stores in the United States were open on Sunday. I drove around the small city of Worcester, and finally found a little shop open. Browsing the counters I attracted the owner's attention, even more when I could not tell him clearly what I wanted. Finally I found a polyethylene tablecloth which I guessed (correctly) had about the right thickness. He took my money, and warmly wished me success in my inexplicable use of it. His wishes certainly did no harm. A clean pair of lab scissors did the rest.

Attentive readers of my paper[10] will note on examining the graph of the absorption curve that I plotted the comparison between the natural sample and a synthetic standard in an unusual way. The reason was my desire to conceal from critical (but not too critical) readers that the agreement between the natural sample and the standard was a good deal better than statistical errors would on average permit. Such extra-close agreement may of course (and did) happen by chance, but I have never been so tempted to 'fudge' data (by increasing the scatter) as here, in order to avoid suspicion. Interestingly, no one ever recognized my less improper device.

It was soon after this paper was submitted that Masatake Honda came from Bern to join, and transform, my research group. With him as a collaborator the difficult became easy, and the impossible merely difficult. As one example among many, he developed the method, using the 'Honda column' by which my student John Merrill was able to isolate stable Be from sea water and measure its concentration of less than a part per trillion. This was a record low concentration for a stable species at the time. Early in this post-discovery period we acquired our own shield and counting system.

The correspondence between Bombay and Princeton had continued steadily, and we were able to help each other in a number of ways. I had suggested to Peters that if he chose we might publish back-to-back discovery papers, but he waved me on ahead, following not long after[11]. (For an account of the independent discovery of ^{10}Be by Peter's group, see the article by Lal, this issue.) Soon, when I moved to join my friends in La Jolla in 1958 at the start of UCSD's evolution into a science university, a member of the Peters group named Devendra Lal appeared almost at the same time. The collaboration among Honda, Lal, and myself has been one of the happiest and most fruitful of my life. While we in La Jolla turned our main efforts toward cosmic-ray produced activities in meteorites and later lunar materials, the group led by Lal, in Bombay and later in Ahmedabad, carried on the study of ^{10}Be in deep-sea sediments and manganese nodules, almost alone, until the advent of accelerator mass spectrometry in the late 1970's made ^{10}Be studies accessible to a much wider group[12,13].

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James R. Arnold is in the Department of Chemistry, 0317, University of California, San Diego, La Jolla, CA 92093-0317, USA.

Recollections of an experiment on the charge composition of cosmic rays

Bruce Dayton

A 1968 project to look for relativistic nuclei in cosmic rays combined science and adventure.

In the spring of 1967 Professor Peters invited me to come to Denmark and spend a year working at the Danish Space Research Institute of which he was the director. We were old friends from Berkeley and it had been five years since we had worked together at the Niels Bohr Institute, so I was eager to work again with him and to take part in what sounded like a very interesting project: to build a balloon-borne instrument capable of measuring electronically the intensity of relativistic nuclei in the primary cosmic radiation, identifying individual elements from lithium to iron, and of sufficient size to provide statistically significant data. As he had played such a pivotal role in the discovery of heavy nuclei in the cosmic radiation two decades earlier it seemed most appropriate that his institute would be undertaking this project.

By the end of September, with my wife and younger daughter, I arrived in Lyngby for what turned out to be a two-year stay. It was, indeed, an interesting and successful project and I would like to recall, for the non-specialist reader, something about the experiment and how we carried it out. It is a personal account and subject to the tricks that memory plays after twenty-two years.

By the time I arrived at the institute the basic design for the cosmic-ray telescope was already well advanced and some prototype components were made. While this was not their only project, it was the first major cosmic-ray experiment for the new institute, so a good fraction of the institute's resources were at its disposal. There was an excellent machine shop with some very fine machinists, several electrical engineers led by a most capable designer, Kurt Omo, and a well-staffed electronics shop. A talented young physicist, Niels Lund, was the prime mover on the project. Various others, whom I shall not try to name, worked on pieces of the instrument, and Ole Funch, an electrical engineer and second-in-command at the institute, kept everything on schedule. Bernard Peters had many other things to do in running the institute, but he still found time to keep closely involved in every aspect of the undertaking.

So what was this instrument that everyone was working on? Figure 1 shows schematically the arrangement of the components: Two closely spaced Cerenkov detectors, having radiators with substantially different indices of refraction, are the heart of the telescope in that they provide the signals that are analysed to identify the traversing cosmic-ray nuclei. A scintillation counter at the bottom is used in the coincidence trigger to better define the geometry, but its signal is not analysed. Three spark chambers with digital readouts determine the trajectory of each traversing particle that satisfies the triggering requirements. This enables one to correct the Cerenkov signals to those for equivalent vertical paths through the centres of the detectors by means of calibration data that can be obtained on the ground with fast singly charged particles. Though two spark chambers could define the trajectory, the third spark chamber provides the redundancy needed to verify that the path is a straight line in the presence of some additional sparks due to knock-on electrons.

Let us see now how the signals from the two spark chambers are used to determine the nuclear charge. Cerenkov radiation may be regarded as an optical shock wave produced by a charged particle when it travels faster than the local speed of light in a transparent medium. The amount of light emitted is proportional to the square of the particle's charge and to the following function of its velocity: $1-(1/\beta^2 n^2)$, where $\beta = v/c$ is the ratio of the particle velocity to that of light in free space and

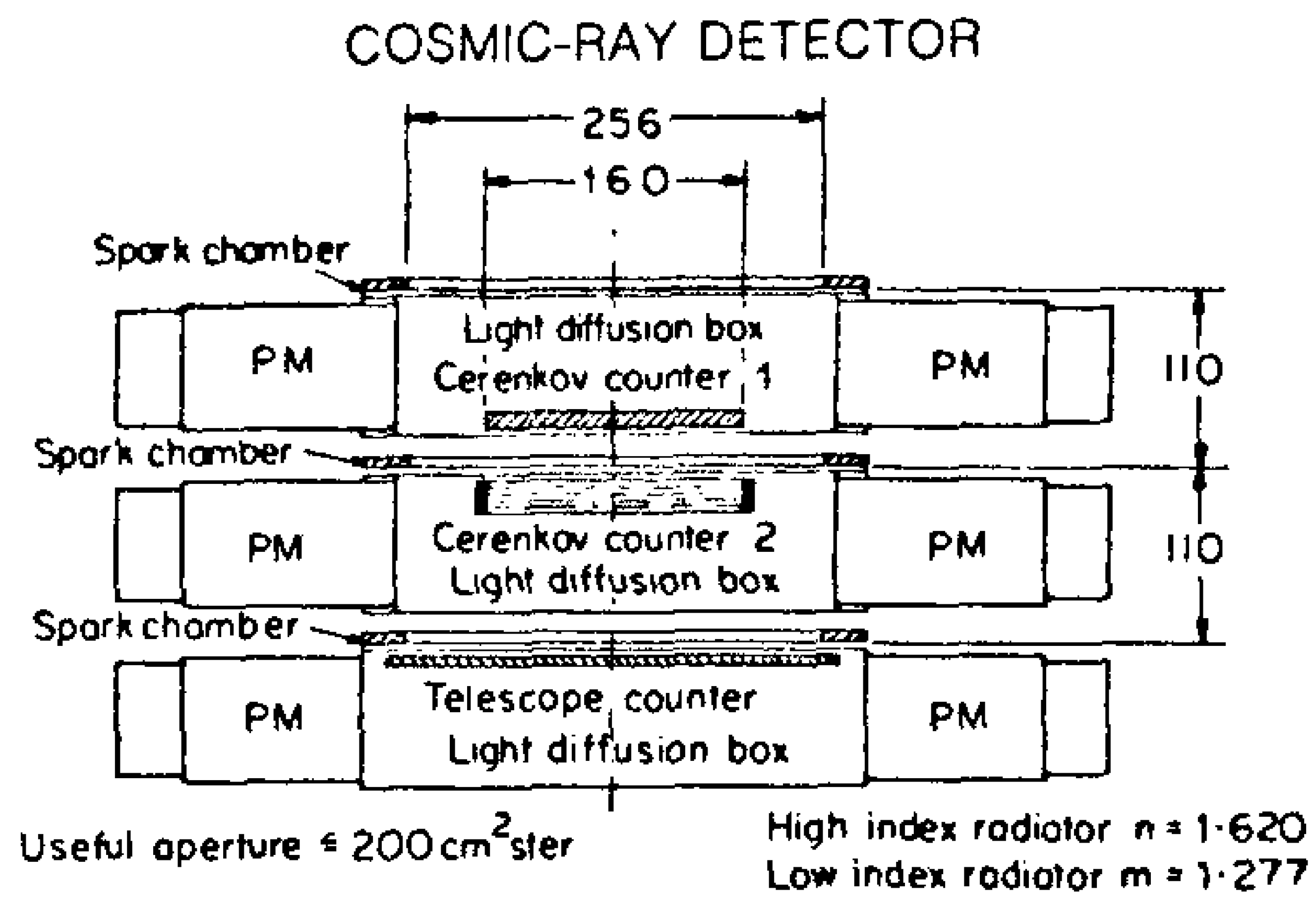

Figure 1. Double-Cerenkov cosmic-ray telescope (schematic).

n the index of refraction of the medium.

Since the cosmic radiation consists of particles with various charges and velocities a single Cerenkov detecter is inadequate to identify the charge values uniquely. However a combination of two Cerenkov detectors with substantially different indices of refraction makes this possible. If the two detectors are arranged such that some nucleus traverses both, the responses from the two detectors may be represented by:

$$x = x_0 Z^2(1-1/\beta^2 n^2)/(1-1/n^2)$$

for radiator with index n,

$$y = y_0 Z^2(1-1/\beta^2 m^2)/(1-1/m^2)$$

for radiator with index m,

where x_0 and y_0 are the responses for highly relativistic (i.e. $\beta = 1$) singly charged particles and Z is the atomic number of the nucleus.

Figure 2 shows the Cerenkov light yields in media having $n = 1.620$ and $m = 1.277$, the actual values used in the detector, for nuclei of energies up to 10 GeV per nucleon. Note that there is no Cerenkov radiation when β is less than $1/n$ or $1/m$ in the two cases.

The ratio of the responses $R = x/y$ is independent of Z and depends only on the particle's velocity, while a certain linear combination of x and y is independent of β and depends only on Z. Figure 3 is a plot of y vs x showing the straight-line locus of points corresponding to particles of different velocity for each discrete value of charge. The certain linear combinatior of x and y is:

$$u = x \sin \alpha - y \cos \alpha = Z^2 u_1,$$

independent of β;

i.e. a rotation of the coorlinates so that the u-axis lies perpendcular to the family of lines of constant charge. Having reduced the depeidence to that of a single variable, experimental Cerenkov outputs, suitably corrected for angle of incidence and position in the detectors, may be thus ombined and

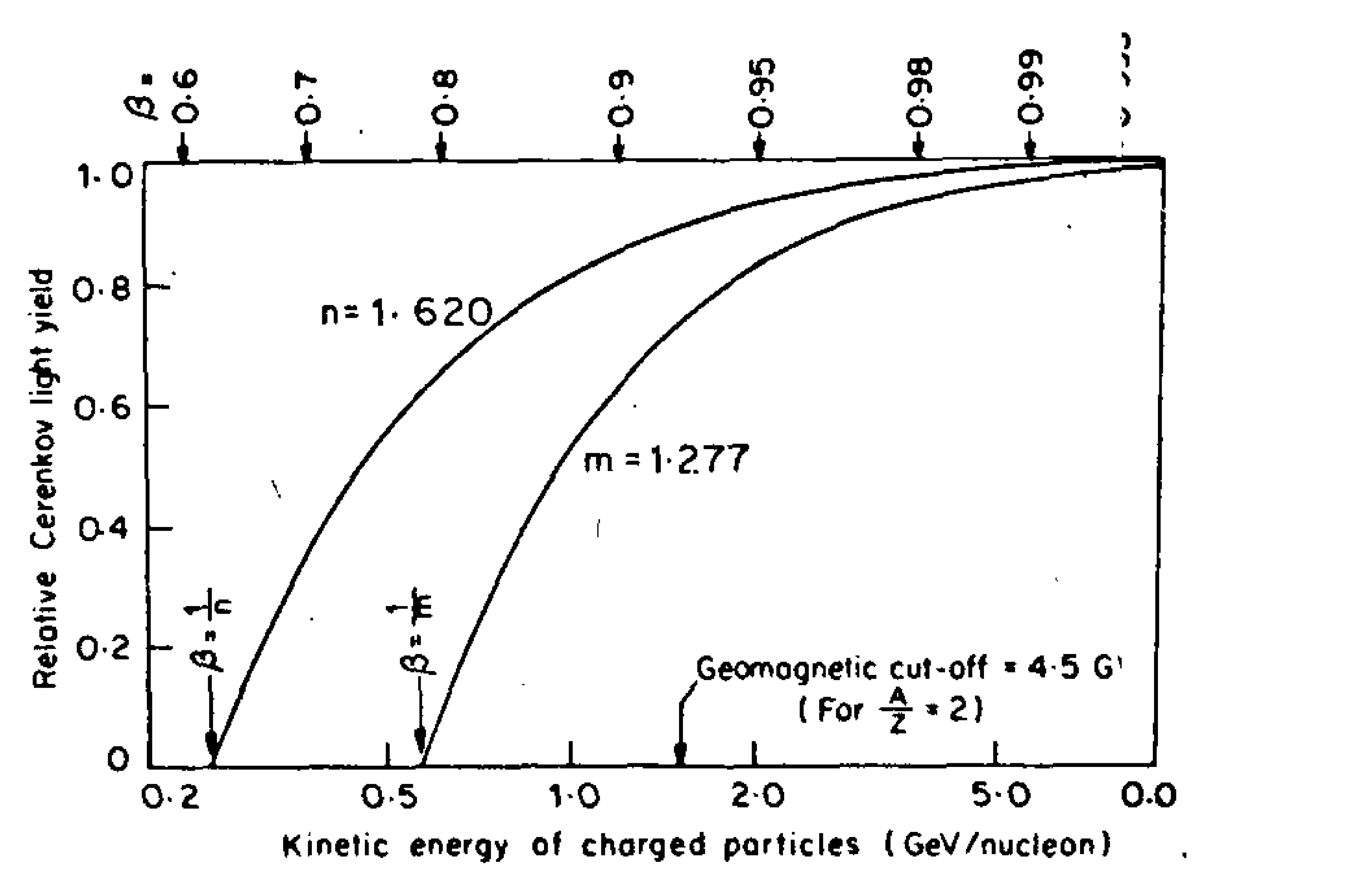

Figure 2. Velocity dependence of Cerenkov light yields, shown as a fuction of kinetic energy per nucleon (logarithmic scale).

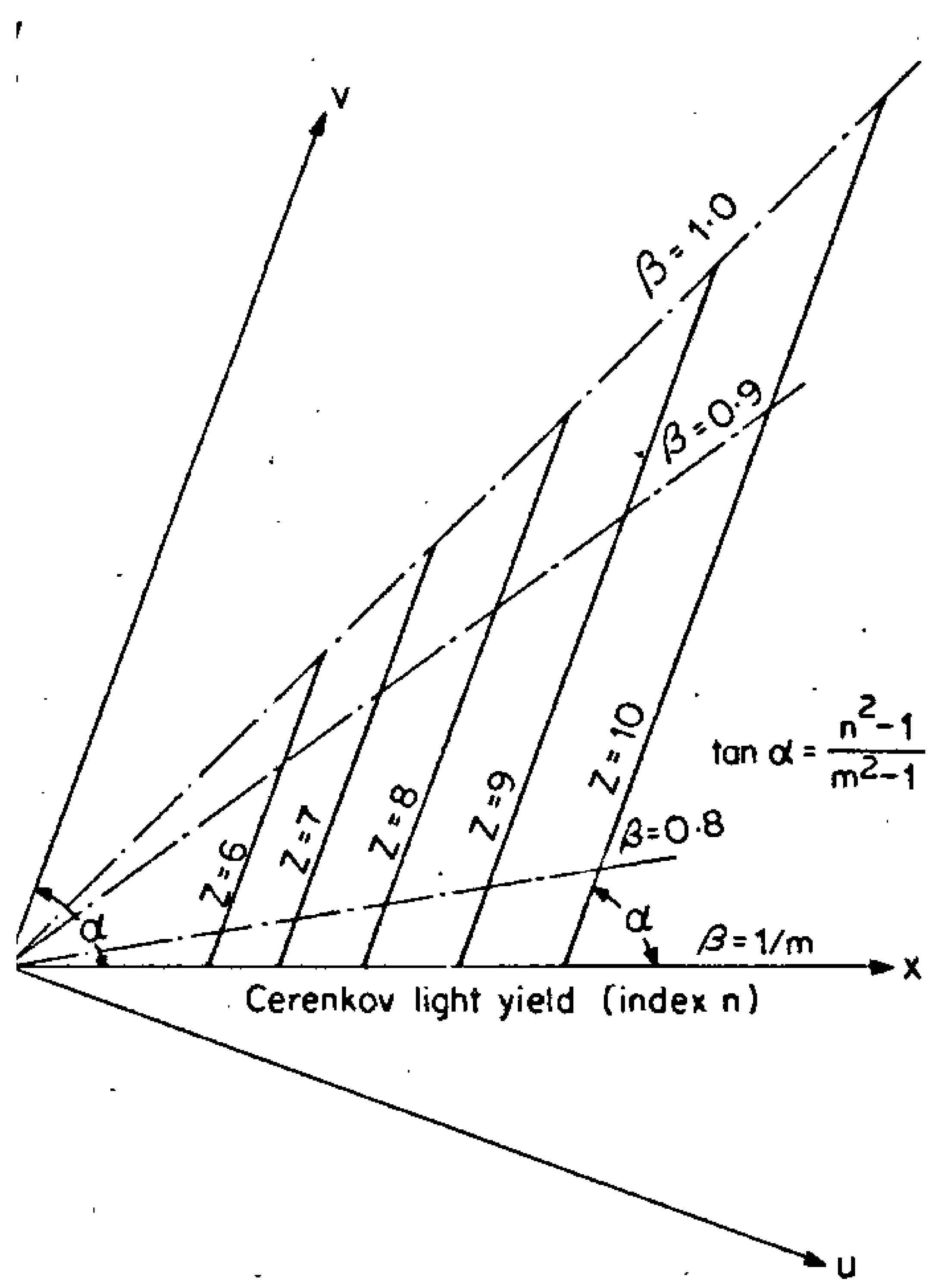

Figure 3. Cross-plot of normalized outputs from idealized Cerenkov detectors for nuclei of various atomic number and velocity. In the rotated coordinate system the u-axis is perpendicular to the family of solid straight lines.

plotted as a histogram along u, or better still, a histogram along $u^{\frac{1}{2}}$, for then the peaks corresponding to successive values of Z will be equally spaced.

Unlike scintillation light the Cerenkov radiation displays no saturation, i.e. a nucleus of charge Z_e will produce the same average amount of light, including that of high-energy knock-on electrons, as Z^2 independent singly charged particles having the same velocity and following the same trajectory. Kristensen, Lund and Risbo had pointed out that this has the consequence that the performance of the telescope for heavy nuclei can be predicted reliably from measurements with singly charged particles. That was important because accelerator beams of relativistic nuclei heavier than helium did not then exist. Though the pulse height distribution for singly charged particles is quite asymmetrical due to fluctuations in the additional Cerenkov light from high-energy knock-on electrons, it follows from the central limit theorem of mathematical statistics that the statistical sum of Z^2 such independent distributions approaches the normal (Gaussian) distribution with mean value Z^2 times greater and standard deviation Z times greater than the corresponding values for the single charge distribution. From computer studies they showed that, already for $Z=6$, deviations from the normal distribution were insignificant.

The relative standard deviation (i.e. the standard deviation divided by the mean value) varies as $1/Z$, which means that it is a constant in terms of charge units. When we represent flight data as a histogram linear in Z the widths of the peaks should be the same for all atomic numbers.

Returning to the construction of the telescope, the Cerenkov light is collected by means of photomultiplier tubes that look into an extremely white light-tight box containing the radiator. The high-index radiator is a one-centimetre-thick plate of a high-lead glass while the low-index radiator is a two-centimetre-deep aluminium box with one-millimetre-thick glass window. The glass surfaces were ground but not polished to avoid the trapping of light in the radiators by total internal reflection. The glass counter initially had two photo tubes but two more were added before flight. The liquid counter had four tubes and the scintillation counter had two photo tubes. Because much of the Cerenkov light is in the UV we found we could nearly double the signal from the liquid detector by saturating the liquid with a waveshifter. It produced no measurable scintillation.

The spark chambers were made from thin printed circuit boards, each having 128 etched lines. Each chamber was constructed by sealing two such boards to a fibre-glass frame with lines perpendicular and facing each other across a 6 mm gap through which the spark chamber gas flows. Within less than a microsecond after a coincidence between the two Cerenkov detectors and the scintillator announces that a multiply charged particle has passed through the telescope, a high voltage pulse is applied across each of the three spark chambers. Sparks occur where the gas is ionized by the traversing charged particle, the coordinates of which are determined by detecting which pair of lines in each spark chamber experienced large current pulses. Each of the 128 wires on each side of each spark chamber passes through a tiny ring-shaped ferrite memory core, so that the magnetic state is flipped in those cores experiencing the current pulse. After the pulse height analysis of the Cerenkov signals is completed the cores are interrogated sequentially by a resetting pulse and a sensing amplifier which identifies the reset cores.

A spark chamber creates a very noisy electrical environment, so timing is critical. The charge outputs from the photomultiplier tubes must be summed and stored on a well-shielded capacitor before the spark chamber is fired. Then, after the noise has quietened down, the Cerenkov pulse heights are analysed, the cores reset and all the information digitized and telemetred to the ground. The dead time for each event is about 1/10 second. Between events the telemetry keeps busy sending down 'housekeeping' data such as temperatures, pressure, battery voltages, etc.

Looking ahead to the possibility of a satellite experiment, Peters saw the need eventually to join forces with a compatible group having substantial resources and similar interests. Professor J. Labeyrie had just such a laboratory, Service d'Electronique Physique at the French National Center for Nuclear Studies in Saclay. This group, led by an energetic lady named Lydie Koch, already had considerable space experience from their work on ESRO's gamma-ray satellite. It was decided to collaborate on a second instrument in which an additional glass Cerenkov detector would be used (to better discriminate against nuclear interactions) and with photographic recording of tracks in a multiplate spark chamber. The Cerenkov detectors and the overall structure would be built in Lyngby while the spark chamber assembly would be done in Saclay. Both groups would take part in calibrations and in some of the balloon flights. It was planned to fly the joint instrument in the summer of 1969.

Back to our telescope. We wanted to test the limiting resolution of our Cerenkov detectors by placing them in a beam of relativistic protons from the Proton Synchrotron at CERN. So I set out for Geneva with the Cerenkov boxes, two pulse-height analysers and a lot of beam electronics. I was joined there by P. Mestreau, from Saclay, who brought their valued dual-parameter analyser. As I recall, Niels Lund joined us for the run after we were set up. Professor G. Cocconi kindly arranged for us to use a very pure beam of 19 GeV elastically scattered protons, magnetically deflected to remove all lower energy secondaries, and focused onto a 3-cm-diameter spot where we would set up our detectors.

An accelerator beam and the cosmic-ray 'beam' are very different in that the particles from the accelerator come in bursts whereas the cosmic-ray particles are distributed randomly in time and of much weaker intensity. The photomultipliers in our Cerenkov detectors were selected for their high efficiency and good uniformity at the expense of being rather slow, requiring about 100 ns of integration time for each event; so in the accelerator environment it was necessary to take special precautions to avoid 'pile-up'. A thin scintillation counter upstream, in coincidence with a 10-cm-diameter scintillator 14 m downstream from our detectors, provided the event trigger and started an electronic gate that would invalidate any event in which two beam protons arrived within 120 ns of each other. To exclude non-beam particles a large anticoincidence scintillator, with a 5-cm hole, was placed directly in front of our apparatus; any signal in this counter during the 120 ns gate would disqualify the event. With such a well-collimated beam the requirement of passing through the small downstream counter would eliminate most nuclear interactions in either Cerenkov detector. We were thereby assured of a very pure sample of highly relativistic protons for our analysed events. The Cerenkov outputs were processed in separate pulse-height analysers out to eight standard deviations. Part of the time, the two outputs were fed into the dual-parameter analyser.

We collected about a million events, half with the glass Cerenkov in front and half with the liquid Cerenkov so situated. The better configuration, with the glass counter in front, gave a relative standard deviation $\sigma_x/x_0 = 37\%$ for the glass counter and $\sigma_y/y_0 = 24\%$ for the liquid counter. From the dual-parameter distribution we obtained a correlation coefficient $r = 0.15$. From the data gathered in this calibration experiment, we could predict, with confidence, that at least 91% of relativistic nuclei of any charge would be assigned the correct Z value, provided that the position and angle dependence of the light gathered in the detectors could be sufficiently well mapped.

It was not feasible to get sufficient beam time at CERN to carry out a detailed mapping, but our French colleagues were able to arrange for doing this in a beam of 370 MeV/c pions ($\beta = 0.935$) from the Saturne accelerator at Saclay. The purity of the beam was not well known, so the measurements would not be directly comparable to the CERN results; but we only needed mean pulse heights for different positions and angles compared to those from pions striking the centre perpendicularly. I took the detectors and held them at various space angles to France and spent several days mapping. The important thing we *learned* is that the standard deviation of the distribution for the extreme angles permitted by the geometry of the telescope was within 1% of that for normally incident particles and that, within the precision of our track determination, we should be able to correct our pulse heights with an error less than 1%.

The CERN and Saclay results persuaded us to add two more photomultipliers to the glass Cerenkov, with an estimated improvement of correct identification from 91% to about 94%.

In early August of 1968 the telescope was completed, so we scheduled a September flight from the French National Balloon Facility (CNES) at Aire-sur-Adour, near the Pyrenees. I drove down with my family, for we planned to vacation in Spain after the flight. Altogether we had quite a few participants in the flight campaign since each part of the telescope and the ground-support equipment had its own expert who might be needed. Ole Funch was in general charge of the Danish part of the campaign, and the actual launch and tracking would be in the hands of the CNES crew.

It took some days to check everything out and to accumulate several tapes of muon data for later production of calibration maps at the computer in Lyngby. Then there was not much to do but wait for 'turn around', the time when the wind at 30 km elevation stops blowing toward the West and is quiet before starting to blow toward the East. Only at that 'window-of-opportunity' can one expect a long duration flight without having the balloon drift beyond telemetry range or get into an area where recovery would be difficult. At least once a day the meteorologist would send up a small balloon and track it and say 'not yet'. The tracking balloons do not go as high as the big balloons, so a certain amount of extrapolation, based on past experience, is necessary.

While waiting we had some nice excursions — to Biarritz, to the Cirque du Gavarnie in the Pyrenees. With Bernard Peters we drove to Bagneres de Bigorre where, at the international cosmic-ray conference fifteen years earlier, he had reported on the decay of the tau-meson, work done at the Tata Institute, Bombay; then to the top of Pic du Midi where we visited the venerable cosmic-ray monitoring station. At Aire we visited the facility where they fabricated the stratospheric balloons, sealing them together from a continuous strip of coated mylar into what become, when inflated, giant bulging tetrahedrons. Not as graceful, perhaps, as the elegant creations made from tapered gores, but much cheaper and they work.

Finally the meteorologist said he thought that 'turn around' had arrived, though the measurements of upper winds had been rather anomalous that year. After a final check-out of all the detectors and electronics the flow of the spark chamber gas was set to about 1 litre per hour and the gondola was bolted together, the thermal insulation strapped on and the accoutremonts of flight (telemetry transmitter, altimeter, buckets of ballast) attached to the harness; altogether just over 100 kg. On the field the preparations for launch began well before dawn — laying out the balloon and parachute with only a small part of the top of the balloon, with its filler tubes, extending beyond the clamp. Then the filling with hydrogen began, filling until the gas volume is about 0.3% of the balloon volume (in the expectation that the balloon will reach a ceiling altitude at about 3 millibars pressure).

With the telescope already hanging on the crane of the launching truck, the vent in the gondola was closed and the umbilical cord for external power was removed and replaced by the short cable from the internal stack of mercury batteries. When, at sunrise, the balloon was released from the clamp, the launch vehicle, carrying the payload, was prepared to move across the airfield, so as to be directly under the ascending balloon when it lifted the payload from its hook; but there was little wind and the launch went very smoothly.

A balloon launch is always a tense time and I suppose some of the tension here was related to the use of hydrogen. My previous balloon experience had been in India, so I was accustomed already to its use, but I still remember the story about the group of American experimenters who came to Secunderabad with their asbestos suits and face masks and who watched the gas filling from afar.

At first everything seemed to be working well — the counting rates were growing as expected, but then we noticed that the gondola pressure, instead of slowly increasing, started to decrease. After a large drop in pressure the counting rates went wild and we realized that there must be a leak in the gondola and that the high voltage on the spark chambers or the photomultipliers had gone into open discharge. There was nothing to do but abort the flight. However that turned out not to be easy for the upper winds had taken on a strong drift to the south-west and soon the balloon was over the Pyrenees on its way to Spain and above terrain where recovery might not be feasible. The CNES decision was to let the balloon continue until over flat country where it was cut down by radio command from their aircraft. From the tracking record they expected that the parachute would land the payload a bit North of Burgos. Ole Funch and the driver of the recovery vehicle set out for Burgos without delay.

The next we heard was a telephone call from Ole in the Burgos jail. It seemed that they had arrived at the site of the landing, out in a field where a large crowd of people had gathered, and ran over to the telescope. Ole was just reaching for the battery disconnect when he was grabbed at the shoulder by a man in the uniform of the Guardia Civil. Ole and the driver were hauled off to police headquarters while the telescope, which had landed softly on its egg-carton crash pad, was picked up by some men who dumped it into a pickup truck and started off on a bumpy ride that, finally, would take it to Madrid. The next issue of *La Prensa* in Madrid carried a telephoto picture of a curiously shaped object in the sky, with an article speculating that it might be a visitor from outer space, an espionage device or even an apparition of the Blessed Virgin.

The immediate problem was to get our friends out of jail and to get back our telescope. This required intervention by the French Ambassador to Spain. Ole and the driver arrived back shaken but none the worse for the experience, but the telescope came back badly battered. Opening it up we found that the leak was from a cracked weld, but the damage to the instrument looked repairable. The instrument was taken back to Denmark where a newly designed gondola would be made. My family and I then went off on our vacation in Spain.

Back in Lyngby, the new gondola was built and the telescope repaired. I don't recall that we made any significant changes in the instrument, but two important things were added for ground support: A more powerful computer was acquired for processing flight data and Corydon–Petersen built a 'quick look' facility for the next flight. This device extracted the Cerenkov pulse heights and the spark chamber coordinates from the telemetry data stream for an event, these appearing on an oscilloscope screen long enough to record the numbers. Now, twenty-two years later, this seems primitive, but then it was state of the art and would serve us well.

Lund and Risbo developed more sophisticated software for data analysis, and extensive ground calibration of the telescope was carried out on-line with the computer using cosmic-ray muons. Now, when the next flight took place, the analysis could proceed without delay.

In the meantime work was proceeding on our part of the French–Danish telescope. I was now getting more involved with that and spent a fair amount of time at Saclay.

By May it was time to go to Gap for the spring 'turn around'. The launching facility was at a small World War II airport in Tallard, a picturesque village, a dozen kilometres south of Gap. This time there were no problems with the telescope. (While preparing the instrument for flight I took a photograph of it with the top of the gondola removed and this is shown in Figure 4.) 'Turn around' arrived on schedule and in late May we had a beautiful launch at dawn. This was a hand launch, in which the payload was supported by a fully inflated small balloon until the big balloon was released.

The balloon seemed almost overhead for hours and then drifted slowly westward across the Rhone valley for a total time of 9½ hours at the 3½ millibar ceiling. For this flight we set gains low enough that the iron nuclei ($Z = 26$) would be on scale, but with the limited dynamic range this meant that carbon nuclei ($Z = 6$) would be the lightest particles satisfying the trigger threshold.

We had set up our own telemetry station on a hillside and there watched the data pour in. I remember that for several hours my wife called out the numbers from Corydon's 'quick look' while I wrote them down. Over lunch Bernard and I plotted these data, using a simple graphical device to correct for the angle of incidence. We had only a sampling of the data but there the peaks stood out for carbon and oxygen and a little nitrogen peak between. Even without the mapping corrections the resolution looked good. It was a very satisfying lunch.

The recovery was made quickly and there was no damage. (Olrik's egg-carton crash-pad was very effective.) We were prepared for a second flight, with another balloon, crash-pad and a set of batteries; so within a few days we were ready to launch again. The upper winds were still very low. This time we set the gains 4 times higher to include the lithium, beryllium and boron peaks, though this meant that everything above magnesium would be off-scale. This flight also went well and lasted for a full nine hours. Again the resolution was good. Then my wife and I drove further south to visit the Grand Canyon of the Verdon and meandered north along little-used roads in the Alps on our way back to Denmark — a nice cap to a successful campaign.

Niels Lund and Torben Risbo were already at work on the computer analysis of the flight data by the time I returned to the institute. We wanted to present our results on charge composition at the international cosmic-ray conference in late summer in Budapest, so the analysis concentrated on getting the best possible charge resolution out of the data. The data also can yield information on the momentum spectra above the geomagnetic cut-off, but this had to wait for a later time.

We settled on a presentation of the

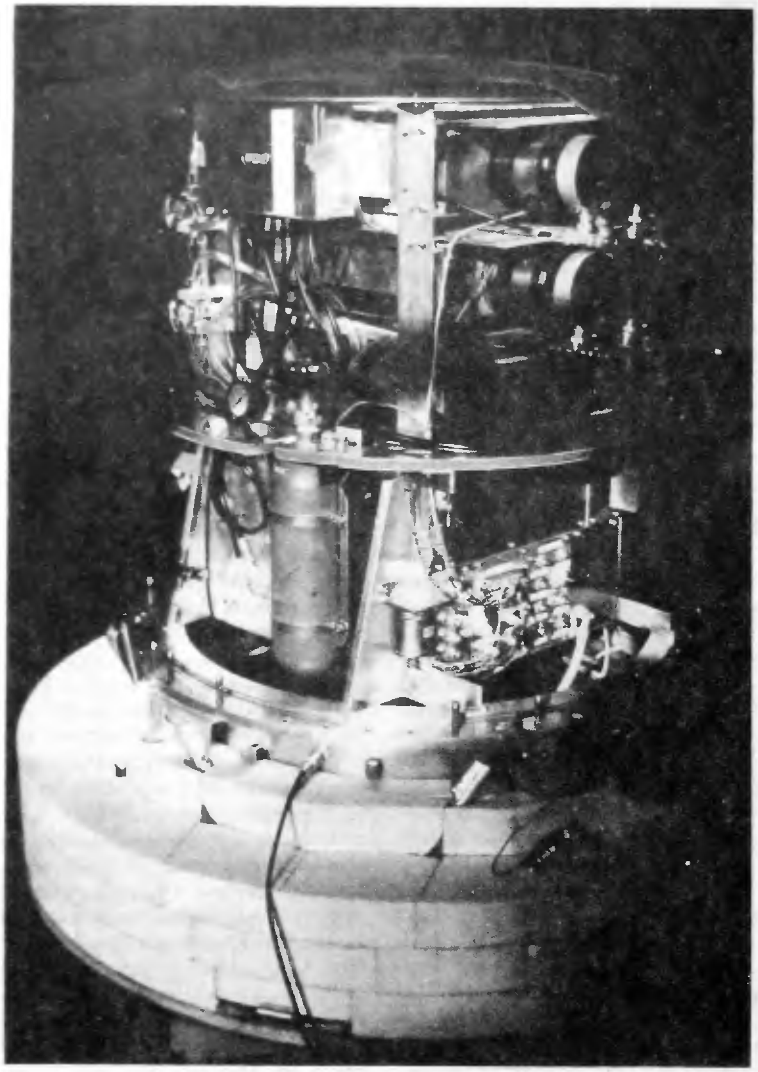

Figure 4. The Danish double-Cerenkov cosmic-ray telescope

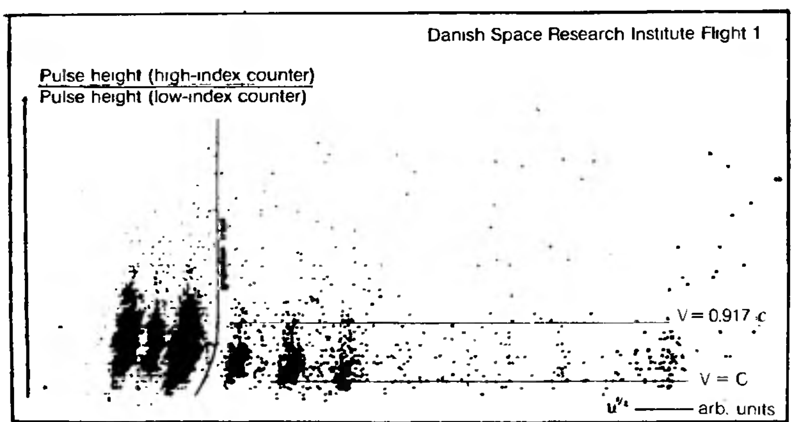

Figure 5. Data from flight 1 The ratio of Cerenkov outputs, normalized and corrected, is plotted against $U^{1/2}$ a scale linear in Z. The horizontal line $v = 0.917\,c$ is the estimated geomagnetic cut-off velocity for the flight

corrected flight data suggested by Peters such that, for each event, the ratio of the pulse height from the glass counter to that of the liquid counter was plotted as a function of $u^{\frac{1}{2}}$, a scale linear in Z. This is shown for flight 1 in Figure 5. Here the nuclei are distributed vertically according to their velocities, the lower velocity particles being higher on the plot. The horizontal line marked $v = 0.917\ c$ (corresponding to a magnetic rigidity of 4.6 GV) marks the lower velocity limit for nuclei to arrive vertically (in the region of the flight), because of the sweeping effect of Earth's magnetic field.

It turns out that when the role of the Cerenkov light from the fast knock-on electrons is considered carefully, the points for the fastest particles are pushed slightly to the left on the diagram. Curved lines, parallel to the one marked 'projection track' on the diagram seem to provide the optimum separation between adjoining charge values. The events were summed into bins, separated by such lines, and plotted as a histogram for each flight. These are shown in Figures 6 and 7. The resolution for carbon and oxygen are about what we predicted from the accelerator results, but for iron the fraction of correct identifications is about 88% compared with 94% for the lower Z values. While the theoretical analysis showed that the statistical fluctuations should be the same (in charge units) for all Z, this is not true for errors in mapping or in determining the angle of incidence, which errors grow with Z. Evidently, there was room for improvement in track determination and calibration. However, the paper we prepared for the Budapest conference had what was probably the best available data on relativistic heavy nuclei. It also showed conclusively that the 'iron peak' is indeed $Z = 26$; not a neighbouring element.

Though Lund, Risbo and I were the authors of the paper that Lund read at the Budapest conference, it presented results of the work of many people. Bernard Peters, in particular, always

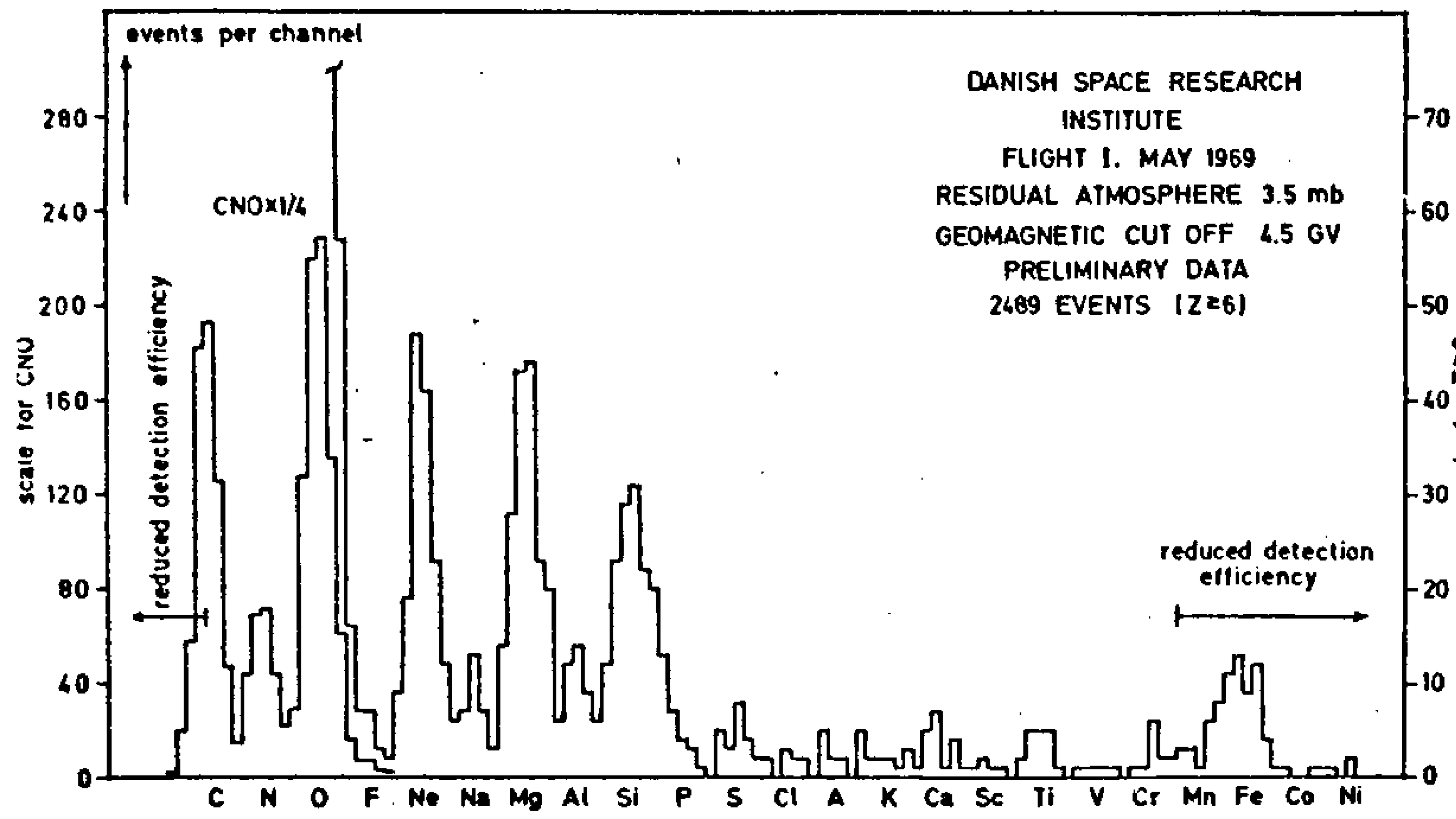

Figure 6. Charge histogram for flight 1.

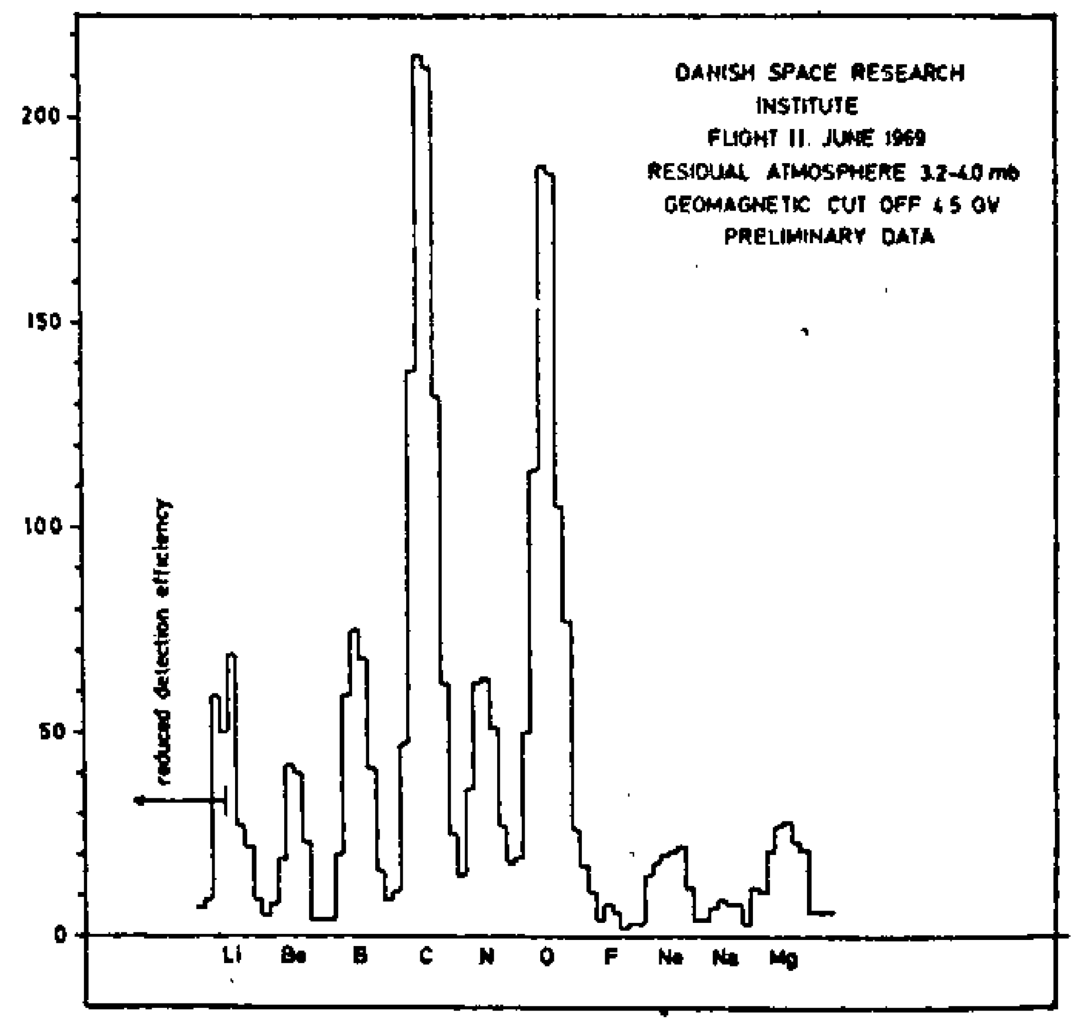

Figure 7. Charge histogram for flight 2.

had an uncanny ability to separate that which was important from that which was not. My role in this experiment was now coming to an end, as I was committed to being back in Los Angeles before the end of summer, but I did return to Gap for a few days in early July to take part in the first flight of the instrument built by the French–Danish collaboration. We had a successful flight of five hours duration at ceiling, though I did not see any results from its until after I had returned to the United States. Then my family and I returned home with much gratitude for the fine hospitality we had received from Bernard Peters and his fine institute.

The work continued; first with more flights of the French–Danish instrument, from which was learned the importance of the redundant glass counter to better rule out undetected nuclear interactions. The Danish telescope was modified to include a redundant Cerenkov detector in place of the bottom scintillator, and it was flown three times in the following year. The modification improved the charge resolution. These experiments led, eventually, to the placing of such an experiment on the HEAO satellite by the French–Danish collaboration; but that is another story.

Bruce Dayton is Professor Emeritus, Department of Physics and Astronomy, California State University, Los Angeles, USA.