

# **Reflections on the Rossi X-ray Timing Explorer**

**A Personal Account**

**by**

**Hale Bradt**

An expanded version of the banquet talk at the Symposium,  
“Exploring Physics with Neutron Stars”

in honor of the 65th birthday of Fred K. Lamb

Tucson, Arizona, 18 Nov. 2010

## **Introduction**

When I retired in 2001 I had to reduce my office footprint because I was to share the office with Gordon Pettengill, another faculty retiree. Upon coming across files relating to the RXTE mission, it occurred to me that the 20 years we spent in bringing this mission to fruition (i.e. launch) was quite interesting and that the documents and correspondence in my files might well be donated to the MIT Archives. I thus left this pile, about 12 inches high, on top of one of my file cabinets with the intention of organizing it a bit and providing a brief contextual document to accompany them.

Some months ago, Dimitrios Psaltis called and asked me to give a talk on the selling of the RXTE mission, and I said, “Why me and why that?” “Well,” he said, “this Symposium is to honor Fred Lamb and he was instrumental in helping bring about the mission. I recall that, at a conference in Rome, over dinner or lunch, you told me all about the adventures of getting RXTE into orbit and a lot of it was very funny, and I thought it would be appropriate for this symposium.” So, I agreed, and realized this was the goad I needed to address that pile of documents that had been untouched for 9 years. So, procrastinating as usual, I finally got to them three days or so ago. I had never kept a proper notebook of the events leading to the RXTE launch, but these documents brought back memories, and pinned down some of the dates and events. There are still large gaps and some uncertain dates, but the broad outline is pretty well established.

The following are my recollections bolstered with dates and facts from the correspondence and documents and conversations I have had here with Rick Rothschild and Fred Lamb. I hope to have similar conversations with Jean Swank and Bill Mayer and possibly others. Bringing about a mission such as RXTE is a complicated business and in many respects is a struggle. Bureaucratic, financial, and technical problems that at times seem overwhelming must be solved, and, in the process, the varied interests of individuals and organizations come to the fore. We at MIT were deeply involved as were the managers, engineers, and scientists at NASA’s Goddard Space Flight Center (GSFC) in Greenbelt, MD, the University of California at San Diego (UCSD), and NASA HQ.

Of course, when there were difficult issues where perspectives differed, we at MIT usually considered ourselves the “good guys” and the others, not “evil:” but possibly a bit misguided in their understanding of the issues. The view on the other side could well have been similar, but, of course, it is also possible that we at MIT were more arrogant

than most other people, and that others took a more balanced view. At times the rhetoric and/or emotions would become rather warm, but, all in all, the people involved all wanted RXTE to fly and were working toward that goal, and our respect and friendship for each other never was compromised. This latter statement is not the platitude it seems; many of us truly have deep and long-lasting friendships that persist today.

In recounting these events, I am trying to see and present the other side’s view a little more clearly than I did at the time. Nevertheless, my MIT bias, if not paranoia, is sure to persist in these recollections. This is very much my personal story; it is not an objective history. Others surely would tell it differently. Please keep that in mind and make proper allowances as you read this.

Earlier versions of this document have been read, in part and in some cases totally, by Alan Levine, Bill Mayer, Fred Lamb, Jean Swank, and Charlie Pellerin. Their comments have been helpful in clarifying many aspects of this story. Fred generously dug into his files and clarified the dates of some of the various critical meetings where XTE was up for discussion. Any remaining errors in this document belong on my doorstep.

***What is this RXTE?***

It is the Rossi X-ray Timing Explorer, but for a long time before launch it was known simply as XTE (X-ray Timing Explorer). It was one of NASA’s Explorer series of satellites, but it was not small; it’s cost including two years of in orbit operations, was close to \$200 million in 1989 dollars. This is a lot of money, but not much as space missions go; the Hubble and Chandra observatories each cost ten times that.

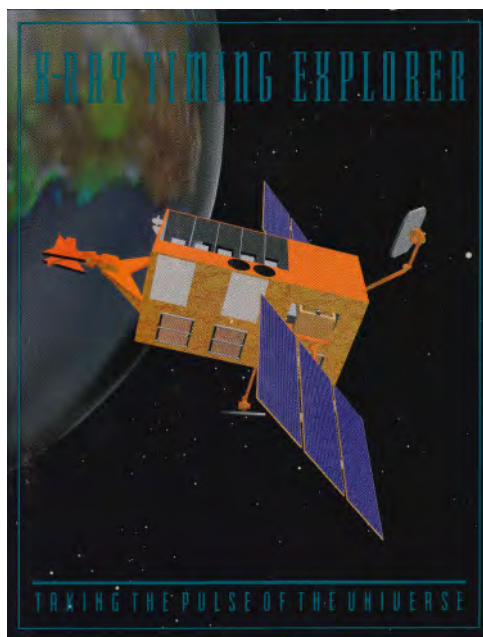


Figure 1. Artistic rendering of RXTE in orbit on the cover of the brochure we distributed at the January 1992 meeting of the American Astronomical Society.

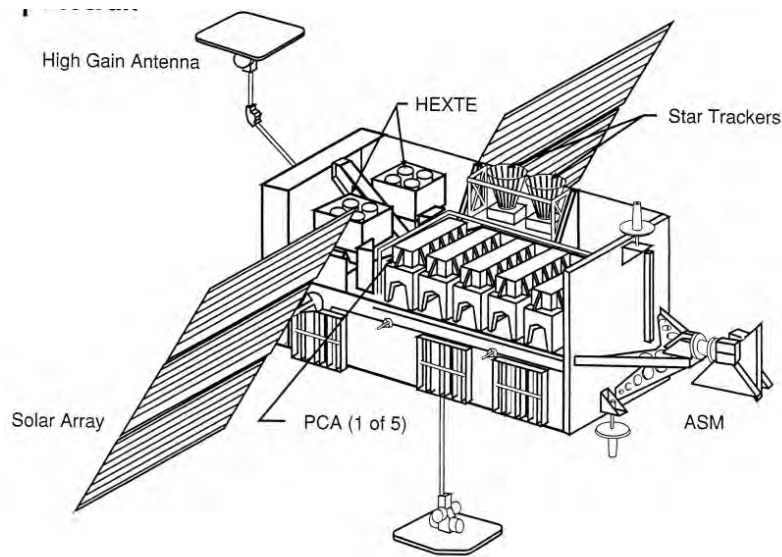


Figure 2: Sketch showing the RXTE final in-orbit configuration

RXTE (Figs. 1 and 2) is an x-ray observatory in space designed to study mostly compact objects, primarily neutron stars and black holes, which are often strong emitters of x rays. Such objects have strong gravitational fields, which are often associated with very hot gases, gases so hot that they emit x rays copiously. The gravity is so strong that the x-ray emitting matter near neutron stars or stellar black holes would move distances comparable to the stellar “size” in only a few milliseconds, and one might thus expect variations in the x-ray intensity on such time scales.

The idea of RXTE was to fly a large area of detectors (proportional counters) so we could collect as many x-rays as possible in a short time, at least a few in a millisecond for the brighter sources. The instrument that made such measurements was the “Proportional Counter Array” (PCA) designed and built by GSFC. Stephen Holt was the Principal Investigator, but well before flight, when he became chief of the science directorate at GSFC, Jean Swank replaced him.

This instrument did not take photos of the x-ray sky such as are done with reflecting x-ray telescopes on the Einstein and Chandra missions. In contrast, RXTE detected and counted x rays in short time intervals while also providing rough spectral information. It’s angular resolution was a rather crude  $1^\circ$ . The focusing missions were more sensitive, had greater angular resolution (down to  $\sim 1''$ ), and could detect fainter sources. However, they could not accumulate large numbers of x rays in short time periods because of their limited effective areas, nor could they reach to the higher-energy x rays that are important for modeling the emission processes. Thus RXTE provided no pretty pictures of the sky; sorry about that. However, we did produce from ASM data jazzy virtual views of the x-ray sky showing, movie style, the changing brightness of the individual sources over the first few years of the mission.

The x-ray sky is highly variable with sources changing intensity and actually disappearing altogether and then possibly reappearing later. It was important that we use the large area instrument of RXTE to study these sources at interesting times, such as when they were bright, so the spacecraft needed eyes to watch the entire sky to keep track of the brightness and spectral states of the brightest 100 or so sources in the sky. That instrument was the All Sky Monitor (ASM) designed and built at MIT. I was the PI

initially; after my retirement, Al Levine took on that role. The other scientists primarily involved with this instrument were Ronald Remillard and Edward Morgan.

The importance of an All Sky Monitor had become evident with the great success of the ASM on the British Ariel V (launched 1974). The experiment consisted of a pair of pinhole cameras built at GSFC. Steve Holt was the PI and his student Lou Kaluziński produced paper after paper of interesting results. Before I met Lou, I thought maybe that Steve had constructed a paper-writing machine he dubbed “Kaluziński.” The slat collimators of MIT’s SAS-3 (launched 1975), with long fields of view, also detected changes in source brightness and new sources. It too illustrated the value of a sky monitor.

The PCA instrument detects x rays primarily in the energy range 2–20 keV but extending to about 60 keV. A third instrument carried on RXTE extended this range up to 200 keV; it was the High Energy X-ray Timing Experiment (HEXTE) designed and built by UCSD.

The RXTE was launched on 30 December 1995 from Cape Canaveral and has been producing data ever since, though the funding by NASA is coming to an end. It will probably re-enter the atmosphere during the current solar maximum, which leads to an expanded atmosphere and greater atmospheric drag. It has produced a wealth of important rapid temporal results many of which have been the subjects of talks at this workshop.

This account is primarily chronological and extends over 20 years. To give reference points, I divide it into three parts:

I: The Proposal Phase (1974–1980). This ran from the first proposals submitted with the concept of large-area pointed observations (rather than scanning as in most earlier missions) to the submission of the final proposals, some of which were selected for flight. This period involved selling the mission concept to our peers and to NASA and its final acceptance by NASA.

II: Selection and Holding (1981–1989). This period included the selection of flight experiments, studies of spacecraft options, but no actual engineering design or instrument fabrication. RXTE was waiting for prior missions to be completed so that money could be allocated to it. To reach the implementation phase it was necessary to again justify the mission to our peers and NASA. After a decade, was RXTE still necessary and relevant? The mission had its detractors and their arguments had to be countered. Also the mission had to be de-scoped to bring its cost within NASA management’s limits.

III: Implementation and liftoff (1989–1995). The instrument teams and the spacecraft builders at GSFC had real money, so design, fabrication, and testing could proceed. Further descopeing was imposed by NASA. The spacecraft type and launch vehicle had to be resolved. Finally, all parts had to come together on schedule, and all groups had to resolve technical and financial issues. When finally defined, the program came together on cost and on schedule.

IV: Liftoff (Dec. 30, 1995): There were serious instrumental problems immediately after launch that were fortunately overcome, or resolved themselves!

V. Science Highlights (1996–2012): RXTE’s greatest achievement was finding the rapid time variability it had sought. This opened a new window into neutron-star and black-hole systems. The ASM provided a unique view of the long-term variability of the x-ray sky and, as planned, served as the eyes and ears of the powerful PCA instrument.

VI. Final Reflections

Appendix: Letters from George Clark re the RXTE mission.

## Phase I: Proposals (1974–1980)

### *LAXTE on a SCOUT*

In 1974, we at MIT with the Leicester group submitted a proposal to NASA entitled “Large Area X-ray Timing Experiment (LAXTE) on a Scout Vehicle” (Fig. 3). I was the Principal Investigator and Ken Pounds of Leicester was the co-PI. It was in response to an Announcement of Opportunity, the infamous (to x-ray astronomers with long memories) AO 6&7 as it (or they) were known. These proposals were for small “Explorer” missions that would follow the HEAO (High Energy Astronomy Observatory) series of three large satellites. Two of the HEAOs were for x-ray astronomy and were to be launched in 1977 (HEAO-1) and 1978 (HEAO-2, or Einstein). The planned lives of the HEAOs were only 1–2 years.

The argument for a large detection area and pointed observations was pretty obvious from the dramatic results of Uhuru (launched in 1970). Keep in mind that this was in 1974 before the launch of MIT’s SAS-3 (1975 launch), which carried out sustained pointed observations of celestial x-ray sources. There were also proposals for such missions from other institutions, notably, I believe, from GSFC and Smithsonian Astrophysical Observatory (SAO; in Cambridge, MA). It is notable that the MIT proposal did not include a sky monitor experiment. I do not know the details of the other proposals.

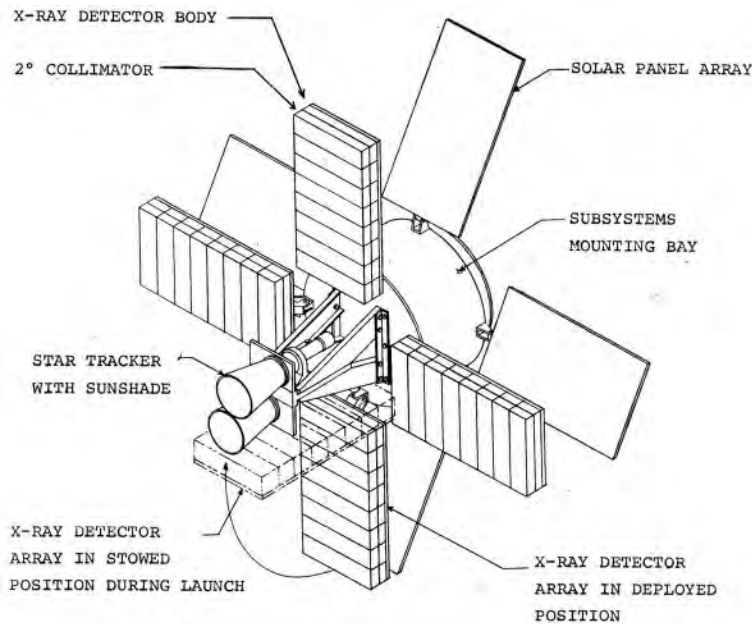


Figure 3. Large Area X-ray Timing Explorer from MIT proposal with University of Leicester 1974 in response to the 1974 Announcement of Opportunity, AO 6/7.

This AO had a significant downside for x-ray astronomers. We had long been the prima donnas of space astronomy because x rays can only be detected from space. Proposals to NASA generally generated only a few proposals from the very few institutions with scientists doing x-ray astronomy at the time: MIT, AS&E, NRL, GSFC, Columbia, and LLL, and not all would propose to a given opportunity. There was space

activity from other wavebands or fields that required observations from space, e.g. gamma-ray, ultraviolet, cosmic ray, plasma, etc., but it was relatively limited. Space opportunities tended to be directed to one field or another. Thus one could propose for a mission directed toward your field with a fair chance of it being accepted.

However, by the mid seventies, scientists working in other wave bands (microwave, infrared, optical, ultraviolet) began to wake up to the opportunities of space and to realize the advantages that could be gained from it, even if they could do observations fruitfully from the ground. For example, infrared astronomers were restricted to narrow frequency bands when doing astronomy through the atmosphere and optical astronomers had to deal with the blurring of images by atmospheric turbulence. Space did offer real advantages to these fields. The AO 6-7 from NASA allowed for experiments in all wavebands and, as a consequence, there was a plethora of proposals from many wavebands. The x-ray astronomers had real competition for a change. It was also distressing because the judging of the science merits from one waveband to another could be rather subjective. To some extent it was like choosing between apples and oranges. Which is better? This made selections potentially more political, and here the x-ray astronomers were at real disadvantage. “Hey, look at all the missions those x-ray astronomers have had!”

It took some time for the proposal selection process to play out. The immediate result of the first peer reviews was that NASA requested six different proposers of x and gamma ray space missions to get together for a study of whether a viable single mission might be created with elements from their proposals. Thus the six competitors were locked in a room together to see who among them would emerge alive. They were Doyle Evans (LASL), Bradt (MIT), Paul Gorenstein and Harvey Tananbaum (SAO), Stephen Holt (GSFC) and Seth Shulman (NRL).

### *ATREX*

In Dec. 1976, this group submitted to NASA the result of its study. We proposed a viable mission (Astrophysical Transient Explorer; ATREX) that encompassed both x-ray and gamma astronomy. The x-ray instruments were a narrow field of view, large area PCA and a sky monitor patterned after the one launched on Ariel-V in 1974 (Fig. 4). The gamma instruments were a wide-field sensitive burst detector (i.e., the BATSE later flown on Compton) and a slit system for determining burst positions. Its collection of four instruments could be viewed as a bit of a “Christmas Tree,” a derogatory term for missions that try to do too many things, rather than doing one of them well. However, that was not a fair description. It addressed two major problems: the nature of neutron and black hole systems and of gamma-ray bursts in fundamentally new and powerful ways. In retrospect, it was an important mission that could have made very important discoveries in unexplored domains.

Unfortunately, the selection committees and NASA did not share our view of its importance, and it was not selected for flight. Instead, missions that later became quite well known were selected: COBE (microwave), EUVE (extreme ultraviolet), and IRAS (infrared). These were all relatively small and inexpensive missions called “Explorers.” At the same time, more or less, the Great Observatories program was getting underway. It led to the launch of the Hubble optical observatory in 1990, the Compton gamma-ray observatory in 1991, the Chandra x-ray observatory in 1999, and the Spitzer Space Telescope (infrared) in 2003. The net result of all this effort was that it heralded more

than a decade (1981–1995) of *no* sustained orbiting U. S. x-ray astronomy missions. That was no small accomplishment (!) and it was brought about principally by the infamous (to x-ray astronomers) AO 6 and 7 and by overruns by the Hubble telescope program.

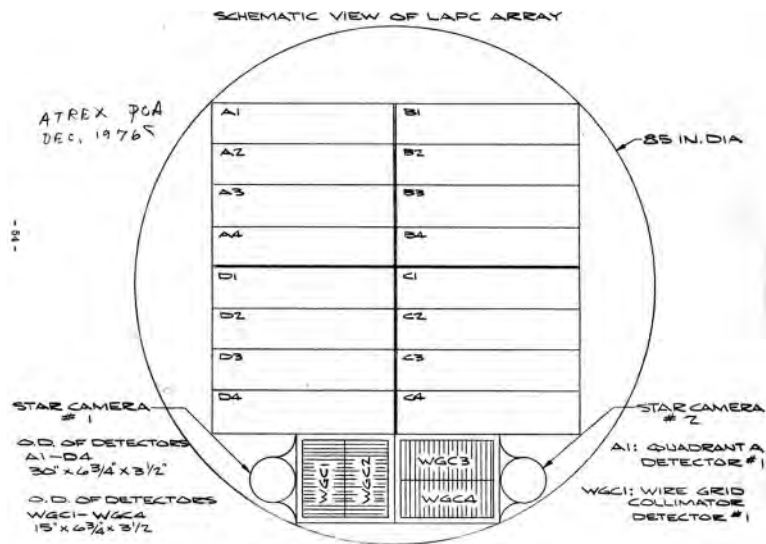


Figure 4. Large Area Proportional Counter on ATREX the x-ray/gamma-ray mission proposed by the high-energy study group selected from the AO6/7 proposals.

***Gorenstein points the way***

In the 1970s, U. S. x-ray astronomers had Uhuru, SAS-3, OSO-7, OSO-8, HEAO 1 and Einstein. It was a truly a great tragedy to shut down a field just as it was blossoming. It had demonstrated that it had the full richness of potential as optical and radio astronomy. It could investigate with high statistics the strongest gravitational fields, the highest temperatures, the greatest densities, the highest particle energies, and the highest magnetic fields in objects as diverse as normal stars, neutron stars, stellar black holes, supernova remnants and active galactic nuclei. It needed space platforms to exist and could not go forward without them.

During the 1970’s, I was on Al Opp’s advisory committee (High Energy Astronomy Management Operations Working Group; HEAMOWG). Dr. Opp was a NASA Headquarters official managing high-energy astronomy programs. He was the source of money for our research programs in x-ray astronomy. This committee had about a dozen scientist members who worked in x-ray, gamma-ray and cosmic-rays. It was intended to advise Dr. Opp on issues facing him. At our first meeting after the dismal AO6&7 announcements, in 1976 or 1977, at Goddard, I believe, Opp suggested that we recommend what space program or programs in high energy astronomy NASA should carry out on the next round.

On committees such as this, the members are supposed to represent their entire field fairly and not to push their parochially favorite programs. My baby, of course, was the pointed large-area observatory concept, but I dared not speak up. There was silence around the table, because everyone was probably dealing with the same conflicted feelings. It was awkward in the extreme. Then, up spoke Paul Gorenstein of SAO in his typically agreeable and thoughtful manner that emanates reasonableness, “Well, many of

the objectives we are interested are in line to be addressed (e.g. gamma rays with Compton and focused x-rays with Chandra), but a very important one that was is not is the pointed large-area x-ray observatory concept. I think we should propose that as our highest priority,” or words to that effect. Paul was an x-ray astronomer, but had other scientific interests, so his words carried great weight in the committee, and, as I recall, the committee endorsed his view. I loved Paul for that. In fact, I had known him from our collaborations with the AS&E group in the early days of x-ray astronomy and had always liked him, as did most people. He was always so thoughtful and reasonable.

Our job was now reduced to convincing the community that such a mission was an important goal in x-ray astronomy, a goal that did not become easier when the Einstein mission (launched in 1978) began producing spectacular x-ray images of celestial objects. That reduced non-focusing experiments such as the large area concept to “old technology.”

Such a view misses the point that at the dominant aspect of x-ray astronomy had been studies of temporal phenomena with broad-band spectra: pulsars, orbiting binaries, x-ray bursts, transients (“x-ray novae”). It made no sense to stop such observations simply because powerful new techniques opened up new exciting domains of study (imaging and high-resolution spectroscopy). It requires lots of x-ray counts to measure variability down to millisecond periods, the time scale of matter motions near a neutron star or stellar black hole. Focusing missions do not provide the needed area; large-area detectors can.

### ***HEAMOWG at St. Louis (1977?) and a lymphoma***

A follow up meeting of the HEAMOWG was convened by Al Opp in St. Louis at Washington University for the purpose of finishing up a report on science priorities. I took special pains to be there and to make sure the XTE concept was prominently included. I remember Al Opp keeping us working at writing until quite late for a couple of days. We produced a report that may have played a role in keeping the XTE concept alive.

This was probably only my second time in St. Louis as an adult. I recall eating down by the river one evening and seeing the moonlit Mississippi River from the famous Gateway Arch, a breathtaking sight indeed. This meeting probably took place in 1977. I remember that it was not long after my bout with non-Hodgkins lymphoma, which was detected in March 1976. Chemotherapy and radiation treatments continued for most of that year.

XTE was not my whole life by any means. We were in the midst of the MIT SAS-3 mission (1975–79) and had seen the launch of HEAO-1 in August 1977. I was heavily involved in both of these missions, not to mention my teaching duties and my family life. My daughters were 17 and 13 in late 1977.

### ***LAXTE on the Multimission Modular Spacecraft***

In 1977, we at MIT decided to submit an unsolicited proposal to NASA for the mission highlighted by the HEAMOWG report. Since the previous multi-institutional proposals (LAXTE/Scout and ATREX) had failed, we chose a simple, single-institution approach that made use of some favorite NASA programs of the day. We proposed to use



the standardized “Multi-mission Modular Spacecraft (MMS)” which had been developed by GSFC for the Solar Max Mission (launch 1980); it was to be launched by the Shuttle whose first flight was in 1981. At that time, the Shuttle was being touted as a low-cost truck ride into space and a boon to scientific research. We further proposed that MIT do spacecraft integration and that 1/3 of the observing time be for guest observers. The instrumentation was again 1 m<sup>2</sup> of proportional counter array, but included detectors with slat collimators for periodic scanning the sky by means of spacecraft maneuvers (Fig. 5).

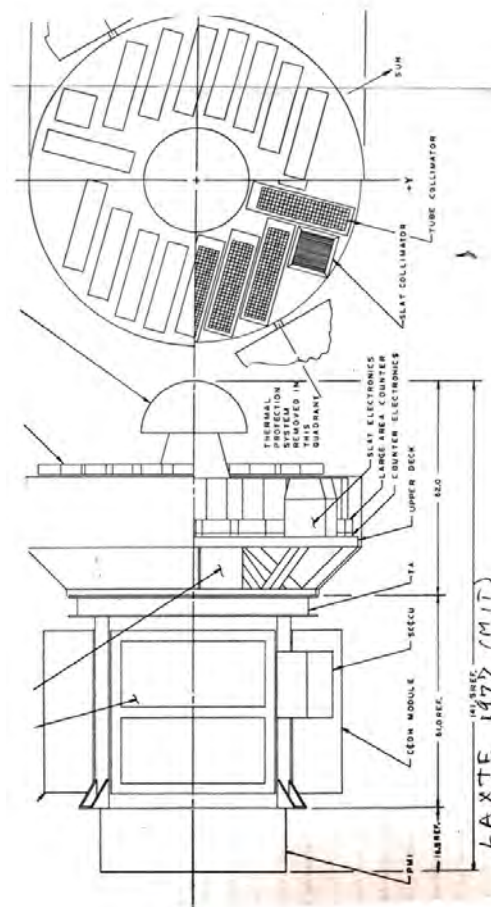


Figure 5. LAXTE on the Multimission Spacecraft (1977). This is from an unsolicited proposal from MIT to NASA that was politely ignored.

This was a complete proposal that represented a lot of scientific and engineering effort. It may have seemed futile to make such an effort in the face of no announced opportunity by NASA. Needless to say our colleagues at other institutions took a rather jaundiced view of this, but we were in an aggressive, fighting mood after the AO 6/7 debacle, and felt it worthwhile.

In the end, NASA simply ignored our proposal. It is possible that we got a nice refusal letter after some time, but I am not sure of that. Perhaps it helped the case for x-ray timing at NASA Headquarters, but perhaps not. I am reminded that Sam Ting (Nobel laureate) somehow convinced the NASA Administrator (Goldin) to fly his cosmic ray experiment, AMS, in the absence of any announced opportunity. Could I have made such

a pitch that carried any weight without the Nobel in my pocket; I doubt it, but perhaps I should have tried.

Basically, the XTE concept was dead in the water.

### *Theorists to the Rescue*

In 1979, David Pines and Fred Lamb, highly respected theorists at the University of Illinois, knowing the value of a timing mission to the physics of neutron stars, decided to do something about it. They organized a workshop in Washington DC, on April 20-21, and edited the proceedings, entitled “Compact Galactic x-ray Sources: Current Status and Future Prospects.” This was the famous Orange Book that graced our bookshelves as a sort of bible for years afterward (Fig. 6). Fred reminds me that the attendees were required to construct a plan of a viable mission and that the doors were locked until the mission concept was agreed upon and the writing justifying it was completed. The core mission was defined to be a large area pointed experiment (1–30 keV) and a sky monitor to detect transients, etc. The value of extended energy ranges, higher and lower, was stressed, but they were not part of the core program.

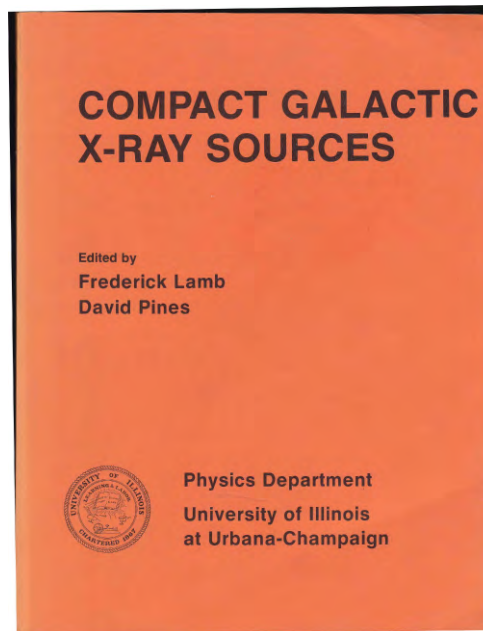


Figure 6. Cover of the proceedings of the workshop organized by Fred Lamb and David Pines, which proposed an x-ray timing explorer mission, the XTE. This was the “Orange Book.”

The Pines-Lamb proceedings with its many papers on the science that could be extracted from such measurements, was a foundation block that set the stage for wider acceptance of the concept. This and the HEAMOWG endorsement led to endorsement by the highly visible committees of the National Academy of Sciences, specifically the Committee on Space Astronomy and Astrophysics (CSAA) and the Space Sciences Board.

*CSAA endorsement and a gambit by a supporter*

About this time, I was newly appointed to the Committee on Space Astronomy and Astrophysics (CSAA), for the three year period beginning July 1, 1979. This was a subcommittee of the Space Science Board of the National Academy that consisted of possibly 15 members from all disciplines of astronomy and astrophysics. I was a little awed to be on such a committee and was of course interested that there was to be a review of the Explorer program at the first meeting I would attend, Sept. 27-28, 1979. COBE, EUVE and XTE were each discussed. Fred Lamb presented the XTE case. (The week before he had made a presentation arguing for XTE to the High Energy Panel of the 1979 Astronomy Survey Committee.) The EUVE case was presented, I recall, by Stu Bowyer, the PI and long time proponent of such a mission. I have always had a soft spot in my heart for him since he sent me a congratulatory note after our 1968 rocket flight detected x-ray pulsing from the Crab pulsar. Unfortunately, we were scooped by only a few days by the NRL group. Stu wrote, graciously, that he only believed the result when the MIT group confirmed it, which was nice to hear. (The NRL result was actually quite convincing by itself. Our results did offer some new aspects.)

So, this was all fine; the presentations went well. But then the discussion turned to the real issue - probably after the presenters had left the room - of whether or not to recommend a reversal of the order of launching so XTE would be launched before EUVE. This was a radical idea as EUVE was already well established in the queue and Stu Bowyer had also gone through many hoops to get it there. Any move to alter the order would be viewed as highly disruptive and would surely be resented. I was pleased that XTE was getting new attention, but was this going too far? I was flabbergasted; I had no idea such a possibility was on the table. I was also highly embarrassed because of my clear conflict of interest, not to mention that I was the new kid on the block. In fact, I should probably have left the room. As it was, I just sat there totally mute during the discussion, and in the end the Committee chose to endorse all three mission and to make no change to the sequencing. It also recommended an early issuance of an Announcement of Opportunity for XTE. This was a huge step ahead for XTE.

I always wondered where the initiative for this proposal had come from, and I heard that Stu considered it to be me. Any warmth he might have felt for me surely went up in smoke then. However, I plead innocent!

It was only at this meeting (in 2010) that I learned who the initiator of the reordering idea might have been. If that is correct, he would have been a very senior academic supporter of the XTE concept who was also on the Space Sciences Board, the committee above the CSAA. As such he was in a position to suggest the CSAA reexamine the mission sequencing on scientific grounds. From a purely scientific viewpoint, this might be a sensible question to ask. On the other hand, from a management viewpoint, it could be highly destabilizing, because, under constant threat of rescheduling, managers of approved missions would be unable to lay out their routes to launch with any confidence. Charlie Pellerin recently told me that, even if the CSAA had suggested a reordering, he most likely would have ignored it.

*Announcement of Opportunity for “X-ray Variability”*

All this momentum led NASA to plan the release of an Announcement of Opportunity. A number of groups, perhaps a dozen geared up to submit proposals for the expected NASA Announcement of Opportunity. It may have been Frank McDonald at GSFC, the head of the Goddard Lab for High Energy Astrophysics, who suggested that MIT and GSFC collaborate on a proposal. Both groups were strong in the field, and such collaboration would probably have been unbeatable for the core instrumentation defined in the Lamb-Pines study. Thus Steve Holt at GSFC and I entered into intense discussions as to the nature of such a collaboration. We quickly agreed that the mission should consist of two instruments, a 1 m<sup>2</sup> of pointed proportional counters and an All-Sky Monitor for detection of transients, etc.

In July of 1980, the Announcement was released. It was specifically directed to “X-ray Variability.” The general scientific objectives were specified, e.g.,

1. Measurement of physical parameters of compact objects
2. Physics of accretion disks, plasmas, and stellar magnetospheres
3. Geometries of source emission regions
4. Normal stars through variable mass outflow
5. Nature of bursters, transients, and Sco-X1 like sources
6. Compact extragalactic objects.

But the instrumentation and specific implementations of these objectives were left to the proposers. It further specified that the mission would fly on a Dutch satellite, TIXTE, along with the Dutch multi-pinhole (“Dicke camera”) instrument. (This instrument, later flew on the Italian BeppoSAX in 1996 where it was known as the Wide Field Camera (WFC).) This of course again complicated the mission and applied constraints on the US portion. A secondary fall back position was that the selected instruments would fly on a solely US mission. Proposals were due in October 1980.

The deadline for proposals was October 17, 1980. The discussions between Steve and me were getting nowhere. We each agreed fully that a 50-50 collaboration was what we wanted, but we could not agree on the form that would take. Basically, Steve envisioned GSFC carrying the fabrication responsibilities for both the PCA and ASM, while MIT would provide the digital data processing and be the lead institution in the operations, data processing, and guest observing aspects. This was not an unreasonable stance because GSFC had experience in building large counters and had flown an ASM on the Ariel-5 spacecraft. Steve truly felt he had reserved for GSFC only a builder’s role and that he was offering MIT most of the juicy, visible, rewarding aspects. Also by drawing on technology already demonstrated in their labs, he felt, correctly, that he was strengthening the proposal and keeping costs under control.

However, we at MIT, with long experience as hardware builders, though not in-house proportional counters, saw it differently. We believed the development of an instrument was the essence of a space experiment and that doing so gave one the moral suasion to control how the entire mission was configured. Without any such role, we felt we would be saddled with a lot of bureaucracy while being left pretty much on the sidelines. We would be the tail on the dog, and it is not the tail that wags the dog. So, after much discussion with my colleagues and much soul searching, we decided we would prefer to compete for the entire core program, even at the risk of losing it all. We preferred to lose it rather than being at the wrong end of a phone line, as we saw it. If that happened, we could more productively put our scientific energies elsewhere. I thus wrote a formal letter to Steve indicating our intention to proceed independently. He told me later that he just

sat there and stared at the letter, shocked, hardly believing what he was reading. My view was and is that his shock was rooted in his inability to see how deeply we felt about the issues we had raised.

In retrospect, we could have made Steve’s conception work for us as it did in many other collaborations. For example, our role in the HEAO-A3 collaboration with SAO was similar to (or even less than) the role Steve envisioned for MIT and it worked extremely well, even though our formal responsibilities were minimal. I now realize that it is mostly the energy and creativity that one brings to a collaboration that determines how involved and influential one is; the formal definitions count for less. Nevertheless, our view was that formal responsibilities can matter a great deal when budgetary or technical challenges arise. We valued a primary role and believed we could do it well, and were concerned that a collaboration with GSFC with its broad management and engineering involvement could easily become quite one sided despite Steve’s good intentions.

In this case, a competition between MIT and GSFC turned out to be a benefit to the final mission. Now, in a deadly competition with the strong GSFC group, our proposal efforts intensified and we strove to find new creative ways to accomplish the prime goals of the mission. It was clear that GSFC was in an advantageous position with its in-house detector capability; it had demonstrated in space both large-area and all-sky-monitoring instruments. We could offer a lot of detector experience through our usual vendor, LND Inc. on Long Island, substantial operational experience, and highly competent science credentials, but these were not the core of the mission. We definitely felt that we were at a disadvantage.

### ***The MIT Proportional Counter Array (PCA)***

For the large array detectors (PCA), we proposed sealed Be-window detectors made by LND similar to those flown on many previous missions with a net effective area of 0.76 m<sup>2</sup> and multiple-cell anticoincidence, shown to be effective in the GSFC detectors. We then addressed the fundamental issues of how to manage the high bit rates of the PCA data stream and what kind of sky monitor was optimum. Here the creativity of my MIT colleagues came to the fore.

### ***The MIT Experiment Data System (EDS)***

The fundamental problem with data retrieval was that it is impossible to get all bits describing each x-ray detection (bits for pulse height, anode number, and time) through the telemetry link when the counting rate is high, as it must be for millisecond timing. In previous missions the data were compressed through binning time and/or pulse height. Several modes would be hard wired into the flight circuitry, perhaps one with high spectral resolution and low time resolution, and another with the inverse. The observer could select the preferred mode by command. This entailed the a priori selection of a few data modes that could not be changed during flight. This inflexibility, we felt, was dangerous in that celestial surprises could well require a mode that had not been foreseen.

Our colleague, Garrett Jernigan, then at MIT, pointed out repeatedly that basically the binning amounted to constructing a secondary set of bits to describe the desired binning

address (pulse height and time) for each possible primary set of event bits. Basically, he pointed out, this amounted to choosing which of the primary bits to retain. Thus what we needed was a scheme that would allow us to construct the secondary bits according to an *arbitrary* formula. The ability to invoke any arbitrary binning scheme in flight was the goal.

Garrett is an extremely creative guy who has a propensity for seeing problems in a different light than most of us. His insight was creative, but the problem was implementation. The obvious procedure was to have flight software reconfigure each event’s bits to the proper binning address on the fly as the events came in. However, the required processing speed made this scheme prohibitive in power consumption. How to proceed was a puzzle, and we even organized a small workshop of local data processing experts, hoping a solution would emerge from the discussion.

It was at that workshop that our very own John Doty came up with the “obvious” answer: table lookup. The bit stream from a single event would address a table that had an address for each possible primary bit set. At that address would be the appropriate binning address, which would be incremented by unity. The lookup was rapid and could be done on the fly. Re-loading the table for a new binning mode could be done leisurely between observations. Why didn’t I think of this? I knew trig functions and logarithms were often obtained from table lookups on the slow computers of those days, and perhaps still are, but its application here did not occur to me. Neither did it occur to Doty until after some time reflecting on the problem.

We called the logic chain that executed the rapid binning an Event Analyzer. Another important feature of this data system was the inclusion of multiple Event Analyzers so the data stream could be binned in multiple ways simultaneously. Of course the outputs would have to share the available telemetry. Two of the eight EAs were dedicated to the data from the proposed All Sky Monitor; they operated somewhat differently.

This then was the essence of our proposed EDS. How useful was it in practice? We launched with a fixed set of modes that observers were likely to need. It turned out that very few additional modes were required postlaunch. However, the generality of the design made possible an important reconfiguration of the data stream and command structure late in the life of the mission. We considered the EDS to be one of the truly innovative features of our proposal.

### *The MIT All Sky Monitor (ASM)*

Our sky-monitor experience was grounded in our SAS-3 experience (1975–79). Much of that mission was in a scanning mode with long FOV slat collimators that effectively located new transients and sources with changed spectral states. At the same time, the GSFC ASM on the British Ariel-V was also finding transients quite successfully, scooping MIT several times, most notably in sighting the X-ray Nova A0620–00, now a known black-hole binary.

Previous sky monitors were the scanning slat collimators (e.g. SAS-3), the single pin-hole camera (Ariel-V) and the multi-hole “imaging” Wide-Field Camera (WFC) camera being advocated by the Dutch. In fact the latter was the instrument that could well fly with the XTE if the Dutch-US collaboration went forward, but it was designed to take long exposures of selected celestial regions, not to scan the entire sky.

After much thought, we decided on a camera that had been suggested by George Ricker of our group at MIT. It was a one-dimensional version of the WFC, it consisted of a random slit mask over a one-dimensionally position-sensitive proportional counter. A one-dimensional capability, with its reduced telemetry requirement, was sufficient because the sky consists of a limited number of point sources; unambiguous full sky imaging was unnecessary. Two-dimensional positions would be forthcoming from two such cameras with slit masks rotated with respect to one another. The relatively large aperture of the masked camera compared to the single pin-hole camera provided much better statistics for obtaining position centroids and intensity measurements on short time scales. This, we felt, greatly offset the background source confusion inherent in such a system.

The data processing of the shadow camera data was also quite innovative. We envisioned a continuously rotating camera and Garrett Jernigan came up with a clever binning and Fourier analysis technique, which would be quite efficient given steady scanning with minimal rotation about the field of view axis. In the end, this proved impractical, and the “scanning” was carried out with sequential 100-sec exposures with the camera orientation held fixed during each exposure.

Knowing we were up against a proposal containing a pin-hole camera, our proposal was careful to clearly describe quantitatively the relative merits of the two systems; both had their strengths and weaknesses. In such comparisons, it pays to be objective and fair, because that gives credibility to one’s conclusions, and I believe we were.

### *Proposal submissions*

Our proposal for a “Proportional Counter Array with a Scanning Shadow Camera for the X-ray Timing Explorer” with me as the PI was submitted to NASA on 17 October 1980. It was a great relief because we had worked terribly hard to make that proposal innovative and accessible. Our conception of the instrument configuration is shown in Fig. 7. About a dozen proposals were submitted in all. Notable, because it was eventually selected, was Rick Rothschild’s proposal for a hard x-ray instrument for high-energy x rays (20–200 keV) to complement the core instruments. Rick was at Univ. of California at San Diego, a member of Larry Peterson’s x-ray astronomy group. He had formerly been at GSFC and was well acquainted with the large-counter technology at GSFC.

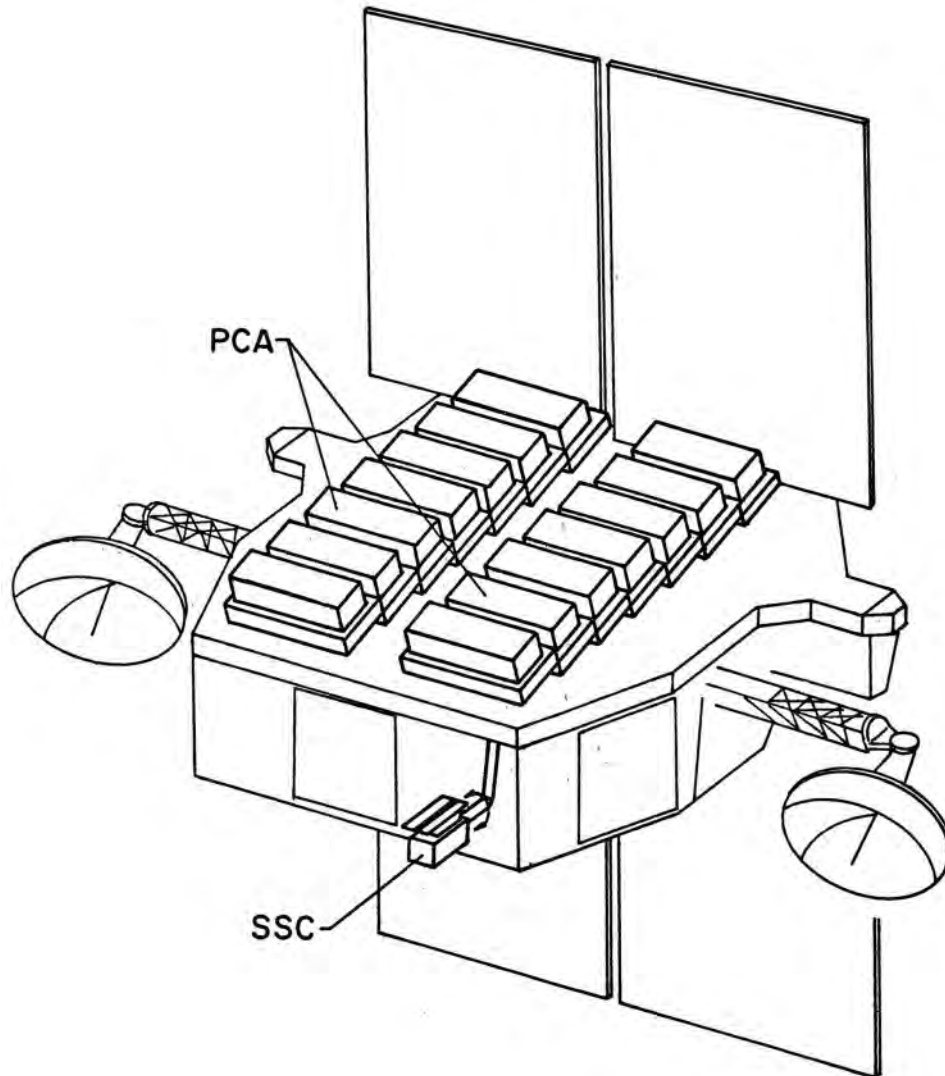


Figure 7. XTE spacecraft in the MIT Proposal (October 1980). The Proportional Counter Array (PCA) consists of 14 beryllium window detectors. The All Sky Monitor (ASM) (labeled SSC) consists of two Scanning Shadow Cameras (SSCs) with crossed fields of view.

## Phase II: Selection and Holding (1981–1989)

### *The George Clark missile*

NASA formed a review committee with Bill Kraushaar of U. of Wisconsin as chairman. Fred Lamb (U. Illinois) was also on the committee, he just reminded me. The others were Doyle Evans (LASL), Terry Matilsky (Rutgers), Francesco Paresce (STScI), Richard Catura (Lockheed), Roger Chevalier (U. Virginia), and Frederick Seward (CFA). Alan Bunner of NASA served as Secretary. The committee members' names were embargoed and we did not know who they were until we appeared before them in May 1981. If we had known, there would not have been even the glimmer of a thought that



Kraushaar might favor MIT because he had previously been on our faculty, and indeed in our “cosmic ray group.” He had tenure at MIT and left MIT for Wisconsin for personal reasons, as far as I knew, perhaps in the mid 1960’s. We knew him as an eminently fair and highly knowledgeable scientist. He was an excellent choice. The rest of the committee were also well qualified and thus it appeared that the proposals would be fairly judged by the scientists. But, there were larger issues involved in the selection.

The MIT group had a history of space experiments in several fields (gamma ray, x-ray, and interplanetary plasma) dating back to the early 1960’s. Much of it had been productive and rewarding, but of late one or more experiences in the plasma area had disenchanted members of our group with NASA’s evolving selection procedures, which they felt had deteriorated substantially in several respects. The evolution was probably due to ever stricter requirements on commercial procurements, but the MIT group felt they were degrading the quality of scientific selections. This had been the subject of strong letters between Herb Bridge (head of the plasma group at MIT) and high NASA officials, one in 1977 in my files was instigated by the categorization of an MIT plasma proposal as Category 2 by the committee. Although, this letter might be written off as “sour grapes,” it is clear from the letter’s quotes of the committee’s comments that the rating was based on an incorrect understanding. Walter Lewin, in 1978, made similar points to the NASA Associate Administrator about uninformed committee decisions and recommended steps to improve the process.

Added into the mix of concerns, in the present case (XTE), was that we were competing head to head with a NASA institution (GSFC) and that NASA (Headquarters) was to be making the selection. Could they be objective? We, of course, were concerned that Goddard’s financial well being might just happen to carry more weight with NASA Hq than would MIT’s. At some later time, in discussing this problem with, I believe, Carl Fichtel of GSFC, he told me the concern at Goddard was that Headquarters in their sensitivity to the conflict issue would bend over so far backwards to be fair that they would thus be biased against Goddard. Of course this discussion just reinforces the reality of the conflict of interest on the part of NASA Headquarters.

All this was background noise to me as my small team was totally occupied with getting the proposal put together and submitted. It thus was a total surprise to me when our colleague George Clark revealed to us that he had sent a letter “missive” to our Senator Ted Kennedy and our Representative, Tip O’Neill (then Speaker of the House) outlining our concerns with the current NASA selection process. He made no bones that a \$25M proposal from Cambridge MA (their districts) was at stake. George had been supportive and helpful, but not heavily involved, in our proposal effort. He had discussed his letter with none of us and probably not with our director (Herb Bridge) either. He knew such a proposal would generate heated discussion about whether it was wise to totally anger NASA Hq., the ultimate deciders and “the hand that feeds us,” at such a critical moment. He sent it on 16 October, the day before proposals were due! It was George Clark’s Sidewinder missile. It is attached to this document as an Appendix.

He made three points:

1. Proposers had no opportunity to rebut erroneous understanding or conclusions on the part of the peer committee members, and such was increasingly likely because it was becoming common that most of the US experts in a given field would be involved in the submitted proposals and thus would be excluded from committee membership.

2. The committee was only allowed to categorize proposals; it was not allowed to rank them. NASA Hq. would select the proposals to fly from Category I proposals. The peer committee’s judgment as to the highest priority for flight was thus, at least formally, not taken into account. In the recent past, the peer committee had been asked to rank the proposals, but the procedures had changed.

3. The costs of civil service man hours was not included in GSFC proposals, thus giving a financial advantage to the government institution.

Needless to say, the Kennedy and O’Neill offices sent George’s letter to NASA for comment and it was not happily received. George had sent copies to NASA’s Frank Martin who was justly angry for not being given a chance to deal with the issues earlier. This generated an intense set of letters over the next six months that went back and forth between George and Martin and a second letter to Kennedy and O’Neill. Martin’s response, simply stated, was that NASA was largely constrained by procurement law and that an Announcement of Opportunity (AO) for peer-reviewed scientific observations, as distinct from a competitively bid RFP, was a highly vulnerable process. In response to George’s three points, he argued that

1. Allowing personal presentations or rebuttal, while not excluded, would hinge on the eloquence and personality of the proposer which could introduce bias. The obvious answer: yes, but that is a lot better than bias by the eloquence of an uninformed prime reviewer on the peer selection committee.

2. NASA sometimes asks for informal rankings, but it must ultimately make the choice taking many factors into account. Answer: yes, but open ranking by the committee would be a valuable input to the final decision that could then not be ignored.

3. NASA makes cost allowances to level the playing field. Answer: Fine, but will that be an open visible process, so that it is clear to all that realistic numbers are used that include overhead, benefits, etc.?

### *Selection*

NASA did modulate its selection procedure in response to all this. It set up a two phase process for the Committee review. The Committee would meet on May 21, 1981 to review the proposals. They would generate a set of written questions for the proposers, and the next day, each of the proposers would have 30 minutes with the committee to answer them. Subsequently, the proposers would have a month to prepare written responses, and they could also submit modifications or clarifications to their proposals, within the original proposal page limits. The committee would then reconvene to consider the written responses before making its report. They were charged with evaluating the strengths and weaknesses of the several proposals. Then an ad hoc subcommittee of the NASA Space Science Steering Committee (SSSC), composed of a group of full-time government employees (!) would categorize the proposals. Those proposals “with the greatest scientific merit” will be further evaluated by the project office at GSFC for cost, engineering, etc. The Associate Administrator for Science would then make the final selection based on these reviews.

This process gave the proposers an opportunity to counter misconceptions, but it seemed to remove the ranking even further from the scientists. Nevertheless, we were grateful for the opportunity to clear up confusing points in our proposals and to make

minor modifications. By then we knew that the committee had sufficient expertise to make unlikely serious errors in this regard.

Well the process played itself out and, on Sept. 28, 1982, (two years after the proposals were submitted!), we received the result from the Associate Administrator, Bert Edelson. Goddard’s PCA and MIT’s ASM were both selected for a definition study on XTE. Thus, Steve and I were again urged to collaborate on the mission. It was not exactly “*déjà vu*” because this time we at MIT at least had an instrument that had been endorsed by NASA itself with the clear mandate that both be included in the study. Edelson wrote, “It is our intent to combine the operation of your instrument with that of the Large Area Proportional Counter proposed by Dr. Holt.”

This led to more discussion with Steve. He felt very strongly that we jointly should recommend the pin-hole camera system that GSFC could provide essentially “free.” On re-reading the selection letter now, I find it remarkable that he would still propose this in that the review had unambiguously selected the MIT ASM over the GSFC pinhole camera. Nevertheless, I believe it was within our purview to select any elements that were in our original proposals to make the optimum mission. Steve felt very strongly that the all-sky monitoring was very important to the mission, but that it was highly vulnerable to being eliminated in one of the inevitable costing exercises we would encounter down the road. An essentially free, off the shelf, system would be much less vulnerable. His arguments certainly had merit, but we would not buy it.

This led to a memorable meeting at GSFC with both of our directors present, Herb Bridge of MIT and Frank MacDonald of GSFC. We debated the merits of the two approaches inconclusively and ended with a standoff - both sides would go home and think about it. This time there was little correspondence back and forth as I felt our case was a strong one, both politically and scientifically. I thus simply kept reiterating my belief to Steve that the ASM should be the Scanning Shadow Camera system we had proposed. Eventually, it may have been Frank who convinced (or told) Steve that he should let MIT have their ASM.

I took my lesson here from Herb Gursky, a colleague formerly at AS&E, when we were defining the HEAO-1 experiment and trying to arrive at a sensible collaboration. I spent an entire summer doing calculations and working up arguments as to why my approach (a deep survey along the ecliptic with a rotating modulation collimator) was preferable to his approach (a scanning modulation collimator to cover the entire sky). Every week I would do some calculations and trudge over to AS&E, then adjacent to the MIT campus, in an attempt to convince him my approach had the greater merit. He would hear me out and simply restate that he “thought the all-sky approach was the way to go,” and never did a bit of work to justify it, because the relative merits depended on one’s subjective view - the apples and oranges choice again!

In the end, we agreed to put both approaches in the same structure and that AS&E would do all the fabrication, thus avoiding two management structures. Later, when the HEAO-1 program was restructured, the RMC portion was removed, which was regrettable, but in accord with the scanning nature of the mission. By that time, my small group was well involved in the collaboration with Gursky’s team. Nevertheless, from those discussion with Gursky, I had learned that if one is confident that one is on the correct side of a winnable issue, one can simply continue to calmly restate your convictions and things will likely eventually fall your way. But, you must be very, very sure you are on the winnable side.

Steve and I then worked up a memorandum of agreement that outlined a mission with the Goddard PCA, the MIT ASM and the MIT data system (EDS) and formally notified NASA of it. It then became apparent through the grapevine that NASA was debating the possibility of including Rothschild's high-energy experiment, HEXTE. For me it was a hard call because it added good science but also added cost and complexity to the mission; remember the Christmas tree. However, it did complement the PCA nicely, and I knew that theorists really valued the information in the high-energy x rays, even though there were very few of them relative to the low energy flux. Thus, when I received a call from NASA, perhaps from Al Opp, delicately asking how I felt about having a high-energy experiment on XTE, meaning, I gathered, “would you create a ruckus if NASA included it in the mission?” I indicated my consent, and it was shortly thereafter that Rick received an acceptance of his proposal to his great delight.

One might ask if all the letter writing about process did any good or whether MIT may have muscled its way to an unfair advantage. I doubt it had a significant effect on the selection; the conclusions were hard to fault and the committee was quite knowledgeable. However, it could well have biased the case one way or the other because the NASA administrative persons are humans with emotions. Was George wrong to interject such perturbations into the selection process? I think not. The issues raised were very real and a real threat to a fair evaluation. In fact, NASA's response improved the process, and the persons involved were certainly sensitized to the possible shortcomings of the aspects they could not change for legal reasons. These issues remain with us today and are good to keep in mind.

### *The SAS-3 incident*

It reminded me of a run-in I had with George Clark, my friend and former thesis supervisor who was the PI on the MIT x-ray Small Astronomy Satellite (SAS-3) on which I was a frequent observer. At one point in the SAS-3 operations, he had made a ruling that I could not have all the observing time I had requested for our team. The amount he had shorted the program would have made an important substantial difference to our program, but, I believed, was small enough to not impinge significantly on the priorities of other observers. I felt it was a rather subjective call; he of course felt it was based on scientific grounds. When he persisted in his decision in a phone call, I became very upset and emotional with raised voice, etc. He then told me that, OK, he would give us the time, but only because I was so upset.

I then responded, heatedly, that he could not make me feel bad by saying that, because, if it came down to a judgment call, it was reduced to a simple power play. As the PI, he had the ultimate authority, and all I had was my loudly expressed unhappiness. If he was invoking his authority as a weapon, I had the perfect right to use my weapon, and I did not feel the worse for it. I told him all this in that one brief and hot outburst. I hope I ended the phone call by thanking him for the extra observing time.

A hour or so later, George, being the gentleman that he always is, called me back to say that it was important that we maintain our long-standing cordial working relationship, not to mention our personal friendship. I assured him that this did not in any way impair our relationship to my mind. In fact, I was probably the one who should have called him back. I value to this day his willingness to have made that additional phone call.

The moral of this story is that sometimes, when one is up against an ultimate authority, the only recourse is to make your case with a loud scream. In fact this is exactly what George did when he wrote Kennedy and O’Neill about XTE. Of course, this approach to a dispute is a very dangerous tool that can easily inflict serious self damage. It must be used with great care.

### *Astronomy Survey Committee endorsement*

In July 1981, in the midst of the selection procedure for RXTE, George Clark found himself again facing the XTE issue, this time from a more lofty perspective. George was chairman of the High Energy subcommittee of the Astronomy Survey Committee at the time, and, in this role, wrote a politic letter to Hans Mark, Deputy Administrator of NASA. He described the science XTE could do and stated that his ASC subcommittee “strongly endorsed” the X-ray Timing Explorer concept. He further stated that the ASC report “will endorse XTE.” The ASC was the decadal study of that time, so this was an endorsement of great significance. Given the fact that there had been no proposal competition for the timing concept, only for the instruments, such endorsements were crucial. On August 13, 1981, Fred Lamb made a pitch for XTE directly to NASA Administrator, James Beggs, and his staff.

### *Waiting for funding*

Well, the spacecraft instruments were selected. Were we all set to go, to proceed with design and fabrication? Well not really. In May 1982, prior to the instrument selection, I gave a talk at the Ottawa COSPAR conference, entitled “X-ray Timing Explorer.” It went over the science justification, but did not describe the instruments. It also stated that the launch would be delayed until “late in the decade” because COBE and EUVE preceded it in the Explorer queue, and the entire Explorer program had a very limited budget. The Dutch TIXTE plan went nowhere, and we were thus left with the need for a spacecraft. Perhaps it would be the aforementioned MMS. Its proponents argued that if NASA would buy several such spacecraft, the cost for each could be dramatically lowered. Unfortunately, NASA was prohibited from purchasing a spacecraft in the absence of an approved mission to make use of it. It was a Catch 22, but probably a good one, because each mission will place its own particular requirements on its spacecraft. A sedan in the storehouse is no good to you if you really need a motorcycle or a pickup truck.

There then passed a period of several years where the program was under the GSFC Advanced Missions Analysis Office (Bill Hibbard). Various spacecraft possibilities were explored, but with no funds in hand, several possibilities were set aside. We began to receive small amounts of study funds that, with HEAO-1 analysis funds, enabled me to continue support of a single postdoc and a graduate student or two. Al Levine, supported mainly on the HEAO-1(A4) data analysis funds, had helped produce our 1980 XTE proposal. In these lean years, he picked up more of the responsibility for RXTE and was the primary ball carrier during my sabbatical leave in the Winter and Spring of 1983. He recalls preparing a budget for MIT’s portion of the mission with Bill Mayer and being chastised at the general meeting where he presented it because it was significantly higher than those of GSFC and UCSD. As time went on and more realism was imposed on the

instrument teams, he was gratified to find that early MIT budget turned out to be about right whereas the others’ had been optimistic underestimates.

Ron Remillard and Edward Morgan, both former students of mine, began to take an interest in aspects of the XTE program. As the mission developed, these three, Al, Ron and Ed, became the mainstay of the MIT scientific effort. How fortunate I was to have three such talented young scientists pulling to make the MIT portion, and indeed the whole mission, a success. On the engineering side, Bill Mayer and Robert Goeke guided our study efforts. Eventually, during implementation Bill, also a former student of mine, became Project Manager and Bob the Project Engineer.

During these lean years (no NASA x-ray astronomy satellites in orbit and little funding for hardware development), Ron Remillard and I sought to optically identify x-ray sources located with our HEAO-1(A3) instrument. This took us to mountaintop observatories in Arizona, Chile and Australia – beautiful! During this hiatus in US missions, we, and others, made good use of Japanese and European x-ray satellites (Tenma, Ginga, Exosat) for x-ray observations. I spent a sabbatical in Japan in 1983 and visited there several other times. I also became familiar with the Exosat data center in Holland, only a few miles from my sister’s home.

### ***Theorists to the rescue again (1985).***

With good reason, Fred Lamb and his colleagues felt the XTE concept needed another boost by community scientists. Accordingly, in August of 1985, they organized another workshop, in Taos. The result was a summary paper in Los Alamos Science entitled “*Astrophysics of Time Variability in X-ray and Gamma ray sources.*” written by R. Epstein, F. Lamb, and W. Priedhorsky. It was reproduced in a widely distributed 37-page slick brochure replete with coherent discussions of the many types of variability in x and gamma ray astronomy, e.g. low and high x-ray binaries, x and gamma ray bursters, Her X-1, internal dynamics of neutron stars, quasi-periodic oscillations, black holes, etc. It described the features of coming timing missions, i.e., the Japanese *Ginga*, and the US XTE.

The Epstein report gave valuable theoretical support to the science XTE would be able to do. The case was helped immeasurably, of course, by the observations of the Japanese *Hakucho* and European EXOSAT missions, in particular the burst studies of the former and the discovery and studies of quasi periodic oscillations (QPO’s) in low-mass binaries by the latter. It wasn’t all theory!

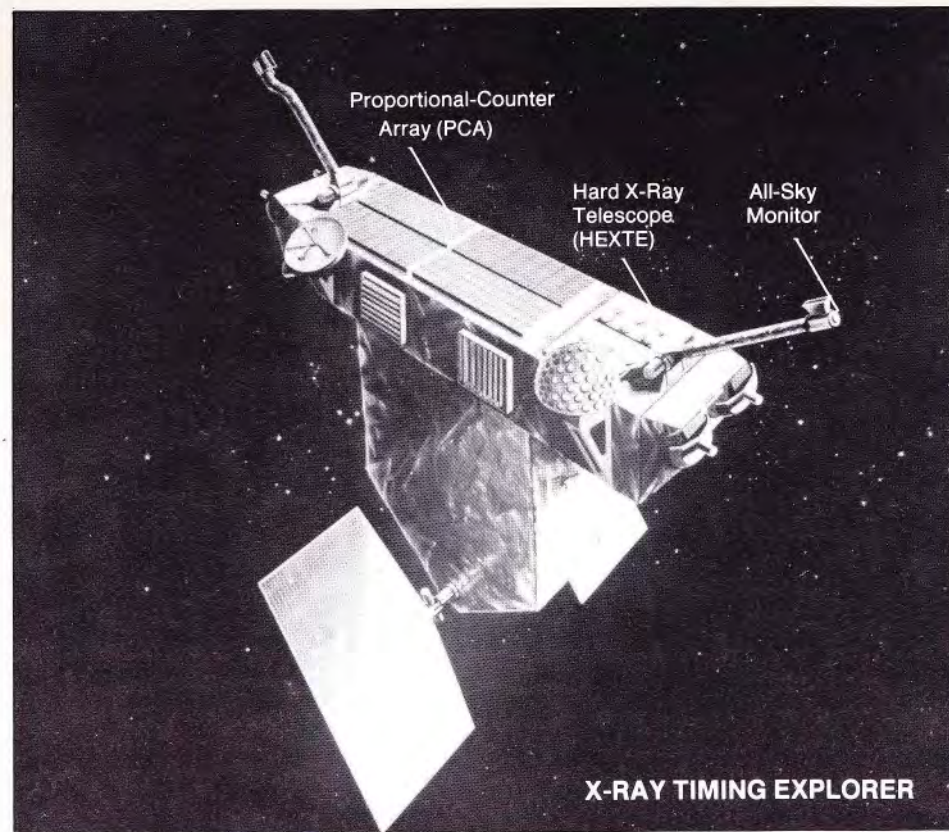


Figure 8. XTE on configured for a space-shuttle launch by GSFC engineers. The long booms for the ASM and the existence of two ASM camera assemblies (instead of the one originally proposed) were not favorably received by Charlie Pellerin of NASA Headquarters.

A figure in the document shows the configuration of the XTE experiment payload as worked out for a space-shuttle launch by GSFC engineers. A notable feature were the two long deployable booms that held the all-sky monitor units (Fig. 8). We had told the engineers it was desirable to maximize sky coverage and hence to minimize blocking of the FOV by the spacecraft. This led to two booms, each with two Scanning Shadow Cameras (SSCs) rotating about the boom axis. Given that the two booms (about 10' long) were at right angles to one another, essentially the entire sky would be accessible. We were pleased to see four SSC detectors rather than the two we had proposed and were a bit surprised by the length of the booms. We were a bit concerned about the complexity of deploying them and holding alignments. Nevertheless, we were pleased that NASA seemed willing to help us optimize the science we could obtain from the mission.

### ***Challenger (1986) and Ginga (1987)***

In January of 1986, the Shuttle Challenger was lost and Shuttle flights did not take place for 32 months. As the shuttle was our current launch vehicle, this was definitely not good news for XTE.

In 1987, Ginga, the Japanese timing mission was launched. Its large area of pointed proportional counters ( $0.4 \text{ m}^2$ ) and sky monitor clearly was the XTE concept. The counters were made by the Leicester group in England; recall that this group was to

provide the detectors for our 1974 LAXTE proposal. Ginga clearly detracted from the singular contributions that XTE might make and raised questions about the viability of a second similar mission. Ground based astronomers in different countries often build competing similar telescopes, but in the space business, because of the high costs, it is difficult to make the case that a second similar mission is worth doing.

It was encouraging, though, that timing science was being pursued. Ginga carried out a wealth of productive studies in both timing and spectral domains. It discovered two bright transients with its sky monitor. It turned out that Ginga lacked some features we were planning for XTE. It did not carry a high-energy experiment like HEXTE; its millisecond timing capability was minimal because of limited telemetry; it could not acquire new targets quickly, and its sky access was limited by solar angle considerations.

### *Explorer platform*

Our future was now largely in the hands of Charles Pellerin, Director of Astrophysics at NASA and his deputy for high-energy astrophysics, Alan Bunner. Alan, a former x-ray astronomer from Wisconsin University, was the trooper who kept XTE alive in the halls of NASA for many years. Charlie was a hard-driving manager whose job, with Alan's, was to get astrophysics missions to flight. Success meant getting a mission to the point where it was returning data from space. We were fortunate to have both men pushing for XTE.

Charlie had come up with a plan for getting XTE into space. At NASA in the 1980s, the hype was all about the Space Station and the Shuttle. Charlie suggested buying into the “platform in space” and shuttle psychology. Sometime previously, Frank Ceppolina of GSFC had proposed mounting EUVE use the “standard” MMS spacecraft, launching it with the shuttle and then at the end of its mission, having the shuttle retrieve it so XTE could, after refurbishment, reuse the MMS. In Charlie's plan, the EUVE and XTE would use the same MMS spacecraft, but the change-out would be carried out, not on the ground, but in orbit in the Shuttle Bay. The spacecraft would now be called the Explorer Platform (EP) - a mini Space Station. Only one spacecraft for two missions sounds like a money-saver, and it could lock the XTE mission into the planning.

This was a bold, but dangerous, concept. It required two successful Shuttle launches, the first to take EUVE into orbit and the second to carry XTE to a rendezvous with EUVE. It also entailed a complex interchange of the two payloads in the Shuttle bay, wherein XTE would mate for the *first time ever* with the EP. Finally, it required that the XTE payload be qualified for flight on the Shuttle, which, because humans are involved, imposes expensive requirements.

This put me, and others, in a tough moral dilemma. Should I, as a PI, raise questions about this plan? However, to question them could destroy our most promising route into space. NASA was known for its bold initiatives, and perhaps I was just too old fashioned to understand the potential of this scheme. On the other hand, in not questioning the plan, I might be failing in my obligations to my colleagues and to the taxpayers. Well, maybe it wasn't all that crazy, and it did help keep the mission alive through the turbulent 1980s. Politically, it may have been a very smart idea.

Another disadvantage to the Explorer Platform from our perspective was that the arrival of a successor experiment would terminate the XTE mission. This could happen



two years after launch, as this was the time NASA planned for the mission. However, with one’s own spacecraft, the mission might operate for many years. In fact, with this in mind, space astronomers had learned to design missions “without expendables” whenever possible, meaning its operation did not require refrigerant for cryogenic cooling or a gas supply for detectors or maneuvering jets, which could be exhausted after a year or two.

Astronomers would also design as much redundancy into the system as possible, so that one failure would not completely kill its data-taking capability. In this way, NASA might continue to support a mission financially for many years if the science yield remained high as determined by peer review. The EP could negate this possibility. However, since missions were often delayed, one could hope the experiment following XTE would not be ready until some years had passed. It was EUVE that was most likely to suffer the indignity of an early turnoff because XTE might be close on its heels.

This plan gained a lot of traction, and considerable effort went into reconfiguring the MMS spacecraft so it could take on XTE after the EUVE mission. This may have been a factor in the subsequent delays in the EUVE launch. It certainly raised its costs.

### *Painful scrubbing by Charlie Pellerin (1987?)*

As the XTE program reached the later 1980s, COBE was well on its way toward its 1989 launch and EUVE was not far behind with what became a 1992 launch. Serious funding for XTE was becoming a real possibility. The study of XTE (Figure 8) also led to a cost figure for the whole mission. As was typical, the total was deemed to be too large for NASA’s Explorer budget. This led Charlie Pellerin to call a meeting of the XTE PIs at GSFC with the intent of scrubbing the mission and hence its cost. Scientists are prone to improve their experiments thus increasing cost, and it is the job of managers to keep this tendency under control. The XTE as configured did not have much beyond the core requirements, so any scrubbing was likely to be quite hurtful. Charlie’s job was to convince us that it was, nevertheless, necessary.

Well, Charlie got us in that room and proceeded to tell us all the reasons why our mission was on the brink of cancellation. He painted a dismal picture of the state of the economy, of the public’s skeptical view about NASA and its expenditures, of congressional pressures and priorities, of NASA’s programmatic and budgetary woes, and the threat arising from skeptics who bad-mouth XTE. XTE, in other words, was no more than a fly on the wall in danger of being squashed flat at any moment, and so on and so forth. The ASM booms came in for particular ridicule (“like a bug’s antennae”) as did other elements of the payload.

Only if we could shave 20-30% off the mission cost did it have a chance to fly. (I forget the actual percentage, but it was not small.) The cuts had to be real; not just superficial reductions. Reductions in manpower without reduction in actual hardware were deemed non responsive; they were “only smoke and mirrors.” He wanted to see genuine reductions that very day. There was some merit in his plea, because in fact, our detector numbers and areas had inflated somewhat since our original proposals, at least the ASM had. There were two SSC assemblies in the GSFC model, whereas our original 1980 proposal had only one.

It was a superb performance; no Shakespearian actor could have done it better, and it scared us and convinced us cuts were necessary. Present, in addition to the three PIs were

one or two other scientists and a project person from each of our three teams (MIT, GSFC, and UCSD) as well as manager/project types from GSFC and NASA headquarters. I don't think the spacecraft people were there nor were they under this same pressure; the EP/MMS was mostly well defined through the EUVE program. Given the size of the demanded cuts, it was clear that the ASM and the HEXTE were both quite vulnerable, the latter more so than the former. However the loss of either one would seriously hurt the mission, making it difficult to survive a critical scientific review.

It is very difficult to commit murder (killing another's instrument) or suicide (giving up one's own instrument) in public. Charlie suggested the PIs and GSFC managers sit down together and work through the possibilities. The managers would be able to comment on feasibility of possible cuts, etc. However, I had learned long ago that difficult interpersonal negotiations take place more successfully in private when there is no audience. It may have been me, then, who suggested that the three PIs (Swank, Rothschild and Bradt, each with possibly with one or two of our scientific associates) retire to a conference room with no one else present. We were smart enough to understand feasibility without the managers. We then went into the small Building 2 conference room, closed the door, and looked around as if to say, “Well, what now?” while thinking: “Will my instrument survive this?”

Then, a remarkable thing happened, and I cannot remember who suggested or initiated it. Perhaps it was Al Levine, my MIT associate, who began to list the positive aspects of the UCSD HEXTE instrument. Rick Rothschild, the HEXTE PI, then picked up and started to list the positive aspects of the ASM and why it was essential to XTE. This was incredible; people were not defending their own instruments, but the other fellow's and the integrity of the mission as a whole. Absolutely amazing!

The following discussion was very constructive and non-confrontational. It was agreed that the loss of either the ASM or the HEXTE was completely unacceptable and would damage the mission irreparably. The loss of the high-energy capability would remove a major improvement over the Ginga mission. The loss of the ASM would blind the mission; it could not seek out sources when they were doing new and interesting things.

This wasn't getting our job done. There was no way we could not leave that room with nothing to offer Charlie. Thus each of us examined how we could cut the scope of our experiments. Each experiment took a hit. The PCA went from 8 to 5 detectors while retaining (or introducing?) small background detectors (PCA Jr.). The HEXTE went from 6 to 4 detectors in each of its two modules, and the ASM got rid of the two long booms and retrenched to one short rotating platform. Rick and I both clawed back a bit of the lost capability. Rick made each of his 4 units per module slightly larger so HEXTE lost only 20% of its effective area rather than 33%. The ASM, rather than cutting back to one pair of cameras on the one rotating platform, would add a third camera to the platform. This materially increased the sky coverage. This third SSC could be considered an “extra,” but its cost increment over the two-SSC system would be negligible, so it was easy to defend.

We thus emerged from the room with substantial cuts to offer Charlie, not as much as he claimed to want but enough. He accepted them at least until they were evaluated for cost, etc., and it appeared that XTE had survived to another day. As for the nature of the cuts, and the way we managed to claw back some of the cut capability, he was heard to mutter something like, “those guys are awfully clever.” I thought we were; we trimmed

the mission substantially, but without jeopardizing its fundamentals. Best of all, we did it as a coherent team, with no bitter contentiousness.

It occurs to me now that Charlie was probably as nervous as we were about the outcome of the meeting. He had a lot riding on getting this mission accepted and in orbit; that was his job. If we had proved to be a recalcitrant group of prima donna scientists who wouldn't pull back our goals, the mission could well have been lost. That would be his failure as much as ours. For our parts, we were all experimenters with substantial space experience. We had learned how to make technical and programmatic compromises in order to get our data. We had all learned the hard way that it is dangerous to push a system or an engineer too hard. We also knew that one had to draw a line in the sand to protect the integrity of the mission so it can withstand scientific scrutiny. Getting the right balance was, of course, the hard part.

### *A fateful CSAA meeting, Jan. 1988.*

Funding for XTE became more likely as the missions ahead of XTE gained traction in their The next step was to resell the mission to the national committees of our peers, again! It had been almost a decade since the 1981 ASC endorsement, and several prominent critics were still out there. With rare exceptions, I never encountered such criticism directly, but only heard about it. In perhaps two cases, though, I saw disparaging letters written to cognizant officials. In one case, the arguments were measured and sensibly presented, but the other was absolutely virulent. In that case, I only saw a retyped version with no indication of the author, though rumor associated a name with it, the name of a person highly visible in the NASA system. That possibility was so shocking that I still entertain the possibility that the letter was fraudulent or misattributed. The arguments were not new: old technology and science that was being upstaged by Ginga and a flawed selection process. I have misplaced my copies of those letters.

Not being a strategist at heart, I was rather unaware of the need for XTE to regain committee endorsements. Thus I had nothing to do with the agenda of the Committee on Space Astronomy and Astrophysics when it took it upon itself to review the case for XTE. (I was no longer on the Committee.) I became aware of it when Steve Holt, by now Director of Science at GSFC, called to ask me to attend a forthcoming meeting as support for Jean Swank who would be presenting the XTE case. Jean was and is a highly competent scientist who surely would do a fine job of presenting the science, but Steve knew her presentation style was rather laid back and subdued without the dramatic flare that some value (and others deplore). Thus, all in all, Steve felt it a good idea to have another voice in the room, especially one with, at that time, probably more visibility in the community than Jean. I thus agreed to fly to Washington for the January meeting,

This was to be a cakewalk for me. I had nothing to present and Jean was more than competent to do the job. This was stark contrast to most meetings we went to in the Washington area. Almost always, at such meetings, there was something big at stake – e.g., dollars or a mission. Inevitably my adrenaline would be flowing freely and my stomach churning. This time, all was calm. The committee members were academics from all branches of astronomy. The meeting promised to be pleasant and interesting. Well that was my mistake. After Jean's well-organized presentation, she was asked some questions which she competently answered. Then David Helfand of Columbia University, whom I had long known and admired (and still do), turned to me with a question I should

have been ready for. He asked, quite pleasantly, “Hale, can you give us an example of a ‘great discovery’ that XTE could with some likelihood make?”

Well, that question was totally fair, but I was not ready for it. I had been thinking of XTE as probing important unexplored phase space of timing, not of particular landmark discoveries it might make. Great discoveries are, of course, great because they haven’t been anticipated. Yet, plausible scenarios for major progress based on current knowledge are a must to sell a mission. The Lamb studies and our proposals were full of such scenarios, but many of them were explorations of known phenomena, not breakthrough unexpected surprises. What would knock us over with astonishment? If any answer was in my head, I might have said: “Seeing oscillations from hot spots of matter orbiting a black hole.” and I might even been right, given the later discovery by RXTE of kilohertz quasi-periodic oscillations from black hole candidates. But, without having examined that possibility more thoroughly for its feasibility, I might be vulnerable to accusations of naiveté by knowledgeable committee members.

So, what was my response? It was reminiscent of Ted Kennedy’s when he was asked why he wanted to be President of the U.S. His and my answer can best be synthesized with “Duuuh?” though I probably muttered something general about learning about the physics of neutron stars and black holes. Jean probably tried to recoup for me, though I forget what transpired in my intense embarrassment, but her answer did not exactly reveal the holy light either.

That was probably the most embarrassing moment of my career. I was really, really down about how I had let down the entire XTE community and possibly lost the whole mission. I can remember discussing it with Ron Remillard very shortly thereafter during a long, long drive in Australia en route to Coonabarabran from Sydney for an observing run at Siding Springs. Ron was very supportive, trying to buck me up with reassurance that I had not totally destroyed all our XTE efforts. I think he was doing the driving and I am glad this did not distract him from staying on the left side of the road, if, there was in fact, a center line to guide him, which was not always the case on the smaller Australian roads.

My failure of preparedness reminds me of another nugget that is helpful in the space business. In the mid seventies, we had just finished presentations of the several experiments on the reconstituted HEAO program at Marshall Space Flight Center. I remember this as being quite important for the HEAO-1 program because its instruments had just been reconstituted, so our presentations were a kind of defense of the newly defined mission and were thus undertaken quite seriously. On the other hand, the HEAO-2 mission (later *Einstein*), was fully accepted and in place. Nevertheless, Giacconi and collaborators gave a beautifully organized and elegant review of their mission that obviously had taken a great deal of work. I asked Herb Gursky why the HEAO-2 team had gone to so much trouble when the mission was already in the bag. He told me, simply, “Hale, you’re always selling.” I should have taken that more to heart at that CSAA meeting.

### *The NATO Workshop in Cesme, Izmir, Turkey (April 1988)*

In April I attended a NATO Advanced Study Institute on “Timing Neutron Stars” in Cesme, Turkey. This, of course was right down the alley of XTE science, and I gave a talk on “Future U.S. X-ray programs related to timing neutron stars.” Despite the elegant

title “NATO . . . Institute,” this was actually your normal science workshop or symposium where attendees present talks on their research and listen to others’ talks in a darkened room. There was time between sessions, though, to admire the waters of the Aegean Sea and to visit the rug shops downtown.

Let me point up the value of such conferences a bit. In preparing for such conferences, one is always stressed about getting one’s paper ready for presentation. This often means a rush to complete some aspect of your research so the presentation can include fresh new and hopefully exciting results. It is like an exam in school. Going to such a conference means pressure before during and after (when the talk has to be written up for the proceedings). It is not all about lounging around the pool.

Another advantage is that conferences provide the time and place for informal discussions among scientists. Ideas are traded and argued, collaborations formed, papers are begun over coffee, etc. This pushes the science along and helps the taxpayer get maximum science value for his or her dollar.

As an illustration of such progress, I recall vividly three conversations with colleagues at this Cesme meeting that related to XTE.

(1). Fred Lamb patiently explaining to me the “Loose screw” theory of spin-rate fluctuations in neutron stars. “If you kick it, it will respond differently if there is a screw loose.” Now this might seem simple to the lay person, “Who wouldn’t understand that?,” but I had trouble relating it to an actual neutron star with torques being applied by accreting matter. I remember how patient he was with me going over this as we sat on a pile of rugs drinking tea in one of the Cesme rug shops.

(2) Present at the conference was a quite eminent scientist who was, so I had been told, a vocal critic of XTE. I had also heard that he had been particularly irritated at some comments I had made in some meeting – perhaps pertaining to the utility to my group of the small amount of XTE funding during the study phases. I do not recall making any such comments, but it could have happened and it could have been irritating to others who had no such funding. I thus asked this person, whom I respected and liked, to sit down with me for coffee or lunch to talk about XTE.

I started by reassuring him that I consider him a much better scientist than I, and that I considered my efforts on behalf of XTE to be more of a service to the community than of benefit to me. I also pointed out that XTE was nearing reality in NASA’s planning and that killing it “because it would not be worth the \$200M cost” would not release the money to another astronomy project one might consider more worthwhile. It would be a lost opportunity for astronomy. I further asked him to look at it from the community viewpoint and to set aside any personal feelings he might have about me. All this was received graciously. I left with a warm feeling that we had reconnected.

I have just now (2013) was reminded of the existence of a letter this scientist, not an x-ray astronomer, wrote to Charlie Pellerin only a few weeks later challenging the usefulness of the XTE mission. Ouch! (I had been forwarded this letter at the time, but have forgotten receiving it.) Fortunately Charlie chose to follow the advice of the CSAA and many other prominent scientists who believed the mission would probe important fundamental science questions.

(3) At this time, I was still reeling from the CSAA debacle (my view) and had been mulling long and hard on the most cogent arguments for advocating XTE. It was at Cesme, in the line at a cafeteria-style restaurant – with all those wonderful Turkish

selections – that my friend, Jan van Paradijs, now sadly deceased, made a great point. He simply mentioned to me that the *dynamical time scale* for matter near neutron stars was in the millisecond range, and that that should be the fundamental argument for XTE because it was designed to study such time scales. This time scale is the “free fall time” for matter in the presence of gravitational matter. It is proportional to the inverse square root of the matter density. The greater the density, the shorter the time scale.

Now I knew that millisecond time scales were important for neutron stars and black holes; for example orbital times were about 1 ms, and I knew about the dynamical time scale from my teaching astrophysics courses. However, the general applicability of the dynamic time scale to XTE’s objectives had not occurred explicitly to me. It says that *any* gaseous motions in the vicinity of a neutron star or stellar black hole with the density of a neutron star *must* occur on a time scales of milliseconds. XTE would be observing such motions because x-rays originate directly from the extremely hot gases in the vicinity of such stars. The “dynamical time scale” is a much more fundamental and general concept than the more specific “orbital time scale” or “free fall time.” In short, it was a basic fundamental argument for XTE.

From then on, if there was some erudite conversation about the science that XTE may or may not be able to do, possibly so erudite that possibly I could barely follow it, I could interject in an offhand way the comment that “and, of course, the dynamical time scale of matter near a neutron star or black hole is right in the range XTE can study.” I used that successfully in more than one critical committee meeting. You could almost feel everyone pause and think: “Oh, yeah; that’s so, isn’t it.” Thank you, Jan, for that.

I can hardly remember some of the places I purportedly gave talks in those years. However, I will never forget Cesme and nor will I cease thanking Hakki Ogelman, also deceased, for organizing it.

### ***Report to the CSAA (June 1988)***

The response of the CSAA at their January meeting, just described, was quite sensible. They felt that they needed a more in depth review of the science XTE could address. The report would be due at their meeting in five months, in June (1988). This really focused our minds; it was clear that the XTE program was on the line. Without CSAA endorsement, the mission was dead.

We planned to prepare a written report on the science that would be presented. This may seem like *déjà vu* after the comprehensive documents put together based on the Los Alamos and Taos workshops, but they were old hat by now, dating from 1979 and 1985 respectively. In the meantime, for example, Ginga had been launched and was making important progress and discoveries.

In preparation for that report, Steve Holt proposed that we go to the community for letters of support, which we did. On March 14, we wrote 45 of our colleagues in astronomy and astrophysics asking them to write how they would use XTE to further their own science interests. We received 39 responses with what amounted to science proposals for observations of all types from cataclysmic variables to black holes, supernova remnants and AGN. They showed the breadth of interest and the richness of the science in a concrete way; no smoke and mirrors here. The writers included: M. Burbidge, M. Elvis, E. Feigelson, R. Giacconi, D. Lamb, F. Lamb, A. Parmar, H.

Ogelman, N. Shibasaki, I. Tuohy, M. Urry, J. van Paradijs, and M. Weisskopf. Most were from the U. S., but Dutch, Australians, and Japanese were also represented. Together they made a powerful impression. These letters would become a part of the report to the CSAA. Steve sure earned his pay the day he suggested this.

The body of the report came together with the writing help of experts in the several science areas, and it emphasized the actual observations that would probe defined questions. I believe Jean Swank, as Project Scientist, took it upon herself to organize the scientific contributions, as she did in so many times after launch. The essence of the completed report was headed: “The Key Scientific Objectives of XTE,” which consisted of seven objectives such as the “Structure of Accreting Neutron Stars” and “Behavior of Matter Close to Black Holes.” Each of these was accompanied by one to three scientific topics, for a total of fifteen, e.g. “Angular Acceleration Fluctuations” and “Neutron Spin Frequency,” each with specific observations XTE could carry out. There were also lesser sections on “Multiwavelength Science.” “X-ray missions of the Next Decade,” “XTE Community of Users,” etc.

The report thus far amounted to 66 pages, quite a bit for a committee member to read in its entirety. To make the report more accessible, we added a much more compact “Summary Report” which amounted to 14 pages. Here we synthesized each of the 8 objectives (we divided the seventh) and pulled out and specifically labeled each “Observation,” of which there were twelve. The 14 pages were more than we had hoped, so we preceded it with a “Synopsis” which shortened the argument to three pages. We thus had a version for everyone, except for those who would like a one paragraph abstract, which we did not have. This Synopsis emphasized very briefly the actual observations XTE would do. For example, I present in its entirety the text under “Behavior of matter close to stellar black holes,”

“XTE will determine the character of millisecond variability (aperiodic and quasi-periodic) which will provide strong diagnostics of the innermost regions of accreting black holes, including possibly (quasi-)periodicities from relativistic matter in the innermost stable orbits.”

Preparation of the report generated a lot of enthusiasm and new confidence in the merit of the mission. I remember the final days of preparation working on it at GSFC. I helped with the Summary and Synopsis. There was some chuckling about adding a third layer to a document that we hoped (probably in vain) the Committee members would absorb in its entirety. The entire bound report was 92 pages. In addition, it included the 39 letters with observation “proposals.” These letters were not circulated with the more widely distributed copies because some of the ideas in them were proprietary. The document made a powerful convincing case for the mission. We were all quite proud of it.

At one point, Steve came by and we were discussing how the presentations would go. Steve suggested that the report should not be passed out to the Committee until *after* the verbal presentation because, otherwise, the committee members would busy themselves perusing it and thus not focus on the presentation. This we did for the HEAMOWG and possibly for the CSAA.

We had been planning, I believe, that I would present the case to the HEAMOWG before Steve presented the case to the CSAA. The former would be a kind of rehearsal for the CSAA presentation; the HEAMOWG, consisting of high-energy astronomers was presumably an easier sell, but the CSAA was the big deal. Since I had worked hard on the document and was fully familiar with the science and observations described therein, I

was raring to go before the HEAMOWG. I also thought it would be efficient for me to go before the CSAA with the same pitch, instead of Steve who would have to familiarize himself with some of the details.

I had the temerity to suggest this to Steve. We were in the hallway in Building 2 at GSFC, and Steve became really upset - perhaps angry would be the word - and possibly I responded similarly. When I saw the emotions ratcheting up, I suggested that we continue the discussion in private, knowing that privacy can calm emotions and pointed to an adjacent office. As we made steps toward the office, Jean Swank who was standing nearby bodily moved between us, though we were not in bodily contact, calling out, “No, No, No!” I or we had to assure her that this was not a western bar-room fight heading outside for fistcuffs or worse; we just needed privacy to calm ourselves down.

Once inside the room, Steve explained to me that Charlie Pellerin had asked him to give the presentation and there was no way he would back out of that. I, of course, readily withdrew my suggestion, and, in a matter of a few minutes, all was again sweetness and light between us. Steve was nice enough not to mention my shortcomings of the previous CSAA meeting as a factor, and maybe it wasn't. But then again that may have been why Pellerin wanted Steve to do it. After all, Pellerin had a lot riding on this too.

My presentation to the HEAMOWG, on June 2-3, 1988 went quite well I thought. Given the beautiful and organized case made in the report, it was not difficult as my talk tracked the report closely. One member, in the question period, commented that the presentation was so effective that Bradt should make the presentation to the CSAA also. I appreciated the comment, but countered that that was not an option; I was not going there again! I believe Steve gave an overview of the program to the HEAMOWG as an introduction or follow-up to my presentation.

I was not at the CSAA presentation, which took place on June 23. Steve gave an overview and summarized briefly our report. This was supplemented by three short talks by three members of the wider scientific community: Fred Lamb (who talked on neutron stars), Jeff McClintock (black holes), and Richard Mushotzky (active galactic nuclei). Each talk was 15 minutes. The committee then gave XTE its strong endorsement. This gave NASA the assurance it needed to “start” the XTE program, i.e., to release the money for the design phase.

All in all, the Report to the CSAA was a hugely beneficial exercise. It crystallized the case for XTE in the current era and got a large team of scientists thinking about the science they could do with it. Would it have been the same if that earlier meeting in January had gone better and resulted in a possibly less enthusiastic endorsement? Possibly not. I would like to believe that the written report to the CSAA carried weight in subsequent high level discussions, and it might not have existed had the endorsement come earlier. Perhaps my blunder at the January meeting served a good purpose in the end.

The report cover shows a sketch of the XTE on the Explorer Platform (Fig. 9) as envisioned by me, based on no engineering considerations. The ASM is restricted to one short rotating mount, and the PCA and HEXTE have the reduced areas that resulted from the Pellerin shakedown. Notable is that there are small detectors with offset fields of view from the PCA for measuring background. In house, they were known as “PCA, Jr.” As we see below, they were later removed.



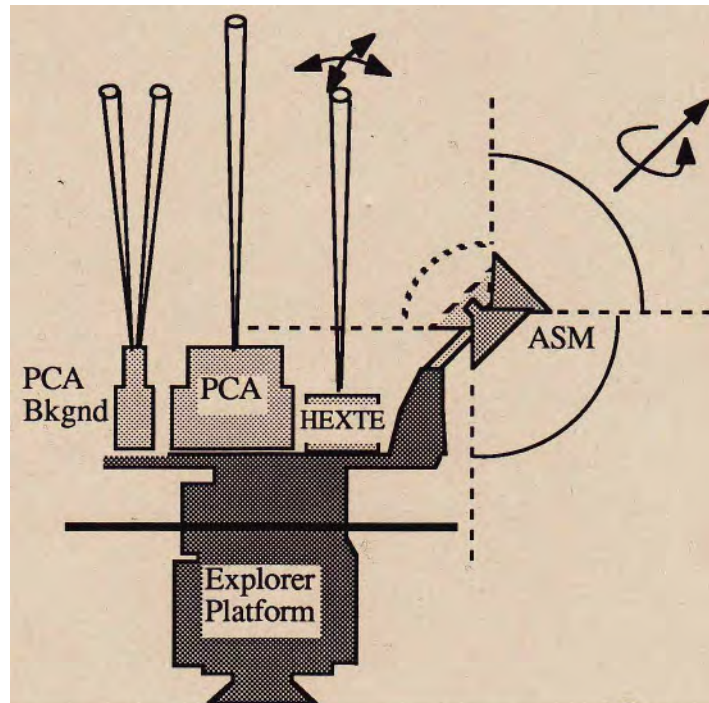


Figure 9. Schematic of XTE on the Explorer Platform as envisioned by Bradt, after the Pellerin de-scoping. Ref. COSPAR XXVII, Espoo, Finland, 1988; Adv. Sp. Res. 10, 297 (1990). This talk was given in Finland by Claude Canizares because I was getting my coronary arteries expanded at the time.

### *A health issue*

It was in the summer of 1988 that I was diagnosed with coronary artery disease. It was cleared up with an angioplasty procedure wherein the coronary artery is opened up with a balloon on the end of a catheter. At age 58, I thought that was the beginning of the end of a long downhill trend. Twenty-five years and one bypass operation (in 1995) later, I am, luckily, still going strong.

### *A 100% Guest Program*

In the midst of all this, Steve Holt came up with another winning idea, namely that every bit of XTE data should be made available to the general User program. The PIs would reserve no data and would compete for observing time along with others.

In the early days of x-ray astronomy, all the data from an experiment belonged to the institution that proposed and built the experiment. This was in the classical tradition of physics experiments. However, the morality of this soon became suspect because these experiments were built with public (NASA) funds and such funds also paid for the launch vehicle and the data analysis efforts. Also, maintaining public support for such missions argued against a single investigator or institution reserving (hogging) all the data. Why should investigators in Massachusetts have all the data that was paid for in part by

citizens in Michigan when there were highly competent scientists there who could make good use of it?

“Guest” programs began in a small way in the 1970s with outside investigators collaborating on observations with the prime observers, but then NASA began to set aside small amounts of funds and observing time for Guests, and review committees began to demand that proposers include guest programs in their proposals. The size of those programs grew and became the dominant portions, but always the PIs retained a significant part of the observing time. Data were generally reserved to the winning proposer only for a year or two, after which time they were made public. Guest programs required extra work, time and money, because the hardware systems had to be well documented and the data analysis programs transparent and usable by visiting scientists. Gone were the days where the prime investigator could fuss with the idiosyncrasies of his or her data and take his or her time in publishing results.

Steve’s proposal was unique in that we the principal investigators would retain *no* exclusive rights to any portion of the data at any time. What about all the creativity, sweat and tears we had put into our experiments, from conception to launch? Didn’t we deserve a payoff of having at least some portion of the observing time guaranteed to us? Well yes, but didn’t we also want to sell the mission? Hadn’t the whole exciting experience of developing an instrument for flight with NASA funds been rewarding in its own right and hadn’t it brought additional young scientists into our organizations that enlivened the intellectual environment? And finally, our proposal did not shut out the PI teams, they would be able to propose competitively for observing time with the rest of the world. Since they knew the instruments better than most, they would be in a strong position competitively.

After receiving Steve’s call wherein he broached this possibility, I mulled on it awhile and discussed it with my younger colleagues who had put large fractions of their recent years into developing our XTE instruments: Alan Levine, Ed Morgan, and Ron Remillard. What was fair to them and to the community; what was right? Without much difficulty, we came down on the side of “100% Guest.” Ron Remillard in particular was always sensitive to, and vocal about, the viewpoints and rights of scientists worldwide, whereas my tendency was to be more concerned about the rights of the PI teams. However, even I was quick to see that 100% guest participation would be beneficial for XTE in many respects.

It was also possible, we realized, that a competent individual on a PI team could well obtain *more* observing time than he or she might with guaranteed time. In the standard model, no more than ~25% of the total observing time would be available for all the PI-team observers, but divided among perhaps 15 PI-team scientists at the three institutions, it could be quite limiting. With the opportunity to compete freely for observing time, there would be no such limit.

The other PI teams agreed with this approach, and this plan was clearly enunciated in the CSAA report. We reserved only the first 30 days for calibration of the instruments, and the data from those observations were released to the community. All the observing time thereafter would be allocated by a peer committee based on competitive proposals. The ASM data required rapid analysis for alerting us to transients, etc. and not being target specific, were not particularly amenable to observing proposals. We thus planned to carry out rapid analysis to obtain source intensities and to make the light curves publicly available in near real time. These data products would be available to all to use

as they saw fit for planning PCA and HEXTE observations, for showing context in papers reporting their pointed results, etc.

### **Phase III: Implementation (1989–1995)**

#### *Real money; contracts signed (1989)*

The instrument teams were brought under contract in 1989. Each institution had to provide detailed budgets for design and construction and for two years of post launch activity. The MIT contract for this period of time came to \$25M, a huge amount from my perspective. Of course, it would be made available to us only gradually over the years as it was needed. It would support several scientists, namely Ron, Al, and Ed, a graduate student or two, as well as an engineering and technical team. We were fortunate that Bill Mayer would be our (MIT) project manager with Bob Goeke, electronic engineer, as his deputy. These two had long experience with managing large space projects and knew the intricacies of planning the details of complex jobs with PERT charts, financial spread sheets, etc., and most important knew how to deal with the NASA engineers, managers, and bureaucrats.

We were fortunate to have such a competent team. My scientists had an eye for and appreciation for good engineering, and Bill and Bob an appreciation for the science we were trying to accomplish. In fact, Bill had earned his physics doctorate as my student with a rocket flight experiment. When problems arose, the solutions were worked as if we were one group; I recall no confrontations between the science and engineering teams. Despite having been my student, Bill was in no way beholden to me. He was his own man and always seemed sure-footed in his view of the path to follow. I usually saw him as my senior in his broad view of the managerial and technical issues facing us. Bill would bring me into play when, on occasion, he needed PI clout to make a point with NASA or the MIT administration.

For example, as we were negotiating the contract with GSFC, Bill approached me to explain that there was a clause in the standard NASA contract that we at MIT did not want included. It was known as the “Technical Direction Clause.” It allowed that NASA could direct MIT how to proceed if a technical problem arose. While this made sense if NASA was purchasing a star tracker, we felt it subverted the intent of a “scientific investigation.” The scientists carrying out an investigation were the best judges of how to get around a problem while still maximizing the science yield. A NASA direction was more likely to be guided only by programmatic issues (cost, schedule, and functionality) to the detriment of the science. Of course, the scientists’ solution had to comply with budgetary and schedule constraints, because NASA could refuse to support overruns. The GSFC project office claimed that, of course, they would factor in the science goals, but Bill assured me that the Directive Clause could lead to real harm down the road. He needed visible PI support to get this removed from the contract. I bought the argument and weighed in with my superficial knowledge of such matters, and we did succeed in getting the clause removed.

Bill Mayer recently gave me more background on this, as he recalls it:

“The existing NASA Federal Acquisition regulations at the time exempted Universities from the “Technical Direction Clause,” so we were surprised to find it in the

proposed XTE contract. When we pointed out to the GSFC contracting officer that MIT is a University (and hence exempt), he admitted that, without consulting us or UCSD, he had gone to NASA Headquarters and obtained a waiver to include the clause in the XTE contracts (and it would be embarrassing if he had to go back and ask Headquarters to undo the waiver). We tried to negotiate a middle ground but he wouldn't budge. We got the MIT contracts office on our side (they didn't want a precedent to be set), and they sent a letter to him saying MIT wouldn't sign a contract with the clause included. UCSD did the same thing. In the end, he had to go back to Headquarters and get permission to remove the clause. He was replaced shortly thereafter.”

Such confrontations were irksome to our well-intended management team at GSFC as it reflected negatively on their ability to hear out and act on our concerns. Nevertheless, we knew that, sometimes, bureaucratic pressures on them could sometimes override good intentions, and we, being the managed rather than the manager, felt we needed formal protections for the occasional crunch. Indeed, a few such issues did arise during Implementation that caused us to be thankful the Technical Direction Clause was not in our contract.

*Another painful scrubbing, by George Newton (~April 1990)*

As the instrument teams began their design efforts, in 1990, the XTE was still to fly on the Explorer Platform with a change out of instruments (EUVE to XTE) to take place in the Shuttle bay. The risks associated with this complex operation were becoming apparent upon closer examination. Also, the 1986 loss of the Challenger and subsequent hiatus of Shuttle flights made clear that dependence on the Shuttle being operable carried its own risk.

An alternative plan was thus put forward, namely the use of a dedicated spacecraft (S/C) launched by a Delta rocket for XTE. A study by GSFC of this plan was initiated. The plan was that GSFC would build the spacecraft and integrate the experiments with it making use of its in house team of engineers and technicians. At the conclusion of the study, there was a review of the concept led by George Newton of NASA Hq. As usual, the total cost came to a higher value than had been hoped and there was again great pressure on both the S/C people and the instrument teams to bring that cost down. If we were to escape from the Explorer Platform, it was essential that the dedicated S/C plan be made attractive to NASA Headquarters.

I remember being around a big table, possibly around April 1990, with everyone being on the spot to give up something to get the cost down. It was made clear again that “Smoke-and-mirrors savings are not acceptable,” i.e., reductions in man-hour estimates did not cut it. Only giving up real hardware or complete tasks would count. This meeting included spacecraft engineers as well as scientists and headquarters types.

At stake were the integrity of the science experiments and the spacecraft systems that we desired for an effective mission, i.e., large PCA sensitive area, rapid maneuverability, nearly continuous telemetry, (nearly) all-sky pointing (which required rotating solar panels), and a high telemetry rate. The mission as designed by the GSFC engineers carried two antennas for access to NASA's telemetry satellites (TDRSS), which would allow the XTE to contact TDRSS from any XTE orientation and from almost any point in the XTE orbit. We could maneuver the PCA to any source, e.g. a transient, at any time at

any place on the sky without losing our telemetry link. This was very important attribute for our mission.

At the meeting, the pressure was huge to give up one of the two antennas and the transmitter that went with it. It was clear, though, that the GSFC spacecraft engineers did not want to lose it either. They considered the two-antenna system to be elegant and important for simplicity of operations; one could download data without maneuvers. Nevertheless the headquarters people kept the pressure on, hinting that the alternative might be to remove an experiment. We took a break for lunch, and Rick Rothschild (PI of HEXTE) was behind me in the lunch line. He leaned over and quietly said, “Hale, I think we are going to have to give up that antenna and transmitter.” Rick knew that his experiment that would be first in line for elimination.

My response was almost (or did I really do it, I am not sure) to grab him by the shirt collar with my face inches from his, to vehemently say, “Rick, don’t give up now; that antenna is terribly important, and even the Goddard engineers are in favor of it!” Somehow, with his stiffened backbone, and some other giveaways, we survived with the two transmitters and antennas. Rick recalls the rescued transmitter as a high point of those negotiations. We were glad we had it because, well into the flight, one of the two transmitters failed.

Unfortunately, reductions of the spacecraft systems did not reach the cost level desired by George Newton. The rest was up to the scientists. Again we scientists went off to a private room to see what we could come up with. From the previous de-scoping, the instruments were at the edge of losing their claim to being a major step forward, and that in itself could kill the mission. So, we were on the spot. Well, we managed to find some meat for the lion.

Jean Swank gave up the “PCA Jr.” background detectors because her group had come to the conclusion that background could properly be determined and subtracted without them. Rick gave up building an engineering unit of his experiment, something he considers to have been a mistake to this day, because it would have revealed dead-time difficulties that later had to be tolerated in flight. MIT gave up the purchase of additional spare detectors for the ASM. This was a highly dangerous move because the detectors were technically complicated and possibly spares would be essential for fixing problems. However, we had nothing else to offer the cost-scrubbing gods that was “real” and not “smoke and mirrors.”

It turned out that we could and did order the spares while absorbing the funding cut elsewhere; it was “smoke and mirrors,” but it was a real cost reduction. We were able to do this because of Bill Mayer’s costing expertise. There are two kinds of reserves, those that are open and visible and required by NASA, and those that are not readily apparent. It was the latter that made the spare purchases possible. Our managers at GSFC would not be aware that we purchased them because, while they reviewed our financials monthly, they did not see our purchase orders because we were an independent entity (a university). Ron Remillard was a junior member at these negotiations and remembers being appalled at the loss of the spare detectors and quietly asking Bill how we could possibly get by without them. Bill responded with words to the effect, “Don’t worry, you’ll get your counters.” Thank goodness we did; we needed every last one. All in all, we survived this scrubbing, but we were sure getting thin.

The lack of forthrightness engendered by the system is disturbing, but how else could the system work? The pressures on the management to hold costs is severe as is the need

to preserve the scientific capabilities on the part of the instrument builders. The fudging of the truth is insidious when organizations fudge the truth by claiming they can proceed within budget while knowing it to be impossible. In our case the fudging was in overestimating the true cost so we had some hidden reserves. And indeed, we did deliver the instruments to NASA on budget. However, arguing that we could lower cost without giving up the spares would have been dismissed as “smoke and mirrors,” and justifiably so because, how could anyone outside the project know whether an extra six months of technician labor was needed or not. Bill and Bob knew, but they would not have been believed.

The instrument complement on the Explorer Platform after this meeting is illustrated in Fig. 10. The PCA Jr. detectors are missing.

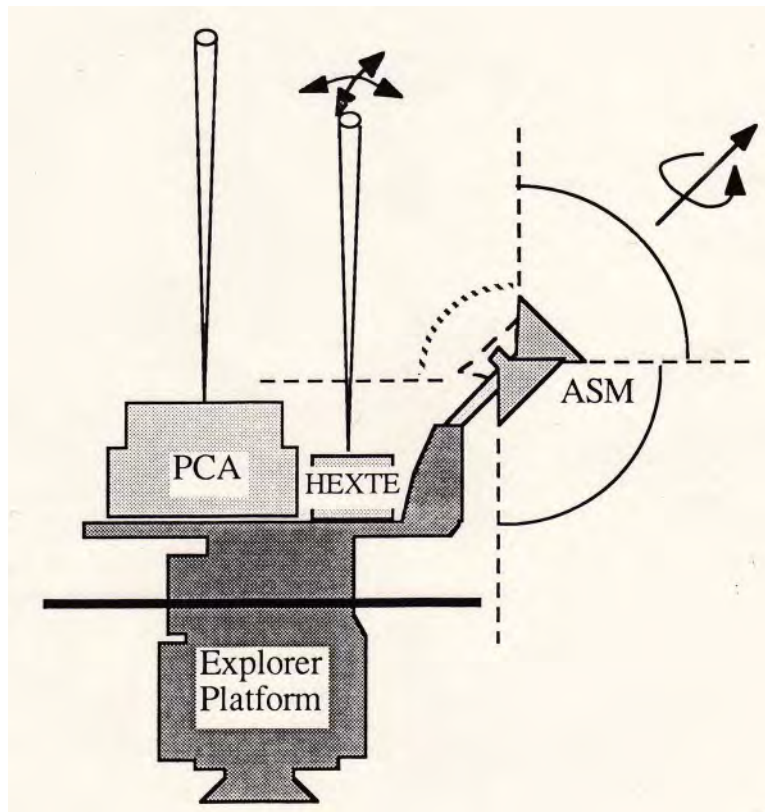


Figure 10. Instrument complement on the Explorer Platform after the de-scoping by George Newton in 1990. Note the missing PCA background detectors that are seen in Figure 9. This was an easy design modification to make because this configuration was solely a figment of my MacDraw program.

Hopefully, we would not fly on the Explorer Platform, but on the dedicated spacecraft under discussion at this meeting. It would be ideally matched to our goals; one couldn't ask for more. It was quite autonomous. For a maneuver, it was only necessary to command it to the new attitude, and onboard software would take care of solar panel rotation, antenna commands, and the roll-yaw-pitch commands while avoiding PCA exposure to the sun. And most important, thanks to the rotatable solar panels, it could detect and maneuver the PCA to transients almost anywhere in the sky at any time.

The spacecraft would be built and integrated with the instruments right at GSFC, thus making use of a first rate team that happened to be available. This would obviate the complexity of searching for a S/C vendor. The downside is that building a new S/C for XTE would obviously raise the price as compared to reusing the EUVE spacecraft, the Explorer Platform. But the Shuttle costs associated with the launch and change-out weighed in on the other side. We hoped and prayed that NASA would adopt the Dedicated Spacecraft model for XTE.

In July 1990 I gave a paper at a COSPAR meeting in The Hague wherein I showed the NASA figures illustrating the scary change-out procedures if we were to use the Explorer Platform. The procedure was this: EUVE would be carried into orbit mounted on the EP by the Space Shuttle. It would be released into orbit and would carry out observations for the scheduled one or two years. When the EUVE mission was complete and XTE ready, the XTE instruments would be carried into orbit in the Shuttle as a single package called the Payload Module (PM) attached to a Flight Support System (FSS) which was designed to fit into the Shuttle Bay. Once in orbit, the Shuttle would rendezvous with the EUVE/EP and capture it with the Remote Manipulator System (RMS), which would mount it on the top of the FSS. The RMS would then remove the EUVE payload from the EP placing it (EUVE) on a temporary mount on the FSS (Fig. 11). Then the RMS would detach the XTE PM from the FSS (Fig. 12) and mount it onto the the EP (Fig. 13). The XTE/EP would then be released into space. The EUVE would then be securely mounted on the FSS and returned to earth. Got that? Or should I run through it again?

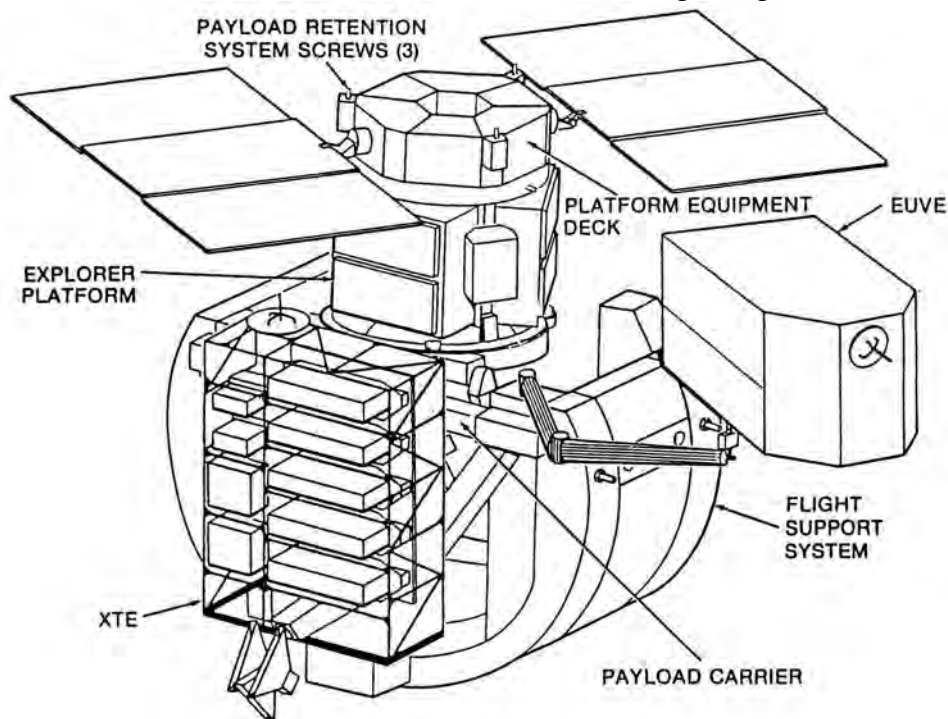


Figure 11. Intermediate stage of the change-out of EUVE and XTE in the Shuttle Bay. The FSS fits cross-wise into the aft end of the Shuttle Bay. XTE is in its configuration for the Shuttle launch. The EUVE+EP has been captured, and the EUVE science payload has been removed from the Payload Equipment Deck of the EP and is on a temporary mount. Drawing by Goddard Space Flight Center. Presented at COSPAR, The Hague, July 1990 and published in Bradt, Swank, and Rothschild, *Adv. Sp. Res.* 8, 243 (1991).

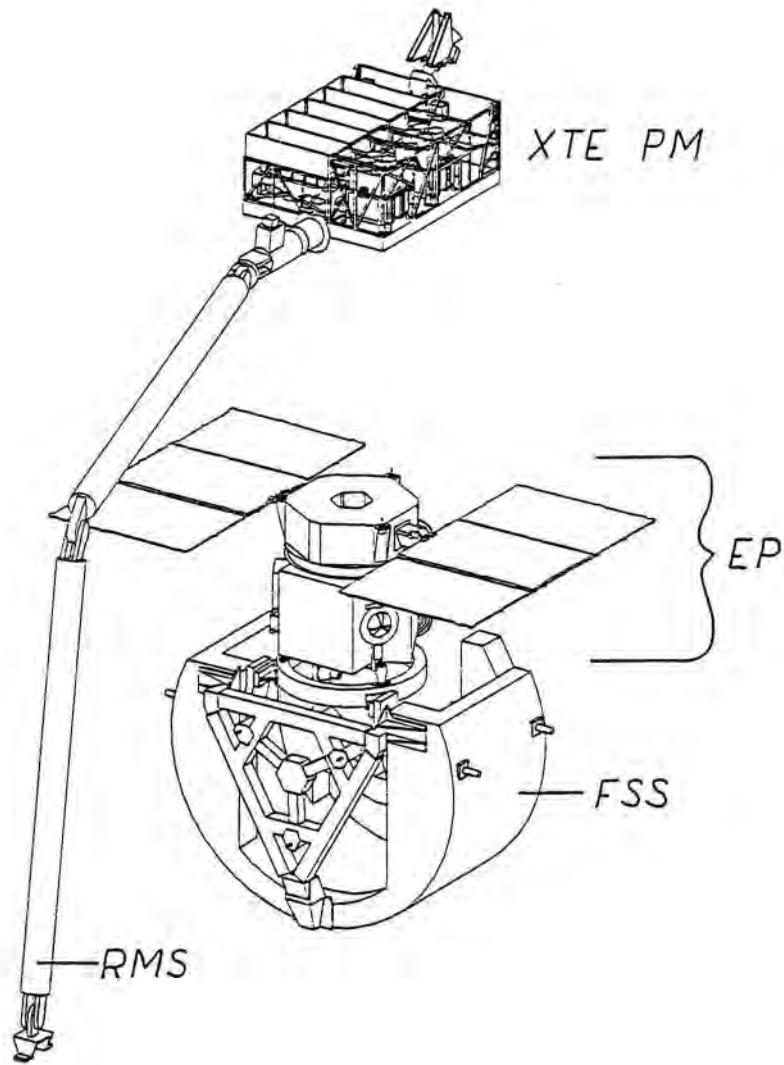


Figure 12. Later stage of the change-out. The XTE payload module is being moved to the Explorer Platform. For references and definitions, see caption to Figure 11.



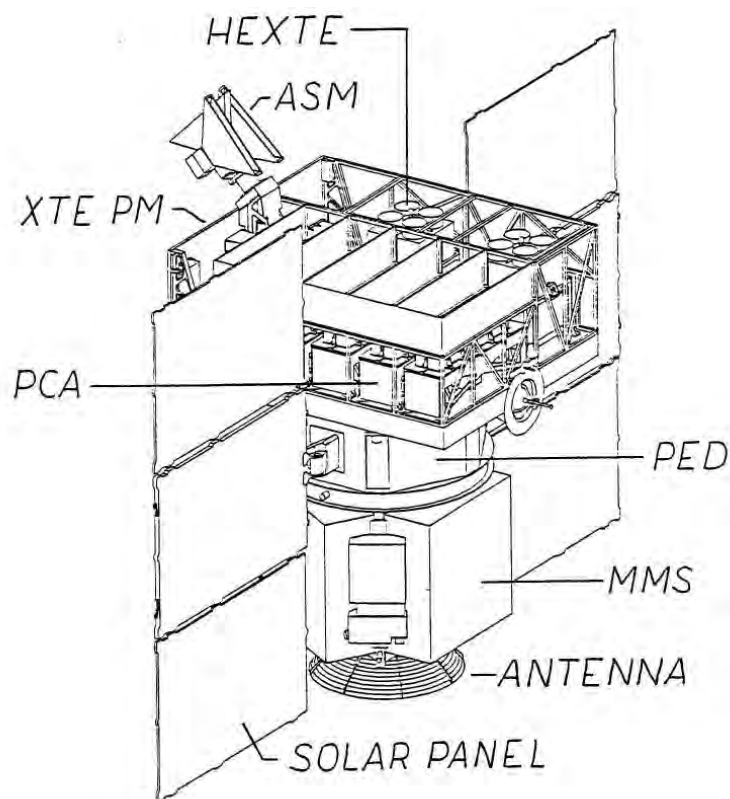


Figure 13. XTE payload module (PM) on the Explorer Platform (EP) as it would be launched into space. The core of the EP is a Multi-mission Spacecraft (MMS). The Payload Equipment Deck supports the rotatable solar panels. For references and definitions, see Figure 11.

I like to think that by publishing these sketches, I helped make clear that this is quite a complicated and potentially risky deal. It sure made that point clear to me. I wasn't so sure it was any more risky than other Shuttle related tasks, but I did want people to recognize it for what it was. I was actually a little in awe at the thought of our experiment going through such gyrations.

At one point, during the long interval of several years when this was the unquestioned way the mission would be carried out, I brought up the topic to Riccardo Giacconi with an air of concern in my voice. He asked me why I had allowed such a seemingly far fetched (he might have said “ridiculous”) plan to go forward. After all, Riccardo is one who can tell the government how to do something, not the other way around. Well, perhaps I should be faulted for that, but on the other hand, this was Charlie Pellerin's way to keep the mission alive when there was no money for it: namely he would ride the “platform in space” craze while it was popular, and get off it when Shuttles start blowing up and the costs of joining the manned program become evident and painful. So perhaps my inaction on this point could be viewed as smart - or perhaps I was just lucky in having such really good people pulling for XTE at NASA.

Later that year (1990), Jeff McClintock wrote a letter to Charlie Pellerin making the case for XTE in view of threatened cuts to the Explorer budget. The Explorer Platform preparation for EUVE was having difficulties also. EUVE would not launch until 1992.

*Dedicated Spacecraft (1991)*

In the early months of 1991, NASA Headquarters was in the midst of deciding whether XTE would follow EUVE on the Explorer Program or whether a dedicated spacecraft was preferable. By this time, the Shuttle was seen as a clear source of high costs due to its being a manned program and delays, which further escalate costs. Charlie Pellerin, by this time was pushing the dedicated spacecraft option, despite the (superficially) higher cost, and we scientists were fully behind it. I wrote letters to the GSFC Director, John Klineberg, and to Len Fisk (Associate Administrator for Science at NASA Hq.) extolling the value of that plan. In July, GSFC made a presentation to Len Fisk, Associate Administrator for Science at NASA Hq. on the two options, and later that month Fisk endorsed the dedicated spacecraft option. This was a huge win for the XTE concept.

It turns out that Ed Stone of JPL played a positive role in the decision by writing a letter to Fisk. Ed was an investigator and possibly the mission scientist on the Advanced Composition Explorer (ACE), which followed XTE in the queue, and he was also Director of Jet Propulsion Lab beginning this year (1991). It is possible that ACE was scheduled for the EP also. His support of a dedicated spacecraft was very important for XTE as well as for ACE.

Ed was a cosmic ray researcher whom I had known from the HEAO days. Since then he had risen to quite eminent heights through his directorship of the Jet Propulsion Laboratory (JPL) and his visibility on the Voyager satellite and Keck telescope programs. I ran into him at the Mauna Kea Hotel on the Big Island Hawaii during the time of the July 11, 1991 total solar eclipse. I was the hotel eclipse lecturer, and he was there with a group of Keck donors and was busy selling the idea of a second Keck telescope as well as explaining eclipses. His letter to Fisk apparently followed this visit, but I had never once mentioned the issue to him at Mauna Kea. We had chatted once or twice but never had a one-on-one talk. His support, which was very important, was totally independent of any machinations by me, in case any of you out there might be wondering.

The solar eclipse and the dedicated spacecraft decision that same month made July 1991 a banner month for me. Morale among the XTE teams jumped to new heights. We had a spacecraft to die for (if you were a timing enthusiast), three instruments under contract and ready to go. I sent off a thank you note to Charlie with my heartfelt thanks with copies to others I had been pestering. It was he, to my knowledge, who turned around the whole thinking. Again, he saved XTE from extinction. Of course, everyone involved deserved credit, the scientists, engineers, managers, and administrators at GSFC and the PI institutions, who all helped to put together a viable mission within reasonable cost guidelines. But to my thinking, Charlie deserves the first XTE medal.

We finally now knew the configuration of the mission and had contracts in hand, so all systems were go. However, the community familiar with the role XTE would play in astrophysics was not great. Accordingly we scientists prepared a glossy 44-page document to distribute at the January 1992 AAS Meeting to describe the mission and the science it could address. Its cover is Figure 1 of this document. I think the motivation for creating this came from Goddard, but I know it was laid out and printed at MIT. I and many others worked hard on it, and many hundreds of copies were distributed at the Meeting.

***Data retrieval***

The concept for our data retrieval was that we would be almost continuously in contact with one of the NASA TDRSS satellites. They are at geo-synchronous altitude (5.6 Earth radii), which is a long way up given that XTE would be orbiting at an altitude of only 0.1 Earth radii. Our data from XTE would be broadcast up to TDRSS from whence it would be relayed to White Sands NM and then to NASA and thence to the scientific investigators. The large transmission distance to TDRSS required powerful transmitters and antennas with good gain. This added cost and complexity to the mission. It would seem to be a lot easier to simply broadcast the short 500 km to the earth’s surface. However, since there would be only one or a few ground stations along the orbital path, the downlink time could be quite limited. A not-to-scale sketch of the data path is shown in Figure 14.

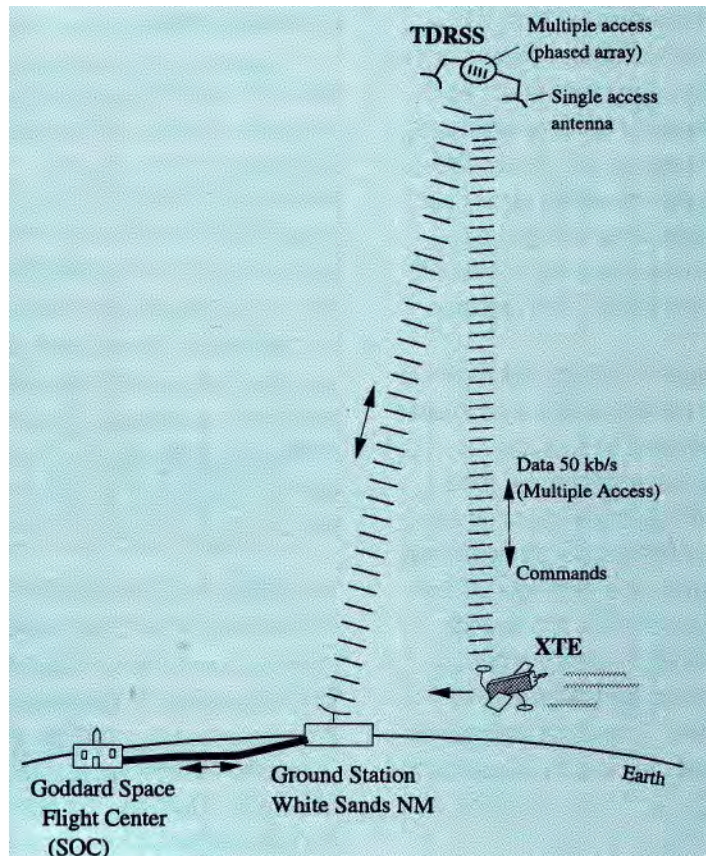


Figure 14. Path taken by data from XTE to GSFC. The scale is distorted: the TDRSS altitude is about 5.6 earth radii, which is about eight times the distance shown relative to the illustrated curvature of the earth’s surface. The altitude of XTE, 1/13 earth radius, is shown at about its actual height. This is how we got the data during the actual mission. (from XTE 1992 brochure. Sketch by H. Bradt).

***Implementation adventures***

The three instrument teams and the spacecraft group went to work on the design and then the fabrication of their instruments. Each group had its crises but none were

show stoppers. I outline here some of the highlights, problems and lessons learned by our group at MIT.

### *Experiment Data System (EDS) defined*

The EDS was essentially a computer; it consisted of ten boards of which eight were Event Analyzers, each with a fast CPU (actually a Digital Signal Processor, DSP) and associated circuitry. We had never built a flight computer, and much thought was given as to the best way to end up with a robust reliable system. We began with a requirements document that listed all features desired by the scientists, each with categories of desired performance (minimal, OK, best). This document was negotiated with the engineers two times with growing understanding on both sides. Ed Morgan kept in constant contact with the engineers at both MIT and GSFC throughout its development, so misunderstandings or problems could be quickly resolved. The result was that there was very little if any escalation of requirements during the design and fabrication phases, and the final product performed as expected, except for one huge problem that surfaced late in the game as described below.

### *The great detector deception*

There was the problem of finding a worthy vendor for the ASM proportional counters. These were gas filled “boxes” of gas with delicate beryllium windows (to admit the x rays) and delicate quartz fiber anodes coated with carbon to which is applied a very high voltage (~1500 volts). The gas absorbs x rays in the gas volume, which gives rise to electrical pulses that can be recorded.

We had planned to have these detectors made by LND Inc on Long Island. They had made counters for most of our prior missions and had always come through with reliable detectors, but often with scary schedule issues. It was a small firm with a strong commercial business building simpler detectors. Our detectors were much more challenging, and therefore of interest to their engineers. But it was their secondary interest. The ASM detectors being position sensitive in one dimension were more complicated than previous detectors they had built for us. We therefore decided to ask LND to build us a prototype detector and to ask the same of another firm that had been recommended to us, Metorex, of Helsinki Finland.

When this plan was presented to the XTE project manager at GSFC, Dale Schulz, he objected on the reasonable grounds that we would be much better off focusing our attention on LND rather than spreading our efforts over two firms, especially with one as distant as Finland. In that way we could monitor schedule and solve problems promptly. Also, building a second prototype was quite expensive, about \$30,000(?). We argued that the risk of depending on only one vendor was huge, and that our allocated funds would cover the cost. But Dale held his ground and directed us to stick with LND.

Well, our project manager at MIT, Bill Mayer, felt that, given our mixed experience with LND, we had to try both vendors. He knew that, according to our contract, it was our prerogative to decide such issues; this was a scientific investigation, not a procurement, remember? He also felt that a verbal battle with Dale was not worth it, especially if we ended up choosing LND to build the flight detectors. So, he ordered the counter from LND and also ordered one from Metorex without notifying Dale. We managed to keep the purchase under the radar on our monthly financial reports to GSFC by paying for it through an American affiliate of Metorex. This uneasy deception carried

on for six months or so during which Metorex made steady progress and LND missed deadline after deadline.

Finally, when the Metorex counter was delivered and we had selected them to do the flight counters, it became necessary to tell Dale. We asked his liaison to MIT, a young woman engineer whom we had also kept in the dark on this, how and when we should tell him. Her answer was, “by phone and quickly” because, by this time, she said, Dale was going sleepless and was practically physically sick about the lack of progress at LND because it could jeopardize our entire instrument. Needless to say, he was happy to hear that we had a successful (almost) prototype. He did ask Bill, though, “What else don’t I know about that I should know?” Bill assured him that there was no other hidden issue, and there wasn’t.

### *Detector horrors*

The prototype detector from Metorex exhibited a mechanical problem that was revealed in a shake test. The connections from the high voltage feed-throughs to the inner frame failed. A design change was incorporated into the flight counters that were by then under construction. When we received the first ones, we held off on testing them because such a test was scheduled after we had mounted electronics on them, perhaps a year hence. We had confidence in the new design and doubly testing the counters seemed unduly risky. The ultra-thin beryllium windows were so fragile we were always nervous about them.

Unfortunately, it was in the testing a year later that we found our design change had an unintended flaw that revealed itself in the thermal test. Finding this at this late stage would seem catastrophic, especially when the counters were welded shut. Well, Metorex saved the day. They had designed the counter with extra material at the weld, so one could mill off the weld, open the counter, fix it, and re-weld it shut, and repeat this one or two times if necessary. In this manner, a successful fix was implemented. However, I wondered if our (or at least my) fear of an early additional test was misguided. Perhaps, my fear of bad news, i.e. of finding a flaw, inclined me to accept, more readily than I should have, the argument that an extra test could be physically damaging.

Occasional visits to Metorex were required to inspect counters before they were welded shut for example. I believe I went along twice, once with Al Levine and another time by myself. We were having troubles with high voltage breakdown in the detectors and were carrying out tests and developing new procedures at Metorex, one of which was a very careful inspection of the counter anodes for dust particles before sealing. For a couple of days, I sat for hours, scanning along the anode wires with a microscope. Be assured that I, a rank amateur, was not the primary scanner, but rather was a backup and was also gaining a feel for the process.

One detector had had a breakdown that had gone on for some hours before it was noted and the high voltage shut down. I noted that the carbon resistive coating on one of the fine filamentary quartz anode wires in that detector had been burned completely off along perhaps 1/3 of the anode length. Apparently the arcing had worked its way gradually along the wire, burning off the carbon as it went, until the voltage was shut off. This observation stood us in good stead later when, after launch, we experienced several potentially catastrophic breakdowns.

Another problem we had were pin-hole leaks in the detector windows. As we prepared for launch, one of the three detectors on the flight instrument had a known very slow

leak. At MIT, I pushed hard to complete testing on a detector that had no such leak just in case the opportunity to access our instrument for a detector change became available, though the chances were slim. These tests were being carried out when the entire science payload was sitting on top of the Delta rocket and the countdowns were proceeding at Cape Kennedy. As expected, we never got the chance to replace that counter.

The gas in the detectors were just a bit above atmospheric pressure, so some oxygen leaked into the gas volume during the many weeks the counter awaited launch. This oxygen slightly “poisoned” or “quenched” the gas, and this made it less likely to undergo breakdown. It turned out that this detector was the most stable of the three after launch; it suffered no breakdowns. It was fortunate we never got the chance to replace it.

Of course, after launch, the counter was in a vacuum, so it was not further poisoned, but the counter gas very, very slowly escaped. This served to gradually increase the counter gain, but this was easily compensated for in our electronics and analysis for the first half dozen years. Thereafter, the counter remained required increasingly difficult recalibrations, but it remained in service for almost all the 16-year flight. To this day, I wonder if we might have avoided all the breakdown horrors if we had simply added a bit of air or a bit more quench gas to all our detectors.

*ASM detects radioactive Principal Investigator.*

I had open heart surgery (quadruple bypass) in the late summer of 1995 and recall that one of the trips to Finland was a couple of months after that. I visited my sister Dale Anne in Holland on that trip. My right leg was still swollen because they had extracted a long vein from it. My attitude in taking this trip so soon after the operation was quite in contrast to my attitude shortly after my first angioplasty in 1988. At that time I felt so vulnerable that I felt like camping just outside the emergency room at Mass. General Hospital, just in case.

During the testing prior to that operation, I had a stress test with a radioactive tracer to diagnose my coronary circulation. When I was loaded up with radioactive material, I went to the lab at MIT where we had an ASM detector on long term test to see if breakdown would develop. This was the detector I wanted to be ready for replacement should the opportunity arise.

When I arrived, I grabbed a student who happened to be working there and also Bill Mayer, telling him “this should be interesting” and took them into the room where the detector was under test, clicking away from the natural radioactivity of the room and cosmic ray hits. The detector count rate remained low as I stood just inside the door. Then I took a few quick steps closer to the detector until I was perhaps 6 feet from it, at which time, the counting rate indicator really jumped way up, as I knew it would. I immediately stepped back and the rate dropped to normal again. Bill said, “Wow, that was scary!”

I don’t know if he was concerned about me because I was so loaded with radioactivity or because the count rate increase looked so much like a breakdown event that it could mean our replacement counter was defective. I think it was the latter. Figure 15 is the record of my radioactivity on 17 August 1995. Note how close this date is to the launch date of 30 Dec. 1995. We were really pushing the envelope on detectors.

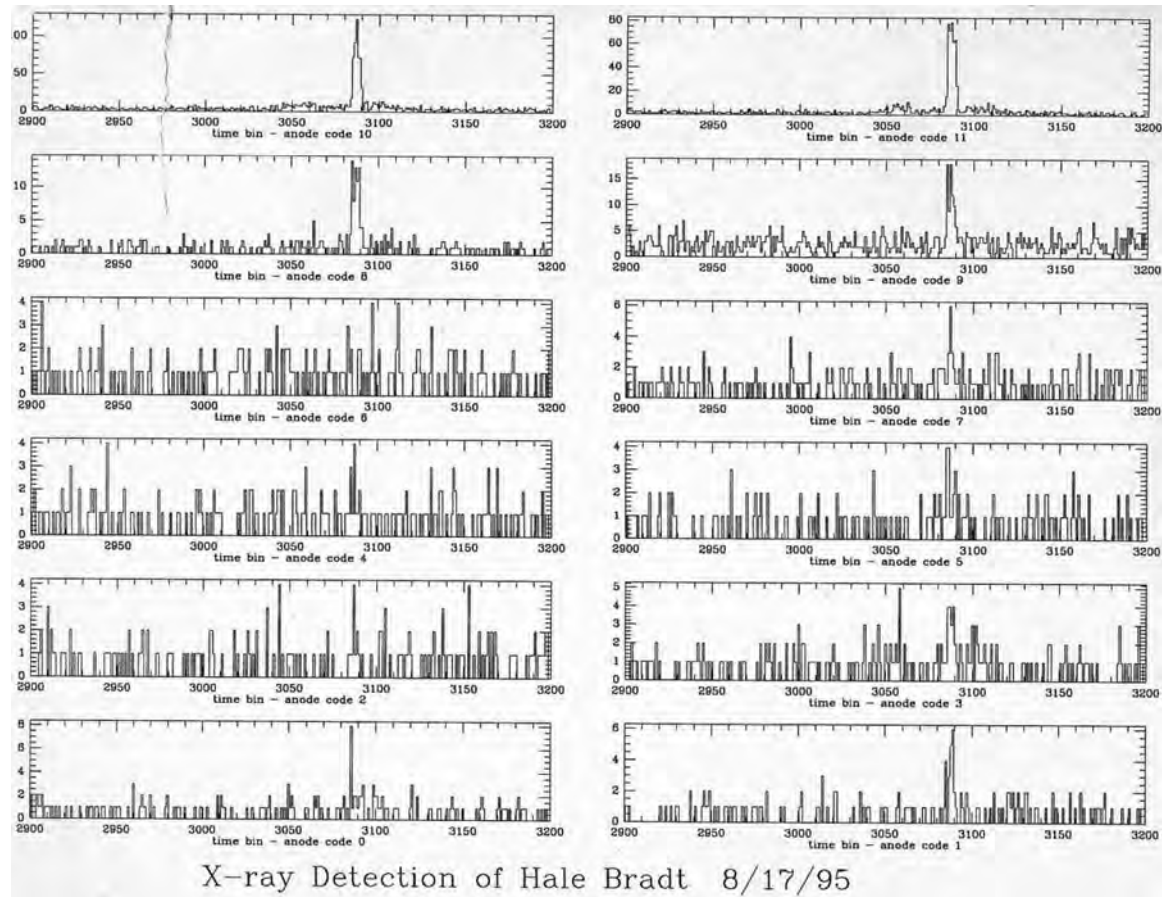


Figure 15. Detection of radioactive Hale Bradt with one of the ASM test detectors. The several panels represent the counts from the several anodes of the detector; time runs from left to right for 300 s. At time “3090,” all rates jumped up for several seconds.

### *ASM software*

Ron Remillard recalled for me the ASM software crisis. As he tells it, the programmer working on the analysis software of the ASM data had structured the task so it was lacking in milestones with which progress could be measured. Then some three months before launch, he quit, with very little or nothing to show for his time on the job. This was another potential catastrophe. Ron was able to bring to bear his own prototype analysis software, which was filled out and upgraded to professional standards by Alan Levine. Ron credited the availability of his software to a conversation with Gunther Hasinger a lead scientist on the German Rosat mission. He had told Ron a similar crisis regarding their analysis software and how they had used Gunther’s prototype software. Gunther’s strong advice to Ron was that, during the implementation phase, keep your prototype software up to date, so it can serve as a backup, and he had done so.

Why doesn’t this crisis loom large in my mind today? I frankly had forgotten about it. The answer is surely that Al and Ron had taken over the responsibility for the ASM so effectively that I had relinquished much of the worry to them. They were so competent that my confidence in them was complete. For example, they realized the shortcomings of the analysis scheme in our original proposal and initiated the more straightforward multiple dwell operation of the ASM. They also made an extensive study of possible

mask patterns and introduced the differing mask patterns on a single mask that provided elevation information normal to the high-resolution direction. As it flew, the ASM instrument was truly theirs.

*The great EDS switcheroo*

The EDS development proceeded without major trauma. It went through all its testing, functional, thermal, shake, etc. successfully under the watchful eye of our engineers and Ed Morgan, the instrument scientist. Its delivery and integration into the spacecraft at GSFC was scheduled early in the program, as it had to be in place to accept the signals from the ASM and PCA when they were installed into the spacecraft. It was a beautiful rectangular box of approximate dimensions, 30 inches long and 10 by 12 inches in cross section with connector ports on the ends. It was completely anodized so it had a golden color and also a big MIT logo (Fig. 16) on its side. We were quite proud of it.

That logo was of some consternation when the EDS was received at GSFC because it was felt that it should have had a NASA logo on it, not MIT's. After all it was a NASA procured item, no? “Well, not really; this is a scientific investigation, remember?”



Figure 16. Edward Morgan, instrument scientist, and James O'Connor, technician, with the Experiment Data System which consisted of eight identical computers in a rectangular gold-colored box. Note the MIT logo on the side of the box.



The day before it was to be delivered to NASA/GSFC, it was in the lab, connected to a computer to which Ed had remote access. Ed Morgan from his home in Lincoln decided to give it a quick check, and found that one of the 8 “computers” would not boot (turn on). This was a disaster; it had been fully tested and was to have been delivered to NASA the next day. This produced lots of angst and diagnostics. A connection on one of the internal layers of the multi-layer board had failed. The defective board was shipped to MIT’s Lincoln lab for further testing. Was it a minor isolated failure or was it endemic to the entire system? We did not know. The EDS was needed for all of the ASM and PCA data so this was a big scary deal that attracted lots of attention.

That day or the next, we were huddled in Bill’s office reviewing our options, and the GSFC project office was desperately trying to reach us, and we did not want to be reached. I forget just what the concerns were, but Dale Schulz at GSFC probably was concerned that we might take steps that would jeopardize the integrity of the EDS which had already undergone all its testing successfully and which he needed for the spacecraft integration. Perhaps on these grounds, he was objecting to us sending the board to Lincoln for diagnostics, before more thought and discussion with GSFC, or perhaps because he would like GSFC labs to do the diagnoses. On the other hand, we had already sent the board to Lincoln and were focused on getting the heart of the problem as fast as possible. Dale’s directions might well interfere with our efforts. We had the EDS; it had not yet been delivered, and we believed we owned this problem; this was our scientific investigation, remember?

So, when the phone rang in Bill’s office, he did not answer it, saying he knew it was Dale. Then, the phone rang next door in Bob Goeke’s office; Bill or Bob, said “Don’t answer it.” The phone could be heard in several other offices too, and they were not answered. A bit later in our meeting, one of our young engineers, a woman, came down the hall, stuck her head in the door, and said, “I just got a call from the project manager at GSFC who says he can’t reach any of you. What’s going on? I never get called by him.” Bill said, with a twinkle in his eye, “Why did you answer it?” and she responded “You didn’t tell me not to.” Such was life in the Albert Hill building in Kendall Square where all this was being done. That building is actually known in MIT style by the unpoetic name, NE80.

Well it turned out that the problem was indeed endemic to all the circuit boards in the EDS. These boards were “multilayer” boards with as many as six (I think) layers of conductors sandwiched together. The optimum way to solder them is to mount all components on them, to heat the entire board and components to an appropriate temperature, and then to pass the board through a “wave” of molten solder. We had not used this method because of the delayed delivery of some components. To maintain schedule, then, we had hand-soldered components so we could mount those that arrived earlier. One location of the boards had a large ground plane on one of the inner layers, so additional local heat had been applied locally to the solder points, and this led to unseen damage to the board material that in turn led to internal broken leads.

A contributing factor was that the board material was different than that with which we had had prior experience. The new material (polimide) was recommended as superior by GSFC, but it was not superior in this one respect, namely its susceptibility to extra local heat while showing no visible symptoms. We and perhaps GSFC were not aware of that shortcoming when we decided to hand solder the components. The issue probably was not on anyone’s radar because standard practice was to use wave soldering, so the problem only rarely arose. Is there a lesson here? Use standard practice whenever

possible? Perhaps, but that might quench valuable innovation. For space hardware, too much innovation can be dangerous.

That is the story as I remember being told it, but Bill Mayer recalls that the problem was eventually tracked to a board fabrication issue. The vendor had not adequately cleaned the individual layers before lamination, and residual etchant chemical (acid) etched away conductors (traces) with time.

This type of issue arose more than once. When buying a product for a space mission, one looks for the qualities one needs, such as performance in a vacuum or under temperature extremes. Goddard’s quality department is one resource of such information because of the wide variety of space missions in their history. However, the catch is that they might come up with a list of approved materials from a previous mission where the needs were somewhat different. Hence a “better” material or item might not be better for your particular use.

We went through this in trying to determine the most reliable lubricant for the rotation bearing on the ASM. I forget the details, but we got opinions from industry experts as well as from NASA and some of the more credible recommendations were totally opposed to one another. The resolution of this issue depended on our assessment of the source of the recommendation. In the end we followed that of vacuum lubricant expert at Ball Brothers.

This failure of the EDS was a huge problem. To rebuild the boards would take months, and this would delay the integration and testing of XTE because the EDS was needed for both the PCA and ASM data flow. Such a delay would invoke huge additional costs, not to mention a major loss of face also. An ingenious solution was found; I believe it was Bill’s or perhaps he and Bob concocted it together. They always ate lunch together in Bill’s office, and most of the difficult issues facing them were cogitated on, or should I say chewed on, between bites of sandwiches.

The plan was to install a jumper on the boards at the locations where the broken lead problem had arisen. The EDS with the patched boards would then be delivered for integration and for testing of the spacecraft, which could then go forward on schedule. After the testing, the defective EDS would be removed and replaced by an entirely new unit that MIT would build while the integration and spacecraft testing were underway; this was the “great switcheroo.” It involved fabricating and assembling not only new boards, but also all the board frames, connectors and wiring. We would then put the completed new EDS through testing somewhat more rigorous than the usual practice to compensate for the tests it missed aboard the spacecraft. All this was more work, but it would keep the program on schedule. That is what reserve funds are for.

This plan was adopted and a completely new EDS was fabricated. The EDS that flew never went through the extensive testing on board the spacecraft. The defective unit had performed perfectly throughout the tests despite having flawed patched-up boards. The question then arose from our manager at GSFC, “Why not fly the latter unit?,” a quite reasonable question because it had been fully tested. We at MIT felt strongly that it was still vulnerable to board failures due to residual etchant and because of the DSP problem (described below) that would have limited the EDS life to perhaps 2 to 4 years in orbit. Such a limitation would carry little weight in a showdown because our mission was planned for only 2 years. Fortunately, we were able to install the new EDS because some of the thermal-control materials had not been installed in the flawed unit because they were not relevant during spacecraft testing. Because of this, the unit was clearly not

qualified to fly; a change-out was required. Had those materials been omitted by our managers (Bill and Bob) with this likely future confrontation in mind? Perhaps so.

*Procurement near misses*

We learned a few things in the process of building the new EDS unit. There was a problem with getting enough memory chips for the boards. GSFC had made a bulk buy of one particular type, radiation hardened, so the several instruments teams could benefit from the lower cost of the large purchase. Bill asked GSFC how many they had left that could be used for our new EDS. They had a significant but insufficient number. Bill still needed several dozens, say 100, more of them, so he called the manufacturer asking if they had the item, referring to the part number on the DSP case. They replied that yes they did. Bill asked if they had 100 and they replied that yes, they had several thousand. Bill then asked what they cost and they said, some very low number as space qualified parts go, like \$20 each. Well, that seemed like an easy solution.

Bill then informed the GSFC Parts or Quality Office, as required, of his plan to use these and they informed him that he could not use them; they were not “radiation hardened,” which was a necessity for space use. Bill then protested that the \$20 items had the same serial number on the case as those that Goddard had provided us. for the first EDS. He was informed that in preparing the bulk order, Goddard had obtained units with that part number but had then had them hardened elsewhere without changing the number on the case! Needless to say, we did not buy the \$20 units. Bill managed to locate just enough odd depositories of the qualified memory chips to fill his needs.

A breakdown in communication led to another parts problem we uncovered during the fabrication of the new EDS. When we notified GSFC Quality office that we would use the same Digital Signal Processor (DSP) as on the first EDS, specifying the part number, we were told that this chip was not protected against “latchup,” a catastrophic short circuit that destroys the chip due to a cosmic ray depositing ionization energy. We thus had to design protection circuitry for the DSP on each of the eight new boards. Without this fix, we estimated that we would have lost roughly one of the eight boards a year. The original EDS had this problem, which was unknown to us until we were building its replacement!

We looked back to see how this use of an unqualified part had occurred during the initial fabrication. It turned out that we had sent a list of EDS parts to GSFC for approval. This was required and also necessary because GSFC had information about quality issues not available to us. Approval of a document by GSFC was often delayed because the document had to pass through a number of cognizant offices, and this could interfere with schedule. Often though, we could learn what was necessary informally. In this case, we had received an informal response indicating that two of the items on the parts list were not qualified for space flight. We took this to mean that the other items were qualified, and the (unqualified) DSP chip was among them.

This was a continuing problem we had with GSFC (from our perspective). Word-of-mouth agreements created a the risk that interfaces would not match at integration. Bill felt this was jeopardizing the program, not only at MIT, but elsewhere in the XTE program. In a strong detailed letter, he urged Dale’s boss, Jim Barrowman, to take strong action. I am not sure if there was ever a clean resolution of this, but the integration did go quite smoothly. There was good informal communication between the MIT and Goddard engineers, and this compensated for inadequacies in the formal documentation. But, the value of the formal documentation is clearly demonstrated in the case of the DSP chips.

An interesting quid pro quo developed between Bill and the project manager at GSFC, Dale Schulz. Although, most of Bill’s interactions with GSFC were in person or by phone, if he felt something needed serious attention, he would write a letter. His letters were clear, full of facts, and usually right on the mark, e.g., the letter regarding the interface control documents. Sometimes they might be rather more forthright than many would consider politic. Dale was the usual recipient and sometimes referred to them, with a chuckle, as “Bill’s nasty-grams.” Dale made a point of never answering these in writing, but rather would phone Bill, so that the tensions would not ratchet up. Dale was good at staying calm when things were tense and that helped. Bill could flare up (as could I), but would quickly return to business in a constructive and friendly way. His great competence and knowledge of the program were huge pluses. He could really see the big picture and this helped discussions get over the rough spots.

On one occasion, during one of our obligatory trips to GSFC, Dale invited us and other XTE attendees to his home on the Maryland shore for a drink and then we all went to supper together (Dutch, I believe). He did it deliberately to enhance the interpersonal relationships. I thought that was especially nice and always meant to return the favor when Dale and his team were in Boston, but never quite got to it. I still regret that.

### *Management issues*

In 1994, as in-orbit operations were being planned, I raised an issue that caused some consternation at NASA Hq and possibly also among my colleagues on XTE. Since the mission was to be 100% guest observations, it became apparent to me that the PIs could well be relegated to bystander status in the control of the mission, e.g., re targets, long term planning, financial decisions, etc. There would be a Users Group with the Principal Investigators (Jean, Rick, and me) as members, but the PIs would be a minority and there would be a non-PI as chairman. As Guenther Riegler of NASA Hq. stated in one communication, “the Project Manager and Project Scientist already have ultimate responsibility for all decision-making and implementation, including directing all mission operations and data analysis.”

As a counterbalance to this, I proposed a “Mission Management Working Group” consisting of the three PIs only, with ex officio members being the Science Operations Center (SOC) director, the program scientist, and the chair of the User committee. Its purpose was to:

1. Advise NASA (Hdqtr, Project Scientist, Project Manager) regarding
  - Operations priorities
  - Implementation of policy, short and long term
  - Implementation of recommendations of User Committee
  - Allocation of resources.
2. Propose new policy as needed.

Meetings would take place only as needed; frequent consultations could take place by telephone; meetings would be open to non-members.

Looking at this now, it seems eminently reasonable; it would be a kind of executive board of the User Committee - empowered only to give advice. Headquarters and Jean Swank, who was the NASA Project Scientist, on the other hand viewed it as unnecessary

cumbersome management. They assured me that NASA would listen to the PIs without it. Since my proposal was received with such dismay by Headquarters and my colleagues had little enthusiasm for it, I let it drop.

I have since felt rather embarrassed about this episode as it perhaps exhibits an undue paranoia on my part. As the mission developed, Jean Swank, as Project scientist and also PI of the PCA, worked very closely with the other PI teams. As noted previously, it is mostly the work one puts in that determines one’s influence. The PI teams, knowing the mission capabilities so well, had a large influence on the User Committee’s deliberations. In fact the User Committee worked well as a single group with few or no serious disagreements. Thus in the end, my concerns might seem unwarranted.

But in principle it could have turned out differently given different personalities and working styles. I think Jean Swank in fact ran the mission as if my proposed PI Working Group existed, but she needn’t have done so. I also think the objectors did not fully factor in the implications of going to 100% guest operations. Does that mean the PIs must give up having a separate voice in giving policy advice? If the conception of the mission was theirs, I think they should formally have a pivotal influential role.

The mission was monitored before launch by a group of scientists known as the Science Working Group. It included the PIs as well as several outside scientists, including John Deeter, John Grunsfeld, Shri Kulkarni, and Dan Schwartz and may have begun meeting in 1991. Membership evolved with time. Dan Schwartz was the first chairman. Upon launch the committee became the “RXTE User Group.” In 1997, Fred Lamb took over the chairmanship and continued until May 2008. John Tomsick then served in that role until the end of the mission in 2012. The committee would meet, perhaps twice a year and would serve as a sounding board on a wide variety of issues. It was particularly attentive to issues affecting the wider user community. To my recollection, the operations throughout the 16 years of flight proceeded without significant controversy.

## **Phase IV: Launch (December 1995)**

### *Groundhog day, seven times*

Launch was scheduled for early December; I forget the exact date, but it was near my 65th birthday on December 7. By the time I arrived at the Cape the payload had already been hoisted onto the rocket. I took part in the “last visual inspection” of the payload, high in the tower, prior to the closing of the shroud. Would I be astute enough to note something out of order when I had no prior experience or list of things to check, or was my presence simply a chance for me to see our baby one last time before it was projected 500 km beyond our reach?

It was mostly the latter I think, though I did remember the story of one of Columbia University’s sounding rockets being launched from White Sands Missile Range. (I felt close to this as we also launched rockets from there and once were there while Bob Novick’s Columbia crew was there.) The launch crew failed to hook up a wire from the tower to a valve on the rocket engine. When the solid fuel booster started the rocket moving, the wire would pull free allowing the valve to open, and thus starting the liquid fuel rocket engine. In their case, with no connection, the booster got the rocket moving at

high speed, but the liquid fuel “sustainer” engine never ignited. The rocket containing all the unburnt liquid fuel was like a German V2 rocket heading for London. After going a mile or more up, it returned to earth nose first, guided by its nose cone and fins. The Columbia polarimeter in the nose of the rocket was not much to look at after the impact.

All this took a minute or more. After only two seconds of flight, the booster burnt out, its roaring sound stopped, and there was a deafening silence. Usually the noise of the sustainer engine would continue for another minute or so. Needless to say there was a sudden traffic jam at the entrance to the blockhouse by those who had stayed outside to see the launch. If I had been there and made a last minute pre-launch check of the tower, would I have noted the unconnected wire? Probably not, as I didn’t even know about that wire until I heard this story.

Friends, associates, and families and also NASA officials came to Kennedy Space Center for the pre-launch festivities and launch. I expected to have more than 20 visitors, part of whom were the large family of a cousin who lived in a nearby Florida town. One of my daughters, Elizabeth, was there with her family, as was my sister Valerie, her husband, at least one of her sons and a grandson, and my cousin Carol from nearby Georgia. Also, my Aunt Ruth and her husband Jack drove over from their winter residence on the west coast of Florida. Keep in mind that neither NASA nor MIT paid travel expenses for visitors, so living nearby made attendance more likely.

NASA put on a wonderful dog and pony show for the visitors the afternoon before the scheduled launch. A session in a small theatre featured talks by NASA officials and the PIs (Jean Swank, Rick Rothschild and me) who explained the scientific goals of the mission. This was followed by our own private bus tour of the Kennedy Space Center. That meant we had our own bus, which took us, more or less, on the standard tour including the Shuttle facilities. The highlight, of course, was a special stop at the Delta Launch site where our rocket and payload were situated. Dusk was approaching and the white rocket, illuminated by searchlights, ready for launch the next morning, was a beautiful sight. Previously, the tower had been surrounded by staging, so the rocket was not easily visible, whereas now it was exposed under bright lights for all to see. Since the bus was full of XTE supporters and friends, it was a wonderful occasion.

The day of the launch was clear but windy. The morning countdown preceded with several holds as the various rocket and safety systems as high altitude winds were checked by launched weather balloons. Finally after substantial delay, the launch was cancelled due to excessive winds, either locally or in the jet stream; I think the former. During the several hours, of the countdown, the visitors waited patiently in the chilly morning on the bleachers set up some distance, a mile or two, from the launcher. John Grunsfeld, an astronaut and MIT alumnus who had worked in our group, being a friend of XTE, came around in his space suit. I encouraged our 5-year old grandson Ben to shake his hand and have a word with him, but the suit was too frightening and he shied away from it. Also, my nephew Dale’s wife fell and broke her coccyx that morning. All in all it was not a good day.

Two more attempts were made the next two mornings and both were aborted due to winds. All in all, there were six attempts before the successful launch of 30 December 1995. Some visitors stayed for the first few, but they were long gone by the time of the actual launch. I had returned to MIT for my teaching duties and to continue testing the potential replacement counter should the opportunity to change it arise.

These launch attempts were hard on the technical staff. The early phases of each countdown started in the evening and continued until the projected morning launch. After the launch was scrubbed, a series of steps had to be taken to ensure the safety of the rocket and to get all systems in place for the next launch attempt. For example, fuel may have to be removed from the rocket. Thus, some technicians would not be freed from their duties until noon or later. Then that evening, or the one after, the whole process would start again. It was like the movie “Groundhog Day” where the protagonist was condemned to live that Feb. 2nd over and over until he finally got it right.

There were teams for every system, the Delta rocket, the second stage Aerojet motor, the weather, the range safety, and the science payload, and others. We never saw most of them as they were tucked into different locations at KSC. You knew they were there as the Launch Director polled each of them as to their readiness at several stages during the countdown. We scientists were not allowed into the main control room; only Dale Schulz (and possibly an assistant or two) had a chair with computer screen in “mission control.”

The Instrument PIs, were considered absolutely useless to this enterprise, but were allowed into a glassed-in VIP room that overlooked it. We could tune into various voice circuits to hear what was going on - reports from the weather team, for example, etc., but we could not talk into those circuits. In fact our experiments were unpowered on the rocket, so there was nothing for us to monitor within our expertise. I found it quite exciting to listen to the reports during the launch countdowns and actual launch knowing how many systems had to operate flawlessly.

### *Main engine shutdown*

The most nerve-wracking launch attempt occurred on perhaps the 5th attempt. The rocket had been on the launch pad for a couple of weeks and condensation had frozen in the valve that controlled fuel injection into the main engine, so it did not open. The countdown proceeded right down to zero, with the vernier (side) engines igniting a few seconds earlier, as expected. But then, when sensors detected no main engine ignition, the launch was aborted; the solid fuel boosters were not ignited. What a letdown!! Someone in the Mission Control room yelled: “ Oh s - - - !”, and that was picked up faintly by the microphone of the official NASA announcer, whose desk was in the back corner of the room. He had just depressed his mike button and was just about to announce the shutdown. It was barely audible, but the inflexion made its meaning unmistakable. This led to some cautionary advice from NASA Headquarters.

I did not see this launch attempt as we were just getting ready to test the replacement ASM detector that morning at a test facility in Boxborough MA. Of course, if the launch went well, we could abort the test. So we took a break to call Ron Remillard who was listening to the countdown from his home in Boston. When he told us of the main engine shutdown, we went back to our testing. Perhaps, if these failed attempts continued, the second stage rocket would need refurbishing and that might lead to an opportunity to replace the counter. The likelihood of that being possible was considered to be so small as to be laughable, but my colleagues humored me by continuing the testing. You never know . . .

### *More rocket problems*

I went back to KSC for the final launch attempt or two. There had been issues with the rocket and the launch director so NASA called a meeting of all concerned to review how these had been resolved. It was a large room with 50 or more people from all the different involved teams. A high NASA official and the senior Delta team person from McDonnell-Dougllass Aerospace hosted the meeting. They went through the several issues one by one. As I recall through the fog of the intervening years, the discussion went more or less along these lines.

Keep in mind that this discussion is taking place in the context of tremendous pressure to get this mission launched. The previous launch attempts and delays were stretching budgets and people to the breaking points and the second stage Aerojet engine may have to be removed for refurbishment if the next attempt is aborted. On top of this, the US Government had been shut down since Dec. 16 in a budget crisis, though the launch teams were kept on the job. These problems were made abundantly clear to all at the meeting. Getting a successful launch soon was a very high priority; shades of Challenger!

#### *Problem #1 – a spark*

A senior respected technician was performing a task inside the main engine of the Delta with pliers in his hand when he saw a flash or spark. This was alarming and could not be reproduced. Perhaps he imagined it, but he was considered to be a highly reliable and responsible person. Since many other inspections by others and functional tests revealed no fault, this was deemed not a significant risk to the mission.

#### *Problem #2 – a washer*

A small washer was dropped into a part of the main engine and could not be found despite an aggressive search. It was possibly lodged invisibly behind a gasket. In that position, it could conceivably lead to serious damage but this was highly unlikely. This too was deemed to be an acceptable risk.

#### *Problem #3 - potentiometer*

Prolonged exposure to moisture on the launch pad caused a film to accumulate on the potentiometer(s) that control the pointing direction of the two vernier rockets. These are the small engines on either side of the main engine. Like the main engine, the nozzles can pivot in either direction (pitch and yaw). Since they were offset from the main engine, they can control the roll angle of the rocket. The film had been detected earlier and many diagnostics were carried out, and it was determined that the film consisted of mineral deposits from the moisture in the atmosphere, or some similar natural effect. This film could render the controls ineffective. Since the film had been removed, or the potentiometer replaced, recently, it was deemed unlikely that deposits could have accumulated sufficiently to cause problems on an imminent launch. Furthermore, the vernier engine motions would be tested in the last few minutes before launch. Also, vibrations of the rocket firing were likely to shake off any accumulated deposits. It had been determined also that main engine offsets could apply sufficient torque to overcome any wind torques in the jet stream even if the vernier engines could not be pivoted. Thus, for these reasons, this too was not considered an impediment to proceeding to launch.

During the question period, a member of the audience asked, “If the vernier engines fail their pivoting test immediately before launch, would you proceed with the launch?”



The NASA official at the podium in the right front corner of the room said, “Yes, because it has been demonstrated that the main engine has sufficient torque to traverse the jet stream successfully.” While he was saying this, the McD-D Delta person in the left front corner at his podium was busily shaking his head from side to side unnoticed by the NASA official. When he had a chance to talk, he said simply, “We do not launch with a known failure.” That surely illuminated the different perspectives!

Finally, the conclusions of the several studies were summarized and an authorization to proceed to launch was recommended. The assembled multitude was then asked, “if anyone here wishes to take issue with this recommendation.” I could feel the heat on me, as did everyone else in the room; there were three problems that had not been completely resolved, and the launch was on the line. On the other hand, the likelihood of them causing a problem was small. What to do? I did not raise my hand nor did anyone else. We were all thus co-opted into the decision to launch. Any launch will have problems that could jeopardize it, and if launches were delayed until each problem was completely resolved beyond any doubt, one might never launch.

On the other hand if one accepts this philosophy and a failure results, one would regret taking this stand. On balance, it is a judgment call based on the perceived level of risk. The danger is that one’s perception of risk can change, perhaps without justification, with time and experience. One can see how this dilemma and the pressure to succeed could lead to risky decisions such as those that led the losses of two Space Shuttles.

### *Launch and a naming*

Finally on Dec. 30, 1995, on the 7th attempt, the XTE was successfully launched into orbit. I never did see the actual launch because I was listening to the reports on the voice circuits in the VIP room. However, I could feel the engine vibrations through the walls, and I did walk outside to see the bird a bit after liftoff.

By pre-arrangement, the satellite was named the Rossi X-ray Timing Explorer after Bruno Rossi, the founder of the MIT “Cosmic Ray Group.” Under his leadership, it evolved into research groups in x-ray astronomy and interplanetary plasma research. It was Rick Rothschild who proposed this name, which was gratifying to us at MIT who had worked with and under Rossi. We proposed this to Jean Swank and her colleagues and to Alan Bunner of NASA Headquarters, and all agreed it was a good idea. We also checked with Rossi’s family making sure they understood that, if things went awry, the Rossi name could become a subject of ridicule or scorn, as had happened with the Hubble mission.

I was deputized to check with the Italians who were about to launch their own x-ray astronomy satellite and might have been thinking of naming their satellite after Rossi who was Italian. I called Livio Scarsi, the prime mover of their program. He was pleased to hear our plan, because as he said. “This frees us to name our satellite after Giuseppe Occhialini,” who was another famous Italian physicist, two years Rossi’s junior and an early student of Rossi. In fact, he had one of Bruno’s first PhD students. The Italian satellite, named “Beppo-SAX” was launched successfully on 30 April 1996. “Beppo” is the nickname for Giuseppe, and “SAX” (Satellite per Astronomia X) the original name of the satellite.

### *Counter breakdowns after launch*

About a week after launch, we turned on the ASM and over the next several days experienced breakdown on several of the anode wires of the ASM detectors. These were desperately unhappy times as they threatened to render our entire ASM instrument inoperative. This would blind the entire RXTE and greatly reduce the science it might accomplish. Also, it surely would blacken MIT’s reputation in space science.

Each detector had 8 primary anodes so the loss of one of them was acceptable. However, the electrical noise of such a breakdown swamped the signal on the other anodes so they were useless. This occurred in two of the three detectors, the two that had not experienced oxygen poisoning! We spent about three weeks analyzing data trying to understand the problem. During most of this time, we had shut down the offending counters by turning off the high voltage poser to them, to limit any damage the breakdown might be causing.

Those were black days indeed. As we were considering our options, I recalled seeing how a breakdown had stripped an anode bare at Metorex and suggested that we let the breakdown proceed by leaving the high voltage turned on. Perhaps, after it had worked its way down the entire length and burned all the carbon off that anode, the breakdown would cease. Then we could use the undamaged wires in the counter. We tried it on a prototype counter in the laboratory and it worked as hoped.

So we let the breakdown continue for one day and then for another whole day. I was giving up on it ever stopping, and was preparing myself mentally for being the focus of universal scorn for being an incompetent instrumentalist. Then, finally, on the third day or even later, the breakdown sputtered to a stop. With the cessation of the electrical noise, the other wires performed flawlessly. We did this on, perhaps three occasions, until finally, we had stable counters that worked beautifully and then adequately, albeit with a few missing anodes, for the next 16 years.

What could we have done better? Perhaps adding a bit more quench gas or lowering the high voltage would have solved our breakdown problems, and perhaps earlier testing would have found problems earlier. During the first in-orbit breakdown events, which at first were quite puzzling, we had a meeting at GSFC with the interested parties, including scientists from their PCA group. There, just as the meeting was breaking up with no clear conclusion on how to proceed, Pete Serlemitsos, a superb instrumentalist at GSFC, commented under his breath that “I can not understand how anyone would build a proportional counter without adjustable high voltage.”

This was quite a damning statement, which I took to heart, because taking such precautions was part of being a good scientist. On the other hand, our engineering group had a philosophy of keeping systems as fundamentally simple as possible, because every additional control or complication is another potential failure point. This approach had led to a string of highly successful flight instruments, and in the end succeeded, even in the case of RXTE. Peter had a point, but there was another sound way to proceed.

We scientists on the XTE-MIT team were so pleased to finally having a well functioning RXTE in orbit and especially a working ASM that we gathered around the engineering-unit ASM for an impromptu photo opportunity (Fig. 17).

## Phase V: Science Highlights

Was the scientific payoff worth all the effort? There was no certainty that this would be the case. The mission was following Bruno Rossi’s dictum that it pays to explore new domains of physical phenomena. Unexpected discoveries can occur, and RXTE did indeed make them. Was there an absolutely mind-bending headline-grabbing earth-shaking Nobel-winning discovery? At present, it seems there was not. But, there were important new surprises that were impressive to the scientific community and which led to major advances in our understanding of compact objects, i.e., neutron stars and black holes.

Over 2000 papers with RXTE data have been published in refereed journals (through 2009) with an average of 22 citations and 60 “high impact” papers that had more than 100 citations. Ninety PhD theses have been written based at least in part on RXTE data sets. Another mark of its science yield is that the High Energy Astronomy Division (HEAD) of the American Astronomical Society awarded its Bruno Rossi Prize for RXTE-science on four different occasions, in 1999, 2003, 2006, and 2009.

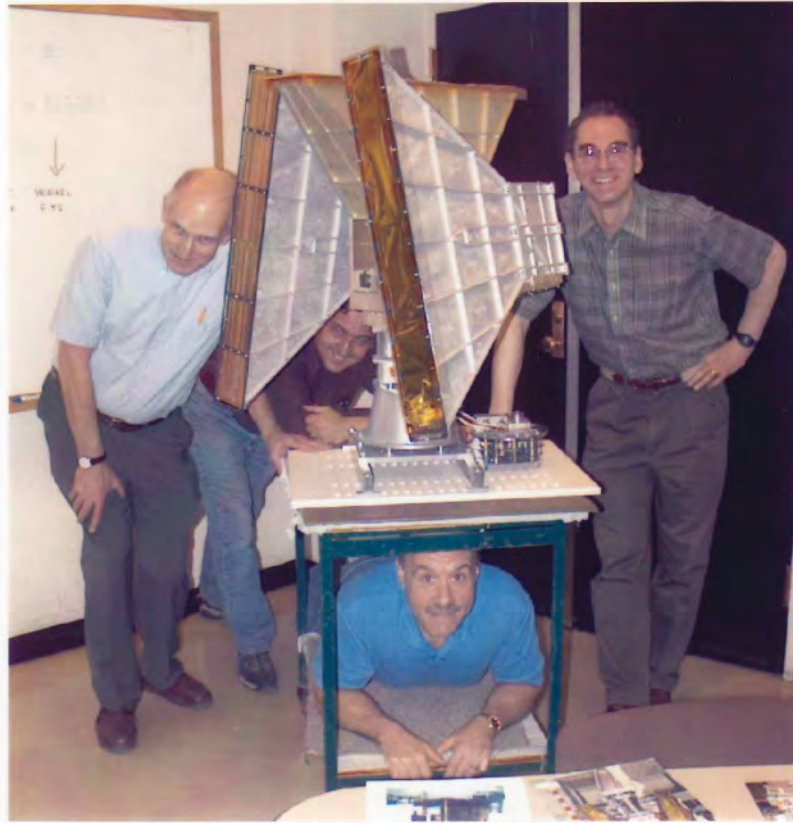


Figure 17. All Sky Monitor engineering unit with our science team, after launch. From the left: Hale Bradt, Edward Morgan (peeking), Ron Remillard (below), and Alan Levine.

Finally, there were the results of the biannual “Senior Reviews” wherein RXTE went head to head with other NASA missions to seek funding for continuing operations for another two years. The science merits of the several missions were judged by a committee chosen from a wide range of astronomical disciplines. The uniqueness of

RXTE capabilities and the quality of its scientific productivity merited funding for full operations for sixteen years. RXTE operations ceased in early 2012. It still orbits the earth at this writing (2013).

My role in extracting science from RXTE was not particularly strong. Younger colleagues were much more active than I. They had the energy, the knowledge for complex analyses of data, and the imagination for new types of investigations. I worked closely, though, with three of our students on RXTE results: Bob Shirey on studies of the neutron-star binary Circinus X-1, Linqing Wen on studies of the black hole candidate Cyg X-1 and searches for periodicities in the ASM data, and Don Smith on studies of gamma-ray bursts in the ASM data. I enjoyed continuing discussions with my MIT colleagues Ed Morgan, our fast-timing expert, who facilitated and participated in many of the exciting kilohertz results from RXTE, Al Levine who worked on and reported many results from the ASM monitoring and from PCA pointed observations pertaining to compact-star binary orbits, and Ron Remillard who carried out observations, both x-ray and optical, of stellar black-hole systems. In addition these three scientists monitored and maintained the in-flight operations of the EDS (Ed) and ASM (Ron and Al).

### *The Rossi Prizes*

Another insight into the science that RXTE yielded is to peruse the citations for the four Rossi prizes based in large part on RXTE data.

The 1999 Rossi Prize of the High Energy Astrophysics Division of the American Astronomical Society is awarded to Drs. Jean Swank and Hale Bradt for their key roles in the development of the Rossi X-Ray Timing Explorer, and for the resulting important discoveries related to high time resolution observations of compact astrophysical objects.

The 2003 Rossi Prize of the High Energy Astrophysics Division of the American Astronomical Society is awarded to Robert Duncan and Christopher Thompson for their prediction, and to Chryssa Kouveliotou for her observational confirmation, of the existence of magnetars: neutron stars with extraordinarily strong magnetic fields.

The 2006 Rossi Prize of the High Energy Astrophysics Division of the American Astronomical Society is awarded to Tod Strohmayer, Deepto Chakrabarty, and Rudy Wijnands for their pioneering research that revealed millisecond spin periods and established the powerful diagnostic tool of kilohertz intensity oscillations in accreting neutron star binary systems.

The 2009 Rossi Prize of the High Energy Astrophysics Division of the American Astronomical Society is awarded to Charles D. Bailyn, Jeffrey E. McClintock, and Ronald A. Remillard for their measurement of the masses of Galactic black holes.

The 1999 award recognizes Jean and me for our contributions to the development of RXTE but fails to recognize others who played equally important roles, e.g. Steve Holt, Charlie Pellerin, Rick Rothschild, and Fred Lamb. Not to be overlooked were the efforts of the high-energy-astronomy team at NASA Headquarters, Albert Opp, and later Alan Bunner and Lou Kaluzienski who kept pushing for the mission. As one Rossi-Prize committee member later commented to me: “The discussion is driven largely by the nominations.” If they were not nominated, which is quite possible, they would not have been considered. The science part of our award primarily refers, I would think, to the kHz discoveries explicitly recognized with the 2006 prize. I was not directly involved in that work, whereas Jean worked with Tod Strohmayer in his investigations. The kHz discoveries derived from the focus of the mission on time scales appropriate to neutrons

stars and stellar black holes, so all of us who brought the mission to fruition deserve some of the credit for the kHz discoveries.

The other Rossi prizes are for specific contributions or discoveries made by the awarded scientists. Although the mission was designed to explore the domains in which these discoveries were made, it required the prior knowledge, creativity, skill, and hard work of observers to pull the results from the sky; they did not come for free, far from it. Finally, I will note that there were many RXTE observers not recognized in the awarded Rossi prizes who made important contributions that could well have been recognized by this or other prizes. One of these would be Michiel van der Klis who was instrumental in many of the RXTE kHz discoveries. He had earned the Rossi Prize in 1987 for earlier work, and thus was probably never considered for a repeat despite his central role in the kHz investigations and discoveries of RXTE.

Examples of specific scientific results are given below. Many others were forthcoming, but these give a snapshot of the impact RXTE had.

### *Kilohertz oscillations*

As stated above, a prime goal of RXTE was to search for, and to study, if found, temporal variations on time scales of milliseconds, that is, with kilohertz frequencies. Figures 18, 19, and 20 show three types of such oscillations discovered with RXTE, and each has opened up a major area of research. The captions explain the physical implications but give only a hint of the richness of information follow-on research provided.

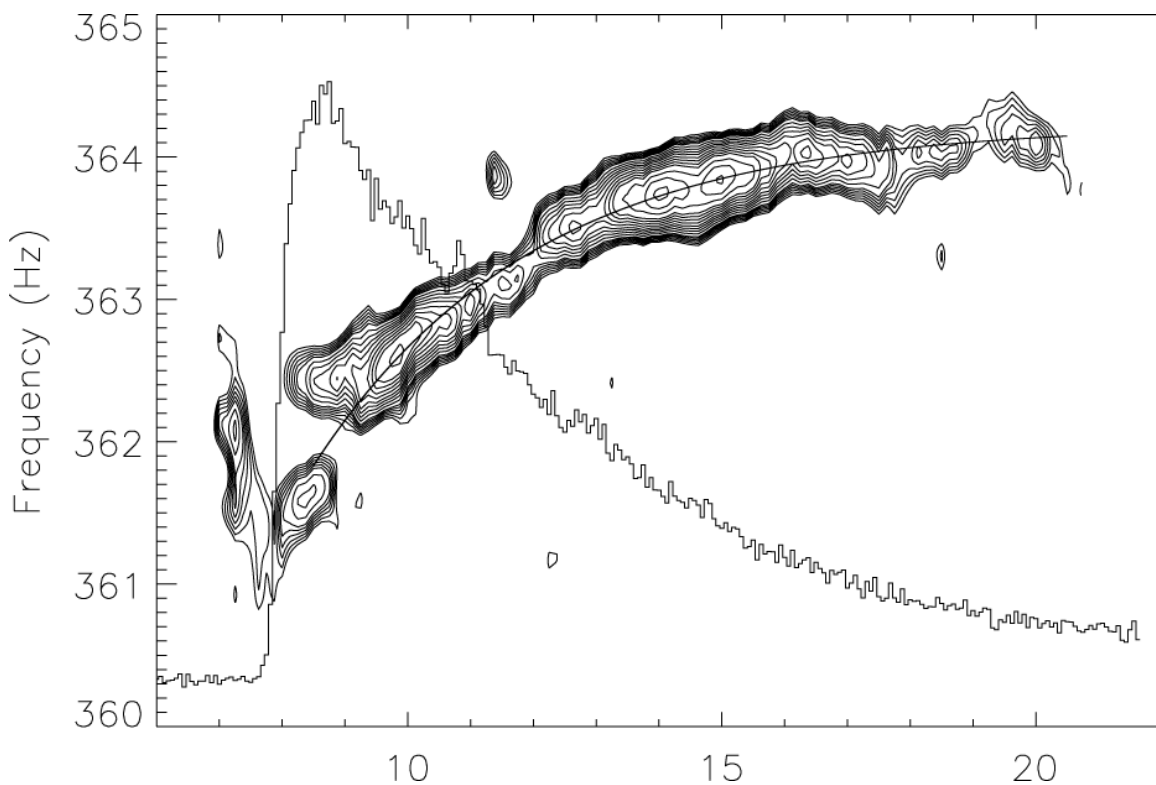


Figure 18. The intensity and frequency drift of an x-ray burst from the x-ray accreting binary system, LMXB 1728–34. Matter from a normal star spills over onto its binary companion, a neutron star, giving rise to a copious steady x-ray flux. The accumulated accreted material on the neutron star surface can erupt in a huge nuclear explosion that emits a “burst” of x rays. The histogram gives the intensity of such a burst as a function of time; note its short duration of only about 20 seconds. The contours show the strength of oscillations in the x-ray signal in the vicinity of 360-365 Hz. The data exhibit a strong ringing at about 363–4 cycles per second (hertz) with a frequency that drifts during the burst to an asymptotic value of about 364.2 Hz. The frequency is transient; it lasts only as long as the burst itself. The 364.2 Hz frequency is now known to be the rotational frequency of the underlying neutron star (Strohmayer, pvt. comm.) [see Strohmayer et al. ApJ 469, L9 (1996)].

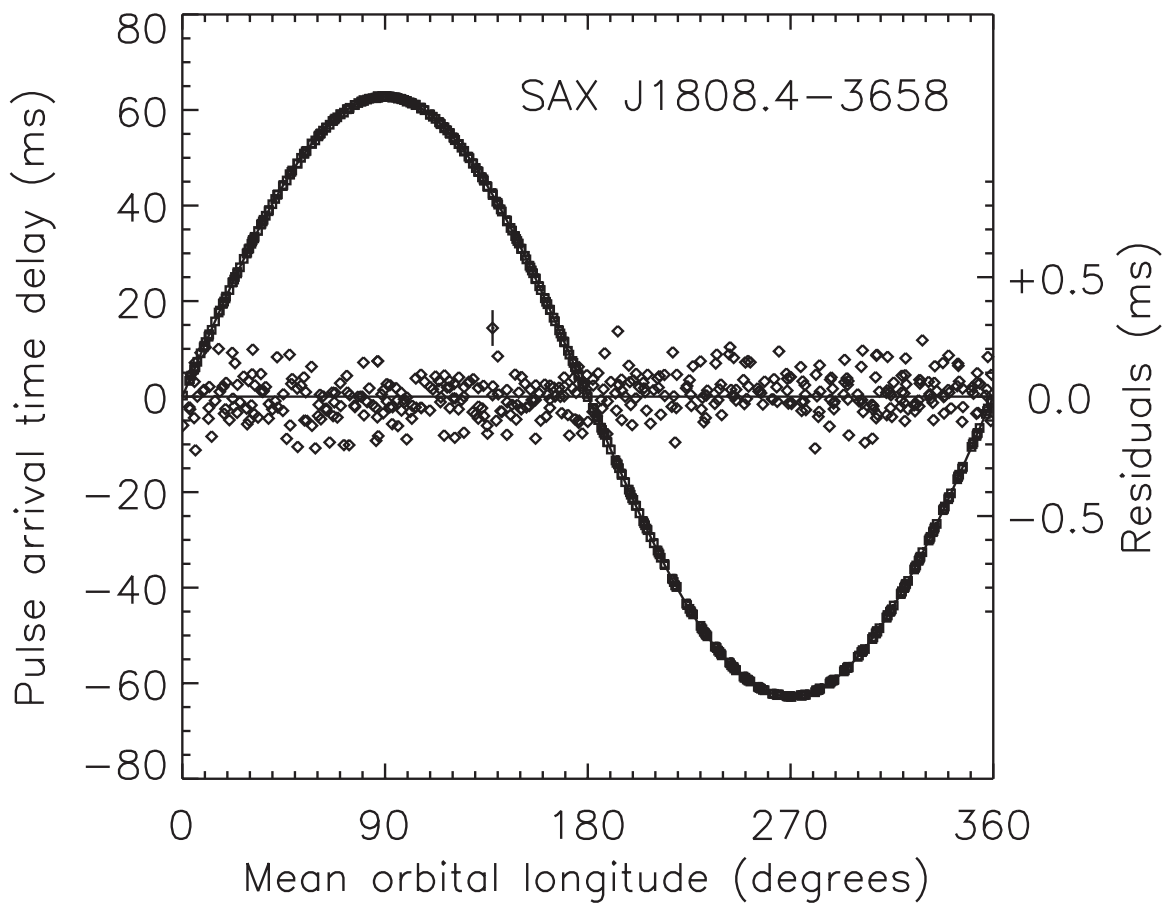


Figure 19. Time delays from the SAX J1808.4–3658 binary system as a function of the orbital longitude of the binary (think “time”). A normal star is donating matter to its neutron star companion. The infalling gas becomes “x-ray hot”(about 10 million degrees) and hence emits x rays copiously. The neutron star is spinning at the high rate of 401 cycles per second, and this causes the x-ray emission to be pulsed at that rate. As the neutron star orbits its companion with a period of 2.01 hours, it moves cyclically toward and away from the earth. At its greatest distance, the pulses received at the earth must travel a greater distance and thus their arrival is delayed, Similarly they are advanced when the neutron star is closest to the earth. The delay curve shown

here thus plots out the orbit of the neutron star with great precision. This was the first known x-ray emitting, persistent accreting millisecond pulsar. Its existence and coherence demonstrates that the radio-emitting millisecond pulsars (neutron stars) were spun up to their high rotational speeds by the accretion of rotating (in a disk like Saturn’s rings) gaseous matter onto the surface of the neutron star. The pulsing in this source was discovered in RXTE data by Rudy Wijnands and Michiel van der Klis (Nature 394, 344 (1998)). The Doppler orbit shown here was reported by Deepto Chakrabarty and Edward Morgan in the following paper (Nature 394, 346 (1998)).

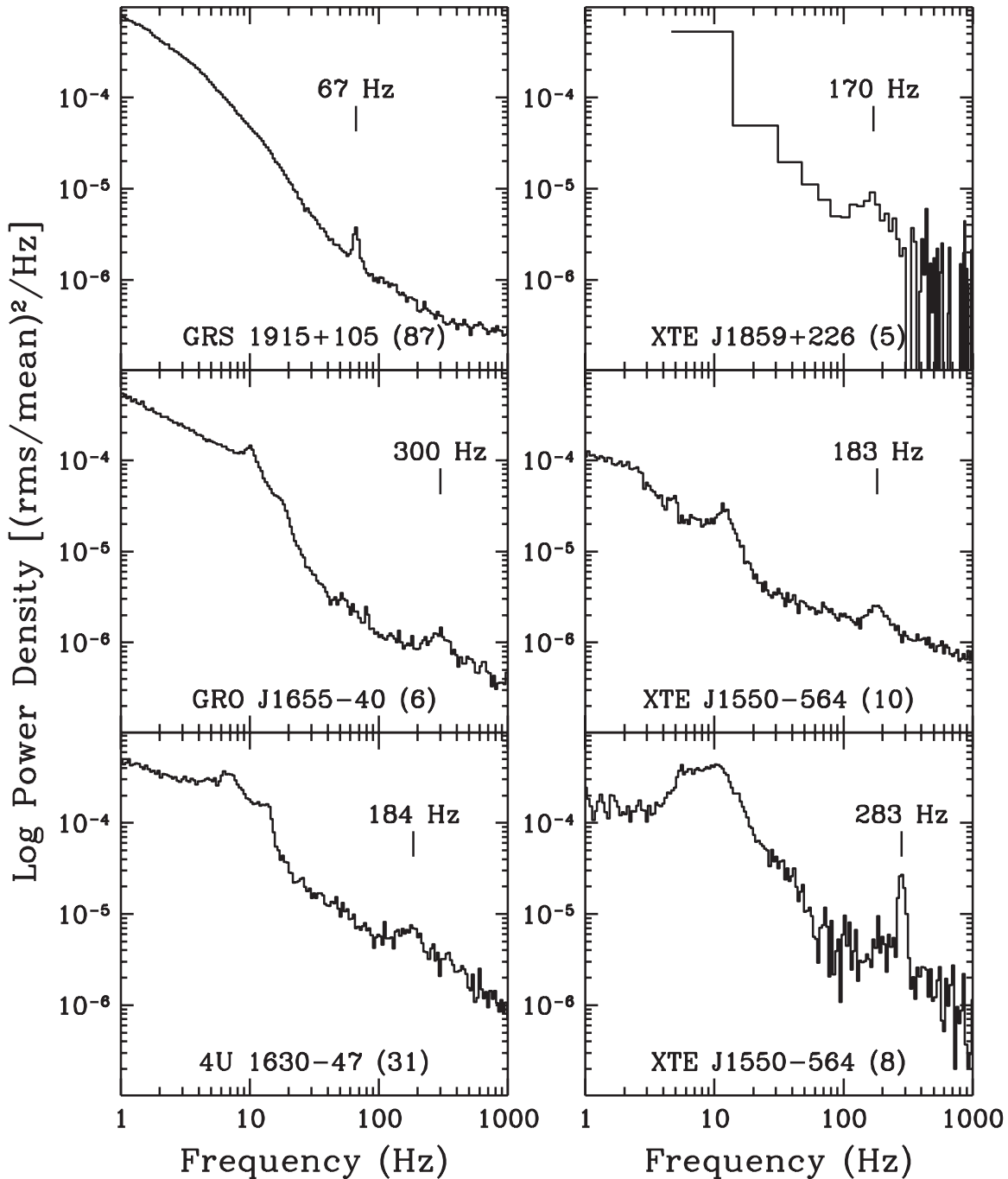


Figure 20. Power density spectra for five black hole binary systems. Each of these systems consists of a normal star spilling matter onto (or into) a stellar black hole. The radiation comes

from the gaseous material immediately surrounding the black hole. The indicated peaks represent ringing (intensity changes) at the indicated frequencies. These oscillations arise from the hot x-ray emitting gases in the near vicinity of the black hole and hence are strong diagnostics of those regions. They could be hot spots orbiting the black hole or oscillations of the accretion disk. [R. Remillard, in “Evolution of Binary and Multiple Stars,” ASP Conf. Series, v. 229, eds. P. Podsiadlowski, et al., p. 503.]

### *All-sky Monitor light curves*

The All Sky Monitor was, of course, our favorite at MIT because we had developed it. Over time, the results from it were spectacular. The assembly of three cameras on one rotatable shaft stepped around the sky, stopping for 90 s to record “images” of three  $10^\circ \times 100^\circ$  slices of the sky. It would then rotate to collect the next three slices for 100 s. Most of the sky would be imaged every 1.5 hours. Doing this hour after hour and day after day and year after year yielded a wealth of intensity points of the brightest 100+ or so x-ray sources. From these we were able to construct “light curves” (intensity vs. time) for these sources. Figures 21 and 22 show the light curves obtained with the ASM for a sampling of sources with interesting behaviors.

Observers used the results as context for their PCA observations, to determine when to observe with the PCA or other telescopes, and to extract results directly from the ASM data such as periodicities, positions of new transients and gamma ray bursts, etc.



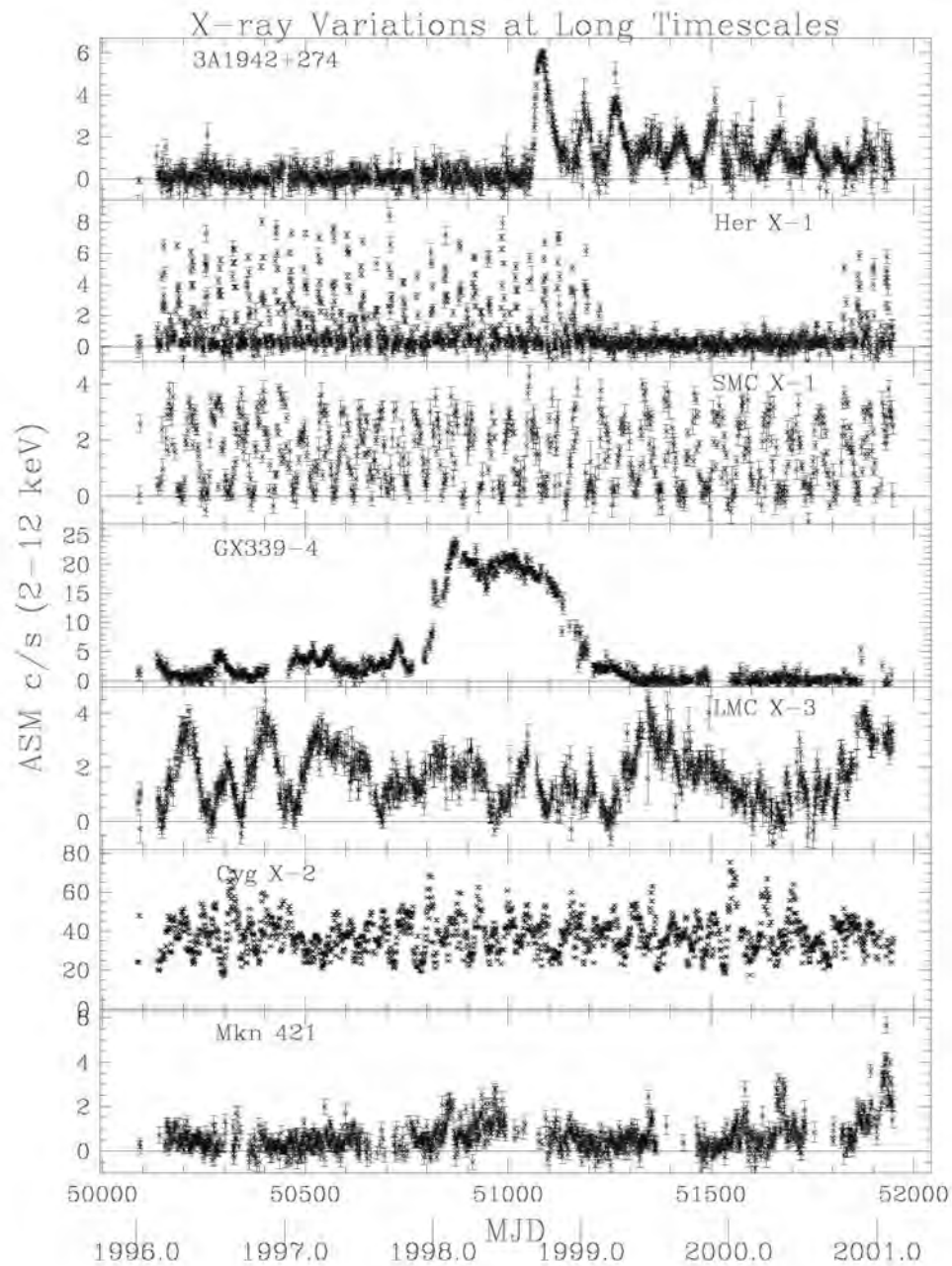


Figure 21. Five year light curves of seven “persistent” x-ray sources from the All Sky Monitor on RXTE. One of these (Mk 421; lowest plot) is a distant “active galactic nucleus” (AGN); the others are neutron star and black-hole binary systems in the Galaxy. These sources exhibit many types of variability, some periodic and some aperiodic. These data give insight into the physical processes taking place in the binary systems. They make possible pointed observations with the PCA or other telescopes at times when a source is in an intensity or spectral state of interest. They also provide observers with context for their pointed observations. Courtesy of Ronald Remillard and Alan Levine (in Bradt, Rothschild & Swank in Proceedings of the MGIX MM Meeting, eds. V. Gurzadyan, R. Jantzen & R. Ruffini, World Scientific 2002, p. 694.).

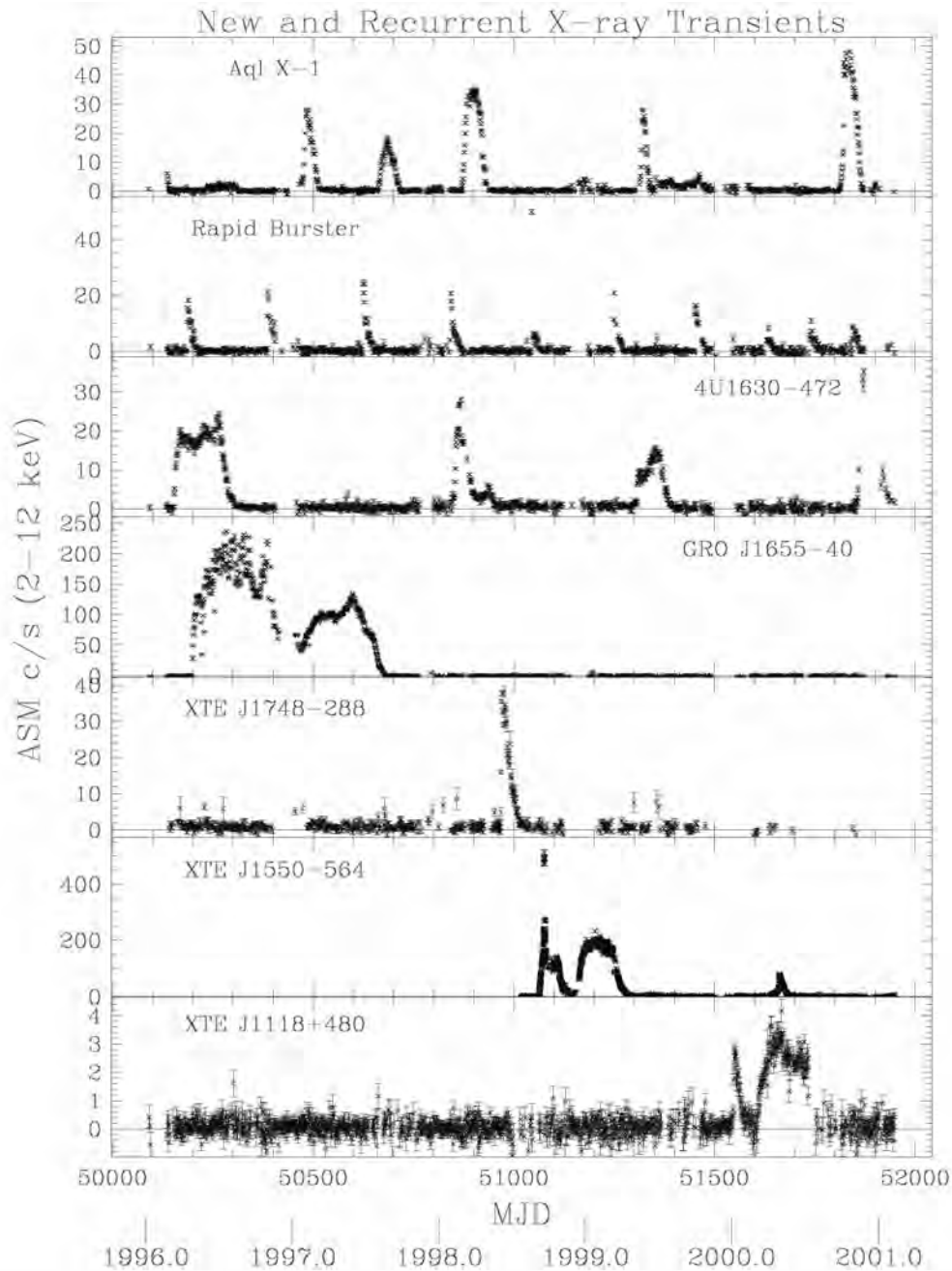


Figure 22. Five-year light curves for seven transient or recurrent x-ray sources from the ASM on RXTE. The accretion in these neutron star and black-hole binary systems is episodic, possibly due to instabilities in the accretion disk. These data allow observers to make pointed observations with the PCA or other telescopes at times when a source is active. Courtesy of Ronald Remillard and Alan Levine. ((in Bradt, Rothschild & Swank in Proceedings of the MGIX MM Meeting, eds. V. Gurzadyan, R. Jantzen & R. Ruffini, World Scientific 2002, p. 694.).

*Cyclotron lines with the HEXTE*

The HEXTE experiment produced a rich yield of solid results also. One of its objectives was to probe x-ray sources for evidence of strong magnetic fields through the detection of “cyclotron lines.” One example of its success is shown in Figure 23.

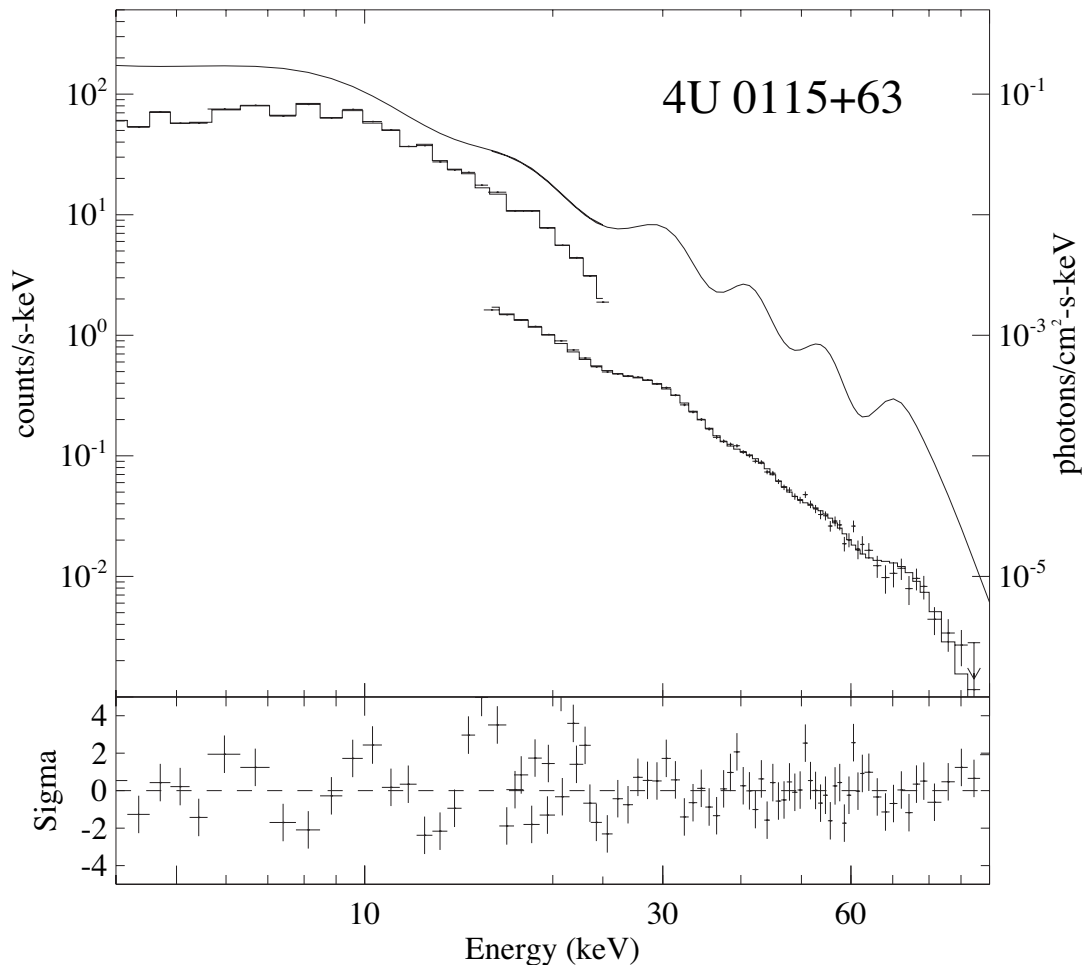


Figure 23. Spectrum of the x-ray source 4U 0115+63 obtained with the PCA and HEXTE on RXTE. The lower curves (histograms with smooth lines) are count rate spectra. The histograms are the data and the smooth curve through them is the model fit to the data. The upper smooth curve is the best fit incident spectrum. It exhibits five cyclotron lines. This was the first time more than two such lines in any source had been found. This allows one to model the emitting system to obtain geometries and an estimate of the magnetic field, namely about  $10^8$  T. It has long been thought that neutron stars emitting pulsed radiation had high magnetic fields of this magnitude, but cyclotron studies provide direct confirmation. It should be noted here that “magnetars” are neutron stars with fields as strong as  $10^{10}$  T ( $10^{14}$  G).

*Microquasar GRS 1915+105*

RXTE has studied in great detail the nature of a source known as GRS 1915+105. This source is highly variable in x rays and exhibits flares in the radio, infrared and x ray bands. Relativistic jets of material emerging from the object have been directly observed by radio astronomers. With strong likelihood, this system is a black-hole binary with a black hole mass of 10–15 solar masses. The object, with its jets, is reminiscent of extragalactic quasars which are powered by massive black holes with masses of 10 million solar masses or more. Galactic sources with their much less massive black holes exhibiting jets, such as GRS 1915+105, are thus known as “microquasars.”

Study of the microquasars can give valuable insight into the behavior of extragalactic quasars. Their much closer distances give greater fluxes at Earth despite their lesser luminosities. More important, the time scales of matter motions near the black hole, e.g., the orbital period, scale as the mass of the black hole. Matter motions can give rise to x-ray intensity variations. Thus a 10-minute variation in a microquasar of 10 solar masses would last 10 million minutes (20 years) in a 10-million solar mass extragalactic quasar. The study of variations in microquasars over hours can help us understand the processes in extragalactic quasars extending over centuries.

The GRS1915+105 source shows extreme x-ray variations over a four-year time scale in the RXTE ASM data (Fig. 24) and a bizarre set of repetitive intensity modes when viewed by the PCA over an hour (Fig. 25). The x-ray observations probe the innermost regions of the accretion disk immediately “outside” the black hole, whereas the radio and infrared are emitted by the emerging jet. Taken together (Fig. 26), the x-ray, radio and infrared fluxes appear to reveal the dumping of accretion disk material into the jet. Such data probe the connection of the accretion disk to the jet.

GRS1915+105: X-ray and Radio Monitors

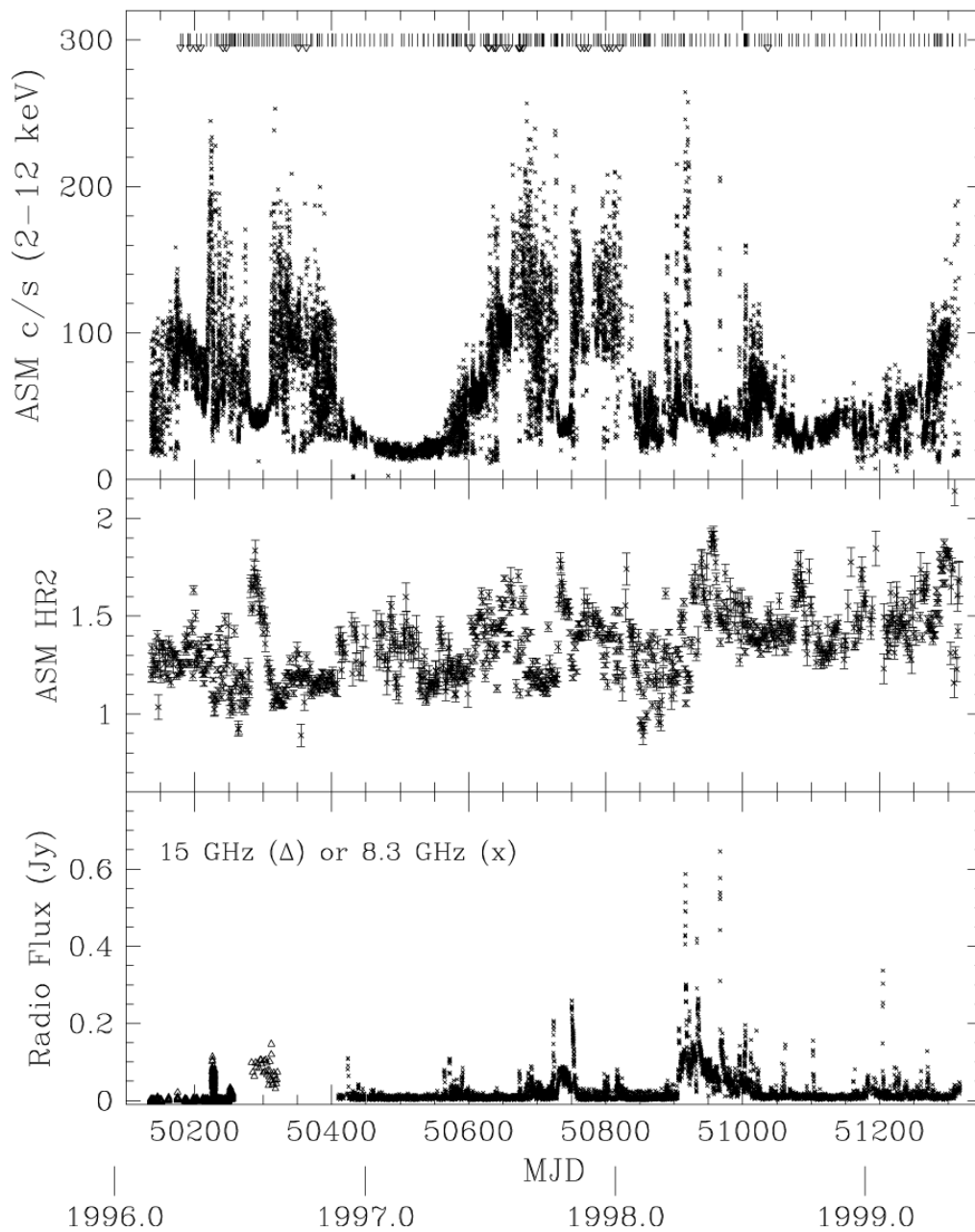


Figure 24. X-ray intensity of the “microquasar “ GRS1915+105 as measured by the ASM over 4+ years. The associated x-ray spectral hardness (from the ASM data) is in the middle panel and radio intensity is in the lower panel. The tic marks at the top indicate the times that RXTE was maneuvered to acquire PCA data from GRS 1915. The source is one of the most wildly variable x-ray sources known. It has a number of different x-ray states, each with its type of high frequency variability (see below), spectral hardness, and radio behavior. [Courtesy of Ronald Remillard]

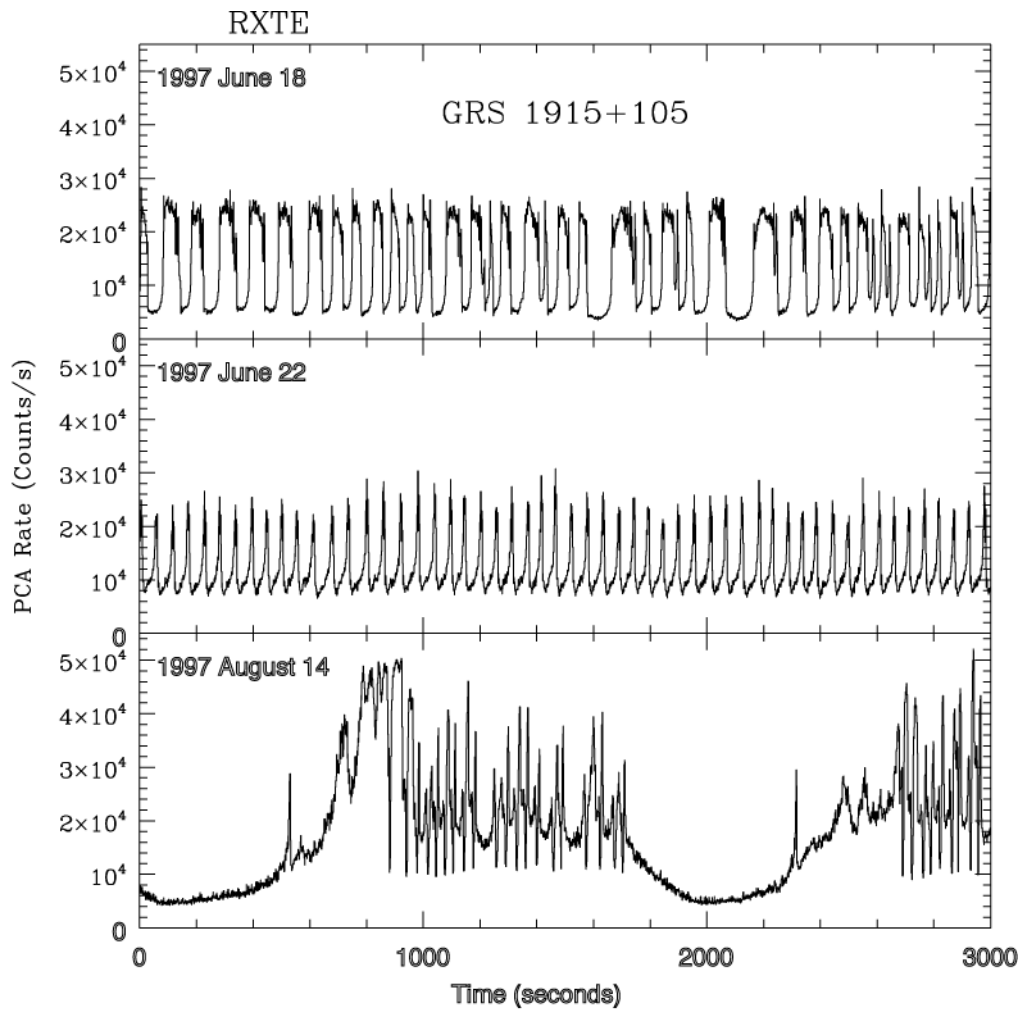


Figure 25. Three AMAZING modes of oscillation of the x-ray flux from microquasar GRS 1915+105 obtained with the RXTE/PCA in 1997 on three different dates. Note the plot extends over only about 1 hour. The lower plot may represent the periodic ( $\sim 2000$  s) dumping of material from the accretion disk into an ejected jet. See the following figure. (Courtesy of R. Remillard. [In Bradt, H. V., *Vulcano Workshop 1998: Frontier Objects in Astrophysics and Particle Physics*, Eds. F. Giovannelli and G. Mannocchi, Italian Physical Society Conf. Proceed.v. 65 (1999), p. 129]

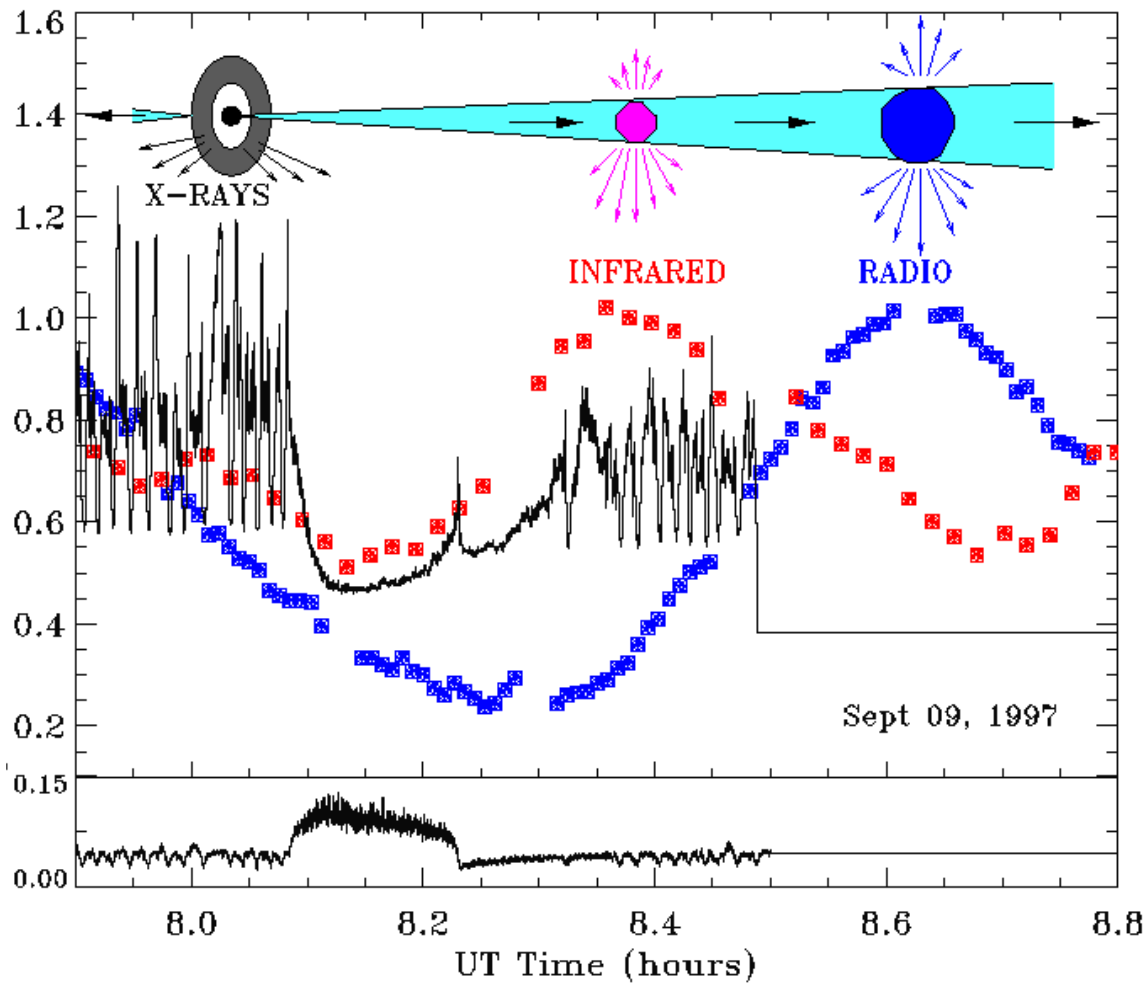


Figure 26. Intensity of x rays, infrared and radio as a function of time over about one hour with the x-ray hardness ratio shown below. The dramatic x-ray oscillations of the third panel of Fig. 25 are seen here to be followed by a rapid hardening of the x-ray spectrum, which in turn is followed by a sharp spike. Infrared radiation peaks immediately after the spike followed by a peaking of the radio flux. This can be interpreted as a relativistic jet of material (seen in the IR and radio) emerging from the “engine” of the microquasar. The x rays reflect the states of the innermost part of the accretion disk. The sharp spike may indicate the initiating event when the disk material is dumped or sucked into the jet stream. The ejecta cool as they move outward. Thus they are first seen in the infrared and later in the radio. (Mirabel et al., A&A 330, L9 (1998))

## VI. Final Reflections

RXTE put a microscope on the regions of strong gravity and high temperatures near black holes and neutrons stars. Our knowledge of these regions and of the objects themselves, their masses, structure and angular momenta, have been enhanced by RXTE observations. All the efforts that went into it proved to be well worth while.

As for our own experience at MIT looking back after 16 years of successful ASM and EDS operation and many fine results, I ask if this success was the product of our skill as engineers and scientists or simply good fortune? I think it was a lot of the former and some of the latter. I have hardly mentioned the many aspects of the ASM that proceeded with little angst that also contributed to the instrument's success: the rotation mechanism and angular readouts, the front end electronics, the mask coding and design, the operations plan (stepping instead of scanning), and finally the analysis algorithms. The EDS, as noted, was free of conceptual design difficulties except for the major board failure and the DSP chip misunderstanding. Its design when properly implemented led to absolutely perfect performance.

Alan Levine, Ron Remillard, and Wei Cui carried out the ASM science activities preparatory to launch and came up with many insights issues that contributed to the overall success. Since the EDS would also handle the ASM data, Ed Morgan was also key to the ASM efforts. Deepto Chakrabarty joined in after his arrival at MIT about the time of launch. The early contributions of John Doty and Garrett Jernigan are not forgotten either. On the engineering side, a professional team under Bill Mayer and Bob Goeke produced two fine instruments, the ASM and the EDS.

This of course tells only the MIT part of the story. Similar teams worked hard on the GSFC and UCSD experiments and on the spacecraft at GSFC. They too had their technical problems, but delivered successful systems on schedule and on budget also. Their stories would be equally exciting and revealing.

The selling and implementation of RXTE was all about people. Many were pulling hard for the mission: scientists, NASA officials at headquarters and at Goddard Space Flight Center, engineers, technicians, and many more. I was impressed with how the three PI teams, at GSFC, UCSD, and MIT worked together in resolving spacecraft issues. It was crucial that scientists talked to and listened to engineers and the engineers reciprocated. Likewise, scientists talked to and listened to NASA officials and the officials reciprocated. Important also was the role played by supportive letter writers and by the critics who forced us to sharpen our arguments, by the peer committees that reviewed and endorsed the mission, by theorists (especially Fred Lamb and David Pines) and observers who provided us with sound scientific arguments for the mission, even as the political and scientific landscape changed over the decades. Finally the entire effort was made worthwhile by the army of smart observers who proceeded to extract great science about compact objects from RXTE data during its sixteen years of flight operations.



APPENDIX

George Clark’s Letters

to

Senator Ted Kennedy and Representative Tip O’Neill

Note: The letter to O’Neill was identical to that sent to Kennedy.

and to

Hans Mark  
Deputy Administrator, NASA

(1980 and 1981)

MASSACHUSETTS INSTITUTE OF TECHNOLOGY  
CENTER FOR SPACE RESEARCH  
CAMBRIDGE, MASSACHUSETTS 02139

October 16, 1980

Dr. Frank Martin  
Director  
Astrophysics Division  
NASA Headquarters  
400 Maryland Avenue, S.E.  
Washington, D.C. 20546

Dear Frank:

Having learned in the past that the correction of the defects in NASA's proposal and selection procedures is blocked by NASA procurement and legal officials, I have written to Senator Kennedy and Representative O'Neill about my concerns. I enclose copies of my letters for your information. I hope a little pressure from the hill will make reform possible.

Sincerely yours,



George W. Clark

GWC:seb

MASSACHUSETTS INSTITUTE OF TECHNOLOGY  
CENTER FOR SPACE RESEARCH  
CAMBRIDGE, MASSACHUSETTS 02139

MEMORANDUM

TO: Herbert S. Bridge  
FROM: George W. Clark *RWC*  
DATE: October 23, 1980  
RE: Call from Frank Martin about my letter  
to him of October 16 with enclosed  
copy of my letters to Kennedy and O'Neill

Frank was angry, and took me to task for not having discussed my complaints with him before writing to the hill. He said 1) that NASA policy is not necessarily against presentations and rebuttals; 2) in-house proposals are fairly evaluated with respect to Civil Service costs; and 3) government procurement officials are pushing NASA toward RFP's for defined scientific instruments instead of A0's. He wants to discuss these matters with me in detail in the near future, perhaps when we meet to discuss the SAS-3 data analysis.

GWC:seb

cc: H. Bradt  
C. Canizares  
W.H.G. Lewin

MASSACHUSETTS INSTITUTE OF TECHNOLOGY  
CENTER FOR SPACE RESEARCH  
CAMBRIDGE, MASSACHUSETTS 02139  
Room 37-611

October 16, 1980

The Honorable Edward M. Kennedy  
The United States Senate  
Washington, D.C.

Dear Senator Kennedy:

In response to a recent Announcement of Opportunity a group of MIT scientists and engineers under the leadership of my colleague, Professor Hale Bradt, is about to submit to NASA a 25 million dollar proposal to develop and operate an instrument for a satellite X-ray observatory called the X-Ray Timing Explorer (XTE). This project will be a key element in the U.S. program of research in X-ray astronomy in which MIT has participated with highly successful experiments on the satellites OSO-7 (1971-72), SAS-3 (1975-79), HEAO-1 (1977-79) and HEAO-2 (1978-). MIT has an exceptionally able group of scientists and engineers with extensive experience in all aspects of the XTE mission. I believe the proposal has a good chance of success if the peer review and final selection are done competently and fairly.

Recent experiences with NASA's procedures in proposal evaluation and selection make me doubt that they will, in fact, be competent and fair. The present procedures have the following three major flaws:

- 1) Several years ago NASA changed its peer review procedures and now no longer permits proposers to participate in the presentation, criticism and defense of their own and competing proposals. At best it is extremely difficult for committee members to comprehend the technical details of the proposals before them and to arrive at well informed judgments of relative merits. The situation is aggravated by the fact that most of the experts in the particular specialty are among the proposers and are therefore disqualified from membership on the peer review committee. As a result some recent NASA peer reviews have arrived at wrong technical evaluations that could have been prevented by open discussion and debate in front of the committee by the proposers. Complaints about this problem to NASA officials have been put off with statements about what NASA lawyers will and will not allow.

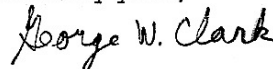
The Honorable Edward M. Kennedy  
Page 2  
October 16, 1980

- 2) NASA also changed its procedures with regard to the prerogatives of peer review committees in recording their evaluations. All proposals are placed in four categories. Proposals in Category I are good in science, meet the mission objectives, and are ready to go. In Category II they are not so good in science though they meet the mission objectives and are ready to go. Previously the committees ranked Category I and II proposals as to relative merit. NASA made the final selection from Category I, or from II if there were no I's, and the selections were made in order of merit in so far as considerations of scientific balance and costs permitted. Now NASA forbids merit ranking and is therefore free to choose from Category I proposals on quite other grounds than relative scientific merit.
- 3) NASA laboratories which submit proposals in competition with private institutions do not include the full cost of the participation of Civil Service employees. Given several Category I proposals, which in the case of XTE will probably include one from NASA's own Goddard Space Flight Center, NASA may choose its in-house proposal on the basis of an unfair comparison of costs.

To achieve a fair evaluation and selection I believe it is essential 1) that proposers participate in the presentations, criticisms and defenses of their own and competing proposals; 2) that the peer review committee be instructed to rank proposals as to their relative merits; 3) that proposals from government laboratories be costed on precisely the same accounting basis as those from universities, specifically including all Civil Service salaries, benefits and proper overhead charges; and 4) that results of the peer review be on the public record.

I ask your help in assuring that the XTE selection will be conducted wisely and fairly, and I ask you further to consider what measures Congress might take to ameliorate the defects in NASA policy which sometimes result in the waste of resources by selection of inferior experiments.

Sincerely yours,



George W. Clark  
Professor of Physics

GWC:seb

MASSACHUSETTS INSTITUTE OF TECHNOLOGY  
CENTER FOR SPACE RESEARCH  
CAMBRIDGE, MASSACHUSETTS 02139

cc: Bradt  
Bridge  
Canizares

July 16, 1981

Dr. Hans Mark  
Deputy Administrator  
Office of the Administrator  
NASA Headquarters  
400 Maryland Avenue, S.W.  
Washington, D.C. 20546

Dear Hans:

Congratulations on your appointment. With a pioneer in X-ray astronomy at the helm NASA can't go wrong. And I think Frank Martin would be an excellent Associate Administrator for Space Science. He is a strong manager and a smart scientist with a clear sense of scientific values and political realities. I have talked with many who share this view.

Among the many urgent questions before you must be:

Whither the Explorer program? and  
Which Explorers next?

The revolutionary impact that Explorer-class satellites have had on astronomy is evident throughout the literature (e.g., the Uhuru Catalog was the most frequently cited of all scientific references in 1974). But do they have a future after IRAS? The list of highly promising candidates for the new astronomy Explorers, and the enthusiastic support of the program by the community as expressed in the Field report suggest they do. Strong cases are made for Explorers in other disciplines. Their record of scientific yield per dollar is good. What they need is a shot in the arm with a funding augmentation to a level of \$80M per year, and strong guidance from Headquarters that will keep their average cost below one annual program budget per mission.

As to specific Explorers, I want to argue the merits of the X-Ray Timing Explorer (XTE) and urge that it be started in time to get it into orbit during 1986.

The XTE mission, with its focus on the physical processes of compact X-ray sources, exemplifies the unity of physics and astronomy. The Pines/Lamb workshop report ("Compact Galactic X-Ray Sources") shows how XTE studies of neutron stars and white dwarfs will provide otherwise inaccessible information about matter under extreme conditions. It will lead to new understanding of superdense hadronic matter, hadron superfluidity, high temperature plasmas in superstrong magnetic fields and in gravitational fields strong enough to manifest macroscopic general relativistic effects [see Pines, Science 207, 597 (1980)].

Dr. Hans Mark  
Page 2  
July 16, 1981

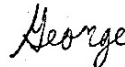
With capabilities far greater than those of previous missions, the XTE will also address questions at the frontiers of astrophysics: the masses of neutron stars, the internal structure of normal stars (revealed through the apsidal motion of their neutron star companions), thermonuclear bursts on the surface of neutron stars, the evolution of close binaries, the sizes (from variability timescales) and mechanisms of quasars and active galactic nuclei. The XTE has a greater chance than any other conceivable mission of establishing the existence of black holes, whose clearest signatures are almost certainly to be found in the character of their millisecond variations.

The XTE will have very great scientific leverage. Its results will feed directly the mature theoretical efforts in neutron star physics, stimulate and complement investigations of optical astronomers concerned with the evolution of close binaries, and delve into new domains of short timescale phenomena where important discoveries are likely to be made. The XTE will exploit the remarkable circumstance, revealed by previous X-ray missions, that the variabilities of celestial X-ray sources are rich in physical information, a circumstance which permits a relatively simple instrument of straightforward design to collect a wealth of significant data. In the specific area of pulsating X-ray binaries, Doppler analysis of phase-coherent pulse timing measurements over periods of days and weeks yields dynamical results accurate to parts per million or better.

Since the mission will employ only proven and highly reliable technology, the chances of overruns in a strongly managed project are negligible. And the mission is well suited to serve the scientific needs of many progressive and productive astronomers whose involvement will assure a high yield of important results.

The High Energy Astrophysics Panel of the Astronomy Survey Committee, which I chaired, strongly endorsed the XTE, and the final ASC report will do likewise. I urge you to give it your careful consideration as you set the new priorities of the agency.

Sincerely yours,



George W. Clark

GWC:seb