% of the X-ray BKG into discrete sources, generally active galactic nuclei (AGNs). The collaboration with the Max Plank Institute for Extraterrestrial Physics has continued through the present.

I must admit that not withstanding the great scientific strides of X-ray astronomy I was somewhat unsatisfied by the pace of developments. Though in the early 1960s and 1970s the progress seemed bound only by our creativity, in the 1980s one could see a much more ponderous approach dictated by the very size of the AXAF project ($1.7 billion) and the much greater involvement of the NASA institutional bureaucracy. I had been spoiled by the great joy of daily discovery with *Uhuru*, and *Einstein* tended to be of greater interest to traditional astronomers than to a renegade physicist.

STScI

Out of the blue in 1980 I received a call from the chair of the Association of Universities for Research in Astronomy (AURA) search committee, Prof. Margaret Burbridge, to ask whether I would be interested in discussing with AURA the possibility of becoming the first director of the STScI. My first reaction was disbelief that I would be considered. Knowing a little more about the clannishness of optical ground-based astronomers than before joining Harvard, I was convinced this would not be terribly well received in the optical community. It was due to my wife's advice that I did not reject this out of hand and went for an interview.

Once again I found that my past activities seemed to fit the requirements of the position. I had my own views of how a research institute should be run. I could contribute a wealth of managerial and operational experience particularly relevant to the *Hubble*. I had a burning drive for excellence in scientific research rather than instrumentation building. I could contribute experience in scientific utilization of a space facility to optical astronomers, few of whom had any space experience. Finally, but not least, from AURA's point of view, I had learned how to work with NASA.

Hubble in 1981

The *Hubble Space Telescope* is a joint European Space Agency (ESA)-NASA mission started in the mid-1970s and originally planned for flight in the mid-1980s. It was launched into orbit by the Shuttle in 1990 and can be serviced by Shuttle.

It consists of a 2.5-meter telescope capable of achieving angular resolutions of 60 milliarc seconds. Although the telescope has a relatively small aperture, its fine resolution permits the achievement of high sensitivity in the UV, optical, and near-infrared regions of the spectrum.

Hubble's resolution has given us a qualitatively new view of complex celestial phenomena from star formation to AGNs. *Hubble*'s sensitivity has permitted us to probe deep into the universe. We find that structures were formed at much earlier epochs than we originally believed. *Hubble* observations were instrumental in the discovery of negative energy. Its impact on astronomy as a whole has been profound.

Here I wish to discuss only another important contribution of *Hubble* to the methodology for astronomy, which stems from the efforts of the STScI.

In 1981 *Hubble* telescope construction had been under way for four years. The spacecraft and optics were contracted by NASA to Lockheed and Perkins-Elmer in 1977 and the instrument complement was being built by Principal Investigators at different research institutions. Apart from the overall MSFC chain of management, a scientific working group including the instruments PI's and other astronomers and led by the MSFC project scientist Bob O'Dell had the responsibility to advise the program on scientific issues including operations.

STScI was started in 1981, deliberately late so that it would not interfere with the decisions made by NASA and this group. Its coming into being was bound to generate some tension as STScI felt itself to represent the astronomical community and so did the working group.

I will not discuss the many controversies that occurred between STScI and NASA, as well as STScI and astronomers, both in and out of the *Hubble* program. With NASA most of the difficulty had to do with funding and some issues of responsibility. The NAS Horning Committee Report foresaw a greater role for STScI than NASA was ready to relinquish, although a strong cooperation was finally established prior to launch. Joe Rothenberg, the GSFC Director, was particularly helpful in forging this partnership. With the astronomers involved in the *Hubble* program, the main issue was the different intellectual approach that STScI was bringing to the program.

The NASA science working group had privileged, although limited, access to the *Hubble* data and a lot of expertise with their own instruments and their major preoccupation was to be able to carry out the research they had planned. This had not encouraged a unified view of *Hubble* as a National Observatory to be operated on behalf of the entire scientific community. To this end it was essential to provide to all general observers high-quality calibrated data with little delay, independently from the special expertise of the PI's. With the help of Don Hall, the first deputy director, I was able, to assemble an extremely competent scientific and technical staff to achieve that goal institutionally, technically, and operationally, thus also establishing a precedent for VLT and ALMA.

The first challenge we found was the problem of pointing *Hubble*. The fine guidance system relies on gyros and optical sensors for fine pointing. The sensors require several guide stars in the rather small focal plane aperture provided by *Hubble*. To be sure to have sufficient stars anywhere in the sky one had to use stars of fifteenth magnitude. No all sky catalogue existed for such faint objects. Under the leadership of the late Barry Lasker, we decided that the approach of preparing each day the necessary stars by scanning the survey plates on the fly, which had been recommended by the working group, was wholly unfeasible and that we would never obtain the necessary accuracy of 0.5 arc sec. STScI therefore embarked on a major effort to construct an all-sky catalogue of 20 million stars to fifteenth magnitude, which has become a scientific as well as a technical resource to all of astronomy.

Rodger Doxsey of STScI reviewed the full operational software system that NASA had entrusted to TRW. It became clear the astronomers had not been involved in the development. For instance, although two instruments would be in focus in the Richey-Cretien focus simultaneously, there was no provision to transmit the data simultaneously. When Bob Brown, a planetary astronomer at STScI, reviewed the pointing system, he discovered that it could not track planets because the required hand-off protocols between guide stars had not been developed. In all, some 700 defects, some major and some minor, were reported. It ended up that STScI took the lead role in the operational software development of *Hubble* under the leadership of Ethan Schreier and Rodger Doxsey.

The biggest controversy with the community came, however, in the matter of data reduction. From the Institute's point of view the data rate from and to *Hubble* was so high that nothing but automated systems would do, both to command the observatory functions and to receive and reduce the data. We conceived of *Hubble* as an instrument that could be described by a physical model and a set of parameters. The parameters would first be calibrated on the ground and verified in orbit. The data processing would use the model and the parameters value to remove the instrument signature and provide physical data such as fluxes, spectra, etc., ready for further analysis and archiving. Equally important, a capable data analysis system would be developed and freely distributed to the community.

When we first proposed this view we were considered by the working group as visionaries who were setting themselves an impossible technical goal. They recommended to the project that we not be funded for this endeavor. We were completely vindicated when the software system of the STScI, developed under the overall direction of Ethan Schreier, was fully successful from the first day after launch. Calibration of complex instruments such as *Hubble* requires a detailed knowledge and control of the status of all components of the observatory and required STScI to institute an observing program for calibrations and configuration management of the Observatory. The development of an archive for all *Hubble* data shared with our ESA and Canadian colleagues became another major and extremely useful technical and scientific enterprise, which has permitted reuse of *Hubble* data by scientists from all over the world. The methodology has been widely adopted by other observatories.

It should be emphasized that these advances required the development and use of technology at the limit of the then available capability, for instance, 12-inch recording disks, much before the current easy-to-use CDs. The use of expert systems for HST scheduling, pioneered by Mark Johnston, then at STScI, was also well in advance of its time. No wonder that Lyman Spitzer, who chaired the Space Telescope Institute Council, continuously stressed at every review (for years) his concerns about our ability to successfully implement our goals.

While worrying about the technical aspects of *Hubble* we worked equally hard to establish the appropriate intellectual conditions for full use of the *Hubble*. We relied on a cadre of senior advisors from the astronomical community, including Martin Rees, Maarten Schmidt, Jerry Ostriker, Malcolm Longair, Chris McKee, Dick McCray, and many others, to advise us on these activities. We sought their advice on the peer review system, the key programs, the balance between large and small programs, and similar issues. We entered into fruitful collaborations with many universities and in particular the Johns Hopkins Physics and Astronomy Department. Our interaction with the department was of great mutual benefit. The support of Steve Muller, the President of Johns Hopkins University during the start-up period, was essential.

We also introduced the *Hubble* postdoctoral fellowship program and a system of grants to observers in order to acquire computational and scientific help in the analysis of their data. We administered the observing time allocation jointly with and under the open scrutiny of the scientific community.

We established a vigorous educational and outreach program that has brought scientific results to the entire astronomical community, to the American public, and to the world. We reached out to the schools, to the teachers, and to the amateur astronomy community. The amateur participation in the use of *Hubble* produced scientific and educational benefits for a tiny fraction of the observing time.

The scientific staff we were able to recruit was outstanding. It included, in addition to the scientists already mentioned, Neta Bahcall, Len Cowie, Garth Illingworth, Duccio Macchetto, George Miley, Colin Norman, and Pete Stockman, among others.

STScI was an institution in which the staff was encouraged to give a high standard of service to the community while producing excellent science themselves. In the first ten years of life it became one of the most productive scientific institutions in the world.

One of the finest moments of STScI came with the discovery of the out-of-focus condition of the telescope. Chris Burrows, at the Institute, discovered and analyzed the problem. We convened at STScI a group of outstanding optical experts from the entire world to consider any and all possible fixes. The group chaired by Bob Brown and Holland Ford of the Institute recommended the fixes that restored *Hubble* vision. Jim Crocker, also at the Institute, invented the technical solution of corrective optics space telescope axial replacement, i.e., COSTAR, which was used in the recovery mission. It was glorious of NASA to tempt fate by risking a manned servicing mission. Its success was a triumph for science and for the agency. The very high degree of cooperation between the NASA operational side and the scientific community through STScI could have become a model for future endeavors. This is not quite happening in the case of the New Generation Space Telescope development, although STScI will be responsible for science operations.

Intellectual Development in Methodology at STScI

The idea of physical modeling of the instrumentation in order to calibrate the data in an automated mode inspired further work. Michael Rosa at ESO introduced what he called forward modeling of instruments. By this he meant that because we build instruments on the basis of known physical principles, one could theoretically predict their behavior rather than deal with them as black boxes as we had done in the *Hubble* calibration. When applied to *Hubble*, this technique was proven in later years to be quite effective in removing ambiguities from the calibration of the faint object spectrograph of GSFC. This same technique was used to predict the behavior of the infrared spectrometer and array camera (ISAAC) instrument on VLT, an imaging infrared detector of very high degree of sophistication. The intellectual rigor required in this modeling makes possible the implementation of powerful on-line automated calibration systems with speed and precision quite superior to what each observer could achieve.

A further extension of this line of thinking is that experiments could be designed by modeling both the hardware and software as part of the initial design. I myself, together with Richard Burg and Chris Burrows, used this approach in designing in the 1980s what I believe was one of the best experiments I ever conceived. The purpose was to scan the sky and to detect distant clusters of galaxies through their X-ray emission. The idea was that it would be possible to equal or exceed the sensitivity of *Chandra* with an X-ray telescope of one tenth the area (and cost). This could be achieved by dedicating an entire mission of a small satellite to this purpose and by designing a telescope that would have a >16-fold increase of the field of view with respect to *Chandra*. Chris Burrows was able to analytically solve the problem of finding the telescope optics that maximized the angular resolution weighted over the field of view. As perhaps we should have known, the result was the equivalent of a Richey-Cretien solution for X-ray mirrors. Taking into account the data analysis system, one could further improve the ability of the system to distinguish between point sources (AGNs) and extended sources (clusters of galaxies).

Unfortunately, all the agencies [NASA, ESA, and Agenzia Spaziale Italia (ASI)] to which this proposal was made by our group and others later on rejected the proposal mostly on the basis of wrong theoretical predictions, then in fashion, regarding the time of formations of clusters, but the brief description given here is intended to further illustrate the progress that can be made in the design of an experiment by a *gedanken* end-to-end approach.

The ability to archive large amounts of calibrated data in digital format developed at *Hubble* was also a significant development. Following the example of elementary particle physics research, large data samples can be searched for rare events. The archives now being assembled in optical, infrared, X-rays, and radio astronomy are so large that this type of research requires new tools for in situ archival research.

What I described above are examples of how I personally came to experience the profound change that ever more capable computers and software systems have brought to astronomical research in the last decades. Similar thoughts must have been common to other astronomers. I was fortunate to be in the position to do something about it. I was reappointed Director of STScI for three consecutive terms. For personal reasons I resigned at the beginning of the third term to become Director of ESO.

ESO AND THE VLT

I enjoyed very much being the Director General of ESO during the development and construction of the VLT and the achievement of first light in 1998. At ESO I was in charge of an organization that was responsible for the construction, as well as the operation, of what was to become the largest optical ground-based telescope complex in the world. Thus all of the technology and management experience I had accumulated in building large systems, as well as the intellectual heritage from *Hubble* in the scientific utilization of a major optical telescope, could be put to use to benefit VLT.

The VLT design rested on years of work by scientists and engineers at ESO well before my arrival there. Lo Woltjer deserves a great deal of credit for having convinced the European community of the desirability of a very large telescope and having initiated the project. The success of New Technology Telescope (NTT) in the 1980s prepared ESO for the much greater challenge of building VLT. A great deal of preparatory work by ESO engineers and scientists had occurred during Harry van der Laan's directorship. It is my opinion, however, that VLT could not have been built successfully in time and on cost without the technical and scientific competence, and the dedication and leadership capabilities of Massimo Tarenghi.

I believe I was able to contribute to the project by changing ESO in a way best suited to support the VLT project. This included modernizing ESO's management approach to make it suitable for large projects, reforming personnel appraisal practices to more clearly reward performance, increasing communications between scientists and engineers both vertically and horizontally throughout ESO, reforming tenure appointment practices for scientific staff, and introducing a new sense of urgency and discipline in the completion of programs. Until the beginning of 1993 the VLT had delayed its planned completion by one year per year. Thanks to stronger management attention this was reduced to about a six- to nine-month delay in the next six years.

Technically I became involved, together with Jim Crocker (who had joined me from STScI), in choosing the material for the secondary mirror (beryllium), which had become a stumbling block in the project. Jim Crocker's contributions in operations planning, engineering and management of VLT and ESO in general were quite substantial. I believe I originated the idea of having two parallel competing contracts for the structure supporting the primary mirror with a view to facilitate maintenance and accessibility in the future.

Scientifically I tried hard to bring a drive for excellence in a world context and a broader view to the somewhat restricted ESO atmosphere and style. I found that much effort was expended at La Silla, operating marginally productive telescopes, and not enough effort was placed in supporting those that could really produce science. With the help of the Science and Technology Committee of ESO, chaired by J. Anderson, we succeeded in closing half of the telescopes and placing great emphasis in bringing the remainder up to world standards. At the suggestion of Joe Schwarz, Jason Spiromillo undertook to completely modernize the NTT control system using VLT software, a fundamental contribution to the VLT project. Jorge Melnick took responsibility for a complete upgrade of the 3.6-meter telescope. We found that, when properly focused, the telescope would yield angular resolution at the limit of seeing. D. Baade led an effort to provide the 2.2-meter Max Planck Garching (MPG)/ESO telescope with a wide-field camera. Alan Moorwood built a prototype of the VLT infrared instrument ISAAC, called Son of ISAAC, i.e., SOFI, which was used extremely effectively at the NTT. Thus ESO participated as a whole in the VLT project. The very far-reaching decision by Harry Van der Laan (my predecessor at ESO) to have most of the instrumentation built by the community (10 out of initial 12) was implemented by ESO with a careful monitoring of progress and by imposing software and configuration standards that greatly improved the chance of successful integrated operations.

Still, I think that the most decisive intellectual contribution I was able to give to ESO was to stress the need to prepare to receive the data and to provide for automated operations and data reduction software. Basically, the full philosophy of science operations of *Hubble* was transplanted to ESO, where it had already been adopted only by the Space Telescope European Coordinating Facility (ECF) of ESA. (The ECF had been placed at ESO thanks to the efforts of Lo Woltjer). Piero Benvenuti, the head of ECF, became the temporary Head of Science Software for all of ESO, until Peter Quinn, who had also spent some time at STScI, came to head that group. In every respect the VLT science operations benefited from the *Hubble* experience, notwithstanding the early skepticism by some of the staff. Today the VLT effectiveness of operations has become the mark by which other ground-based observatories measure their success.

It was a great pleasure, while worried about VLT and budgets, to try to emphasize the role of the Education and Outreach Programs at ESO (again following the STScI model) and sponsor joint activities with astronomy teachers in Europe and with the European Union.

A few particular points of view I tried to introduce were more difficult in a European than in an American setting. I have always felt that when doing a scientific project one should be as effective as a private corporation. The money saved could then be employed to do more science. Massimo Tarenghi, by not using the VLT contingency, was able to restart the Very Large Telescope Interferometer (VLTI) program. Although this is not a popular subject among scientists, I believe there is a certain beauty in maximizing science within the available means and in trying for elegant simple solutions that are less expensive and esthetically satisfying.

At ESO I was able also to reflect on the role of a large international institution vis-à-vis national institutions. In March of 1988 I wrote a paper, "The Role of ESO in European Astronomy," which summarized my views. [Having taken a leadership role on behalf of Europe in ALMA and the OverWhelmingly Large (OWL) telescope project, ESO is certainly fulfilling that vision.]

In this respect it is interesting to note that the difference in funding approaches between the United States and Europe in the different disciplines of astronomy has led to different capabilities and opportunities in the different fields. In optical astronomy the United States has relied mainly on private institutions, whereas Europe is relying on ESO to provide the bulk of its observing capabilities. This places ESO in a better position to lead OWL.

On the other hand the reverse is true in radio astronomy and the National Radio Astronomy Observatory (NRAO) has been successful in building the best radio telescope facilities in the world. ALMA is now testing a new paradigm of international cooperation, about which more will follow.

A description of my activities during this period would be incomplete without some mention of the long negotiations with the Government of Chile to secure access to Cerro Paranal. The happy conclusion and the signing of a new Chile-ESO Treaty was brought about by the wonderful help of Sen. Arturo Alessandri, the Chilean Foreign Minister, Don Miguel Insulza and the Chilean President, Don Eduardo Frei Ruiz-Tagle.

AUI AND ALMA

At the end of my first five-year term at ESO, in January 1998, I was reappointed for only two more years owing to my advanced age (67 at the time). Prior to the end of my term I received an offer from Associated Universities, Incorporated (AUI), in the United States to become President of the Corporation. AUI is the management organization that built and operated Brookhaven National Laboratories for 50 years and has operated NRAO since its beginning.

My contacts with NRAO had first started when a group of European radio astronomers met with me at ESO to discuss a project, the Large Southern Millimeter Array, in which they were interested. ESO had previously collaborated with Onsala University of Sweden in operating the SEST telescope at La Silla. The new project would be much more ambitious than any yet contemplated in radio astronomy with tenfold increases in collecting area and angular resolution in the millimeter range. It seemed difficult for the individual groups in Europe to seek funding or to create *ex novo* an organization capable to carry the project through. However, ESO in collaboration with all other institutions could provide much needed managerial experience and strength in the negotiations with possible U.S. partners.

In the United States there had been plans for a similar project, the Millimeter Array (MMA), of somewhat smaller scope, by NRAO. International participation to share the very high cost of the facility was urged by the U.S. Congress. Thus in June 1997 I found myself as Director General of ESO signing a statement of intent for a joint program with the then Director of NRAO, Paul Vanden Bout. It is not obvious in retrospect that either of us had the authority to sign such a statement, but be that as it may, it started a close technical and scientific collaboration between U.S. and European astronomers on this project.

I had found ALMA to be potentially one of the most exciting of the new observational capabilities being planned. I thus was very interested in the AUI offer and became President of AUI in July 1999. Apart from my duties as President of AUI in the governance of NRAO, I become personally involved in the development of this project as a member of the ALMA Board.

The original plans in Europe were for a millimeter array of 10^4 square meters and thus considerably larger than the six-element array of Institut de Radio Astronomie Millimétrique (IRAM). The main interest appeared to be in extragalactic astronomy at very great redshift. Given this interest surveys were initiated for suitable locations at relatively low altitudes (2800 m). On the other hand, in the United States and Japan the interest was for higher frequencies in order to continue the study of molecules in interstellar space and objects of larger angular extent such as star forming regions and nebulae, in addition to the study of extragalactic and extremely distant objects. Thus the U.S. and Japanese groups started their surveys at much higher altitudes in Chile on Chajnantor (5000 meters), where absorption by the atmosphere of submillimeter waves was minimized.

The U.S. group was further advanced in its technical studies and in its efforts to obtain community, NASA, and Congressional support. A very strong recommendation was contained in the Decadal Survey in astronomy of the NAS.

ESO, on the other hand, was still completing the VLT and was already thinking of OWL. It had within its walls only a modest experience in radio astronomy and there was some uncertainty in the community about whether ESO should expand its horizon to a new discipline. Ultimately the compelling scientific case, the unlikely possibility of creating a new ESO for radio astronomy in a reasonable time, and the prospect of a fruitful international cooperation tilted the balance. I should add that, in my opinion, the retention of management, engineering, operational and contracting skills at ESO in the hiatus between VLT and OWL is a positive gain to optical astronomy as well.

Japanese scientists had collaborated with their counterpart U.S. astronomers in the exploration of the Chajnantor site since the early 1990s and a number of workshops had been held to discuss the results. There was uncertainty in Japan, however, on whether the scientific interest in Japan and the United States could be best combined through a collaboration or a parallel effort. After the signing of the NRAO-ESO Memorandum of Understanding in 1997, some discussion took place about a possible three-way partnership and a "Resolution concerning the expansion of the ALMA partnership to merge the ATACAMA large millimeter array and large millimeter and submillimeter array projects" was signed in 1999 by myself as the European representative, Dr. Martha Haynes for the United States, and Dr. Masto Ishiguro for Japan. But funding for the Japanese contribution was delayed. Thus it was a two-way partnership that initiated the construction of ALMA in December 2002 by the signing of an agreement between North America and Europe. The funding having now materialized for Japan as well, it is hoped that a three-way collaboration will soon be in place. This three-way partnership will make it possible to achieve all of the scientific aims of ALMA: 80 antennas distributed in a compact and a large array will be placed on Chajnantor at 5000 meters; ALMA will cover all atmospheric windows between 350 μ and 10 mm. Angular resolutions of 10 milliarc seconds will result from the largest separation of the array at 10 km. When completed it will be the premier array in the world in the millimeter and submillimeter domain and one of the most powerful telescopes in the entire astronomical arsenal.

We have been helped greatly by the Chilean government in securing access to the land and in providing us with all kinds of exemptions and immunities. Chile is a full partner in the ALMA enterprise as the host nation and through the scientific and technical collaboration of the Chilean Astronomical community. Thus, the ALMA partnership is truly worldwide including Asia, Europe, and both North and South America. The cost of approximately 1 billion US$ (in 2002) will be shared almost equally between North America, Europe, and Japan.

The management of ALMA is a new experiment in international cooperation. Rather than joining financial resources and creating a single new entity to manage the program, the ALMA partners rely on the expertise at already existing institutions to carry out a coordinated program. The purpose of this approach is to insure that the existing observatories, ESO, NRAO, and the National Astronomical Observatory of Japan (NAOJ), fully participate in the program, learn new technology and methodology in the process, and end up strengthened rather than weakened in the process. This is what happened at ESO during the construction of VLT, leaving ESO in a much better position to participate in ALMA and OWL. The ALMA approach is particularly attractive for international joint projects because it benefits all the participants rather than weakens home institutions to create a new international structure. The coordination and management of such a large project is a daunting task. We have been lucky that Massimo Tarenghi has been willing to take it on.

CHANDRA

While worrying about ALMA, I was allowed to continue my scientific research using *Chandra*. I had participated as an interdisciplinary scientist to the *Chandra* working group since its onset. In return I had been given access to a privileged amount of time, about 500,000 seconds. Early on I had decided to use this time in the same manner as I had used 1 million seconds on *Einstein* and on the *Rosat* collaboration, that is, to search for the sources of the X-ray extragalactic background. Piero Rosati and I selected a particular field in the southern hemisphere where the column density of hydrogen in our galaxy was lowest. Using the VLT-FORS (focal reducer/low dispersion spectrograph), we prepared an optical survey of the region to magnitude 25 in R. After the analysis of the X-ray image in the field (called *Chandra* Deep Field South) we obtained identifications and spectra of sources with VLT and high-resolution images with *Hubble*. In the X-ray range the images of the weakest sources we were detecting in a one-million-second exposure consisted of ten photons, accumulated at the rate of a photon per day. Since the discovery of Sco X-1 the sensitivity of X-ray observation had increased by more than 1 billion times!

It gave me particular joy to use in my research three of the best telescopes in the world that I had helped bring about. Our group was able to use these observations not only to resolve the X-ray BKG into individual sources but also to study the properties of distinct AGNs of Type I and II, galaxies, and galaxy clusters. All together some 50 articles were produced (mostly because of the work of the younger people) in the last five years.

It is with great confidence that I see the future of X-ray astronomy and its continued contribution to the study of the Universe.

INSTITUTIONAL SETTINGS

I have been extremely fortunate in having been associated with so many wonderful developments in astronomy and in being given the opportunity to work in so many different environments. As I moved from Indiana to Princeton, to ASE and to

Harvard, I came to realize more and more the importance of process in doing research.

By process I mean two things: first, the learning and intellectual growth that should be pursued while carrying out the most demanding projects. I used to discuss this point with the staff at STScI. Because we had no responsibility for the overall program or for hardware construction we had no assurance that, when launched, *Hubble* would actually work. What we could do was to work hard and learn all we could so that even if exposed to a failure we would have grown professionally and personally. To produce this result we felt that whatever we did had to be done as well as we could, in all aspects of the institute activities. The guide star catalogue, the library, the archive, the operation system, the pipeline data reduction, the academic and community initiatives, the outreach and educational programs, the peer review system, and the management and the personnel activities, all had to strive for excellence. The result was a shared vision with the staff, superb service to the community and an outstanding success, both scientific and institutional.

Further, under this aspect of process I would include the concern for the institutional setting in which the scientific activities are taking place. On the large national scale I believe greater attention should be given to the manner in which different funding agencies operate and what is the result of different styles on the research productivity of the community they are intended to support. The National Academy of Sciences has little influence on these matters.

At a lower institutional level our competitive atmosphere in the United States encourages the start of new institutions and initiatives rather than the continuation, improvement, and re-engineering of the existing ones. Thus the learning process occurs not in a cumulative way, but by jumps in which an existing institution is by-passed in favor of a new one to carry out a new program.

The fact that, notwithstanding its spectacular success with *Hubble*, the STScI responsibilities have not increased but if anything have been reduced in the Next Generation Space Telescope (NGST) program is a case in point. I tried in my tenure at ESO and AUI to strengthen the institutions while carrying out programs of greater complexity and challenge than any they had previously undertaken. At ESO the decision that all of the observatory should be involved and share in the challenge of meeting the new standard of excellence imposed by VLT resulted in a profound change at La Silla as well as Paranal. The scientific publication rate increased rather than decreased during the construction of VLT.

The same attention and regard for strengthening rather weakening the participating institutions is being used in the execution of ALMA. I am convinced that transformation and learning, which are essential for the successful execution of ALMA by NRAO, will result in improved collaboration with and open new pathways of research to the astronomical community in the United States and Canada. I know that the Director of NRAO, Fred Lo, shares this hope. I expect a similar effect on ESO and NAOJ. This is particularly important in large scale international collaborations where the partners wish to join together to achieve larger scale projects than they can do alone while retaining the vitality and the scientific and technical competence of the national institutions.

I also meant to discuss process under a different aspect, that is, in "following due process." Nobody is as sensitive as an intellectual (which natural scientists are) to the evils of authoritarian rule. Painful as it may be, a meritocracy of skills and ideas is the regime that best sustains the creative enterprise. At STScI we attempted, as a group, to introduce participative management practices, and fair and open selection and promotion procedures for the scientific staff. We willingly sought the advice, expertise, and participation of the science community in designing and executing our program. We welcomed the Space Telescope Institute Council and the Visiting Committee advice as a useful look from an outside perspective of what we were doing. We subjected ourselves to audits of operational plans and engineering solutions. We tried to foster a climate of openness and mutual learning and teaching. I attempted to institute similar reforms at ESO and at NRAO.

BIG SCIENCE AND SMALL SCIENCE

In the preceding pages, I have described a way to carry out scientific research that is quite competitive in an international setting and that has resulted in big science enterprises in astronomy. For myself, I was driven by passion, or compulsion, to work in this manner. I could imagine a much more leisurely pace in the progress in X-ray astronomy. After all, it took more than 350 years to go from Galileo's telescope to Keck and VLT. We did not need to progress in X-ray astronomy by an even greater amount in 40 years. At a slower pace we could perhaps have had a greater opportunity of integrating the new discoveries in the general context of astronomy in general. Unfortunately, this could be said of technological and scientific progress as a whole, not only in basic but in applied research. We seem driven to a faster and faster tempo of discovery without a moment to really reflect on the social consequences of what we are bringing to mankind. What is clear is that the psychological and civil development of humans has not kept in step with our technical might and that the political institutions seem unable to cope with the downside of technology.

The only consolation I have had from this questioning has been the interest and joy that I have seen on the face of ordinary people when exposed to the beauty and mystery of the *Hubble* and *Chandra* pictures of the Universe in which we live.

POSTSCRIPT BY GEOFFREY BURBIDGE

Never before in the 28 previous introductory articles have I ever felt it necessary to add anything to what the author has written. But in this case I have decided to do so. This is because Riccardo Giacconi has only in passing mentioned his greatest honor. In 2002 he shared the Nobel Prize for Physics together with Ray Davis and

Masatoshi Koshiba. The award was given for the detection of cosmic neutrinos (Davis and Koshiba) and for the discovery of cosmic X-ray sources (Giacconi). Riccardo Giacconi, together with Herbert Friedman and Bruno Rossi, literally founded X-ray astronomy. I quote from the announcement concerning his award from the Nobel Foundation on October 8, 2002:

> In order to investigate cosmic X-ray radiation, which is absorbed in Earth's atmosphere, it is necessary to place instruments in space. Riccardo Giacconi has constructed such instruments. He detected for the first time a source of X-rays outside our solar system and he was the first to prove that the universe contains background radiation of X-ray light. He also detected sources of X-rays that most astronomers now consider to contain black holes. Giacconi constructed the first X-ray telescopes, which have provided us with completely new—and sharp—images of the universe. His contributions laid the foundations of *X-ray astronomy.*

In this article Riccardo has given us a marvelous account of his career and this work.

The *Annual Review of Astronomy and Astrophysics* is online at http://astro.annualreviews.org

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