Recollections of MIT Oct 1961 - Aug 1968

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In my final year at Trinity College, Dublin, together with a few others, I decided it would be best to go to the United States for my doctorate. I applied to six Universities and got offers of Research Assistant posts from Yale, Princeton and MIT. I did not know anybody with experience of graduate study in the US and almost by chance chose MIT, whose propaganda was the most enticing. I got a travel grant from an exchange programme - this was to prove a nuisance in the long run because I had to get an Exchange Visitor visa that limited my options later on. I had to go through a rigorous health check with an x-ray etc.

I flew to Boston as soon as I was clear of my final TCD exams.

My arrival in Boston was on a warm Saturday evening (7 Oct 1961) and it remains clear in my mind. I flew with John Miller, a mathematician from TCD, and we got a taxi from the airport to Cambridge, where we had arranged to stay in the YMCA near Central Square. This was quite old-fashioned and unpromising. The next day we went to MIT and looked around the campus.

MIT is situated on the Charles River, downstream from Harvard and across from Back Bay in Boston. The part of Cambridge wher it was located was not the best. At least in those days, the area immediately behind it was filled with factories. There was considerable noise pollution from the Carr fastener company that made electronic connectors and the Necco chocolate factory that filled the area around with a sickly chocolate smell. The most pleasant part of Cambridge is that around Harvard, about 2 kms behind MIT. Between the two institutions is the Central Square district of local offices and small houses.

On the Monday I went to the Physics department and was assigned Lee Grodzins as my advisor. He was known for work on measuring the helicity of the neutrino (Goldhaber, Grodzins and Sunyar). Because of Trinity College's awkward final exam dates I was several weeks late for term and the beginning of the lecture courses. Grodzins asked me if I had seen the "Qualifying Exam" paper and I said no. He then put me in a room and had me write it then and there. This was a kind of diagnostic exam and general check.

Though one was encouraged to believe that it would take 4 years, at that time the (unadvertised) average to complete a PhD in Physics was about 4.75 years. As it turned out, for reasons that will be obvious later, I took 7. The general idea was that one had about two years of coursework to prepare for a General Examination in two parts – classical and modern physics, with an oral. Then a thesis should take about 2 years, with a formal examination at the end. In addition one had to take 3 courses as a "Minor" – in my case mathematics – and pass an examination in two languages – in my case French and German. A full-time student could take 4 courses each semester but only those few who had full scholarships could reasonably do this. Assistants usually took 3.

The semesters were long – about 3½ months - and as part of each course one enrolled in a "recitation section", a sort of tutorial with a few dozen students. There were homework assignments each week and a test every few weeks, together with a final exam at the end. It was like going back to high school. This was very different from the easygoing way at TCD! For a research assistant, the vacation was just two weeks in a year, though there were a few short breaks at other times. For each course one got a mark at the end, from A downwards. A was worth 5 points and so on. One's long-term average was called the "cum", for cumulative, and 3 was regarded as the minimum acceptable.

I got registered as student 615536. To become a person I had to get a "Social Security Number" at the Cambridge post office. I opened a bank account at the Harvard Trust Co. On registration I was issued with a pack of punched cards, one of which had to be given in at each course attended. Physics is known as "Course 8" at MIT.

Grodzins must have thought I had done quite well because he advised me to take his own Nuclear Physics Course 8.511/512 and a quantum mechanics course 8.731 given by Felix Villars, a former associate of Pauli. Grodzins' course was known to some students as "Anecdotal Nuclear Physics" and indeed in it I heard many of the now familiar stories about famous physicists, such as Feynman, Pauli, Dirac and others. The course itself was disjointed – maybe that was the nature of the subject - and I found it hard to keep up to date with it. I ended up with a C. Worse still was the Villars course, which I was poorly prepared for and eventually failed. However, I took a course from Stan Olbert on space plasmas etc and this one I got an A in, which saved me from being chucked out.

Joining the Cosmic Ray Group

To support myself I had a Research Assistantship that paid my fees (about $1600 per annum) and gave me the barest minimum (a bit less than $300 per month) to live on in exchange for 16 hours of work a week. Fortunately I also had also a small scholarship for two years from TCD that paid about $400 per annum. I had initially been assigned to the Cyclotron (Synchrotron?) Group but soon transferred to the Cosmic Ray Group, where I was taken on by Frank Scherb.

The head of the Cosmic Ray group was Bruno Rossi who had been one of the pioneers of cosmic-ray research in the 1930s and had invented the coincidence circuit (the original "and" gate) and had been involved with radiation measurement and evaluation of the implosion techniques during the Los Alamos Bomb project. Quite early on I was given one of Rossi's papers to check his English. Not knowing any better, I turned it into English English. The result was that I was never asked to do this again! After a while I grew accustomed to American English and avoided various embarrassments.

The Cosmic Ray Group had several main themes during my time there. They had veered away from the traditional study of Air Showers, though a Meson Monitor was still running in the penthouse of Building 26 and they still had a finger in

Volcano Ranch where some of the highest energy particles observed up to that time had been detected. The Soviet Union's launch of Sputnik of 1957 had deeply shocked the USA and NASA had quickly encouraged the American scientific community to invesigate space. An early result was Van Allen's discovery of the radiation belts around the Earth. The MIT Cosmic Ray Group quickly developed satellites that were the first to detect the Solar Wind and cosmic γ rays (Explorer X and XI respectively). In fact, the decade of the 1960s was to turn out to be an exceedingly productive one for astrophysics, especially with the opening up of spectral regions that were unobservable before.

Several MIT Cosmic Ray Group members were involved in the discovery of cosmic X-rays through their company American Science and Engineering. Later, X-rays became the main theme within the Group itself.

In mid-decade moves were made to connect the various MIT departments involved in astrophysics more closely through new courses and joint seminars such as COMPASS. There were mathematics groups involved in galactic dynamics. Physics included stellar structure besides the Cosmic Ray Group. Electrical Engineering had radio astronomy. Earth Sciences had planetary interests.

I shared an office with 5 other graduate students at 26-453. I gradually got used to being a number, working in a building with a number, in an office with a number and attending courses with numbers. Once I saw a student walking around dressed as an IBM punched card bearing the standard words "Do not bend, fold, spindle or mutilate" and I understood perfectly what he felt.

My supervisor/employer, Frank Scherb, was a bachelor in those days and we sometimes ate together in the MIT cafeterias. On a visit to the Soviet Union to meet some relations, he met and eventually married a Russian [or perhaps Ukrainian] lady. They had two sets of twins in quick succession. He said he did not sleep for four years.

Frank had to lecture 8.031 and 8.041, the main second year physics courses. This was an enormous class, held in 26-100 and maybe had around 500 students. On a certain occasion he told the class he would have a surprise for them in the next lecture. This turned out to be that it was the 100th anniversary of Maxwell's Equations. The class let out a massive groan!

On another occasion, Frank was involved in an unexpectedly spectacular lecture demonstration intended to show the power stored in a charged capacitor by short-circuiting it with a thin wire. The lecture room lights were turned off for the occasion. What he did not notice was that the voltage on the capacitor was accidentally doubled, so that the energy stored was 4 times the usual, and a brilliant fountain of molten iron ensued, leaving him almost deaf for the rest of the lecture! The students had a camera with shutter open to record the demonstration the next time he did it – however at the normal voltage.

My initial work was to examine thousands of print-outs from the Explorer X plasma probe satellite and try to extract whatever information had not already been published. This rotating satellite or rather probe had carried a sort of Faraday Cup that measured the flux of Solar Wind charged particles in a certain energy range and had had a relatively short life, travelling from the uper atmosphere of the Earth to just outside the the geomagnetic cavity. The task was pretty boring but Frank was a sympathetic supervisor. I had not encountered big computers seriously before and learned to programme the IBM709 (the last big vacuum tube computer) of the Computer Center, in Fortran 2, using punched cards.

A place to live

I joined John Miller, who was a grad student in mathematics, in an apartment in the somewhat crummy Pearl/Franklin streets area of Cambridge. We had a third roommate, an undergraduate who eventually walked out, leaving us in an untenable financial position. Luckily I had a small Scholarship from TCD for the first two years. We had to pay more than we could afford to a lawyer to extricate us from the lease. I then found a garret in Center Street, near Cantral Square, at $7.50 per week in the house of a twittery old lady who liked to push religious pamphlets under my door. I set myself up to be able to make breakfasts. My other meals I mostly ate in the various MIT cafeterias. If you paid more than 99 cents you had to pay an additional Massachusetts Old Age tax, so we would always go back later for our 10 cent coffees.

Usually I travelled to MIT by bus down Massachusetts Avenue, though if the weather was nice I would walk. There were thuggish elements around and one had to be alert. I had to equip myself with a heavy coat and gloves to survive the winter, when snow could be on the ground for 4 or 5 months.

In retrospect, there were many things I should have done better. One would have been to reside initially in Grad House, as loneliness was a serious problem for me. I eventually found that the only way to cope with the incessant homework assignments was to collaborate with other students doing the same courses. As an experimentalist, I resented the apparent belief that solving endless boundary value problems was of educational value. My own feeling was that a strong understanding of the theoretical structure of a subject was more to the point. The few homework problems that I enjoyed were about proving little theorems.

Nevertheless I did manage to do a few things outside work. I travelled around whenever I could during breaks and long weekends and made use of various hospitality plans for foreign students. I visited Cecil Graham, a TCD friend at Brown University in Providence, Rhode Island, and I went with John Miller, Diarmuid O'Mathuna, an Irish grad student in mathematics, and another student to Washington DC. I visited my Uncle Sydney Glass who bred horses in West Chester, PA. At Easter in 1962 I went with an English student who had a Marshall scholarship, and therefore a car, to eastern Canada. We stayed with Chester Glass, a cousin of my father's in St Catherines, Ont, and visited the Niagara Falls. Later we passed through Toronto, Ottawa and Quebec, returning through Maine

and New Hampshire just after the border crossing had opened after winter. In Boston I sometimes I went to the English Speaking Union in Back Bay and drank their terrible tea, made in a samovar. I also occasionally went with John Miller or Diamuid O'Mathuna to events such as concerts of the Boston Symphony Orchestra. The conductor when I arrived was Charles Munch. He was followed by the more pedantic Erich Leinsdorf, a Mahler fanatic. One could sometimes get tickets that came from abonnement holders who could not attend for some reason and these were sold cheaply to students to support the musicians' retirement fund. I also enjoyed free Sunday afternoon concerts in the atmospheric Isabella Stewart Gardner Museum, one of my favourite places in Boston. Sometimes also there were lunchtime concerts in the Hayden Library given by MIT students. The Brattle theater in Harvard Square (as with several other so-called "squares", there was nothing square about it) was famous for classic movies, especially Bogart ones, and the various film series presented in 10-250 at MIT led me to an appreciation of cinematic art. I made good use of the humanities library both for borrowing books and for reading the newspapers.

For much of my time there, MIT had a really excellent branch of the Harvard Coop bookshop. My spare cash went on buying the wonderful American paperbacks, which had only just begun to reach England and Ireland. The Paperback Booksmith in Harvard Square stayed open until about 10pm and I often used to go there. Not long after I arrived, the building on the campus containing the Coop (pronounced like a chicken coop) and the WGBH television studios burned down. I was amused on a much later occasion when a huge, prominently labeled, "Fireproof Warehouse" on Vassar Street nearby also went on fire.

One thing I missed at MIT was female company. The number of women students was then a very small fraction of the total and those that existed were usually excessively serious. The McCormack women's dorm was constructed during my period there and I believe that now the sex ratio is much more even. There were a few young secretaries and assistants also. Sometimes I had girl friends but no serious relationships developed.

Many of the undergraduate students were very nerdish. They had often come from high school backgrounds where they had worked very hard to be top of their classes and had found they had to work even harder to stay afloat at MIT. The admissions policy was such that only students very likely to succeed were selected and it was regarded as a failure on the part of the Institute if anyone "flunked out". Only a very few were thrown out for academic reasons each year. The pressure on students seemed to peak in their second (sophomore) year. Frank's class had I think 6 suicides one year and, of course, he had to comment when these were investigated. The class was so big that he often did not know the student involved. It was the task of the "recitation instructors", tutors who worked with small subsections of the class, to cope with individuals.

The buildings were all connected by corridors, some of them very long. Walking along these is one of the abiding memories most MIT people must have. It was said, probably apocryphally, that the original architect of the campus had been,

as a student, unable to afford an overcoat! It was always interesting to pass "Doc" Egerton's office where many of his amazing stroboscopic photographs were displayed, like the famous one of a splashing milkdrop.

There was a kind of sardonic counterculture shared by many of the students. One aspect was the motto IHTFP or "I hate this F place" that appeared from time to time. Once was on the newly constructed ~20 storey Green building, achieved by blocking out the light from appropriate windows. Such "hacks" or practical jokes still occur, it seems.

Quite a few undergraduates worked part-time on the campus to make ends meet. There were jobs available serving in the cafeterias as well as in libraries. Many of them were expert programmers and formed a very valuable resource of cheap computer talent. I was interested on a later visit to see how an undergraduate student friend made up his annual budget from different sources such as a parental contribution, a long-term-loan and a part-time job. Outright scholarships were few and usually for a single year.

At the end of the first year I was quite depressed because of my difficulties with passing the courses. I think it was then that I was told I could not stay registered for a PhD but I got agreement that if I completed a SM (Master of Science) satisfactorily I could be readmitted for a doctorate. After the semester ended I went to New York for about a week of sightseeing, theatres and museums as I thought I might not have another opportunity. Then I returned to Dublin. The possibility was still open to go to Cambridge, England, (non-Mass as Gamov called it) and indeed I visited there, staying with my friend Alan Walton in the still uncompleted Churchill College. I visited the Cavendish Laboratory but somehow did not feel inspired to apply to study there. I decided to bite the bullet and return to MIT in the expectation that I would survive and complete my PhD. Somehow I still had faith in myself.

1962-3

It must have been during this period that I saw Norbert Wiener on the bus, going to MIT. He was one of the most famous people at MIT, though then very old and on his last legs. Years before I had read his book on Cybernetics and I still have his autobiographies. On this occasion he was sitting next to a pretty latina and trying to make conversation in rather bad Spanish. She clearly wondered what this scruffy old man was up to.

In my second year at MIT I stayed in a slightly better garret in Maple Street. This I shared with David Frech, a mature Nuclear Engineering student who was an operator at the MIT nuclear reactor, a little-known feature – at least among the ordinary citizens of Cambridge - of the campus. He was a West Point graduate who had served in the US Army in Korea. On one occasion he showed me around the reactor, which included an operating theatre for treating patients with brain tumours (with little success).

I decided that my Physics knowledge would have to be totally reconstructed and took the graduate Classical Mechanics course 8.711/2 from Lemmer, a South African low-energy nuclear physicist. I also took electromagnetic theory from F Low, known from the Chu, Goldberger and Low dispersion relations. Both these were good lecturers and I got reasonable grades. There were several other students who I shared an office with in 26-453 and we collaborated on solving homework problems, which made the task bearable. Al Klimas and Vytenis Vasyliunas were doing some of the same courses that I was. Al Krieger, Paul Higbie and Henry Helmken also shared this office. Klimas had transferred from the US Coast Guard.Academy and had had enough of naval life. When Captain Arentzen, the administrator of the Physics Dept told him he had "volunteered" to mark exam papers he was furious and gave him a piece of his mind. Koster, the head of Physics, phoned him a bit later and said "You didn't mean what you said to Captain Arentzen, did you". End of rebellion.

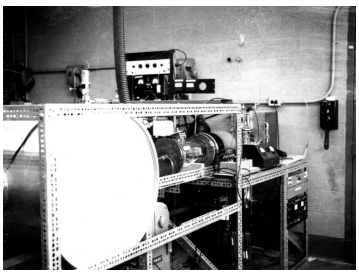


Fig 1: Large vacuum system and ion source for testing plasma probes.

For my Research Assistantship I constructed a large vacuum system with a focused radio-frequency ion source for testing (space) plasma probes. I learned a lot about vacuum system design. The main pump had to be cemented to the floor. Its vibrations disturbed the molecular beam lab below us, so I had to liaise with them every time I ran it.

We had some interesting technicians to help us. My favourite was Bill Smith, who I got to know fairly well, with his family. He had worked at the MIT Radiation Lab on developing radar equipment with the famous electronic engineer of vacuum tube days, Matthew Sands, during WWII. He was a New Englander from New Hampshire and a great admirer of hard work. I sometimes had to take a lot of time off to prepare for exams and he would be quite crusty when I got back. But when he saw me working every day till late he would melt and offer his help. Our machinist, Charlie Fernald, claimed to have been a liquor runner during Prohibition and had some amusing stories to tell of escapes at sea.

The Laboratory for Nuclear Science in which our lab fell was the most productive environment that I ever worked in. We had an excellent design office under Cy Tourtelotte who could produce a bundle of blueprints (no Autocad in those days!) in no time. There was a large workshop with a staff of instrument makers. We had a comprehensive electronic stockroom, like a supermarket. If something had to be purchased, the purchasing department did all the research and procured the item, often on the same day. I think that their ethos had developed under wartime conditions, when everybody had to get the job done and burocracy was minimized. It probably helped that anything space-related was well funded by NASA as the shock caused by the Sputnik launch in 1957 had not yet worn off.

Normally one was expected to take the General Examination for the Doctorate in June of the second year, but there was no way that I could consider myself sufficiently prepared.

Accordingly, I found a job for the summer at Goddard Space Flight Center outside Washington DC with Keith Ogilvie. Here I worked on the development of a detector/mass spectrometer for solar wind particles. I had to be employed through a contractor ("white slave trader"), being a foreigner who could not work for NASA directly. During this period I stayed with Ogilvie in Takoma Park, Maryland, as he was between wives and enjoyed the company. We kept in touch for many years and later I usually spent Christmas in Alexandria with him and his second wife.

From the proceeds of this job I was able to buy a car – a Fiat 2100, a model said to be much favoured by nuns in Italy. This made a big difference to my life and I began to feel more human. I also had to learn to drive, which I did from a Mississipian who I could barely understand. I did my test and got my first driving license in Maryland. Almost immediately I had to drive the 450 or so miles back to Boston.

Finding parking space near MIT was difficult and I often parked around what later became Technology Square. Parking tickets were only $1, as far as I remember, but even this was a lot on a student budget. Sometimes after the police had been around, a student would gather up all the tickets he could find and put them in the nearest mailbox!

1963-4

My living circumstances improved in the new semester. I moved into an apartment at 124 Oxford Street in Cambridge with Al Krieger, another graduate student in the Cosmic Ray group, and Jim Elliot, then an undergraduate who I think had a part time job in our group. This was a 3rd floor apartment with a good-sized room each as well as big living rooms. It cost us about $45 each per month. It was a typical wooden building probably dating from the 19th century. The landlord was a Mr Economy – probably contracted from Economos.

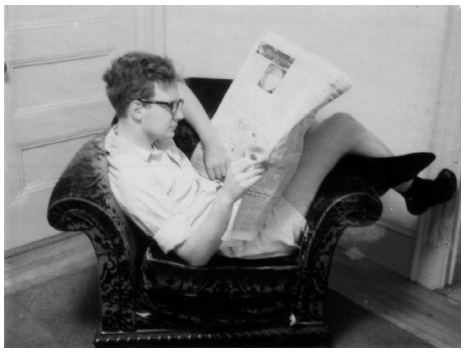


Fig 2: Reading the Sunday paper.

Fig 3: Jim and me in the kitchen at 124 Oxford St

I bought an old TV set from Bud Lyon, an engineer in our group. He gave me "a half-hour guarantee, or until I took it out of the room; whichever happened first".

I liked to watch the educational channel, WGBH, for the news by Louis Lyons, the Curator of the Nieman foundation. He always started with a weather report except on the day when Kennedy was assassinated in November 1963. Most people remember where they were when that terrible news broke – I was in one of Huang's quantum mechanics lectures and remember coming out to see many people crying in the corridors.

Fig 4: Jim and me in the Cosmic Ray Lab, Bldg 26.

Jim, who became a lifelong friend, essentially operated on a 12-hour day, with two sleeping periods in every 24 hours. He sometimes used to sleep under the Meson Monitor in the penthouse of building 26. Al, however, worked on about a 27-hour day, which meant that he would often get out of phase with the rest of the world. He would then get immersed in a novel and read until all hours, getting even further out of phase. Then his supervisor (Hale Bradt) would phone to find out where he was and generally read him the riot act. Krieger was well-read and opened my mind to American writers.

After two years, Elliot became a grad student at Harvard College Observatory and moved out. Keith Ferguson, a TCD botanist who was a post-doc at Harvard, moved in to take his place. He later worked at Kew.

In this year I reconstructed my quantum mechanics in a course given by K Huang, also a good lecturer. At some point I also listened to a quantum mechanics course given by JC Slater, one of the pioneers of molecular theory. He spoke almost word-for-word from one of his textbooks. Philip Morrison offered a course on introductory astrophysics that many of the Cosmic Ray group attended. He was a charismatic lecturer.

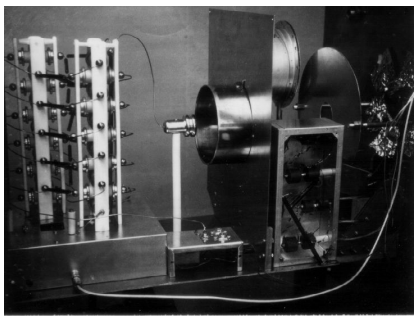
I also remember taking courses from Alar Toomre on galactic dynamics and from Jerome Friedman (later a Nobel prize winner) on particle physics. I listened in on a stellar structure course from Icko Iben. One area of physics of which I remained forever ignorant was statistical mechanics. I did a couple of extra mathematics subjects to complete my Minor. As luck would have it, I think the Minor requirement was abolished just afterwards. I had to pass exams in two languages – I chose French and German, which did not present too much difficulty. A course I took for interest was on assembler-language programming on one of the first minicomputers (TX0 - not then all that "mini"). The PDP1 had just appeared, the Digital Equipment Corp having been founded by engineers from MIT. Whenever I saw it, students were playing an early version of "Space Wars" as it had a video console and a pair of game handsets.

For my SM I worked on a prototype deuterium detector for solar wind studies. This involved accelerating particles to 100KeV towards a tritiated target (1 Curie but fairly harmless!) and detecting the resultant alpha particles with a solid-state detector [using reaction $t(d,n)\alpha$] . To do this I constructed a vacuum system with

an ion source and mass selection and also did some particle "ray-tracing" using an electrolytic bath to obtain the equipotential surfaces. The Achilles' heel of this idea was that the 100KV power supply (a Cockcroft-Walton generator) could not be prevented from occasionally arcing and nobody wanted 100 kV sparks on a satellite. As a detector it worked well, however.

I remember an episode when Frank Scherb was doing some hotplate soldering and was supporting some pieces with blocks of aluminium – except one of the blocks was actually magnesium. It made a most brilliant fire and covered everything around with white dust.

Though the master's degree went well, I did not manage to pass my General Exam at the first try that June.



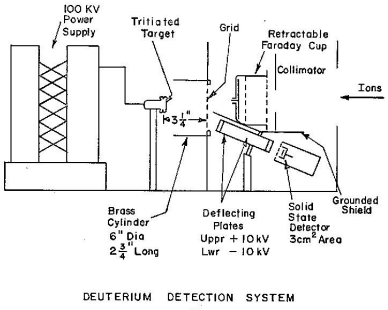


Fig 5, 6: The deuterium detector with 100KV power supply and tritiated target Using reaction t(d,n)α.

1964-5

Frank Scherb, my SM supervisor, was offered an attractive position in a newly formed space physics group at the University of Wisconsin and moved there at the end of the 1963-4 academic year. Accordingly, I had to look for another supervisor. I heard that George Clark was looking for a student to work on celestial hard X-ray sources that could be observed at balloon altitudes. He had just conducted (July 1964) a successful observation of Tau X-1 with a NaI + photomultiplier detector. However, it had been a rather marginal result with low energy resolution and it was desirable to repeat it with better equipment.

George took me on as a Research Assistant for my PhD. I was to find him a very stimulating supervisor. He let me have a lot of freedom but somehow would appear just at the time when I needed some advice as to how to proceed. He also forced me to present at seminars, overcoming my extreme shyness. I repeated my General Exam and this time passed. George was in charge of the oral part, Kraushaar was the second member of the committee and the third member was a rather junior nuclear scientist whose name I have forgotten. This last asked me a question about isotopic spin, which I answered satisfactorily, but he phrased his questions so badly that the other members asked him to explain better what he wanted, which had the fortunate effect of shutting him up completely! Kraushaar was notorious for always asking the same questions, so of course I knew the answers!

The idea for an improved high-energy X-ray detector was based on proportional counters. A proportional counter consists of a thin wire anode in a noble gas box. With a suitable high voltage on the wire, any electron in the gas arising from

an X-ray photoionization is accelerated and gives rise to secondary electrons in a kind of cascade as it approaches the anode. The resultant detectable charge is proportional to the energy of the electron. It soon became evident that Argon, though easy to use and yielding more or less a unique electron energy for each photoelectric event, has too low a cross-section for detecting hard X-rays.

To obtain a higher quantum efficiency it was necessary to go to the next highest atomic number noble gas which is Krypton. Unfortunately, we soon discovered that all recently collected Kr is radioactive thanks to the accumulated fission product ^{85}Kr from nuclear reactors and bomb tests. Once at lunch I told Philip Morrison about this and he proceeded to calculate the total tonnage of ^{235}U that had been fissioned. Though later on, a search of old laboratories around the USA yielded enough vintage pre-fission Kr for some small satellite X-ray detectors, I would have needed an unreasonable volume of it.

At this time we had a beautiful laboratory on the second floor of building 6, overlooking lawns and trees. It was in the middle of the theoretical group, in what eventually became known, at least to some, as the "Ivory Tower". However, one day the head of Physics entered with a person unknown to us, who said to him "Yes, this will do" and we were banished a few days later to a rather old lab on the 4^{th} floor. The new occupant of our lab was Charles Townes, inventor of lasers.

I therefore fixed on Xenon as the gas to use. It was not as ideal as Argon since the pulse response to monochromatic X-rays was double-peaked because the ionization of the atoms and their recombination was not so simple. Nevertheless, it still offered quite good energy resolution for line detection. I built up a balloon gondola with two integral Xe proportional counters and plastic scintillator anticoincidence shielding. A difficulty emerged in that the Xe had to be very pure and the simple Al structure of the counters I had built was in effect too dirty. I experimented with a device to clean the Xe using very hot reactive Mg (or was it zirconium?) turnings but this was not very successful.

At around this time I was joined in our lab by Jay Stein and Jeff McClintock who were doing theses under Walter Lewin, a newish member of the group who had come from the Netherlands. Lewin afterwards became one of MIT's best known physics lecturers. Many years later he unfortunately became a non-person at MIT due to a scandal that I know too little about to pass judgment on. Jeff I stayed in contact with until he died.

In July 1965 I was woken up in the early hours of the morning by a huge explosion. I thought at first that WW3 had broken out. But I was still around. It turned out that hydrogen had leaked from a bubble chamber at the Cambridge Electron Accelerator and had blown off the roof. This was just a few blocks from where we lived on Oxford Street. There was at least one fatality.

Elliot had moved to an apartment in Somerville with Stan Erickson. I recollect visiting him for a Seder that Ken Brecher (then a grad student of Phil Morrison)

had arranged. One of our circle was Mitch Feigenbaum (of later Chaos fame) and I think he was also there on that occasion.

1965-66

During this academic year I was at this time slowly becoming more and more ill. My usual 50% efficiency had dropped to 10%, as Jay Stein put it! In September 1965 I had gone to the MIT infirmary about a persistent cough but had not been treated seriously. Sometime in the first half of 1966 I had gone again but was again brushed off. As will be seen, I was ultimately diagnosed as having a rather serious case of TB.

On 9th November 1965 at about 5pm there occurred the notorious power blackout that affected most of the Northeast USA. Almost immediately our landlord came upstairs and banged on our door. "What are you boys doing" he shouted! It was extraordinarily difficult to get the power stations running again because they all needed power to heat the oil they were fired with. As it happened, MIT had a small power station which was still running and they managed to supply enough power to the Cambridge power station to get it going. They in turn were able to supply Boston and get them going, and so on. The whole event taught the electricity supply companies a lesson.

In June of 1966 our Oxford St apartment came to and end. I found another one in Central Cambridge that I was going to share with Martin Breidenbach (later a professor at Stanford). The night after moving my furniture I suddenly found myself swallowing blood while in bed in the middle of the night. I was alone and went in great trepidation to the Cambridge City hospital, which was nearby. There I was seen be a young intern (I suppose) who said I had had a hemorrhage but it had now stopped. I should go next day to the MIT clinic as I either had pneumonia or TB! This time I was seen and treated very competently by Dr Stein, who was afterwards head of the clinic. I was x-rayed and examined and kept for a week. Then I was sent by cab to the Middlesex County Sanatorium in Waltham, where I was admitted. There I was to stay for around 7 months.

I was the patient of a Canadian lady, Dr Barber, who I found really caring – one of the best doctors there.

To summarize the treatment – I had to stay in bed for a month. After that I had to lie down and/or nap twice each day. I was put on a regime of daily streptomycin injections in my posterior and INH pills with vitamin B12. There was a queue every morning for the injections and we knew which of the nurses were gentle and which not. I also had to undergo a broncoscopy. Sputum samples were cultured as well as given to guinea pigs but the results of the tests took 6 weeks to emerge. Fortunately the variety of TB that I had was not drug-resistant and by August I was not infectious to others. I was allowed to receive visitors and occasionally spend the day out with friends. Towards the end of my stay they decided to remove the part of my lung that had been infected. This involved removing a rib and pushing the others apart for access. The surgeon was a Mass

General Hospital (MGH) resident, called May. He removed my right lower lobe. I was able to leave around mid-Jan 1977 and was given INH, B12 and PAS pills to take. I had to be followed up for about 18 months, though the actual time was shorter. I got to know one of the consultants shortly before I left and he told me he had had a very similar case to mine. However, he had refused the chest operation, which I was annoyed to hear. It carried significant risks, but supposedly it reduced the chance of further problems later in life, as the germs sometimes tend to wall themselves up and appear harmless, though they are not.

Good nutrition was part of the treatment and the meals were usually tasty. My waist expanded and never returned to its original dimension.

Fortunately I did not have to pay for all this. My MIT Insurance covered up to $2500 at $28 per day in the hospital and I think the State of Massachusetts paid the rest.

Fig 7: Checking temperature and pulse.

The hospital was an interesting experience. The ~200 patients were a real cross-section of society, weighted towards the poorer end, from ex-jailbirds to management types. Many were alcoholics. Because the treatment was so long one got to know many of the patients and staff. One of them had a relation in the office of Naval Security and had heard J Edgar Hoover tell stories of the sexual misbehaviour of President Kennedy and Martin Luther King. This was long before it all became public.

Fig 8: In the hospital grounds

The acting head of the hospital was a crusty but generous New Englander, called Payne. He often gave me interesting magazines, such as Atlantic Monthly and New York Review of Books. An Irish lady doctor, Kirwan, would give me the English Sunday newspapers. There was also quite a good library. I bought a small transistorised TV, which was then a novelty. There was a rather battered old piano in a disused recreation hall that I was allowed to play and George Clark brought me several music books. [In the basement were the records of TB patients from the 1930s, before the advent of antibiotics, and their cases were grim reading, though some survived.] The two lady doctors were supporting their husbands who were doing residencies at MGH. The nurses were usually good fun to talk to. Among the patients were some well-educated and agreeable people who I used to chat with on walks in the fields and woods around the hospital. One of them was a kind of misfit – a Master of Theology in Biblical

Languages from Harvard, though he usually kept quiet about this and worked as a machinist.

I read about the disease and its epidemiology. It seems that the new case rate had not decreased much with time, though the number of deaths was by then very low. Drug resistance was not yet too serious, though there was a class of patients who kept themselves TB-positive so that they could be bums in Summer and be comfortably cared for in hospital in winter.

I would get occasional welcome visits from friends such as John Miller, George Clark, Elizabeth Clark, Jay Stein and others. I think it was here that I first met Ray Bates, a lifelong friend who must have come along with one of the others.

I was not able to do much towards my thesis project while in hospital but I think I did some design work on equipment

After the end of my hospitalisation I went to visit my parents in Ireland for two weeks. I always resented the fact that they had never visited me in the USA, even when I was in hospital.

Back to MIT in February 1967

I arranged to stay in Graduate (Ashdown) house for the remainder of my studies when I returned to MIT so that I did not have to walk too far in the winter cold. It was a nice room that overlooked the Charles River. I was advised by the hospital to take daily siestas but this caused too many problems and I soon gave them up. My roommate was Ankoor Bodhe, who was studying Civil Engineering and writing a thesis on optimizing suspension bridge design. My neighbour was Irving Plotkin, a very bright economist who was working on risk vs return in different industries. His findings were music to the ears of the pharmaceutical industry in particular, as they were under attack for excess profits, and he earned exceedingly well as a consultant for them.

Fig 9: Wintry view from my room on the 6th floor of Grad (Ashdown) House, looking towards Boston across a frozen Charles River

I traded in my Fiat, by then almost impossible to start, for a dark green MGB that I bought in Washington as the Boston dealers had some sort of minimum price agreement between themselves. This was unfortunately stolen after about 4 months by a delinquent teenager of Irish origin. He sold the car to somebody else and then stole it back again, after which he was apprehended by a wide-awake detective who knew his reputation. I attended the indictment of the boy, but what happened to him I do not know. The case was complicated by the fact that his parents had in the meantime threatened a witness and had tried to decamp to Ireland. I ended up with quite a

lot of respect for the Boston police, who had had to put up with many postponements designed to wear them down. Simply stealing a car for a joy ride was regarded as not serious – a "misdemeanor" and meant only a small fine if you were caught.

Fig 10: With Jay Stein and George Clark.

I had stayed in touch with Jay and Jeff and returned to our lab, where progress on my gondola was continuing.

The doors of the older buildings had glass panels on which were painted the names of the occupants or the laboratory inside. We tried to get the Physical Plant department to put our names on the door but they refused. So one Saturday morning I got some black paint and a fine brush and did an imitation of an official sign with

PROJECT SIESTA
I.S. GLASS
J.A. STEIN
J.E. McCLINTOCK

"SIESTA" supposedly stood for Scientific Investigation of the Energy Spectrum of Taurus A. The sign went unremarked for several weeks. Then George Clark brought around Nancy Roman, then the critical person at NASA in control of his funds. He walked past, did a double take and came back in. He could hardly be cross, but his nervous laugh indicated that he was not entirely amused.

In fact, George was quite generous towards us students. I remember an occasion when he invited us to the summer property he shared with his first wife's family in Vermont. Present were Jay, Jeff and Jeff's brother (who was later killed in Vietnam). George had converted an old church on the land into his and Elizabeth's place. I called it the "First Church of Clark, Scientist".

When we bought the supply of aluminium tubing referred to below, we had brought our requisition book to George to sign. The amount was $3000 or so and he signed it without looking properly. As we were leaving, he suddenly called out "By the way, what was that for" and Jay replied "A new Volvo". George jumped up and ran to look – he thought he had really signed for a new Volvo!

This was about the era when remote computing became available for the first time. George had an Olivetti mechanical terminal installed in his office (no screen) but he soon found that it was too expensive to use because the rental was very high, we had to pay for CPU time and we also had to rent tracks on a memory drum (I think). We were soon back to submitting our programmes on punched cards.

We decided to abandon my original detector and go for long cylindrical detectors of thin-walled Al. We purchased the entire national supply of 0.015-inch thick Al tubing. For shielding we used copper and tin, surrounded by Ar proportional counters for anticoincidence against charged cosmic rays. Unfortunately there was still a problem with purity in the Xe counters and we also had difficulty in sealing their ends. Eventually we gave a contract to a company called LND to build the counters as they had the necessary experience. Jay had a very practical outlook and had realized that money was not, at that time, a serious constraint! It had been pointless for us to struggle on.

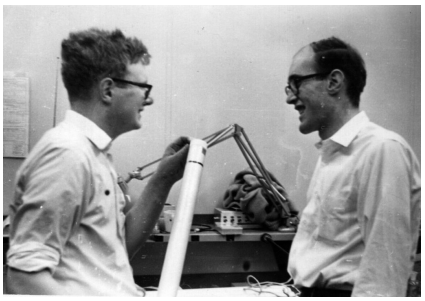


Fig 11: With Jay Stein and one of the LND detectors.

The detector array finally adopted had an area of 5000 cm^2. It was composed of 11 tubular proportional counters of 5cm diameter and length 165 cm filled with 90% Xe and 10% N_2 at 2 atmospheres pressure. The signals were fed into a charge-sensitive preamplifier and a 31-channel pulse height analyser. The outputs were registered in 32 binary scalers (including an overflow channel) made with discrete transistors as the integrated circuits then available used too much power. The gondola was rotated at about one revolution in 9 minutes with the collimator pointing to a fixed elevation angle. The data were recorded inside the balloon gondola on photographic film and information concerning orientation from magnetometers, atmospheric pressure, temperature, time etc were included. Power came from NiCd batteries. Calibration was provided by an ^{241}Am X-ray source that gave a 60 keV line.

Fig 12: The gondola was covered with insulation and reflective foil. It is suspended from a line twister and above it is a chute for lead shot from the red ballast container. The red arms at the top of the launcher swing open to let the gondola float away as soon as the balloon has taken the load.

Fig 13: Just before launch. The partially filled balloon fills out at high altitude. The balloon is released at the right side and the launch vehicle "Tiny Tim" manoeuvres downwind beneath it, holding the payload until the balloon takes the load.

Needless to say, a great deal of technology and testing went into the project. The altitude of operation was intended to be 130,000 ft, using a 10.6 m cu ft balloon, but two attempts during the summer of 1967 from the NCAR Balloon Base in Palestine, Texas, failed due to burst balloons and the final successful flight on 30th September was with a 5 m cu ft balloon that reached 120,000'. At these altitudes and atmospheric pressures corona discharge takes place very easily and all connections to the counters had to be carefully potted. Worse still, during the ascent, the equipment was subjected to extreme cold, which tended to disturb the potting and endless problems were encountered, in spite of rigourous testing in environmental chambers. Every flight was a risk as often a gondola landed hard on the ground and could be dragged along in spite of crush padding underneath and there was each time some damage to be repaired.

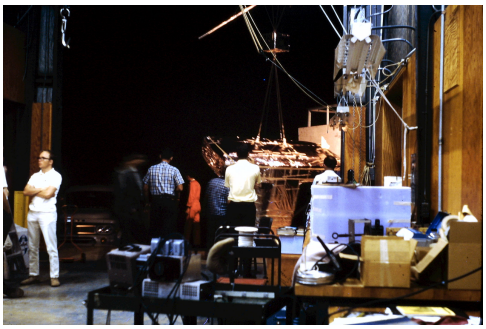


Fig 14: Preparing for a night launch

The data from the recording film had to be scanned using a special machine that I redesigned, with output to a digital incremental tape recorder (a sort of 7-track computer tape unit driven by a stepping motor). The orientation and time data were read visually. To get the use of the recorder, I had to set up in an eerie basement lab stacked with soldered copper parcels containing the bones of people who had painted luminous dials with radioactive paint in the 1930s (with KF Richard, *Rev Sci Instrs* (**39**, 1131, 1968).

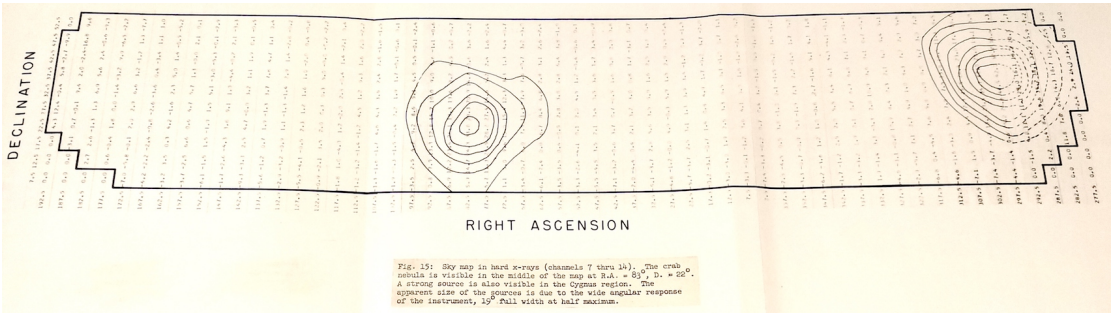


Fig 15: The count rate and other data were combined to make an X-ray map of a good part of the northern sky on which Tau X-1 and Cyg X-1 were clearly visible.

Perhaps the most interesting result of my thesis was the non-existence of spectral lines in the x-ray data. The energy resolution achieved was about 15% FWHM. There had been speculation that the decay time of supernovae was governed by ^{254}Cf, which would have given a clear line at 60 keV - which was clearly not the case. Fits were made to the overall intensities of the sources assuming Power Law and Bremsstrahlung spectral shapes.

The flight operations at the NCAR Balloon Base in Palestine, Texas, were of great interest in themselves and involved quite a lot of logistics. For the first flight, we were accompanied by Robert Mudge, one of the group technicians, but by the third one just a couple of us students went down by ourselves. On one occasion we went as couriers and had a ride in the pilot's cabin of a cargo Boeing 707, witnessing the thrill of landings and take-offs. One had to wait one's turn for a flight at the Balloon Base after getting the gondola ready. The weather on the ground and at altitude had to be right. Filling the balloon and launching it were tricky operations requiring a specialized crew. The balloon was tracked by radar and followed wherever it went by a small plane and a ground crew in a truck. The aviation authorities had to be warned and approve when the balloon was expected to pass through commercial airspace. The gondola had of course to be retrieved at the end of each flight. A ground crew would drive a pickup truck in the direction the balloon was moving and a small plane would follow it as well. Texas has many small airfields left over from WW2 and the plane could land every now and then to allow the balloon overhead to catch up. Finally, the balloon would be cut down by radio control and the descent by parachute monitored so that the ground crew could recover the gondola. Flying around Texas in a Cessna was good fun.

My final flight was of exceptional duration as the high-altitude winds that usually limit the time available were then quiescent.

During the waiting periods we had time to visit Dallas and see where Kennedy had been assassinated and to go to the Space Flight Center in Houston where we saw the astronaut training process. We visited other Texas county towns such as Athens and Crockett – the last almost unchanged since the mid-19th century (there was no oil in its county). We also visited the Winzen balloon factory to see how they were put together. They extruded their own polyethylene tubing from which the balloons were made. This was the only part of the factory that we were not allowed to photograph.

On the rural East Texas roads one saw many dead armadillos that had been run over. We also had to watch out for farmers in pickup trucks emerging at random from side roads. They usually had a gun slung across the rear window. Between Dallas and Palestine you could see many of the old grasshopper-style oil well pumps. At night-time launches in the extremely hot summer the air was full of large flying insects. A cute little raccoon that was half tame used to curl up tissues in its tail and try to make a nest in a little box.

The Cosmic Ray Group moved to a new building – the Center for Space Research – sometime in 1967.

My thesis was submitted in June 1968. The examining committee was George Clark, Prof Harvey and Phil Morrison and went very smoothly. My thesis defense was a fairly friendly affair with the examiners being curious to know what I had done rather than asking hostile questions! I did not attend the graduation ceremony as my parents were not there.

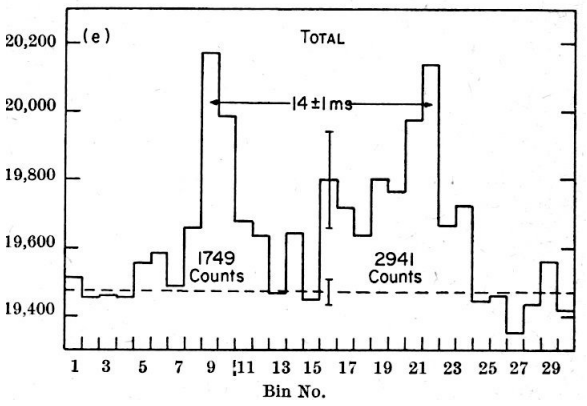
The thesis project's results were published in ApJ (**157**, 215, 1969). I was surprised to have to deal with the editor Chandrasekhar himself when I phoned the ApJ office about a typo.

Post-doc year at MIT (1968-69)

Following my degree, I was taken on for a year as a Post-doc in the X-ray group. My main project was with Frank Floyd, a recently graduated Electrical Engineering student who had built a gondola with a pointed x-ray telescope incorporating a modulation collimator to determine the size of the Crab Nebula X-ray source in hard x-rays. My part was to time each arriving photon so as to pick up the pulsar.

This gondola was operated by radio control and telemetered the pulses in real time to the Balloon Base where they were recorded on a multi-track tape with precise time signals. Some of the data were lost due to the Balloon Base's false economy of using old tapes. These had drop-outs and ran too slowly at times. Nevertheless, thanks to a clever phase-locked loop that Frank designed, I was able to extract most of the pulse data onto computer-compatible tapes to within about 1.3 msec and to sum each detection modulo the known pulsar period

This was the first result to show that the pulsed fraction of the Crab's X-rays increased with energy and also that the so-called "interpulse" became much stronger relative to the main pulse at higher energies.



We published the result in *Nature* (**224**, 50, 1969) with Herb Schnopper. We got into trouble initially for leaving out the latter, who had been Floyd's supervisor but who had contributed nothing to the particular result described!

Fig 16: Histogram of the arrival times of hard X-rays from the Crab pulsar plotted against its ~33msec period.

For most of my post-doc year I shared an apartment on Concord Avenue with Ankoor Bodhe as he had started working for a design firm at the same time. I moved out when he got married.

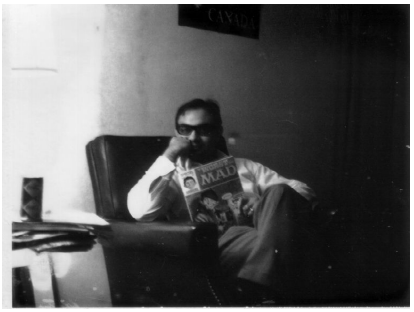
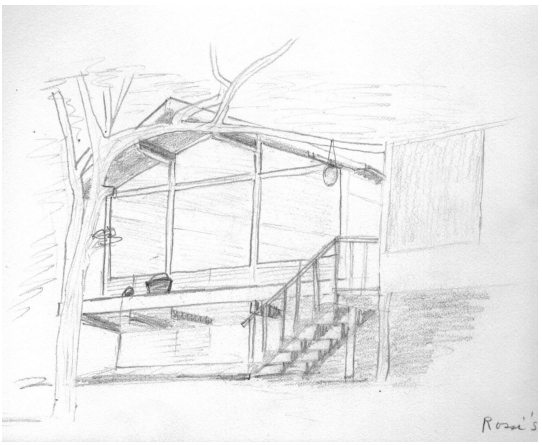


Fig 17: Ankoor Bodhe with his favourite reading material.

I bought another MGB, this time a fairly new 1968 model in white. This I kept until I left the USA in September 1970.

In the summer of 1969 I was asked to look after Prof Rossi's house and got to know Bruno and Nora quite well. She was very fond of playing opera records and would get quite emotionally involved. She was the grand-daughter of Cesare Lombroso, the psychologist who promoted the idea of the "criminal type". Rossi, as mentioned, was one of the pioneers of cosmic ray research in the 1930s and had been involved with the Los Alamos project to develop the atomic bomb, his job being the assessment of the radiation output. Like others who I met who had worked there, he remembered the time at Los Alamos fondly, feeling that what they were doing was valuable towards ending a terrible war. I remember he had a bottle of foamy green glass, formed during the Trinity test blast, in his study. I asked him if it was radioactive, but he did not know. Once, when we were driving to MIT we passed a hoarding on which some student had written "*mene mene tekel upharsin"* which Bruno explained was the original "writing on the wall"!

I had the use of Rossi's cars, which carried the most prestigious kind of MIT parking permits since he was an "Institute Professor". It was amusing to see the faces of campus police when I drove in to park just outside building 37 where I worked!



I also visited the family once at their summer house on the shore of Long Pond near Wellfleet on Cape Cod. This was a most idyllic place and there were several other properties nearby owned by MIT professors.

Fig 18: The Rossis' house on Long Pond

Miscellaneous Recollections and stories

One of the benefits of MIT and its proximity to Harvard was that many of the most famous physicists and astronomers would visit and give colloquia. One of the first I encountered was James van Allen. He was invited afterwards to a small reception to meet the Cosmic Ray Group. Everyone was introduced to him by Rossi, but I was passed over because he obviously did not remember my name. Van Allen noticed this and gave me an understanding wink!

There were, of course, frequent colloquia in the Boston area. Many were held at MIT. The main weekly Physics one was held in the large lecture theatre 26-100 that held several hundred people and was frequently full. There was another one devoted to astronomy and space (COSPAR). I often found those at the Harvard College Observatory the most interesting. One could spend all one's time attending them, so had to be selective.

The professors did not socialize very much with the students, except that we usually lunched together at the various cafeterias. When Icko Iben came, he threw a party and soon afterwards Stan Olbert. Other professors that invited me on various occasions were Hale Bradt and Walter Lewin. Of course, George Clark, my PhD supervisor was very generous and invited me on numerous occasions to his house. Philip Morrison often came to lunch with us. He was a lively conversationalist and liked to keep us thinking and even on occasion reminisced about his work on the Atomic Bomb and his visit to Nagasaki and Hiroshima soon after the end of the War. He was no friend of atomic weaponry.

Jay Stein went on after MIT to apply his knowledge to commercial and medical applications, such as airport luggage scanners and low-dosage x-ray tomography. He started a successful company that built equipment for the diagnosis of osteoporosis. Jeff McClintock stayed on at MIT and later joined the Smithsonian Astrophysical Observatory and had many fundamental papers in x-ray astronomy to his name. Saul Rappaport was another student contemporary who remained at MIT and has published widely in x-ray astronomy also.

The Cosmic Ray Group was often host to foreign visitors over the summers. Several of these were Indian. Vikram Sarabhai was one of their most famous figures and from a very rich family, but he had to be paid a small salary because he could not take any money out of India! Others were BV Sreekantan and UR Rao who later occupied important posts in India. Several others came from Italy. One was Alberto Bonetti, who was very anticlerical and told me to "Beware to the Catholics" (pronounced like alcoholics). Minoru Oda from Japan was a long-term visitor and while at MIT developed the "Modulation Collimator", an important way for finding out the size of x-ray sources before the grazing incidence mirror telescopes and two-dimensional detectors were developed.

On one of Nancy Roman's visits (the NASA person who, as mentioned, was important to our funding) the group was having lunch with her in the of the Graduate House cafeteria. Herb Schnopper plucked a flower from the central flowerpot of our table and put it in Nancy's lapel. Sometime afterwards he said to me "That flower was worth two rockets'.

The average student had short hair and was very conservative in outlook compared to Irish and English undergraduates, as well as being much more hard-working. As the decade wore on, with the anti-Vietnam war protests and the rise of "flower power", it all changed and the students wore long hair. One prof remarked to me that they used to worry about the students with long hair but now they worried about the ones with short hair!

A student friend and very short-term roommate Martin Breidenbach had an Alfa sports car and one long weekend he decided to overhaul the engine. The lab where he worked had a big door to the outside and he expected he could work there undisturbed. However, while he was under the car a loud voice said "What is an automobile doing in a physics laboratory". This was prof Martin Deutsch. Breidenbach, red-faced, told him as nicely as possible what he was up to and – before long – Deutsch was under the car, helping him! [Deutsch was *inter alia* the

discoverer of positronium]. That Deutsch was around on that long weekend was not unusual; if ever I was at MIT on a Sunday or a holiday, I was likely to see him.

Another Deutsch story – possibly one from the "Anectdotal Nuclear Physics" course was this: when Deutsch devised his idea for a beta-ray spectrometer he looked around his lab for a suitable piece of brass pipe to make into a vacuum chamber and found a random piece of about the size he had in mind, which he made use of. Years later he found that everybody else who had built these spectrometers copied his design to the last detail, including the precise length of that random brass tube!

During the exam periods there sometimes broke out "hi-fi" wars between the two rows of facing dorms in East Campus. All the students on one building would connect their hi-fis together and the same on the other side and they would blast out different tunes. When the Green Building (Earth Sciences) was under construction, the pile drivers would drive the students crazy.

The New England weather was very cold in winter, with snow on the ground from November until April. The first snow was picturesque but by April the piles of icy snow were dirty and one could hardly wait for the short spring. The sidewalks became slippery and treacherous and I could count on having at least one bad fall each winter. On the other hand, the summers could be intolerably hot at times. I remember a group of us jumping into a car and heading for the beach one midnight! However, the best season was the Fall, when the countryside was beautiful and the leaves had vivid colours. I liked to take walks with friends around Fresh Pond on Sundays or else go to Walden Pond, the Concord area or very occasionally to Crane's Beach.

In summer, the Boston Symphony Orchestra went to Tanglewood in upstate Massachusetts and I went there once or twice with Ray Bates and George Philander, a South African friend of Ray's. On another occasion I went to Marlborough Vermont where Pablo Casals ran a summer music festival where baroque music was played in a beautifully resonant wooden hall.

There was a so-called humor magazine, Voodoo, published by the undergraduates. It was full of very dubious material and was eventually suppressed.

I think the total number of students was then about 7000, of whom about 3500 were undergrads and the rest grads.

I was much happier in myself after the first couple of years at MIT. On the whole, I felt less pressurized, I did not feel so poverty stricken and of course I knew far more people. The period of my illness caused me to reconsider the way I was living and made me more at ease with the world.

Occasionally I have contact with the friends I had at MIT and have always been glad to hear how their lives have progressed. The best friend I still have from those days is Ray Bates, who was a grad student in meteorology and lives today

back in Ireland. Many others remain fresh in my mind even if we are no longer in touch. Once in a while at international meetings such as the Interntional Astronomical Union I have run into faces from the old days.

I still receive a magazine from the Physics Dept each year and also the MIT Technology Review. Most of the physics staff that I remember have retired and many have died, as might be expected after 50 years or so. I am envious that people can stay working if so inclined.

Stellar Interferometry

With my former roommate Jim Elliot, who was then a graduate student at Harvard under Giovanni Fazio studying Cerenkov light from cosmic rays, I worked in my spare time on a detector for Michelson stellar Interferometry. This was in collaboration with Frank Scherb, my former SM supervisor, then in Wisconsin.

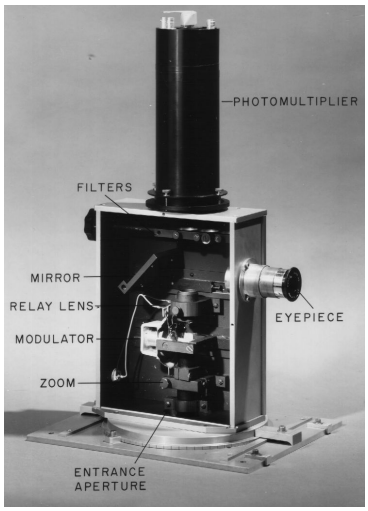


Fig 19: Fringe detecting photometer

We constructed a fringe-detecting photometer making use of a Ronchi (parallel line) grating driven by a loudspeaker movement. Interference fringes were matched to the grating by a zoom lens and the transmitted light was detected by a photomultiplier. However, the fringes moved around due to the seeing. The signal was processed through a phase-sensitive circuit (lock-in), and squared and integrated using analogue methods before recording on a chart recorder. I learned a lot about operational amplifiers.

We worked at the Agassiz Station of Harvard College Observatory, near the town of Harvard, MA. This had previously been called Oak Ridge and is in fact again called that. We had one of the old Metcalfe photographic telescopes to ourselves. Apart from us, the main astronomers around Agassiz were the users of an 80-feet radio telescope and a student called Horowitz who was monitoring the Crab pulsar.

We used the fringe detector for measuring double stars with a Fizeau arrangement (the slits were near the focal plane) and could rotate it. This way we obtained the "rotation curve" or visibility function for the double and could extract the separation, position angle and relative magnitudes of the components using the Zernike-van Cittert theorem. The unsteady Massachusetts atmosphere meant that the results were very poor and erratic, but the principle was demonstrated (with JL Elliot, *AJ,* 75, 1123, 1970).

In 2019 I was pleased to see that our experiment had become part of the history of interferometry, featuring prominently in a book "*Mieux Voir Les Étoiles: 1er Siècle de L'interférométrie Optique*" by Daniel Bonneau, EDP Sciences, France.

Agassiz Station was 30 miles or so from Boston, near Harvard MA, and so I sometimes arrived late for work at MIT after sleeping over there. This annoyed Walter Lewin, for whom I was doing some work on balloon-based detectors and resulted in my postdoctoral appointment not being renewed for a further year. However, this was a blessing in disguise as it ultimately led me into infrared work that was much more to my liking, partly because it was easier for an individual to make significant contributions.

Fig 20: Agassiz Cottage in 1969 with my 2nd MG in front.

Living at Agassiz Cottage, which I later did for a couple of months, had its moments. Jim Elliot, I and the Night Assistant Mike Mattei shared the place. On cloudy nights we would sit around the fire, amply provided with firewood by Arne, the site supervisor. I remember in particular Jim reading hilarious passages from *Oblomov*, by Goncharov, that resonated strongly with us. Adjacent were some apple orchards that provided us with a good supply in season!

In July 1969 we watched the first Moon Shot on TV in the company of friends from the Amateur Telescope Makers of Boston and *Sky and Telescope* magazine who had the full printed scenario for the event. Mike Mattei made a point of following the Moon rocket for as long as possible with the 61-inch reflector and I believe we probably had the most distant visual sighting of it, as a rapidly varying starlike object.

On one occasion we had a rather boozy bachelor party for an MIT friend of Jim's. At around 3am we went for a noisy walk outside the Observatory and it was not long before some local resident called the Harvard MA local police. When they heard we were from the Observatory (you never knew who a Harvard student's father might be) they bundled us into the patrol car and took us back, telling us to make as much racket as we liked, but within the grounds! Years afterwards this episode got magnified into rumours that orgies had been going on at Agassiz. Of course, we kept our mouths shut.

Jim seemed to be very laid-back in those days and it amazed me how very directed he became later. Probably his marriage to Elaine and his graduation drove him to get his act together. After Harvard he went to Cornell to work with Joe Veverka. As is well known, he made his name in 1977 through the discovery of the rings of Uranus with Dunham and Mink. In 1978 he became a professor at MIT. We collaborated later in a number of occultation observations. Sad to say, he died in 2011 of a rare cancer called SNUC.

Looking back on this period of my life, it was generally not a particular happy one and it feels now, half a century later, as if I had been in a kind of alternative world,

reduced to a kind of drudgery during the first few years. Nevertheless, it had its moments.

No doubt I have left out a lot of people and things in this account and some of the "facts" may have metamorphosed over time. I wish I had taken more photographs of friends and acquaintances. [Version: Apr 2023]