Introduction to WRF Life in Physics.

Educated as an Engineering Physicist, I spent most of my life inventing and building massive detectors for high energy particle physics experiments at international laboratories. This required dividing my time between working at those labs, building and testing detectors back at or near the university and of course teaching physics, helping my dear wife raise a family, occasionally sleeping. Some years we even managed to squeeze a family vacation into this rigorous schedule.

I write this long story (after twenty-five years in retirement) as encouragement, or perhaps as a warning, to the young.

Chapter 1. From School Days to PhD.

Early days. I always tended towards the microscopic, but I don't recall learning much about physics at Peterborough Collegiate and Vocational School. I remember a demonstration of light's colour separation by a prism, and of a mass M accelerating down an inclined plane, but not much else. Mathematics was different; we were well taught and encouraged to think. History was well taught too; as were languages and classics, although I got into an administrative struggle with the head of classics, "Helena Maxima", and dropped Latin in mid-fourth year. Fortunately, I had been carrying a 3-course overload for a couple of years so this did not endanger my graduation. Also, Queen's University had dropped its requirement of high school Latin for admission to Engineering, so this merely enforced a tendency I was developing at that stage towards science and away from classics.

Queen's Faculty of Applied Science changed my life. My Dad (BA'27/BSc'29, Queen's) had encouraged me to pursue engineering, but once I arrived at Queen's the math and science part got most of my attention. I remember in a second-year physics lab test, being given a simple problem in "error analysis". Normally, when given a formula like f=x/y, you estimate that an uncertainty of 3% in x, together with an uncertainty of 5% in y, means you have an uncertainty of 8% in f. And if f= x^2/y, then the uncertainty in f would be 2x3+5=11%.) The test presented a somewhat more complicated system: a torsional pendulum with a horizontal pole of mass M and length L_1, suspended horizontally from each end by (vertical at rest) strings of length L_2. I was to compute the uncertainty in its torsional oscillation frequency, given the uncertainties in measurement of L_1, L_2. Unfortunately, my question paper was blurred (as "mimeographed" copies sometimes were) and I did not see that the formula giving frequency as a function of L_1 and L_2 was actually provided on the question paper: the answer required was merely a more complicated version of the above example for f=x/y. However, I thought we had been asked to first analyze the torsion pendulum problem from first principles, deduce the formula, and only then do the error analysis. So that's what I did: almost didn't finish on time, but I got a very good grade on that test. I had other adventures as an undergraduate, for example winning the prize for second year Chemistry; a book "Chemical Composition and Properties of Engineering Materials" which is still on my shelves, and which I consulted recently to find out what was going on at the cement works we observed in Prince Edward County just a few years ago. I also got to be the "Bews Tutor" for that Chemistry course the following year, which helped financially too. I was awarded the medal in Physics when I graduated "magna cum laude" in 1956.

When registering for my third year at Queen's in the fall of 1954, I elected to move into the Engineering Physics program. Then I experienced my first scientific summer job the following summer in the Gas Dynamics Lab at NRC (National Research Council) in Ottawa. That lab had some interesting research equipment captured from Germany at the end of WWII, including a huge air compressor/evacuator system, designed to test jet engine combustion chambers, simulating conditions at any altitude and flight velocity over a huge range. The compressor/evacuator system allowed us to appropriately adjust not only the inlet air pressure and flow rate, but also the outlet conditions. And what were we testing? The AVRO ARROW annular combustion chamber! This was a real innovation. Just ask for a lecture on this design.

Our third-year course in electronics (taken with the electrical engineers) had briefly mentioned a new invention, the transistor (but without giving us any scientific background: we were already aware of solid-state diodes, but this ss device, the transistor, had a third electrode like the grid in a vacuum triode tube). Fourth year was our first exposure to courses in modern physics: first, Atomic and Molecular Physics, and in the final semester, Nuclear Physics. The latter was given by the Head of Physics, B.W. Sargent ("Sarge"), an established man in the field, a Rutherford student, and a familiar face at Chalk River Nuclear Lab. And the new (to us) quantum mechanics required to describe motion at very small distances: everything moves not continuously, but a bit jerkily. Energy of a tiny system was not continuous either but quantized into discrete states. And a small system's angular momentum or "spin" was quantized also. Wow. Something else! Snooker with sub-microscopic balls would be weird.

Anyway, I was about to graduate. In those days, companies came to the university every spring to try to hire new graduates in science and engineering. I got two offers, one from ORENDA ENGINES (In 1956 they were making the engine for the AVRO Arrow) and one from Canadian General Electric in Peterborough, whose new Civil Atomic Power Division (CAPD) was designing the NPD (Nuclear Power Demonstrator reactor, the precursor of the CANDU Reactor). Following my new interest in nuclear physics, I chose CGE, starting in early May. (Good choice, because the AVRO ARROW was soon to be junked by the next Conservative government.)

My life about then became much more complicated, as Frances and I got married on graduation day evening (Saturday, May 19, 1956). A whirlwind honeymoon for May's long weekend in Niagara, but back to work Tuesday morning at CGE.

At CGE I was the second member of the new 2-man Reactor Physics section of the CAPD project, working for Dr. Chick Whittier, who had just come to Peterborough from AECL (Atomic Energy Canada Limited), Chalk River. To solve reactivity questions (like what thickness of what materials would absorb too many neutrons and shut the reactor down before it got started) we had to solve a set of seven coupled differential equations. GE's nearest computer was in Schenectady, NY, so I had to solve these equations in the good old-fashioned way, using Integral (Laplace) Transforms. We did have a bright summer student who would spend the week punching his program onto what he called IBM cards, using a hand-held card punch (about 10x25 cm). He would mount one card in this frame and slide a little bridge along to one of the card's 80 columns. He would then move a tiny little hole punch across the bridge to punch out a number between 0 and 9 (and punching more than one hole per column to code alpha characters). He and his laboriously punched little card deck would then take the train to Schenectady, where the computer tended to be more available on weekends. He would return Monday morning with a pile of printout, called a "dump job", which he would scour to find out his "programmer errors", and then repair his little deck for the next weekend's jaunt to Schenectady.

I never saw how that worked out because my industrial career had already been seriously compromised on graduation day at Queen's. A classmate had suggested I should see Professor Sargent (Chairman of Physics) before leaving the campus, and Sarge advised me to consider graduate work in Physics! Back at CGE, I asked my new boss Dr. Chick Whittier if this was a good idea. Chick said a PhD could be really helpful in an industrial job, but not always: since this was close to flipping a coin, why not ask if Queen's would pay for it. Sarge then offered an NRC studentship of $1200 for the winter term, and a $1200 supplement for the summer. CGE was paying me $4K per year, a good starting salary for those days, but NOT tax free like Sarge's scholarship offer. Sounded too good to miss, and Frances was really itching to get out of Peterborough (actually, for the summer, Bridgenorth. We were living in the apartment over Mom and Dad's boat house, which I had fixed up for the purpose.) So, in September of 1956 I resigned from my first job as an engineer and went back to the University: never got out again until 40 years later in 1997.

Perhaps a word or two about sub-atomic physics would help you to understand my activities in this pursuit. Since the early 20th-century it had been known that atoms were composite: an atom of a chemically pure substance like iron, copper or gold, consisted of a swirl of electrons surrounding a much smaller and very massive nucleus, with the electrons occupying almost all of the atom's volume but providing only about 1 part in 2000 of its total mass. If we could shrink every electron cloud into its nucleus, a 50,000-ton battleship would be about the size of a golf ball: and it would still weigh 50,000 tons. This tiny nucleus itself is composite, consisting of positively charged nucleons (called protons, one for each electron in the outer swirl, to keep the atom electrically neutral) plus a similar number un-charged nucleons, called neutrons. The protons all carry a positive electrical charge, and so repel each other VERY STRONGLY. But once they get close enough to each other, protons and neutrons tend to stick together, somehow overcoming the electrical repulsion. We call this weird sticking force the nuclear force, and we know that it acts only at very short range. Very light nuclei like Helium tend to have equal numbers of protons and neutrons, but very heavy nuclei can only stick together if they have more neutrons than protons. For example, lead's nucleus has 82 protons and 126 neutrons. The repulsion of 82 charged protons confined into such a small volume is a huge amount of stored energy, like a giant compressed spring; but the extra neutrons help it stay compressed. Sounds a bit unstable? Don't worry, it's safe to drop a lead brick on the floor (but not on your foot).

However, there is a limit to how many protons can be held together by extra neutrons. Nuclei of atoms much heavier than lead are less stable: if you jiggle a Uranium nucleus too much, the sticking force loses its grip, and the nucleus springs violently apart, its compressed spring potential energy having been released into its fragments' kinetic energy. The Uranium nucleus fissions, as we say. You can see why some refer to the nuclear bomb as an electric bomb.

If a Uranium nucleus splits for example into two, the parts will become nuclei of some lighter substance like Iron or Fluorine, and there will be more neutrons available to each than are required for their stability. These extra neutrons are let loose, moving rapidly outwards away from the fission event. What happens next depends on the details. An important point is that low flying neutrons-on-the-loose may encounter a second uranium nucleus, possibly causing a subsequent fission event, or maybe even a series or "chain" of such reactions. MUCH more probable is that the neutrons will simply be absorbed by surrounding materials like steel or water. Lots of lighter nuclei can safely harbour an extra neutron or two. To pick an extremely tolerant example, calcium 40 (usually having 20 protons and 20 neutrons) has 5 possible stable configurations or "isotopes": 20 protons plus either 20, 22, 23, 24 or 26 neutrons. A nuclear reactor can only be coaxed to release enough energy to heat steam for power generation by carefully shepherding enough of these extra neutrons-on-the-loose to make sure they have a chance to cause a subsequent fission. The energy release in one fission event is tiny, so the fission rate in a reactor is enormous, zillions per second. For stability it is essential that on the average each fission event causes EXACTLY 1.000 subsequent fission event: if less than 1 the reactor quickly shuts down; if more than 1 it explodes.

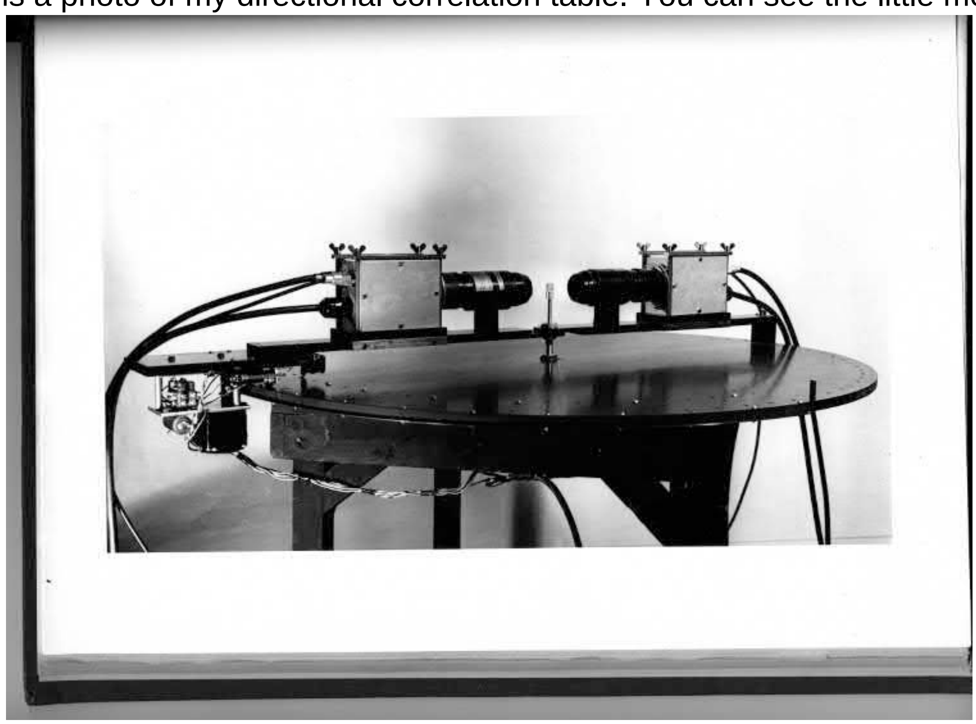
There is much to be understood about atomic nuclei and their constituents, but a central question is "What is this short-range sticky force that can overcome this huge electrical repulsion?" And you might even ask, why can calcium stand to have 20, 22, 23, 24 or 26 neutrons, but not 21 or 25? This sort of detail was accumulated over many years by experiments in many small laboratories: working not with a Large Hadron Collider or equivalent, but by performing detailed studies of the radioactive "decay" of various unstable nuclei. The element to be studied could be made radioactive by exposing a sample to the enormous flux of neutrons available in a reactor, inducing some of its nuclei to take on one or more neutrons than they could handle. In time, these would subsequently decay to a more stable juxtaposition of protons and neutrons and finally juggle themselves down into their most comfortable condition by emitting one or more nuclear X-rays called Gamma rays. A typical example is the unstable nuclide cobalt 60: cobalt 59's 27 protons can manage 31 neutrons, but not the 32 it has been given by reactor exposure. It solves this by allowing one of its neutrons to turn into a proton, emitting an electron to conserve electric charge. Notice that in the process the cobalt 60 nucleus has become a nickel 60 nucleus with 28 protons: but not yet completely stable. It gets rid of its extra jiggling energy by emitting two successive gamma rays. This "decay" of cobalt 60 into nickel 60 takes time, the mean life of a sample of cobalt 60 being about 7.6 years. Ideal for the laboratory, such a "source" outlasting even the slowest graduate student.

My MSc research project involved detection and angular correlation of sequential gamma rays emitted during de-excitation of a nucleus of this sort, stabilizing itself by tumbling down through two or more (quantised, of course) excited states. The idea is that if it doesn't stay in the intermediate state long enough for it to interact with (bump into, for example) something else, the direction of two successive gamma rays would not be random, but correlated in some way, depending on the rotational nature of the intermediate state. Detection of gamma rays was well established, and in those days typically done with crystals of some substance that emitted a flash of light when the gamma ray hit the crystal. This flash was turned into an electronic signal by a photomultiplier tube, and the rest was mere electronics. I had two such "scintillation counters" placed at different angles from the radioactive source. The electronic circuits had to make sure that 2 gamma rays were emitted at very nearly the same time (so I could be sure that they came from the de-excitation of the same excited nucleus). Study of cobalt 60/nickel 60 gamma rays was not original research by 1957 but did serve as a valuable check on my research. My task was to study the much more complicated de-excitation of silver/cadmium 110. My supervisor's idea was to have a graduate student (me) living day and night beside a table with a small radioactive source in the centre and two scintillation counters at different angles, accumulating 1000 such coincident gamma pairs typically in about an hour. Then move the counters to define a different angle, reset and repeat. Student should have a cot and alarm clock beside the table, spending a MSc research year (or preferably two) investigating the sequential gamma decays of various nuclei.

My response was to design an automatic version of this plan. Our electronic "scalers" that recorded the "coincidences" would shut down after reaching a preset number, like 1000. With very little rewiring of this commercial scaler, I arranged for this shutdown to trigger a solenoid magnet which would activate an old "Sept" hand-held, wind-up movie camera that I had found in the attic of the Physics building (Ontario Hall, in those days), which would photograph both the scaler's front panel and a clock, and then advance the film one frame. Here is a Sept of similar design and age, provided by Google.

I installed a little electric motor on the movable counter to advance it to a new angular position, activating a microswitch on arrival. This would reset the coincidence scaler to zero, which would then resume counting. When it got to 1000 again (which took about an hour) it would activate the motor for the next advance. Each morning I would come in before class, change the film, develop the exposed one, and hang it up to dry. After class, I would read the film (one frame for each run) with a magnifying glass and record the data in my logbook.

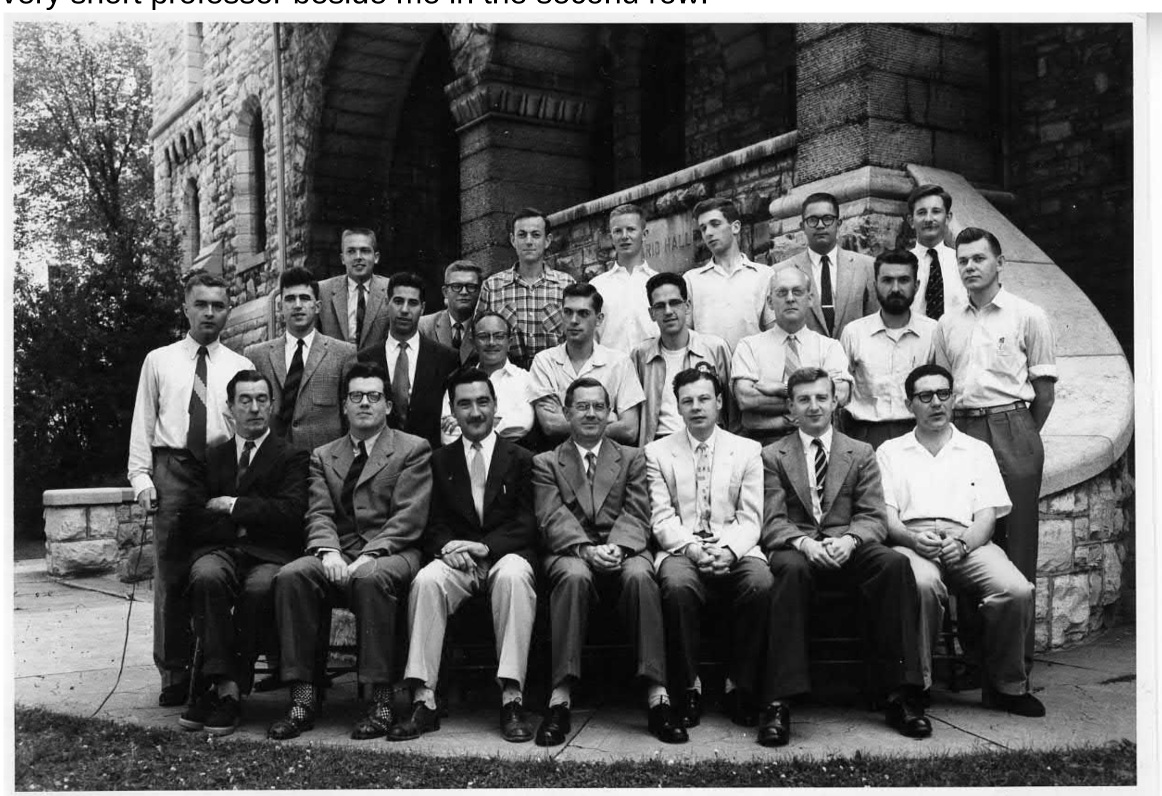
Below is a photo of my directional correlation table. You can see the little motor



and the control relays. The table's little round bumps are rivet heads. The rivets could be placed in holes drilled on a circle in the tabletop. Note there are two circles and two micro-switches attached to the swinging arm that carries the moving scintillation counter. One switch stops the motor at the desired location; the other, actuated during the motion, sends a signal to reset the scalers and prepare to resume counting as soon as the new position has been reached.

Following an incident in which a small fire was discovered in the Physics Building basement one morning (the solenoid that I had rigged up to actuate the Sept camera had overheated), I was provided with a (then) modern camera, designed to be electrically operated.

The next photo shows most of the Physics staff and grad students probably in the spring of 1957. Doug Stairs is absent. Stewart Scott (back row, with his head inclined) shared the basement lab with me. Henry Janzen (second row, at the end on your right) was the technician who ran the little electron synchrotron and was the person who discovered my fire. That's me in the short-sleeved shirt midway in the second row, with Sarge front and centre, directly in front of me; my supervisor, Harry Taylor is in the white jacket, to Sarge's left. Jimmy Allen is the very short professor beside me in the second row.



Anyone desiring further information should consult my MSc Thesis (and the associated logbook, both available in my study) or Phys. Rev. Vol.114, No.1, pp.127-132.

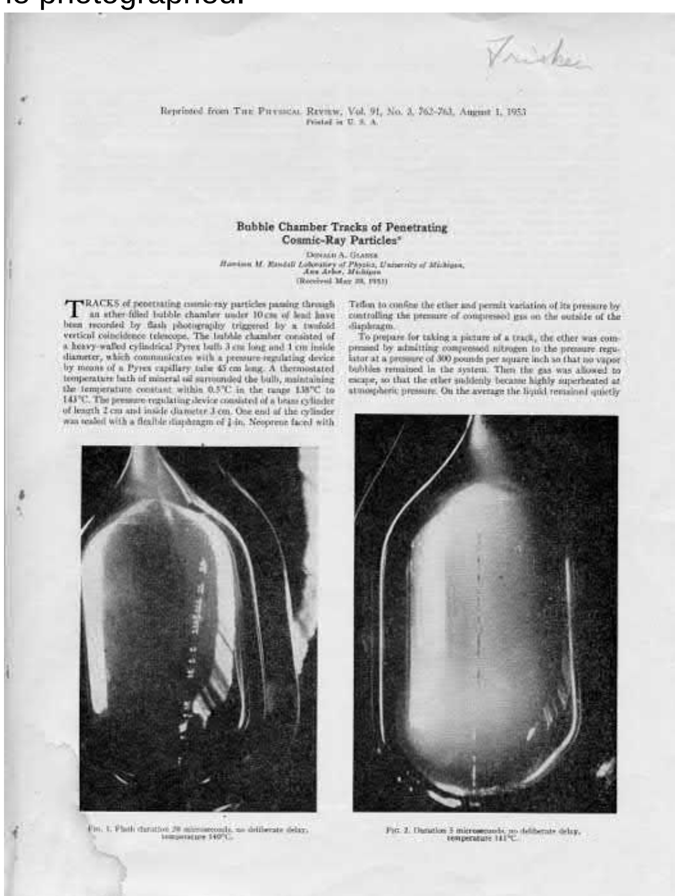
Sarge urged me to apply for a prestigious scholarship funded by the proceeds of the first world's fair, the Exhibition of 1851 in London. This scholarship had a history of funding scholars from the Commonwealth to study in the UK: notable among these colonials was Ernest Rutherford from New Zealand. The problem was that the 1851 required the submission of an MSc thesis and mine would be too late for September 1957, when I hoped to go to the UK for my PhD studies. No problem, it turned out, as there was the R.S. McLaughlin Travelling Fellowship standing in the wings, a one-year Canadian fellowship, with travel expenses included, so off we went. The 1851 award followed just as Sargent had prophesied, one year later. I later discovered that Sargent had a lot to do with the candidate selection process for both scholarships.

His advice to me was "Go somewhere they can make mesons". Why? Because in 1957 the nature of mesons and their interaction with "nucleons" like protons and neutrons was a central question in nuclear physics. Molecular physicists already knew that atoms bound themselves together to form molecules of chemical substances like H_2O (water), NaCl (salt) and CH_4 (methane), and that they bound themselves by sharing their atomic electrons. In 1935 Yukawa had proposed that nucleons might be mutually bound by a similar mechanism but sharing a much more massive particle which he called a meson. Twelve years later new particles were found in cosmic rays, 200 times more massive than electrons but less than 1/5 the mass of nucleons. One of these (the pi meson) was shown to interact strongly with nuclei and was quickly heralded as Yukawa's meson.

The CERN proton synchrotron was not yet in operation. Brookhaven Lab's Cosmotron was having teething troubles, and its later famous AGS (alternating gradient synchrotron) was still under construction. The best possibility was the University of Birmingham, which was operating a small 1 GeV kinetic energy proton synchrotron. It could make mesons, but only just. There I joined a small group: Dr. Giuseppe Martelli and two more senior grad students, Brian Musgrave and John Dowell. They were interested in investigating just how a proton tends to scatter when it strikes another proton: if protons are mushy, a fast proton will tend to go right through a stationary one, changing direction at most slightly. If they are hard spheres, they will bounce at larger angles, just like billiard balls. (This was long before we learned to view protons as complicated objects: not elementary, Watson, but composite, made up of quarks and gluons.) How were we going to observe these scattering events? With a bubble chamber of course, a bubble chamber full of liquid propane. This was 1957, and the bubble chamber had been invented by Don Glaser 3 years earlier.

To operate a bubble chamber, you suddenly and momentarily lower the pressure in a pressurized chamber of the working fluid, just before the short pulse of incident particles (a beam of protons from the synchrotron) comes through. The working fluid in our chamber was liquid propane: when momentarily decompressed it wants to boil, if it can only get started before you recompress it. The ideal starter for boiling is a group of charged particles. And guess what we have: a long trail of ionized molecules left by the passage of very fast particles like our incident or scattered protons. Our liquid propane has been "damaged" momentarily into a trail of separated positive and negative charged particles. Once again, we needed an electrically triggered camera. Actually, this is all in a dark enclosure, so the camera's shutter can be always open. What we need is to have the camera's flash accurately timed and of very short duration. If you get the timing just right, you see strings of little bubbles marking the paths of all the particles that went straight through the chamber (boring), and once in a while you see the outgoing tracks of one that hits a stationary proton (hydrogen's nucleus is a single proton, and propane is C^3H^8). Just like snooker with bubbles!

Here is the front page from Don Glaser's 1953 paper, showing a photograph of his apparatus: a very small glass chamber filled with its working fluid, N-pentane C^5H^{12}.The fast particle leaving the track is a cosmic ray (probably a muon). The flash timing was crude, triggered by a coincidence of signals from two Geiger counters, which have a relatively slow (and uncertain) response time, making for uncertain coincidence of flash timing. Presumably these are the best of many expansions he photographed.



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Bubble Chamber Tracks of Penetrating Cosmic-Ray Particles*

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(Received May 28, 1953)

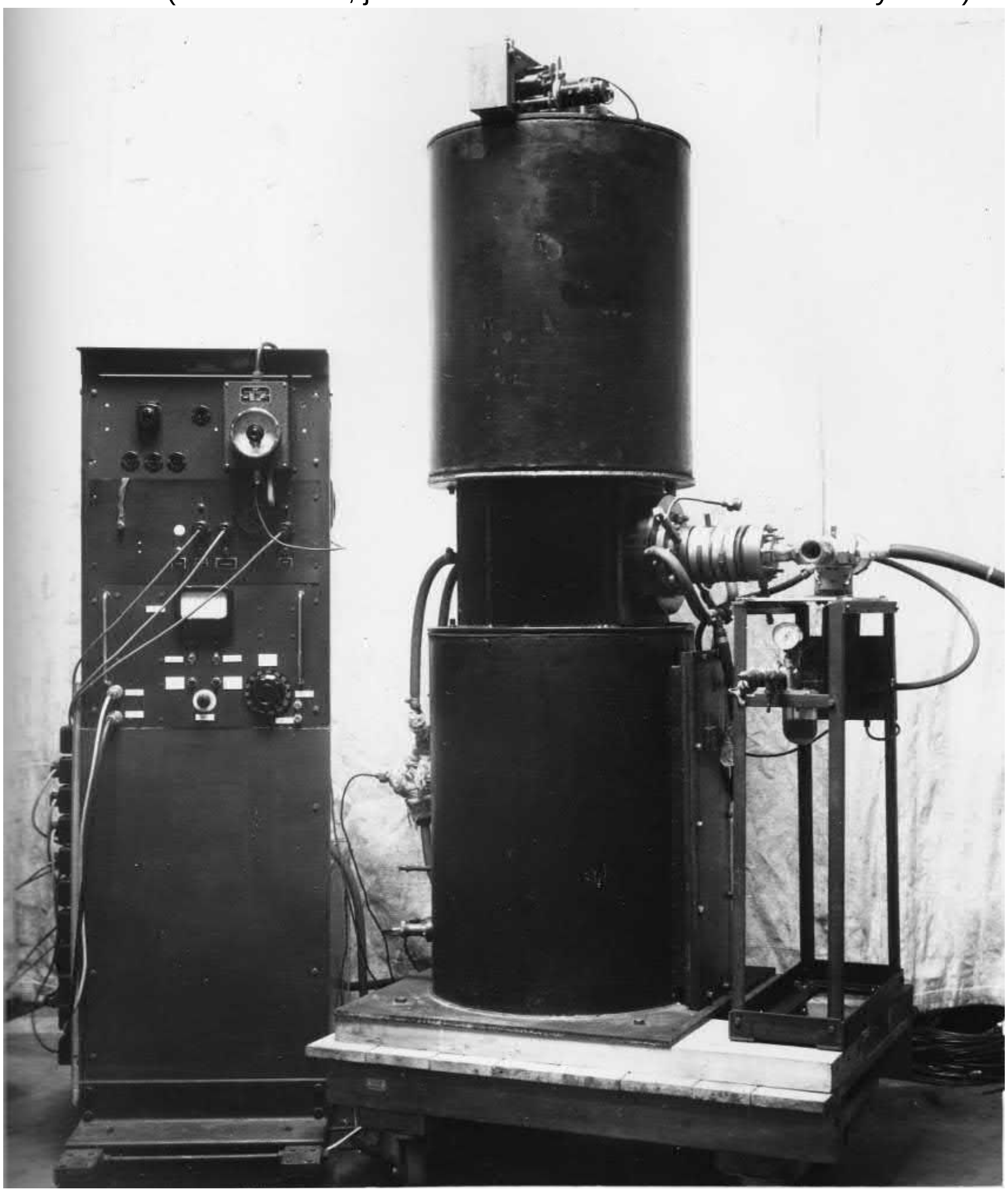
TRACKS of penetrating cosmic-ray particles passing through an ether-filled bubble chamber under 10 cm of lead have been recorded by flash photography triggered by a twofold vertical coincidence telescope. The bubble chamber consisted of a heavy-walled cylindrical Pyrex bulb 3 cm long and 1 cm inside diameter, which communicates with a pressure-regulating device by means of a Pyrex capillary tube 45 cm long. A thermostated temperature bath of mineral oil surrounded the bulb, maintaining the temperature constant within 0.5°C in the range 138°C to 143°C. The pressure-regulating device consisted of a brass cylinder of length 2 cm and inside diameter 3 cm. One end of the cylinder was sealed with a flexible diaphragm of ⅛-in. Neoprene faced with Teflon to confine the ether and permit variation of its pressure by controlling the pressure of compressed gas on the outside of the diaphragm.

To prepare for taking a picture of a track, the ether was compressed by admitting compressed nitrogen to the pressure regulator at a pressure of 300 pounds per square inch so that no vapor bubbles remained in the system. Then the gas was allowed to escape, so that the ether suddenly became highly superheated at atmospheric pressure. On the average the liquid remained quietly

FIG. 1. Flash duration 20 microseconds, no deliberate delay, temperature 140°C.

FIG. 2. Duration 3 microseconds, no deliberate delay, temperature 141°C.

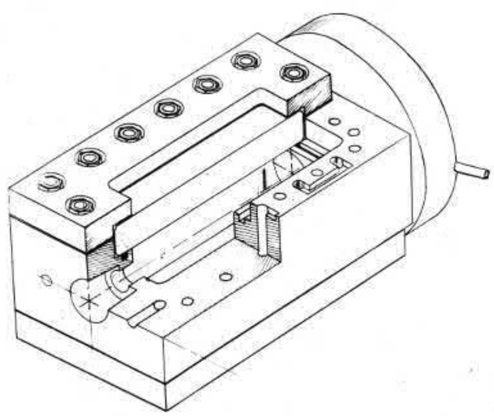
Our bubble chamber at Birmingham was much larger, but soon to be completely dwarfed by chambers built in the US by Glaser himself, and also by Luis Alvarez at Berkeley. The assembled Birmingham chamber is dominated by the big black enclosure towering above and below the chamber per se, forming a special darkroom for the chamber, the camera (John Dowell's design, mounted on top) and its flash (at the bottom, just inside the base of the lower black cylinder).

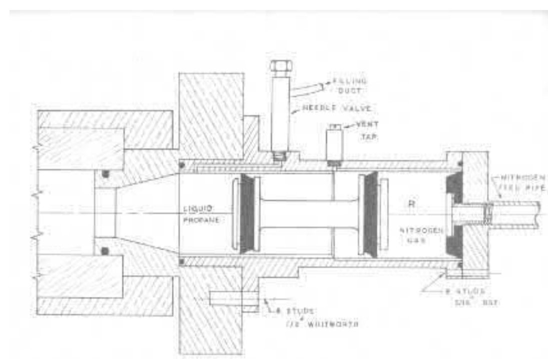


That rectangular bit in the middle contains the chamber itself. The small cylindrical part to its right is the expansion cylinder. A schematic of the chamber is shown on the left below. It had VERY thick glass windows because the previous larger, more ambitious version had exploded just before I got to Birmingham. Malcolm Derrick's pre-arranged post-doc appointment at Carnegie-

Mellon had suddenly become a post-arranged pre-doc, and Brian Musgrave still carries a deep scar in his forehead. Malcolm was awarded his Birmingham PhD several years later for his research while employed as "pre-doc" by Carnegie-Mellon University.

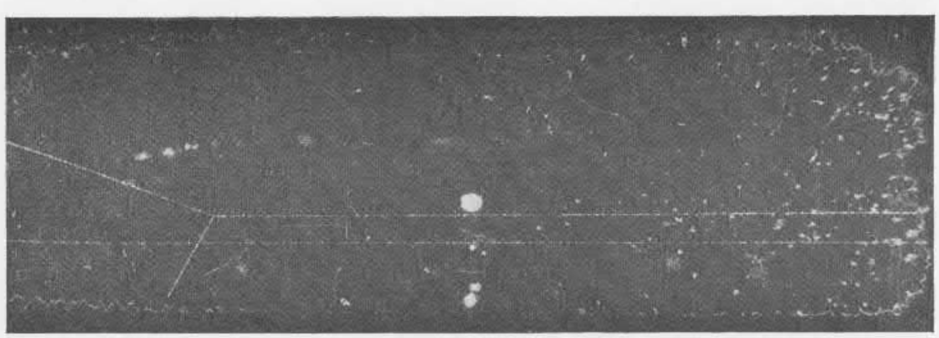
The optical and photographic system was designed by John Dowell, the other senior grad student, and the whole project was inspired by our supervisor, Dr. Giuseppi Martelli, on leave from Pisa. The expansion cylinder, shown on the right below, was designed by me to replace a "more adventurous" (wildly unsafe) version brought by Giuseppi (in his motorbike saddle bag) from Pisa.





In the photo below (from our paper, Proc.Roy.Soc. Vol 74, Dowell et al.) you can see that eruptive boiling also starts around the sharp edges of our chamber. It's a boiling race between the edges and the track: you must achieve sensitivity (by expansion) just a millisecond before the beam proton enters the chamber from the right, quickly crossing the chamber in a mere nanosecond (a millionth of a millisecond). The liquid also wants to start to move towards the expansion cylinder (to the left in this photo), but fortunately that takes much longer, or the tracks would be distorted by currents in the liquid.

PROC. PHYS. SOC. VOL. 74, PT. 5 (J. D. DOWELL et al.)



A typical proton–proton event.

This work led to three published papers plus John and Brian's PhD theses. As the above photo shows, we were playing billiards with protons. The Synchrotron provided a weak beam of high energy (high for those early times) protons: a few per pulse, and the machine pulsed once every ten seconds.

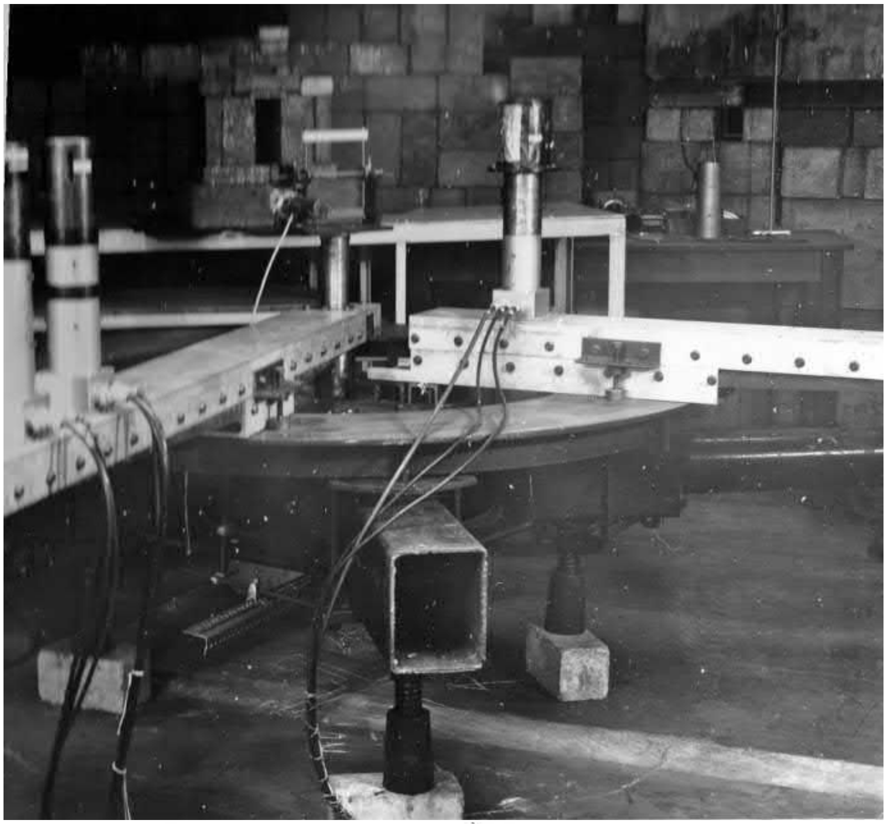
Our results suggested that an almost free proton in the propane scattered the incoming proton (seen entering the chamber from the right end of the photo), behaving as if they were both very small spheres, less than 2 femtometres (2x10**-15 metres, or 0.000000002 microns) in diameter. A bit mushy at the periphery, but with a hard core. Of course, in propane (C^3H^8) there were more than twice that many protons bound in carbon nuclei, but these tended to scatter and recoil at the wrong angles, so this background could be removed from the data sample during analysis. To an incoming proton at this speed (3/4 of the speed of light) the stationary hydrogen nuclei were basically free target protons, relatively unconstrained by molecular and atomic forces.

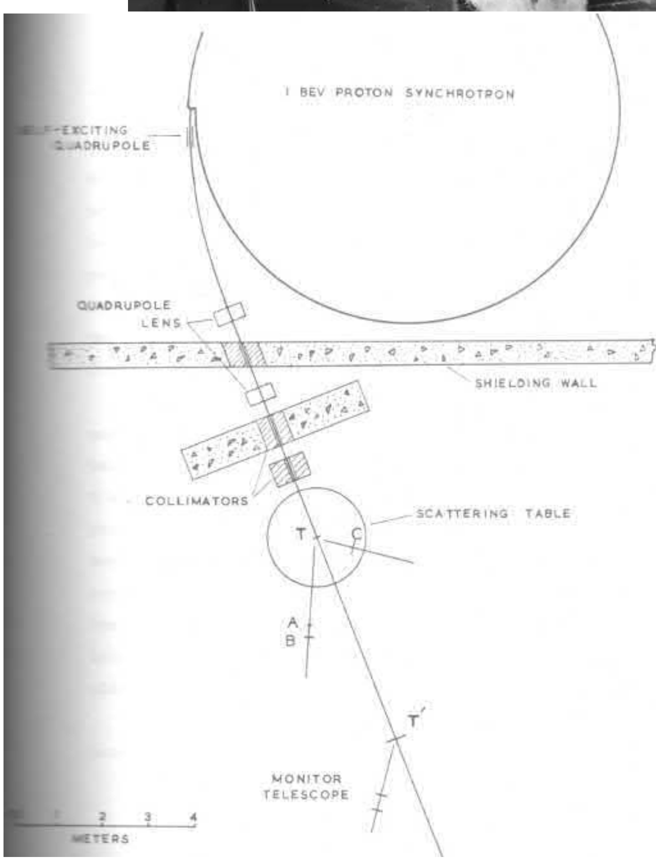
An interesting difference between the present-day operation of high energy physics labs and the 1957 operation of the Birmingham synchrotron lab is that the big labs of the present time tend to run flat-out 24/7 while our old synchrotron lab ran 2 shifts per day (7am-11pm) and 5 days/week; and of course, with the usual university term breaks for Christmas, Easter, and others. By the way, 7am meant the beam would not be up and running before 8am and evening shut down also took an hour, so really just 8am to 10pm for students, not two shifts but one long shift. The school breaks meant travel time for Frances and me, lots of local jaunts around the Midlands (including nearby Stratford-on-Avon), Wales, and even London, plus the occasional much longer trip in an extended break. Of course, the synchrotron had its breakdowns too, which could consume one or more weeks, and once several months.

There were also much longer shutdowns for major improvements of our synchrotron. In 1958 the Italian Physical Society held a summer school at Varenna, on Lago di Como in northern Italy. Giuseppi had encouraged his 3 graduate students to become IPS members because its journal "Il Nuovo Cimento" was then one of the few Physics journals featuring rapid publishing of new experimental results in Particle Physics. Frances and I decided to take the journey to Como; my McLaughlin travelling Scholarship was still covering my travel expenses, and we had our beautiful old 1937 Morris 8 (purchased for 85 pounds the previous autumn) to travel in. We went through France, to Paris, Chartres, and south to the Loire Valley, then east to Geneva (CERN was just getting started in Particle Physics). We then found that we would not be allowed through the sub-Alpine tunnels to Italy because our old car was too slow (yes, it is uphill even by tunnel). So we went south, re-entering France, and then over the Col de Mt. Cenis: half a day in low gear! Hundreds of switchbacks!

That summer school gave me a strong exposure to new ideas about particle physics. Some of the lectures were by Sydney Glashow, later to become famous for his work with weak interaction theory in the mid to late 1960s. I also met some of Giuseppi's Pisa graduate students.

Meanwhile, back at the lab, our bubble chamber proton scattering results were fairly sparse, as we had to analyze a lot of photographs. Most of them showed nothing of interest, just a couple of protons going straight through, or perhaps scattering from carbon to form an unhelpful background. However, our 140 proton-proton scattering events stimulated a demand from theorists for more precise data. At my urging we discarded the chamber and built a "scattering table" using scintillation counters (see my MSc research above) that could deal with an event rate that was several thousand times higher. This allowed us to benefit from a new development of the Synchrotron: a high intensity extracted beam of 10,000 protons per pulse! The bubble chamber could only stand about 4 beam protons per pulse, but the counters could handle the new extracted beam easily, and that part of our research became the subject of my PhD thesis in 1960. At this point we were joined by a new grad student Roy Rubinstein, who was becoming the local expert on (then) fast electronics, replacing vacuum tubes with little solid-state elements called transistors. And this speed we would need. The scintillation counters used plastic scintillators: blocks made of plastic that had scintillating phosphor dissolved in it. These would give a very short flash of light when responding to a particle's passage, 100 times shorter than the crystals used to detect gamma rays in my MSc thesis. The first photo (and its schematic, below) shows the target (T), and upstream of it the hole in the lead wall that the proton beam came through, plus of course the counters that detected the scattered (A and B) and recoil (C) protons. Not shown in the photo is the beam intensity monitor system downstream. The third figure is a close-up photo of the target holder: a "Geneva motion" device to precisely interchange targets of polyethylene (C^2H^4) and carbon, for example (so we could correct for the carbon background). The intense new extracted beam was dangerous, so we had to switch targets remotely, without personnel access to the experimental area. Later we added targets of Lithium Hydride and Lithium Deuteride (deuterium's nucleus is a loosely bound pair: a neutron and a proton) to extend our studies to scattering of a proton from an almost free neutron.

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EXCITING
QUADRUPOLE
QUADRUPOLE
LENS
SHIELDING WALL
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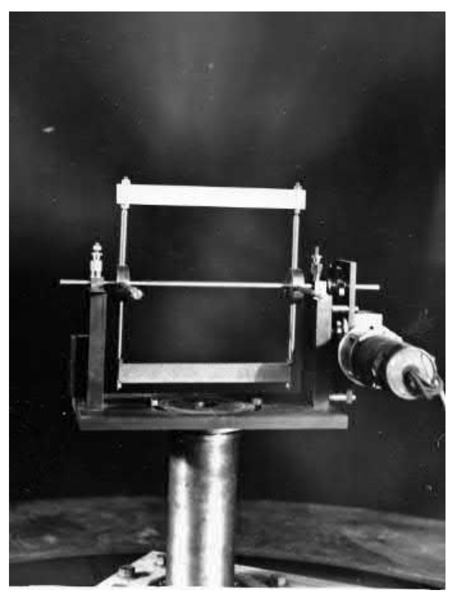

This led to more published papers (for example, Il Nuovo Cimento Serie X, Vol. 18, pp 818-819), to my PhD thesis, my appointment at McGill in October 1960 as Assistant Professor.

Chapter 2. My Introduction to Large Scale Experiments

My physics research at McGill University wasn't really very exciting. In the first place much of my time was taken up scrambling to learn how to teach physics to undergrads in the classroom, and to grad students in the lab. I helped senior grad student Bob Moore with his detailed perturbation of the magnetic field of McGill's synchrocyclotron to produce an extracted proton beam, but that machine was only capable of accelerating protons to 100 MeV, just 1/10 of the Birmingham PS' s energy. No mesons here. I supervised another bright young new grad student (Jonathan Lee), and showed him how to set up a (small) scattering table, this time to look at what had been at Birmingham a nuisance background: the scattering of protons from protons and neutrons bound in nuclei, this time to get an idea of their normal motion within the nucleus before the collision. Before all this could be done, four junior faculty members, Bob (by then Dr. Moore), Murray Kavanaugh, Bill Link and I had to design and build a new addition to the Radiation Lab, designed specifically for experiments using Bob's new extracted beam. Several other experiments were performed with this new beam, one or two of which will appear in my list of publications. Jonathan went on (after I left) to get his PhD, and eventually both Bob and Jonathan became Professors of Physics at McGill. (Not Sargent's idea of the right way to do things, but common at Canadian physics departments other than Queen's. Sarge regarded this as inbreeding.) Murray Kavanaugh went off eventually to AEC Livermore, where he spent the rest of his career in relative secrecy. Bill Link had returned to the Rad Lab from a few postdoctoral years at Brookhaven Lab's Accelerator Division. He told of his exploits working for Canadian physicist John Blewett (joint author of Livingstone and Blewett's magnum opus on Accelerator Physics) and helped set the stage for my next phase.

At the spring APS meeting in Washington DC in 1963, I met a friend, Earl Fowler (Professor at Duke U.) with whom I had worked during his sabbatical year at Birmingham in 58-59. Surmising that I might be bored with low energy nuclear physics at McGill's Rad Lab, Earl suggested I attend the upcoming 1963 summer workshop at Brookhaven Lab on Long Island, where they would be designing experiments for the proposed ISABELLE high energy proton-proton storage ring/collider. This I did: wow! what a difference! Head-on collisions of proton beams at hitherto unheard-of energies and momentum transfers! My job at the summer workshop was to help design detector systems for the many outgoing particles resulting from these collisions. I wrote up several reports on my work there. A few weeks later, in September of 63, I received an offer I couldn't refuse: to move to BNL, with a bump from Assistant Prof to Associate Scientist and a stunning salary increase. I agreed to make the move, as soon as classes were over in May of 1964.

A sobering shock that gave me a lot of pause at this moment: President John F. Kennedy was assassinated!

I was employed by BNL's Advanced Accelerator Development Division (aka A^2D^2) and had been busy with this work for only a few months, when one day at lunch in the late autumn of 1964 I met two former colleagues from Birmingham days, Hywell White and Roy Rubinstein. Hywell was a postdoc at Birmingham when I was there and had moved first to U. Pennsylvania as Assistant Prof and was now Associate Prof at Cornell. Roy was Hywell's postdoc at Cornell. Disappointingly, the ISABELLE project had been put aside "for a while" (eventually becoming known as the WASABELLE project), and my Cornell friends urged me to get permission to join their small team of experimenters (known as the CBUGs or Cornell-Brookhaven User's Group) preparing guess what? An investigation of (then) very high energy elastic scattering of pi mesons (just for a change) from stationary protons, particularly at large scattering angles. In this angular region the momentum transfer is higher, and so the detail of the proton's structure is probed more deeply. Brookhaven's main accelerator was the AGS, a 30 GeV proton synchrotron, producing beams of protons with 30 times the energy we achieved at Birmingham. And it certainly could produce mesons, pi mesons having about 1/7 of the proton's mass, K mesons at half the proton's mass: even antiprotons, having the same mass as protons. We eventually used all of these to probe the structure of the proton.

My boss said OK (I am sure he thought that I could do it nights and weekends), but this work eventually became so demanding and so important to the Lab, that I hardly spent one shift a week at my A^2D^2 desk from then on. Here's a photo of our experiment being set up. It takes up a large area of the experimental hall. Look familiar? Yes, those are scintillation counters again, used now to detect the scattered mesons and recoiling protons. The AGS is not visible, being hundreds of meters distant in the background, beyond a massive concrete shielding wall. An addition is that huge blue magnet, which allows us to measure the momentum of the particles, and thus their mass (so we could tell outgoing mesons from protons). The incoming beam was a mixture of different particles, all selected to have the same momentum, but having different masses. Some particle ID technology required. To have the same momentum, the lighter mesons would have to be travelling faster. They were identified by a Cherenkov Counter, which noticed their higher velocity. In the Cherenkov counter the incoming particles travelled through a long tube filled with a special gas (sulfur hexafluoride) in which the speed of light was less than in the air outside. In the sulfur hex, the fast-moving meson caused a luminous shock wave which accompanied it in its journey through the counter. Thus, caught exceeding the local light speed, each meson arrived with a flash of light rays travelling almost parallel to it: an electronic speeding ticket with its name on it.

When the experiment was ready to run, the high momentum beam of particles coming from the AGS would be coming straight at the camera, between the two rows of red fence. The incoming particle beam is aimed to first run right through the liquid hydrogen target (our target protons) in that light blue tent with the pyramidal roof: the incoming particle scatters to the right (your left), passing through the smaller blue magnet and one of several scintillation counters (covered with black photographic tape). The struck target protons recoil at larger angles through the large blue "picture frame" magnet and one of a wall-like array of large plastic scintillation counters. A schematic top view (from our publication):

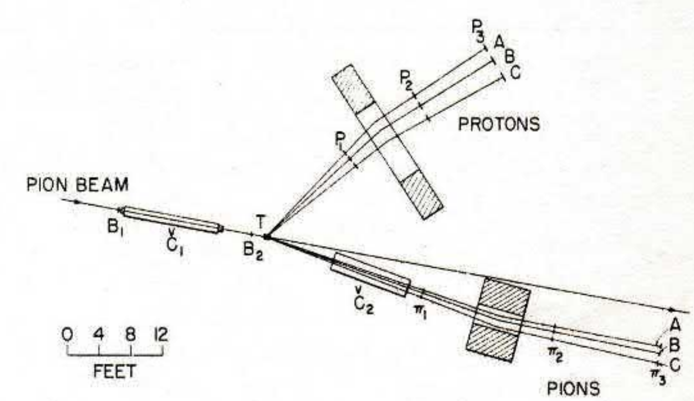

FIG. 1. The magnet and counter arrangement. B_1 and B_2 are the incident beam counters, $\check{C}_1$ is a threshold Cherenkov counter, and T is the hydrogen target. P_1, P_2, and P_3 are the scintillation counters in the three proton telescopes, and π_1, π_2, and π_3 are the counters in the three pion telescopes. $\check{C}_2$ is a Cherenkov counter in the pion telscopes used to eliminate accidentals due to proton-proton scattering.

When the photo was taken, the second Cherenkov counter (C_2) had not yet been installed. Developed by Roy and myself, C_2 consisted of a 3-foot diameter 12-foot-long black polyethylene balloon (made in one evening on our living room rug in Patchogue using a hot iron for sealing its seams). Then at the lab in the midst of the experimental setup we built a light-tight darkroom for it (not shown in the schematic diagram above). To operate, our long balloon was inflated with sulfur hexafluoride (SF_6) to give a shockwave of light for pi mesons, but not for the slower protons. For the interested student, the gas bag's downstream end had a large thin (stretched aluminized mylar) mirror at 45 degrees, then a 24" square Fresnel lens, focused on a large photomultiplier tube. The pi mesons went right on through the mirror, but their light speeding tickets were reflected by the mirror, focused by the Fresnel lens and collected electronically as a voltage pulse from the photomultiplier tube.

The big blue magnet (60 tons) could be moved into a variety of positions, so the experiment could measure scattering at a wide variety of angles. This next page is from a paper of ours that received a lot of citations, because it showed that pions had a strong tendency to scatter very backward, as if the pions were disguising themselves as (changing into) protons, and really just continuing to go forward as expected during their very brief interaction. At that time, we particle physicists were just beginning to realize that "elementary" particles like protons and neutrons (and mesons) were not elementary at all, Watson, but dynamic assemblies of smaller particles (later termed quarks and gluons). I remember C.N. Yang (already a famous Nobel Laureate theorist from nearby Stony Brook) visiting our instrumentation trailer in the middle of the night shift to inquire about our latest results. Hot stuff.

VOLUME 15, NUMBER 7 PHYSICAL REVIEW LETTERS 16 AUGUST 1965

BACKWARD ELASTIC SCATTERING OF HIGH-ENERGY PIONS BY PROTONS*

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and

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A backward peak in the positive pion-proton elastic cross section has been reported[1] for 4-GeV/c incident pions as well as a hint that this peak might persist at higher energies.[2] We have studied backward elastic scattering of pions at 4 and 8 GeV/c, measuring cross sections for positive and negative pions at center-of-mass angles from 170° to 180° in the center of mass. We find a sharp peak in the backward direction for positive pions and a lower, flatter peak for negative pions at both 4 and 8 GeV/c.

The experimental arrangement was similar to that used in an experiment to measure forward elastic cross sections at high momentum transfers, which is reported in the preceding Letter.[3] Figure 1 shows the arrangement used for backward scattering. The backward-scattered pion and the forward-going proton were each momentum analyzed and detected in scintillation-counter telescopes. The solid angle in the center of mass, subtended by each telescope, ranged from 0.5 to 1.5 msr; the momentum resolution ranged from ±6 to ±12%. From two to five scattering angles were measured at one time, using a large fraction of the 120-in. by 24-in. gap of the pion magnet and the 30-in. by 6-in. gap of the proton magnet.

A threshold gas Cherenkov counter together

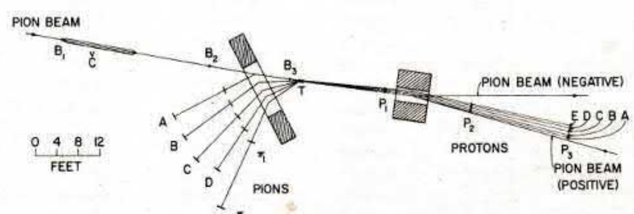

FIG. 1. The experimental arrangement. B_1, $\check{C}$, B_2, and B_3 are the incident beam defining counters and T is the hydrogen target. π_1 and π_2 are the counters of the five pion telescopes, and P_1, P_2, and P_3 are the counters of the five proton telescopes. The incident pion beam passes through the large-aperture magnet before striking the target.

An interesting aspect of Brookhaven was that it relied heavily on the associated universities for its intellectual drive. It did have some very significant staff scientists, but the ideas often came from the universities. BNL was operated by

AUI (Associated Universities Inc.), as an AEC contractor (shortly to become a DOE contractor, as the Atomic Energy Commission morphed into the Department of Energy). I had to have "clearance" to work there. For us younger staff scientists there was an urge to get an academic appointment and move to the outside.

I went north to Providence and gave a seminar at Brown, but they didn't have anything attractive. Then I heard that Case Institute (in Cleveland) was looking for someone at about my stage. I interviewed there in the summer of 1965, and was offered an Associate Professorship, but without tenure. Not understanding these things as well as I do now, I accepted their offer. Shortly thereafter a deputation from McGill came to BNL and offered to match the Case offer, if I would go back to McGill (where I learned I still had tenure). Tempting, but I refused.

The experiment at BNL had evolved to become much more complicated, and now included "spark chambers". The scintillation counters could locate the path of a particle within a few centimeters, but the spark chambers showed the actual trajectories of the particles. Like a bubble chamber, a spark chamber shows a path, but not quite so detailed. However, it is triggerable: if the counters claim something of interest has just happened, you can trigger the spark chambers in less than a microsecond and catch the particle paths before they disappear. (A bubble chamber requires more than a millisecond to get the expansion going, so you have to expand every time the beam goes through and take a picture every time in case something of interest might have happened. And then scanning through all those mostly empty photographs..... Ho Hum...). Fortunately, Hywell had just hired a new postdoc, also a member of the "Birmingham Mafia", Dave Ryan (Canadian from Quebec City, a member of Giuseppe's team after my departure, but overlapping with Roy Rubinstein). Roy had moved from Cornell to BNL as an Assistant Scientist, and Dave was an expert in spark chamber technology.

Meanwhile, my eldest daughter Barbara had started grade school. My wife Frances had gone back to school too. She had taken a grad course at Stony Brook University in Sociology, but was eager to leave Long Island. My Father had survived open heart surgery in Toronto early in 1966 but had then died in March while recuperating in Florida. This was a busy time for me, and I only began to really miss him much later. I was to start lecturing at Case in September, and I was invited to lecture about our experiments at a Summer School of the Italian Physical Society in Erice (Sicily) in July, and afterwards at the Rutherford Lab (near Oxford, UK) on my way home. The Head of BNL's Accelerator Division, Lyle Smith, asked me if there was anything BNL could offer to make me stay, but I had made up my mind.

Somehow, we managed the move, into a rented house on Berkshire Road in Cleveland Heights, just up the hill from Case. Barb started back to school in Cleveland Heights and Frances started grad studies in the Science, Technology and Public Policy Program at Case. After much agitation, I (and other new faculty members) managed to convince the Case administration to extend free tuition for children of faculty to include faculty wives, but just then Frances managed to win a scholarship from the Danforth Foundation that paid hers anyway! I continued to commute to BNL until the experiment was finished a few months later, and then started to look into possible experiments at the more convenient Argonne National Lab in Chicago.

Case had just received a large NSF grant to start a particle physics group, and I was to be its first new member. Unknown to me, Case Physics was in transition. I had been hired by its Chair, Fred Reines, a well-known physicist famous for his detection of (and subsequent work with) neutrinos. What I did not know was that Fred had arranged to move to UC Riverside, to become Dean of Science, apparently hoping to take the new NSF grant with him. This didn't work out for him, which must have been disappointing for both Fred and Riverside. It also left direction at Case a bit unclear as I arrived, which I only gradually learned. This left Professor Tom Jenkins in charge, but open to suggestions. His background (like Fred's) was in detection of neutrinos in deep underground, very quiet conditions, and I had been hired as an expert in running busy experiments crammed into crowded conditions (and schedules) at big laboratories. Tom and I probed various possibilities for research at the Argonne National Lab in Chicago but found their program committee to be fixated on strengthening established groups, rather than encouraging new ones. At Case we had added two more faculty and some graduate students: we wanted to compete as a separate Case group. So we had to propose an experiment that would get the program committee's interest, yet one that had not already been proposed. For a year or so Tom and I tried to press for approval for an experiment in the new area of "weak interactions", but our only route seemed to be to work on someone else's experiment.

We finally got the Program Committee’s approval for a proposed series of experiments that were an extension of my work at BNL, but with the charged pion turning into a neutral pion during the collision, while the target proton turned into a neutron! OK, so now it's obvious, once you realize that pions and protons (and neutrons) are all composed of the same constituents, and that they are just swapping partners on the way past. Technically a bit different, though. You had to detect electrically neutral pions, and neutrons. Hmm.... some invention required. Here's the set-up we worked out.

VOLUME 26, NUMBER 9 PHYSICAL REVIEW LETTERS 1 MARCH 1971

Measurement of the Reaction $\pi^- p \to \pi^0 n$ at Large Momentum Transfers*

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(Received 10 November 1970)

We present results of an experiment to measure the differential cross section of the reaction $\pi^- p \to \pi^0 n$ between the forward and backward peaks. The measurements were made at incident π^- momenta of 3.67 and 4.83 GeV/c. The t range $1.7 \le |t| \le 4.9$ (GeV/c)^2 was covered at the lower momentum and $1.8 \le |t| \le 7$ (GeV/c)^2 at the higher momentum. At the lower momentum the cross section is essentially constant between $|t| = 2.4$ and 4.8 (GeV/c)^2 while at the higher momentum the angular distribution exhibits a broad minimum centered at $|t| = 4.4$ (GeV/c)^2.

We report a measurement of the differential cross section for charge-exchange scattering $\pi^- p \to \pi^0 n$ in the region of high momentum transfer. Incident pions of momenta 3.67 and 4.83 GeV/c were obtained in the 17° beam of the zero-gradient synchrotron (ZGS) at Argonne National Laboratory. Previous experiments[1-3] had already measured the forward and backward peaks and the present experiment was designed to be sensitive in the high-momentum-transfer region in between. Regge-pole models[4] have predicted a dip in the cross section at $t = -2.5$ (GeV/c)^2, and recent data[5] in the elastic channels of the $\pi^- p$ and $\pi^+ p$ interactions show a pronounced dip in the region of $-t = 2.8$-3.0 (GeV/c)^2.

The apparatus is shown in Fig. 1. The π^- beam was momentum analyzed to ±0.75% and focused on a 10-cm-long liquid-hydrogen target. The beam-defining counters consisted of a quadruple scintillator telescope, B_1 through B_4, together with an array of beam-halo veto counters A_{17}-A_{20}. The threshold Cherenkov counter between B_1 and B_2 was used to veto higher-mass particles. The output of this telescope was vetoed by any beam particle which preceded it by less than 1 μsec. The final result was a signal, designated B, which typically ran at 10^5/sec. Thin-plate spark chambers, S_3 and S_4, in the beam located the trajectory of the incident particle.

The basic left-right symmetry of the apparatus

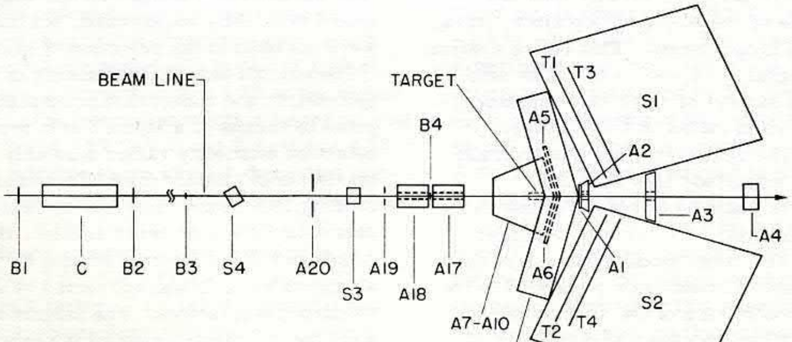


FIG. 1. Plan view of the apparatus. The beam telescope consisted of B_1 through B_4. C is a gas Cherenkov counter. All counters designated A were used as vetos. A_1, A_5, A_6, A_{19}, and A_{20} were simple scintillation counters; all other veto counters consisted of lead-scintillator sandwiches. The counters A_7 through A_{10} formed an enclosure around the target and in turn were surrounded by a rectangular structure consisting of counters A_{11} through A_{16}. Counters T_1 through T_4 were scintillation counters embedded in the chamber arrays (S_1 and S_2) which produced the "T" signal. S_3 and S_4 were thin-plate spark chambers.

The idea was to detect events in which a neutral pion scattered to one side and a neutron scattered to the other side (and in which nothing went backwards: hence the Anti- or veto-counters.) Pi-zeros decay quickly into a pair of high energy forward-going gamma ray photons, which shower briefly in the thin lead spark chambers in the first few layers. Neutrons show up by knocking a proton out of a lead nucleus in the thick-walled chambers deeper in the array. So, look for this signature: a shower in the front thin-lead plate chambers on one side and knock-on proton track deep in the thick-lead plate region on the other. Logistically identical to the BNL experiments, but much more compact. All this got a lot of attention (well, from other particle physicists), so I was awarded tenure at Case in due course. Publications were not a problem for my CV, as they were pouring out of the BNL research, and we managed a couple of interesting ones on the new work at Argonne.

Chapter 3. Climate Change and Meteorology.

Soon it was 1971. We had lived in the USA for 7 years, 2 on Long Island, and 5 in Cleveland. Frances had gone back to university, doing an MS and now just finishing a PhD in Science, Technology and Public Policy at Case. We (mostly Frances) raised 3 kids at home in our spare time. I spent many weekends in Chicago supervising Case graduate students at the Argonne Lab. The Cleveland Rapid Transit station was about a block from my office, and I could be at the airport in 25 minutes; then in Chicago's Midway airport in almost no time at all (saving an hour moving to Central Standard Time). I would bunch my Case lectures into Monday to Thursday, and late Thursday afternoon head for the airport. Sunday afternoon and evening I would be swatting up for Monday's 8 am lecture (Case is an engineering school, not a liberal arts college). I spent even more time at Argonne in the summer months.

By 1970 I had become bored with up/down quarks (the "ordinary" ones in protons, neutrons and pi mesons) and drifted off into the scientific aspects of environmental problems. In US universities at that time, you typically were paid for teaching 9 months of the year: some professors were also paid for 2 months of summer-school teaching or committee work, but always with zero for one month forced holiday. (This was clearly long before faculty unions.) If you had a research grant (as I always did) you paid your 2 months "summer salary" out of the research grant and went on with your research. Engineering professors often did 2 months of consulting. This was always 2/9 of your regular salary, so you were always reminded of the basic 9-month arrangement. If your grant was big enough to pay part of your teaching salary (and mine often was) you could even take a semester of unpaid leave if your experiments became too demanding. In the summer of 1970, I spent 2 months consulting for RFF (Resources for the Future), a subsidiary of the Brookings Institute in DC. My RFF assignment was to look into the connection between climate change and human industrial activity. Part of my job was to attend a weekly seminar at U. of Toronto on pollution problems, so during that summer I typically flew to Toronto to spend Monday and Tuesday at UofT, and then to Washington, to spend the rest of the week at RFF. Lots of other contacts at NOAA (a Paleoclimatologist in DC and atmospheric modellers in Boulder) as well as in academia. I wrote a paper on this work for EOS (Transactions of the American Geophysical Union) entitled Extended Industrial Revolution and Climate Change, which got a lot of attention (for 1970).

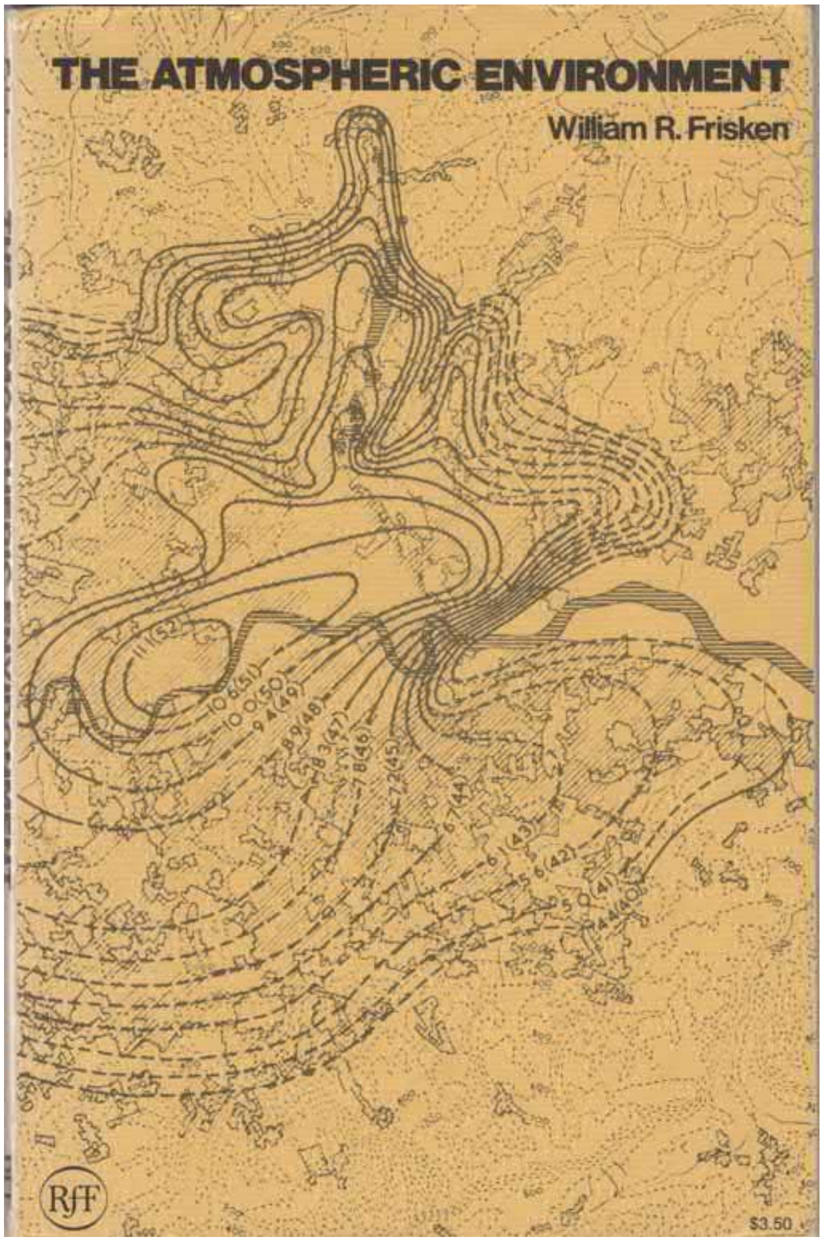
Case Institute had just changed to a "4-1-4" curriculum (the 1 month "intersession" was not for credit) and for January 1971 I organised an intersession course "Physics Today is Everyday" given every lunch hour in the Physics amphitheatre. I borrowed the name from the American Physical Society's magazine ("Physics Today") and arranged for colleagues to give talks about something of general interest. I started the month with three one-hour lectures on climate change.

Just about then, we got a letter from Joan Code (Frances' mother) containing a newspaper clipping showing that York University was looking for an "Environmental Physicist". I applied, not realizing that I was to spend the rest of my career at (or at least based-at) York.

At York University I quickly became involved with Canada's meteorological service (the Atmospheric Environment Service, AES), and with the Canada Centre for Inland Waters (CCIW). The AES was situated near YU on Dufferin Street, and CCIW in their research centre under the Burlington Fly-over bridge, at the entrance to Hamilton Harbour.

I was sponsored by the Atmospheric Environment Service of Canada to give a series of lectures at government meteorology labs across the country during my first year at York. I then designed my environmental research to test the hypothesis that the Great Lakes Basin would tend to remain stable longer than, for example, the great plains, in the event of a rapid climatic change. I took on two theoretical graduate students in Meteorology at York, who focused on computer modelling of meso-scale boundary layer meteorology near the Great Lakes, while I set up a small experiment on the ice to the south of Wolfe Island (near Kingston) to measure the incoming and outgoing radiation (solar and infra-red), turbulent heat transport and heat conduction through the ice. On Wolfe Island I joined a group from Glaciology Canada who had rented a farm there for the winter as a base for their studies, so I could take the bus down from Toronto and bunk in at the farm overnight every weekend. I could even use their skidoo to get out to my station on the ice.

The summer of 1972 I worked for RFF again, this time concentrating on the urban environment. This low end of the meso-scale was trickier, and of course more political, because it had to do with the polluters and the polluted. RFF sent me to interview scientists at various labs, including some in the UK. Summertime: impossible to get three UK interviews lined up in the same week. The only way seemed to be to spend our vacation time in the UK too. Frances and the girls agreed, and so we five spent 6 weeks touring the UK from northeast Scotland to southwest England, managing to be at specific places in time for my pre-arranged interviews. I actually managed to get some work done during this extended UK holiday, and published the results as an AES (Canada) report, "The Atmospheric Environment of Cities". RFF published both my



papers in a small book entitled "The Atmospheric Environment" in 1973, and I was promoted to full professor on my meteorology work.

I had become established at York University as a professor of environmental physics, a meso-scale meteorologist and the local expert on climate change. Meteorology asked, if the climate is changing, can the energy from the sun be assumed to be constant? Physics answered that, aside from temporary effects of surface phenomena like sunspots, solar heat emission can only change very slowly. Even if energy generation in the sun's interior were to change suddenly, gravitational collapse of this huge gaseous body would have to occur before the news got out to its light- (and heat-) emitting outer surface, so before solar radiation felt by the earth would change. This so-called Kelvin-Helmholz collapse time had been estimated to be about 30 million years. Meteorology wanted to know if the sun's surface was gradually warming or gradually cooling but the K-H collapse process kept the rate of change too slow to measure. Was the earth heading for an ice age, or for a warm epoch?

The particle physicist in me knew a way to monitor the solar energy production on a much shorter time scale. If the standard model of the sun's energy production is correct, the sun's energy is released by fusion of its hydrogen nuclei (protons) into Deuterium nuclei and eventually Helium nuclei. A neutrino is emitted every time a fusion event occurs. Passing freely through the sun's enormous massive body (as little neutrinos do) and travelling at the speed of light (as they also do), it would travel as far as the earth in about 500 seconds. That should keep us pretty up to date. However, there was a problem: A Brookhaven group under Ray Davis had been unable to detect enough neutrinos from the sun's interior to account for the sun's present brilliance (and heat emission). Had the sun's internal fires already slowed down but the resultant next ice-age hadn't quite begun on earth yet?

My particle physics ears perked up immediately.

Chapter 4. VERY Large-Scale Experiments.

The solar neutrino crisis of the early 1970s had torn my attention away from Meteorology back to Particle Physics. Models of industrial climate change assumed the sun's brilliance to be constant, but in 1970 Particle Physicists had a solar neutrino problem. They had not been able to detect a large enough flux of solar neutrinos: not enough to account for sufficient solar interior energy production to maintain a surface brilliance anything like that presently observed.

Ever the optimist, I gave 3 invited talks on solar neutrinos: one at York, one at AECL Chalk River, and one at Carleton University (which was looking for a new professor of particle physics). However, re-entering particle physics was not as easy as getting out of it. Meteorology may have received an interesting contribution from a particle physicist, but particle physics was not looking for any contribution from a meteorologist. Moreover, I had never done any particle physics in Canada, or even in association with a Canadian group: I was just another outsider trying to horn in on their rather small research budget.

My first attempts were distracting but hardly exciting, and grant applications were an immediate problem. NRC (there was no NSERC yet in 1974) was not enthusiastic about starting a particle physics group at York University, so I had to start small. One or two of York's theorists were somewhat interested, but I would have no experimental particle physics colleagues at all. I began by analysing data from my last experiment at Argonne and published the results, with my former fellow Case Institute colleagues as co-authors. Then, remembering my success at the ISABELLE study a dozen years earlier, I participated in a summer study called POPAE (Protons on Protons And Electrons) at the new National Accelerator Lab (aka Fermilab) west of Chicago. I submitted a few reports, but this time I got no reaction at all: definitely a tighter funding environment than 1963.

I then joined Professor Tony Key at UofT (University of Toronto) to help with experiments involving the production of K-mesons during electron-proton collisions at Stanford's Linear Accelerator Center (SLAC) in California. Nice location and technically challenging, but I didn't find the physics very stimulating. Technically demanding because it was early days for controlling (OK, at least monitoring) an experiment with a "mini" computer on-line: in this case, Data General's Nova 920, a 16-bit wizard, with a row of settable (to 0 or 1, of course) toggle switches and a "load" button on the front panel. You could instruct its CPU by keying in (and loading, one 16-bit word at a time) a small program which instructed its processor to read the contents of a small tape into its tiny memory. This in turn told it how to read a program from a larger tape deck, which then told it how to monitor the experiment and record its data. All this took about 30 minutes following a power cut (frequent: this was California in summertime). The centre of this experiment was a 1-metre liquid hydrogen bubble chamber: track sensitive, and chock-full of target protons. And a LOT of chemical energy - so no smoking please!

A bubble chamber? Why use old-fashioned 20-year-old technology! You will recall that bubble chambers were not very good for observing rare processes: we would have to look at a semi-infinite number of photographs before we saw an interesting event. Boring! (And costly.) However, SLAC's chamber was a new departure: a rapid-cycling bubble chamber, expanding (to become track-sensitive) and recompressing again about 10 times a second (not a pop every few seconds now, but a dull roar). So, 36,000 pictures per hour? What a mess! No, our camera's flash was only triggered (and then, its film advanced by one frame) if our external detection system demanded it. We had to quickly detect some external suspicious evidence and then trigger the flash. We would only generate a trigger pulse if our external array of scintillation counters (aided by a huge directional Cerenkov counter and electronic wire-chambers) found evidence of an interesting event. (Rapid bubble growth only gave this external array part of a millisecond to make up its mind.) Our triggered flash allowed us to use the bubble chamber (a tracking device very revealing of detail, but otherwise logistically cumbersome) to observe the production and decay of rare particles.

Bubbles in a bubble chamber grow to the right size for photography in about a millisecond, so we had hundreds of microseconds to decide whether to trigger the flash. This could not be done in our (then) new "on-line" Nova 920 "minicomputer”. Decisions and subsequent production of a trigger pulse were instead accomplished by fast external logic units, basically elaborations of the "and-gate" type logic units commonly used by sub-atomic physicists since the '40s (yes, even in my MSc thesis). Basically, forming a special-purpose computer, this rack-full of separate fast electronics units could read out the detectors in a few microseconds and easily produce a trigger pulse within a millisecond. Now you can see why I was welcome to join this well-funded and rather cushy experiment in Sunny California: my UofT colleagues needed an expert in something other than managing a room full of film-scanning personnel.

The data-taking phase took a good part of a year, but then the experiment got to be quite dull, standard bubble chamber physics: yes, an enriched data sample because of the smart flash trigger, but still the room full of scanning and measuring machines, essentially a management project. Ho hum! And then.....

More Neutrinos

In 1977 I met some old friends from BNL days, Linc Read (Fermilab), Lou Hand (Cornell) and Gene Engels (Pittsburgh) planning an experiment involving neutrinos: not solar neutrinos, but very high energy neutrinos, the aim being to use a neutrino's energy to observe the decay of the newly discovered and highly unstable B-quark. The plan was to produce a B-quark out of pure energy in the tiny fireball at the centre of a collision of a high energy neutrino with a stationary proton. A beam of neutrinos would be provided by Fermilab's high energy Tevatron accelerator, and the target protons would be contained in the nuclei of the elements contained in packages containing many litres of undeveloped photographic emulsion (the sticky, light-sensitive stuff normally only found on the back of undeveloped photographic film). Why that??? Well, just as in bubble chambers, charged particles leave invisible tracks in photo emulsion, which, after development, become visible as little rows of silver grains. However, it takes a high-powered optical microscope to see them, as they are very small, about 1 micron [1/1000 of a millimeter] in diameter. Just what we needed because the particle containing the B-quark had an expected lifetime of about 1 picosecond, during which time the decaying particle would travel about 0.2 mm (200 microns. The unstable particles we sought were not electrically charged but would soon decay into two charged particles. So, you look for a big star of several particles leaving the collision centre, but with a "neutral V" suddenly appearing about 200 microns downstream from the star: two new tracks diverge from the point where the B-quark decayed, and then continue travelling with the other particles from the star. You don't see the neutral B-quark itself: it has no electric charge and so leaves no track. Then a "V" suddenly appears when the B decays into two charged particles, one positive, and one negative. In a bubble chamber this would have seemed to take place immediately (within the first bubble): all the "neutral Vs" previously observed in bubble chambers had been from the decay of much more stable, longer-lived particles. The much closer look provided by our "nuclear" emulsions would allow us to prove the existence of our new less stable "neutral V" particles. And to measure their lifetimes? No, actually just how far they travelled during their lives. To get the lifetimes we had to also determine their speed or their energy. How would we do that?

The instrumentation I had developed for our Case experiments with much lower energy particles at ANL was becoming known as tracking calorimetry, and this calorimetry was becoming a favoured method for following the trajectory of a high energy particle, identifying it, and then measuring its energy. As you may recall, a tracking calorimeter requires layers of energy absorbing material interspersed by layers of particle detection, and relies on the tendency of high energy particles, electrically neutral as well as charged, to create a shower of lower energy particles, which eventually stop, giving up their energy to the layers of absorber. Electrons and high energy photons (from pi-zero decays for example) will shower well in thin (2 or 3 mm) layers of lead, while hadrons (protons, mesons, neutrons, etc.) will shower more coarsely in thick layers (50 mm) of iron. The trajectory of the particle that initiated the shower is shown by the shower's position and its energy by the shower's extent. Muons are very electron-like, but 200 times more massive: they don't shower at all but can continue on through a metre of iron plates, just leaving a single track all the way.

Because of the huge momentum of the incoming neutrino, all the particles from the collision move in the same general direction, in a narrow forward cone. This allowed us to perform two tasks: Task 1 to accurately measure the tracks of the product particles close to the stack, so we could extrapolate their trajectories back to help find the event "vertex" in the emulsion stack; and Task 2 to capture these product particles much further downstream in our tracking calorimeter, so they could be separately identified and have their energies measured. That second task became mine, and especially the identification any high energy forward going muons from the original collision, or from the subsequent decay of the B-quark. Of particular interest in those days would be to see two muons in one event, one from the "star" itself and one from the decay of the neutral V.

Our proposal P537 to Fermilab was approved, and we became experiment E537. The experiment's layout is shown schematically below.

Task 1 required surmounting the huge mismatch between the space-time of the calorimeter (macro space but micro time) to the space-time of the emulsion target (micro space but macro time). The spatially huge calorimeter's response time was essentially immediate: within a millisecond capturing calorimeter information from a particular event and storing it on tape and photographic film. On the other hand, the tiny star buried deep in the emulsion stack would remain hidden there until the end of the experiment (many weeks later). Only then would the emulsion be removed to a special darkroom and developed. Then the hunt for an expected vertex star could begin: a needle in haystack problem; each event vertex occupying a cubic millimetre, and a hundred or so of them scattered through 15 litres (i.e. 15 million cubic mm) of our emulsion target. Once found, a star then had to be examined by microscope, including precise mapping of the trajectories of the outgoing particles, including the pair of particles resulting from the decay of the neutral V. Task 1 was undertaken by my Cornell and Pittsburg colleagues, using small gold-plated glass plate spark chambers placed immediately downstream of the emulsion target. An intermediate task was also undertaken by my Task 1 colleagues: the target and gold-plated chambers were followed by a system to measure the momentum of charged particles (like the muons) using a large aperture magnet and large optical spark chambers.

For Task 2, I made a calorimeter in which the entering particles first encountered thin (3mm) lead plates, just like my little calorimeter at Argonne, but with much larger lateral dimensions (3m x 1.5m). These were followed by 5cm iron plates and eventually 10cm iron plates: 2.5 tonnes of lead and 55 tonnes of iron altogether. To reduce the cost and complication of detection between 57 layers of plates I developed "flashtube" chambers (based on an idea originally conceived by Conversi et al. at U. Rome in the early 70s). Instead of the Rome group's delicate arrays of glass tubes, I used elements of corrugated plastic called "coroplast", used for packing material and real estate signs, essentially large sheets of black plastic tubes.

These were filled with neon/helium gas and energized by 10 kilovolt pulses (from a 30 KV power supply). Their resulting flash was photographed from the end of their "tubes" through their plexiglass gas manifold end caps: like a spark chamber, only coarser (and far cheaper). A view of the gas manifold windows on a pair of my chambers is shown here (left) and construction of the large 1.5x3 m^3 chambers in Petrie basement at York U.(right).

Because there were 57 layers of flashtube chambers, 57 individual 3-metre-long strip mirrors had to be mounted and carefully aligned overhead so that a single camera could see all the way down through the 57 thin 1.5-metre-deep chamber modules all at once, to watch the whole event unfold (in part of a microsecond).

Here's the apparatus, from the published Proceedings of the "Neutrino '79" conference in Bergen, Norway, where I had been invited to give a paper on early results from our experiment. The photos underneath show 3 events, each with a muon going right through the calorimeter, several electrons and photons showering in the thin lead plate part, one event showing a charged particle (proton or meson) showering in the 5 cm iron plate region. The calorimeter is working as planned.

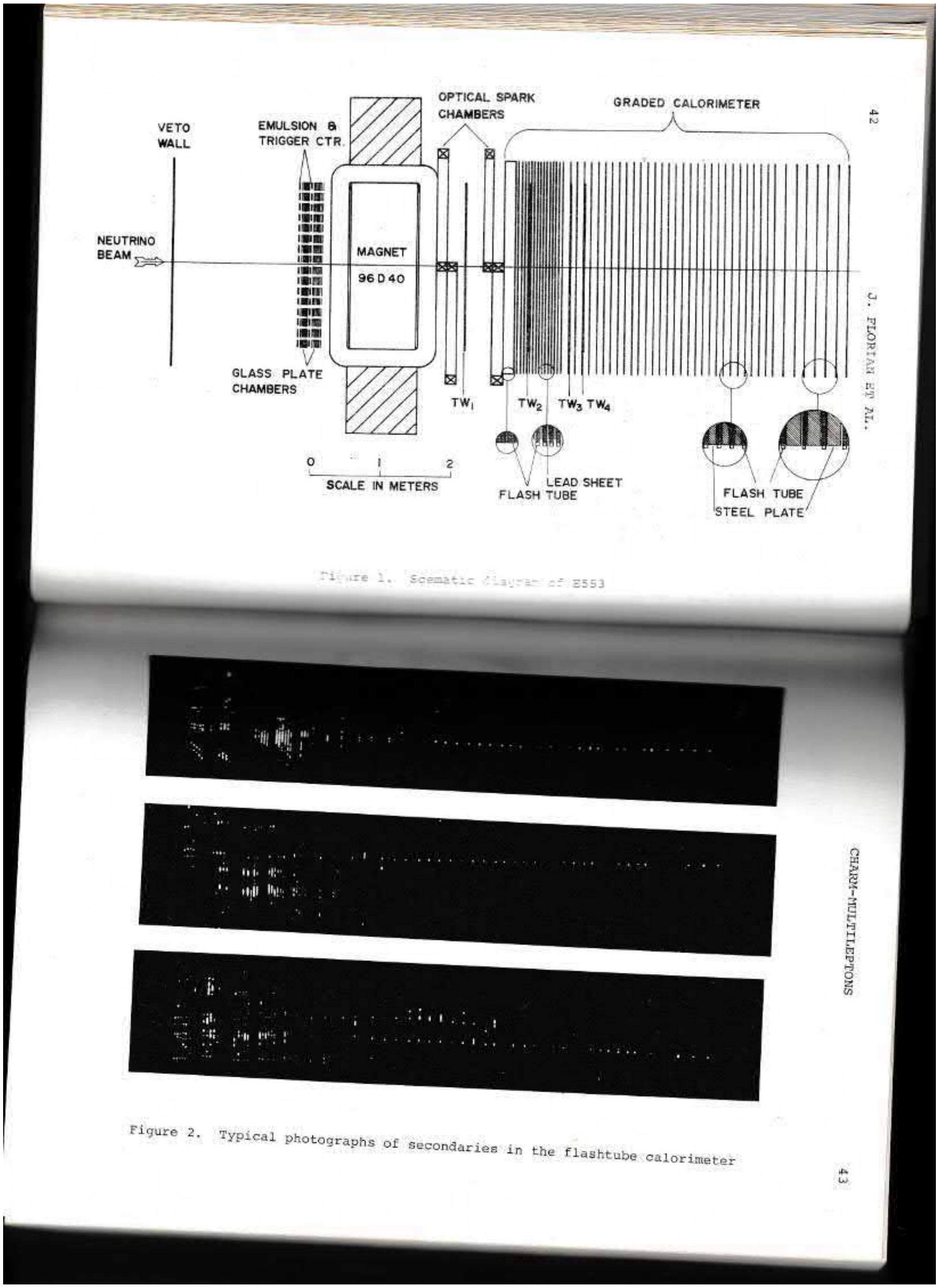
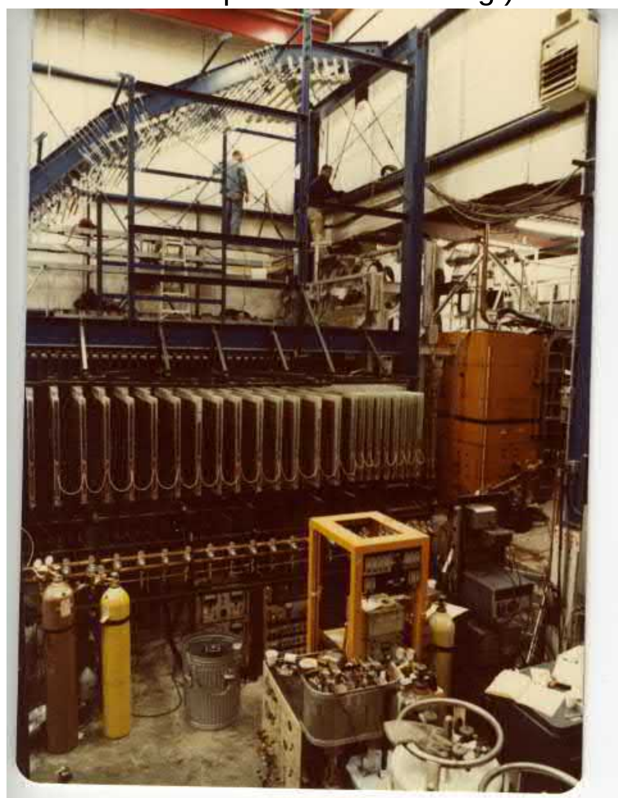


Figure 1. Schematic diagram of E553

Figure 2. Typical photographs of secondaries in the flashtube calorimeter

Each planar chamber of flashtubes had to be given a 10-kilovolt electric pulse within the first microsecond after the event had occurred, before its little ionization trails had faded. This was done by discharging "strip-lines"; patterns of copper etched on both sides of 3ft by 4ft panels of circuit board material (aka "G-10"), one panel for each of the 57 chambers. Each strip-line had to maintain a ready charge of 20 KV, so each pair of strip-line panels was maintained in a nitrogen atmosphere, encased in a lucite box.

They dominate the foreground in the following photographs, the chambers themselves being behind them. The curved array of strip mirrors is visible overhead, each strip mirror training its chamber's image on an almost visible camera. (The room is in total darkness when the experiment is running.)



The etched copper strip lines and the long mirrors we assembled at Fermilab. The flashtube chambers were made at York in the Petrie Building basement and taken to Fermilab by rental truck. The York group at this time consisted of myself and three students who were physics graduates but not yet grad students: Warren Schappert, Ulf Mjoernmark, and Carl Hassanali. For the summer of 1979 we were joined at Fermilab by an undergrad Physics student from Queens, my daughter Barbara Frisken. (To avoid conflict of interest I paid only her travel expenses and a per diem for housing and food out of the research grant.) Warren later got his PhD at McGill and spent the rest of his career as staff scientist at Fermilab, Ulf returned to University of Lund, Carl got a PhD from York, and remained there as director of undergrad physics labs. Barbara went on to PhD in condensed matter physics at UBC, did a post-doc at Santa Barbara and is now professor of physics at Simon Fraser U in Burnaby, BC.

This experiment established me internationally as an expert in tracking calorimetry. After presenting our experiment's preliminary results at the "Neutrino

79" meeting in Bergen, I gave a colloquium about tracking calorimeters at the University of Rome (at Professor Conversi's invitation).

Unfortunately, the Task 1 subgroup was less successful: this part of our experiment (which told scanners where to focus their microscopes inside the emulsion stack to find each event's "star" of origin, followed closely by its "neutral V") ran into serious difficulties. The trajectories, energy measurements and particle identification information were ready and waiting from Task 2 for any event "star" found. We now know there were many events of this type buried forever in this huge mound of very expensive emulsion, but only two or three candidates were ever located, the first of which I presented at Neutrino 79 in Bergen.

We proposed a new improved version of Experiment 537 and then? Ah well, politics controls all forms of human activity, including fundamental science and of course, not necessarily for the better. The Institute of Particle Physics of Canada (IPP) had been simultaneously supporting a University of Toronto group in a competing but somewhat old-fashioned experiment, which mostly consisted simply of a large emulsion target. The rest of the running experiment was mostly tasked with monitoring the neutrino beam intensity. The real work was done "off-line". Their experiment was led by an established collaborator from Japan, whose budget employed a huge room full of women (in Japan) trained to painstakingly scan through the entire stack of developed emulsion looking for the tiny little stars manually, slowly adjusting the focal plane of their high magnification microscopes down through the "nuclear" emulsion, a few microns at a time. Although this process consumed a huge amount of person-power over more than a year, at this point in time their "volume scanning" technique proved to be a better way to attempt this measurement. The IPP wanted to improve the downstream part of this competing experiment, intending for me to abandon my Cornell/Pitsburg colleagues and join the competition at UofT. When I declined, my research funding was drastically cut, forcing me to withdraw from the E537 collaboration. I left my calorimeter behind. My erstwhile colleagues struggled on for a few months, but eventually gave up the quest.

CHEER for Canada

This sort of conflict between Canadian institutions had generated a strong sense within the IPP Council that Canadians should work together in larger groups, so they could provide a more significant part of one of these large international experiments (and maybe even provide improved relations with the host laboratory by building part of their particle accelerator facility). An IPP meeting was convened in late 1979 to consider possible experiments for such collaboration. Several of us were pushing for a collider experiment, and the meeting concluded with a vote to work together on an electron-proton collider experiment. IPP Scientist Richard Hemingway organised a 1980 summer study at Carleton University to study the feasibility of building a huge Canadian high energy electron ring (CHEER) at Fermilab, tangent to that lab's Tevatron proton ring to study head-on collisions of electrons with protons at very high energies.

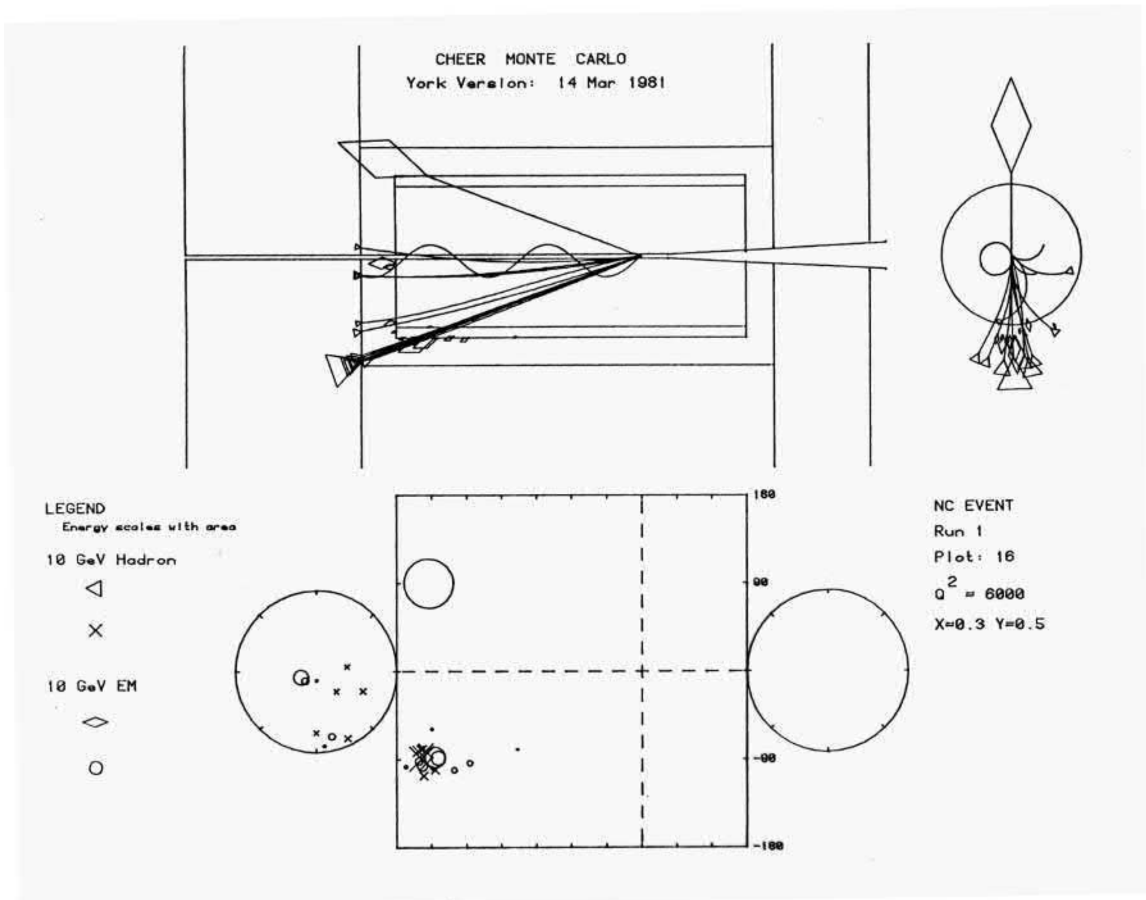
This project was just right for me: I had designed experimental equipment for the challenging geometry of collider experiments during the summer study at BNL in 1963. My last efforts in detector design for the proposed ISABELLE collider at BNL had led directly to my appointment at BNL, enabling my (first) return to particle physics in 1963. Now in 1980 scientists were coming from all over the world to participate in our exciting CHEER study. (I had also participated in the POPAE, Protons on Protons and Electrons, at Fermilab in the mid 1970s but that study was much smaller and less international.)

Studying particle collisions using a collider facility was a new game for most other particle physicists, accustomed as they were to focusing a beam of high energy particles onto a collection of stationary target particles. In such a "fixed target" experiment almost all the particles participating in and produced by the collision can be observed, but with a cost: the incident particle's enormous momentum carries all of them forward in a very narrow cone, from which they have to be sorted out and identified: often with great difficulty. On the other hand, in a collider experiment we would be examining head-on collisions of two incident particles originally moving in opposite directions. These higher impact collisions achieve much higher momentum transfers (and so get a closer look at the structure of the interacting particles), but there is a significant further advantage. If the momenta of the two particles coming into the collision are equal but opposite, the net momentum will be zero, so that the interaction's product particles will leave the collision center highly dispersed spatially and more available for individual analysis. Even if the initial momenta are not equal but still opposite, much of this advantage will remain. Of course, your detector instrumentation must have "4 pi" geometry, that is it must cover the entire sphere of possible exit directions, so it can get very massive, and hard to build and operate. Some assembly would be required (and some invention, too.)

Much of the resulting CHEER feasibility report treated the design and construction of the storage ring itself, but I was responsible for the report's chapter describing the elaborate detection system designed to fully study the results of these collisions. The Carleton group was heavily involved in a CERN experiment at that time, and when this conflict forced Richard to resign from CHEER in the fall of 1979, IPP appointed a formal "CHEER Committee" with me as chairman. Three of us presented our proposal to Fermilab, and two of us (UofT's theory Professor Nathan Isgur and I) continued on to present it at TRIUMF (Vancouver), BNL, and in Europe, as we searched for other institutions to collaborate in this enormous venture. Nathan and I presented the CHEER studies in Germany in the fall of 1980 at a collider workshop in Wuppertal chaired by Professor Volker Soergel, Director of the DESY (Deutsches Electronen Synchrotron) lab in Hamburg. Professor Soergel was impressed with what we had done and invited the Canadians to join in experiments with DESY's newly approved electron-proton collider, HERA (Hoch Energie Ring Anlage). He had been particularly impressed by "Monte Carlo" simulations of electron-proton collider events, generated by our simulation program. One simulated event is shown below. This program had been developed at York University with important contribution from a Swedish student whom I had employed for a year. Gunnar Ingelman had been a member of the Lund University group (collaborators on our failed neutrino experiment at Fermilab) and had found himself at loose ends in 1980. In later years Gunnar became famous for collider event simulation work at CERN and world-wide, eventually being appointed Professor at Upsala.

The forward-backward asymmetry in the CHEER simulation event below reveals that the incident proton entering from the right had 100 times the momentum of the incoming electron entering from the left. (In principle it would be better to have them equal, but it is much cheaper to accelerate a ring filled with protons to high energy than electrons.) So, in CHEER's 100 to 1 design, the electron is turned back towards where it came from, with even more kinetic energy than it began with, while most of the proton's fragments continue onwards. In a collider experiment the incident beams do not show up in the detector, because the vast majority of electrons and protons don't collide and must be allowed to remain in the collider's tubular high-vacuum chamber. Here they continue to circulate around the collider's huge storage ring, one revolution every 20 microseconds (for hours) to provide future collisions. In HERA a short bunch of protons passed through a short bunch of electrons (for about one nanosecond) every few microseconds, but most of these beam particles literally "pass each other in the dark". The DESY Accelerator Division was the world's expert on accelerating electrons to high energy, and HERA's momentum ratio of 30 to 1 produced events that were 3 times more experiment-friendly (more spread out) than our simulations had anticipated.

We displayed our simulated events interacting with a schematic detector consisting of a big tracking drift chamber surrounded by a thin (superconducting, of course) magnetic coil, so trajectories of charged particles are given a curvature inversely proportional to their momentum. The drift chamber is in turn surrounded by massive tracking calorimeters. As this diagram shows (because of the asymmetry in the initial energies of the colliding electrons and protons) the calorimeter was designed to be more massive in the "forward" or initial proton direction and less massive in the "backward" or initial electron direction. Our simulation’s schematic display shows the energy of a simulated particle as the size (area) of the symbol at the end of its track (a diamond in the case of electromagnetic particles like electrons or photons, and a triangle in the case of hadrons, like protons, neutrons or mesons). Neutral particles like photons and neutrons, leave no trace (of course) until they interact with the calorimeter material. The view from the end is also shown, and below we see what an engineer would call an "unfolded cylindrical development" of the calorimeter's inner surface: here the electromagnetic energy is shown in circles, and hadrons in crosses.



HERA - DESY - ZEUS (and ARGUS)

Nathan Isgur returned to Toronto, but I accompanied Professor Soergel back to Hamburg to be introduced to the DESY lab.

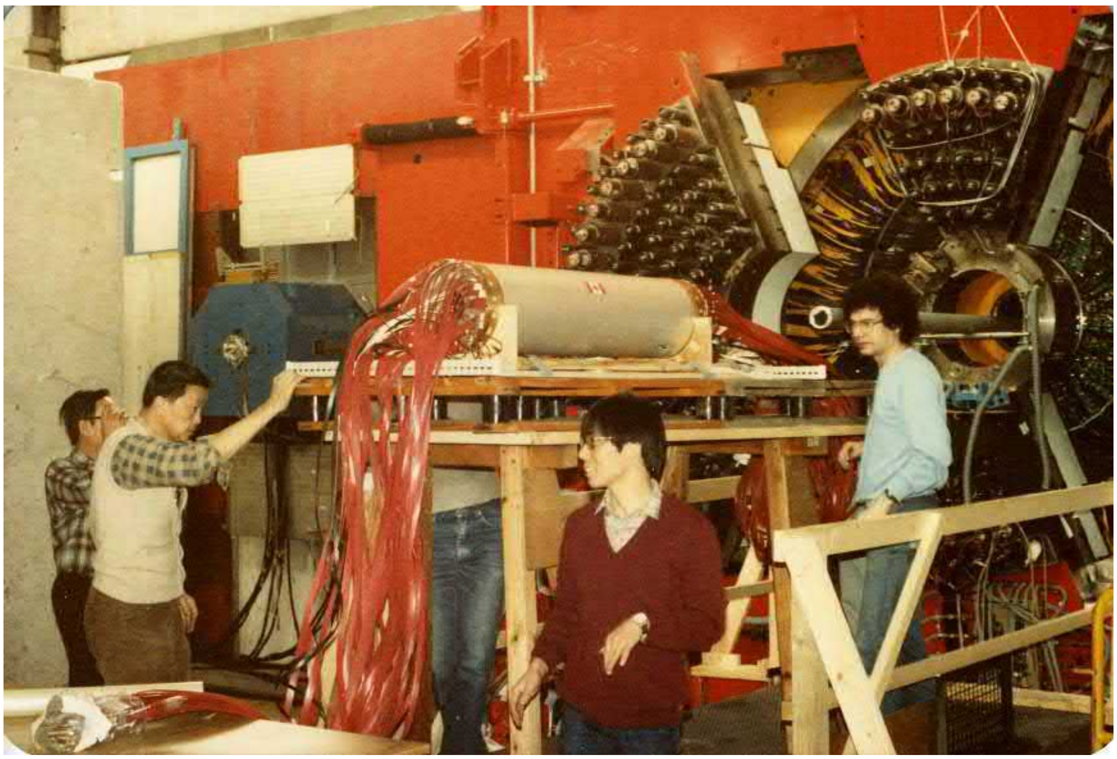
The operative word in Professor Soergel's invitation was "approved", i.e. approved by the German Government, and so actually being funded. Further, to get Canadians started on this long adventure, Soergel invited us to bring some Canadians to join the DESY's ARGUS experiment (electron-positron collisions at a much lower energy: to look at B-quark decays in great detail). ARGUS was already in the early stages of preparation, but needed more scientific, technical and financial support. NRC (and the soon to be created NSERC) reacted very positively, whereafter the Canadians always had a special place at DESY, because we were the first international group to join HERA. We even organised some parts for the HERA storage ring facility to be designed and built in Canada at the TRIUMF and AECL Chalk River laboratories.

Many Canadians joined ARGUS (from Carleton, McGill, UofT and York), and even more came later to DESY to help form the ZEUS collaboration, to design and build the enormous ZEUS detector to perform electron-proton collision experiments on DESY's emerging HERA collider. For ARGUS we agreed to build a vertex drift chamber (VDC) and to provide a computer, together with an on-line operating system which would not only record data while ARGUS was running but also to monitor the performance of its thousands of detector elements. For this purpose, we bought the new (this was early 1983) VAX 11-780 with huge whirling magnetic system-(and data-) disks (40 and 60 Megabytes respectively), each enclosed in a huge cylindrical tank similar to my grandmother's washing machine - except no hand pump handle on the side. Marvelous: people came from all over the DESY lab to see it. Its operating system came with a series of instruction manuals that occupied almost a metre of shelf space. Our Russian colleagues xeroxed every page and sent the copies back to Moscow. The VAX and its disks occupied a separate special air-conditioned (Klimat kontroliert) room in the experimental area.

The ARGUS VDC's role was to detect electrically charged particles leaving the interaction point with very high spatial precision (about 50 microns), while consisting of as little material as possible, so as let the particles pass on undisturbed into the surrounding layers of detector. Even the vertex chamber couldn't see the actual interaction point, because the collisions take place between colliding beams of particles circulating for hours inside the vacuum chamber of a huge storage ring collider. The vertex chamber is just the innermost layer of the 200-tonne ARGUS detector, all surrounding the 10cm diameter vacuum tube of the storage ring.

Our VDC allowed ARGUS to make a precision measurement of the mean lifetime of the newly discovered tau-lepton, a sibling of the electron and the muon, but more massive than the muon and therefore even more unstable. Its mean life was expected to be part of a picosecond, the time to travel a millimetre from the event vertex at light speed, so that taus would typically decay well inside the collider's 10 cm diameter beam vacuum chamber. One of the tau's decay modes was known to be into three charged pi mesons plus a neutrino (a typical event is shown below). The tau's point of decay would not be visible but could be deduced from three charged particle tracks found to be coming from a point in space displaced from the main event vertex by a millimetre or less. To achieve high lifetime precision we would require very high spatial precision.

Here is York physics graduate (BSc 1982) Paul Padley (in blue shirt, right, with the hair) helping his UofT PhD supervisor, Professor Taek-Soon Yoon (on the far side, left, partly hidden), and others load our beautiful little vertex drift chamber into ARGUS in 1984. In the foreground is Carleton PhD student J.C. Yun, and in the white vest Tony Kiang, UofT engineer. Also involved was W.R. Frisken, but at this moment the photographer.



Paul Padley had worked as a summer student with Gunnar Ingelman at York on our CHEER Monte Carlo program and had gone on to use it in his MSc thesis at UofT. He did his PhD research on ARGUS, eventually becoming Professor at Rice University, Houston.

Below, is Professor Soergel's "favourite picture" which hung for years in the executive conference room at DESY. It shows the ARGUS vertex chamber during construction, showing its hexagonal array of fine drift wires surrounding the central aluminum beam tube with its thin black carbon fibre centre-piece (photo by W.Frisken). Designed by Taek-Soon and me, built partly at York, partly at UofT and partly at Carleton (plus precision hole-drilling for precise drift-wire location done at NRC in Ottawa). A drift chamber's wires divide its space into small cells, whose filling gas (propane) is ionized by the passage of any fast charged particle. The electrically charged ion pairs drift quickly to the drift-wires. The drift time tells the computer the drift distance within 1/20 millimetre, and with 10 hits per particle track, we could locate the secondary tau decay vertex. The VDC is the small inner chamber in the schematic reconstruction below right. The large outer chamber (about 2 metres in diameter) measures the emerging particles' momenta (from magnetic curvature), but the small vertex chamber is essential to extrapolate the tracks several centimeters back to the event's invisible origin inside the vacuum chamber of the storage ring-collider facility.

306 ARGUS Collaboration / Physics Reports 276 (1996) 223–405

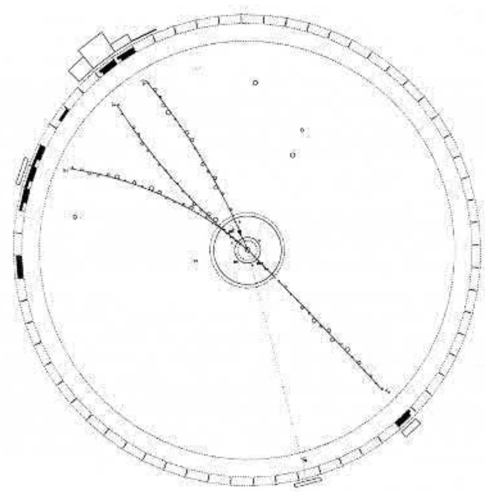


Fig. 4.1. A typical τ event with 1–3 topology produced in e^+e^- annihilation.

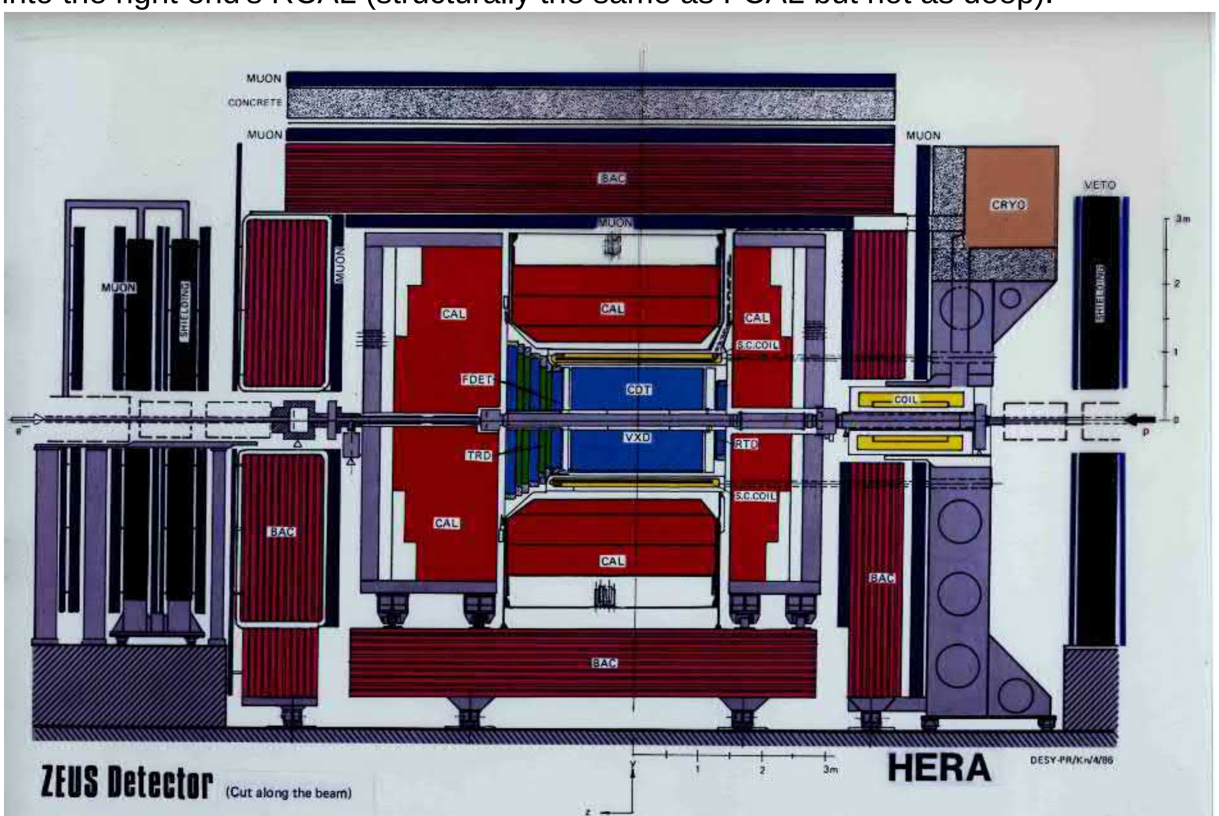
Many Canadians remained on ARGUS until the mid-nineties before moving to ZEUS, and the elegant ZEUS detector operated until 2007. Some of us moved more immediately to ZEUS in the mid-eighties and played an active role in its design and construction, with particular emphasis on its very elaborate tracking calorimeter. In transition, I spent my 1984-5 sabbatical year at DESY, still running shifts on the very active ARGUS, but with the design of ZEUS becoming my main focus.

What was the attraction? We were pursuing two kinds of particle physics, the discovery and examination of new particles, but also the probing of size and possible structure of particles already known. Most collisions of particles in high energy colliders are inelastic: part of the kinetic energy of the two colliding particles being absorbed in the production of new particles whose properties can then be studied by experiments. However, if the energy is high enough, the very occasional perfectly elastic collision (in which the two incident particles simply deflect each other's trajectory) can yield information about the size and structure of the colliding particles themselves.

The ZEUS Experiment

The HERA collider and the ZEUS experiment were designed to examine the proton's sub-structure and to look for possible structure of the electron. It provided a lot of detail about the former, and found that the electron, as many had expected, behaved like an electric charge at a dimensionless mathematical point. ZEUS eventually probed this hypothesis down to 1/2000 of the size of the proton. This and associated discovery of many new particles gave rise to hundreds of published results, and many PhD theses for Canadian graduate students during the ZEUS experiment's 16 year running life.

This schematic of the ZEUS detector shows the calorimeter (CAL) parts in bright red, with the proton beam entering from the right and the lower momentum electron beam from the left. The high momentum of the proton beam will carry most of the produced particles to the left into the deeper forward calorimeter (FCAL), but some into the cylindrical part (shown above and below) and a few into the right end's RCAL (structurally the same as FCAL but not as deep).



The organization of the Canadian Zeus group deserves some clarification. Beginning in the fall of 1980, Canadian e-p collider activity was led by me with important interaction with UofT theorist Nathan Isgur. Very early in the '80s, as the Canadian group began to grow, John Martin (IPP Research Scientist and Professor at UofT) became strongly involved. I recall he and I both attended a large collaboration meeting in northern Italy where our collaborators adopted the name ZEUS. Meanwhile, the associated ARGUS opportunity demanded a lot of my personal attention. I was the only professor of experimental particle physics at York, so in early 1983 the spokesmanship for the Canadian ZEUS collaborators passed to John Martin, while I focused my attention on the design and construction of the ARGUS vertex chamber (VDC). Two years later I spent my sabbatical year 1984-5 in residence at DESY. By then the VDC was up and running and, while still working a few shifts on ARGUS, I was able to switch my attention back to serious design studies with DESY's local ZEUS group. John Martin remained the spokesman for the Canadian ZEUS collaboration for the rest of its entire duration, but NSERC's flexible attitude to HEP funding allowed us to split the enormous cost of building the ZEUS calorimeter mainly between UofT and York.

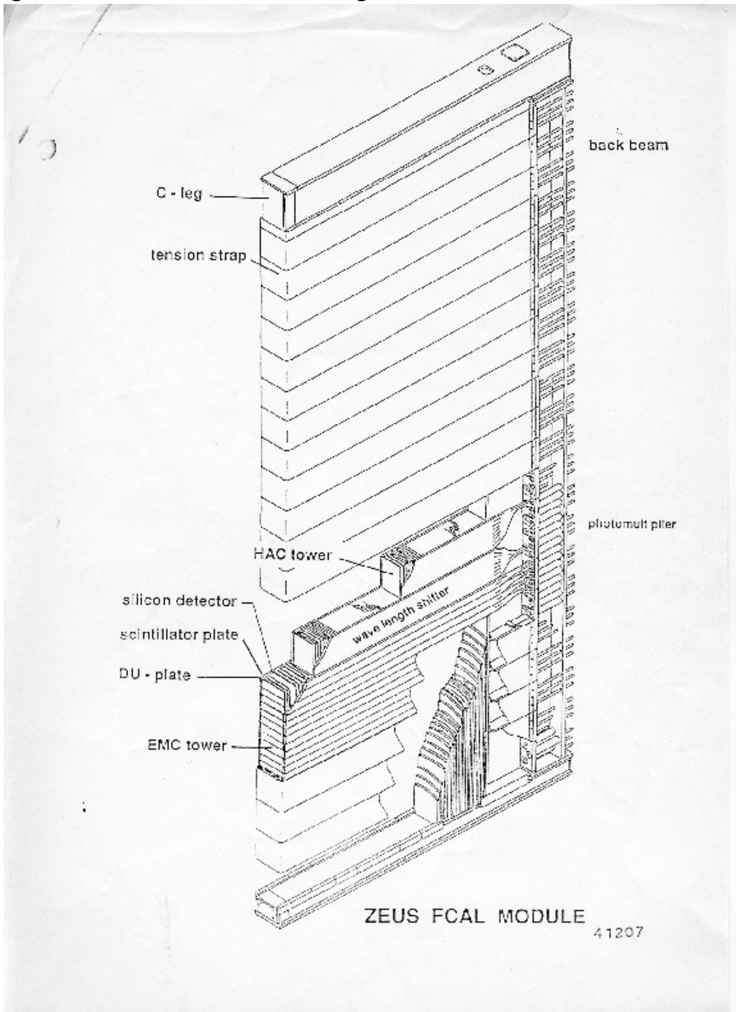
A major responsibility for John was the expensive long, thin DU plates (depleted uranium), precision-rolled and cut to width by MSC (Materials Sciences Corp) and sent to CRNL (Chalk River Nuclear lab) to be cut to length and clad in stainless steel foil before we handled them. MSC was near Oak Ridge National Lab in Tennessee, which was depleting the natural uranium by extracting its U^{235}. The main expense for me was leasing an empty 11,000 sq.ft. light industrial building in Markham ON for 3 years, plus hiring a large crew and buying the required materials and equipment to make it into a calorimeter factory. John and I worked closely together at the Markham factory for several years. At the centre of our effort were two very essential people, my senior Research Associate, Doug Hasell (PhD Manitoba and postdoc/TRIUMF) and John's mechanical (and digital) genius, the UofT HEP group's engineer, Gavin Stairs.

Since tracking calorimetry was my specialty, ZEUS calorimeter design had fallen into my lap, in particular the very sensitive forward FCAL and rear RCAL. Calorimeters for collider experiments must not only detect, locate and determine the energy of strongly interacting particles like protons and mesons, but also simultaneously do the same for electromagnetically interacting particles like electrons and photons. The detection layers of a calorimeter unfortunately respond much more vigorously to the elaborate showers of electrons than to the simpler showers of protons. From the very first, Doug, Gavin and I focused our attention on the emerging concept of "compensating calorimetry" as urged by our DESY/University of Hamburg Nuclear Physics colleague, Hanno Bruckmann. "Compensation" aimed to increase the complexity of the simpler showers of strongly interacting particles (protons and mesons) and thus make the detector yield the same immediate energy signal for protons as for electrons of the same energy. The aim was to avoid the expensive and logistically cumbersome alternative - detailed detection the showers, followed by their off-line examination and finally resulting in a large computed energy correction for each shower. A compensating calorimeter would consist of alternate layers of thin DU (depleted uranium) plates and plastic scintillator plates. A high energy pion or proton would produce a small shower of similar but lower energy particles, just as they did iron calorimeters. But a compensating calorimeter's absorber plates are uranium instead of iron, so primary and secondary shower particles interacting with a uranium nucleus also release many low energy neutrons, which in turn, as they traverse the plastic scintillator, produce knock-on protons which enhance the shower's light flash in the scintillator. So, just get the scintillator/DU plate thickness ratio right, and it compensates!

Not quite. The operating experiment's calorimeter would be made in many modules, each comprising several tonnes of thin plates of DU and plastic scintillator, the plates not lying flat but standing up on their ends. Each module had to remain in a very stable configuration for many years, from its first assembly until the end of its operating lifetime. It even had to withstand the shocks of shipping by land and sea, first to CERN (Geneva) for calibration and then to DESY (Hamburg) for installation. But that was not all. Each half of ZEUS's huge calorimeter would have to be trundled away from the central storage ring beam pipe about a metre for a few minutes at least once a day during the experiment's whole lifetime! Otherwise, the flare of radiation during the daily refilling HERA's electron storage ring would damage to the calorimeter's scintillator. To improve the stack's stability, competing proponents of compensating calorimetry were planning to pin the stack to a substantial steel back-frame by a regular array of many steel rods, each of which had to penetrate the entire DU/scintillator stack from front to back. However, the presence of these rods would produce unfortunate dead spots in the detector and severely limit its compensation advantage.

Doug Hasell, Gavin Stairs and I solved this problem with a design which had no rods at all! It used the strength of the DU plates by standing them on their ends, clamped for precise lateral stability to the strong backbone of a massive steel C-frame by a stretched skin of stainless-steel foil straps. We convinced our Canadian colleagues of the wisdom of this design, but first a design war had to be waged with our Dutch ZEUS collaborators, who wanted to design and build the calorimeter at NIKHEF in Amsterdam. After much discussion the collaboration decided in our favour, but the NIKHEF group remained an important part of ZEUS and eventually built half of the 52 huge FCAL/RCAL modules at NIKHEF, but to our design. These Dutch heroes also played an essential role in the testing and calibration of calorimeter modules at CERN.

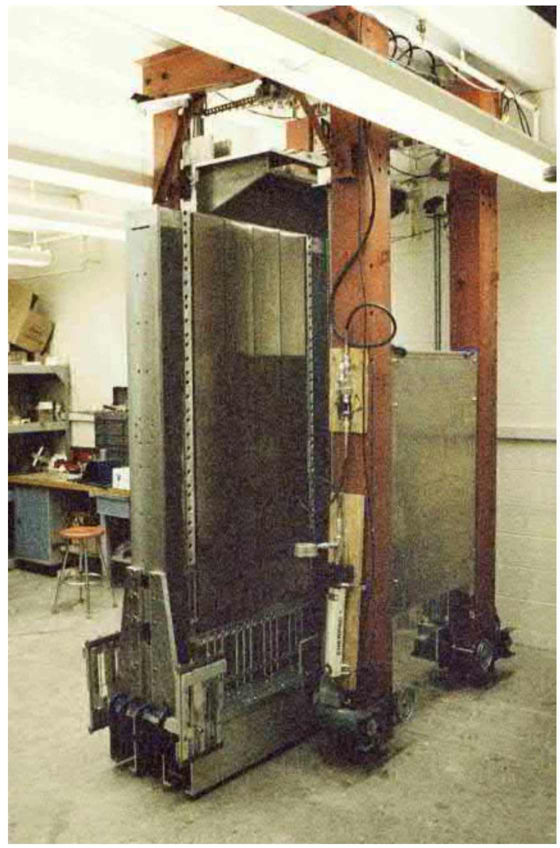
Below is a schematic representation of a full-sized module in its operating position with plates vertical and standing on the lower C-leg. Note that during the construction process, a module lies with the plates horizontal, flat on its back-beam until the stainless-steel foil straps have been installed and properly tensioned. Once installed in ZEUS, it spends the many years of its operating life standing on one of its steel C-leg beams.



Some explanation might help. The 3.3 mm thick by 20 cm wide and 5 meter long DU plates ran from top to bottom, but the light collection system ran horizontally, logistically dividing the module into a vertical stack of 22 long horizontal "towers" of high energy particle energy measurement, each tower 20 cm square by 185 cm deep. The light collection logistics further divided each tower into 6 sub-towers, each of which was read out from each side for a total of 12 photomultiplier tubes per tower in the central region: thousands of phototubes altogether.

As part of the proof of this pudding, we designed and built a prototype of this calorimeter and shipped it to CERN (Geneva) for testing. NSERC agreed to fund this small beginning, and we were assigned a room in York University's Petrie Science basement, immediately adjacent to its large machine shop. (Look familiar? This was the same room in which we had built the plastic flashtube chambers for E537 - see page 33 above). However, this ambitious project demanded much more person power at York than one lone professor and his

Senior Research Associate. As the prototype work began in earnest, we quickly added two Research Assistants (Cathy Farrow, BSc Western, and David Mosscrop, MSc Manitoba) and 4 technicians (fresh out of community college technology courses: Sergio Iaccino, Angel Cerato, Rod Abarca and Peter Greenwood). The UofT group's engineer, Gavin Stairs, was importantly involved.

The left-hand photo above shows the prototype stacker in the background (with Chris Youngman, visiting from DESY, and me in the foreground). The right-hand one shows the first of 4 prototype modules rolling out of the stacker.

In the photo pair below, the top one shows that same module out in the corridor on the crane (at last) and ready for packing and then shipping to CERN for testing. But how did we get this 2.5-ton module horizontal? What about installation of all the light-readout technology: light-guides, phototubes etc? For this delicate part of the process, Gavin had invented the "flipper". The lower photo (viewed through the stacker) shows our first prototype module in the grips of the flipper. With the flipper holding the module horizontal, one strap could be removed at a time, that tower's thin light guides installed and then its strap could be replaced.

This 2 1/2 ton module was tiny compared to the full-sized modules we were planning (see p.57).

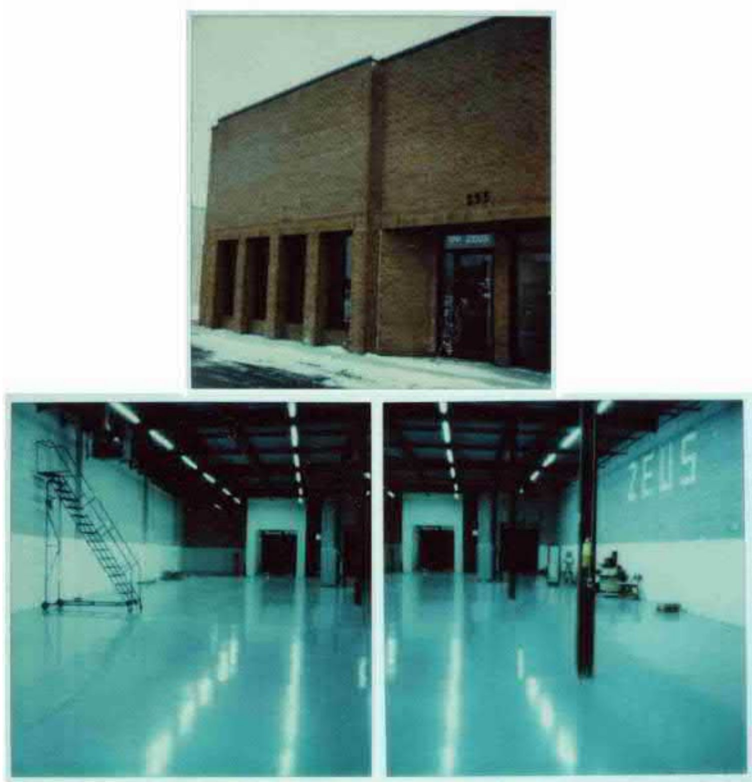
During the summer and fall of 1987 our new technical crew, often assisted by Petrie machine shop foreman Frank Jarvis and his merry men, produced the complete set of 4 identical prototype modules. The modules were shipped to CERN, where the assembled prototype calorimeter performed up to our wildest expectations.

Then we started in on the real "heavy hunting": designing and building 200 tons of delicate instrumentation, and shipping each module first to CERN for calibration, and then on to Hamburg, where the assembled ZEUS expeiment was destined to operate successfully for 17 years. You may have noticed that the structural elements in the above prototype stacker machine itself looked somewhat clumsy, clearly heavy and very much over-designed. Four heavy 10-inch square steel columns, supporting two huge 2.5" diameter 10 ft high steel Thomson bearings for precision vertical stack alignment. This was because our prototype stacker was intended to be simply enlarged longitudinally for stacking the much heavier full-sized modules, as soon as approval (and funding) would be in place.

To move from our successful prototype to the real ZEUS calorimeter I arranged that York University lease 11,000 ft^2 of light industrial space in Markham, Ontario, where we built our calorimeter factory. As we arrived, the site was completely empty. It had a high ceiling, but no overhead crane, so we were starting from scratch. A lot of work had yet to be done.

As we moved our crew to our new Markham site, our chief engineer Gavin Stairs was joined by his colleagues from the UofT particle physics group, IPP RS/Professor John Martin, senior RA Gary Levman, engineer Tony Kiang and grad students Burk Burrow and Milos Brkic. Our Markham factory began as a large bare floor, with two loading bays at the rear and some front office space for desks and drafting tables. We had to make 26 modules of calorimeter, the heaviest of which would weigh 14 metric tonnes, and ship them by land and sea, first to CERN (Geneva) for testing/calibration and then to Hamburg for installation. This was only half of the forward/rear calorimeter, the rest being built by Dutch colleagues at the NIKHEF lab in Amsterdam. Part of the job for Gavin and me was to make sure both halves were the same, so we both made frequent visits to Amsterdam during the next three years.

IPP Canada ad hoc site in Markham for calorimeter factory.

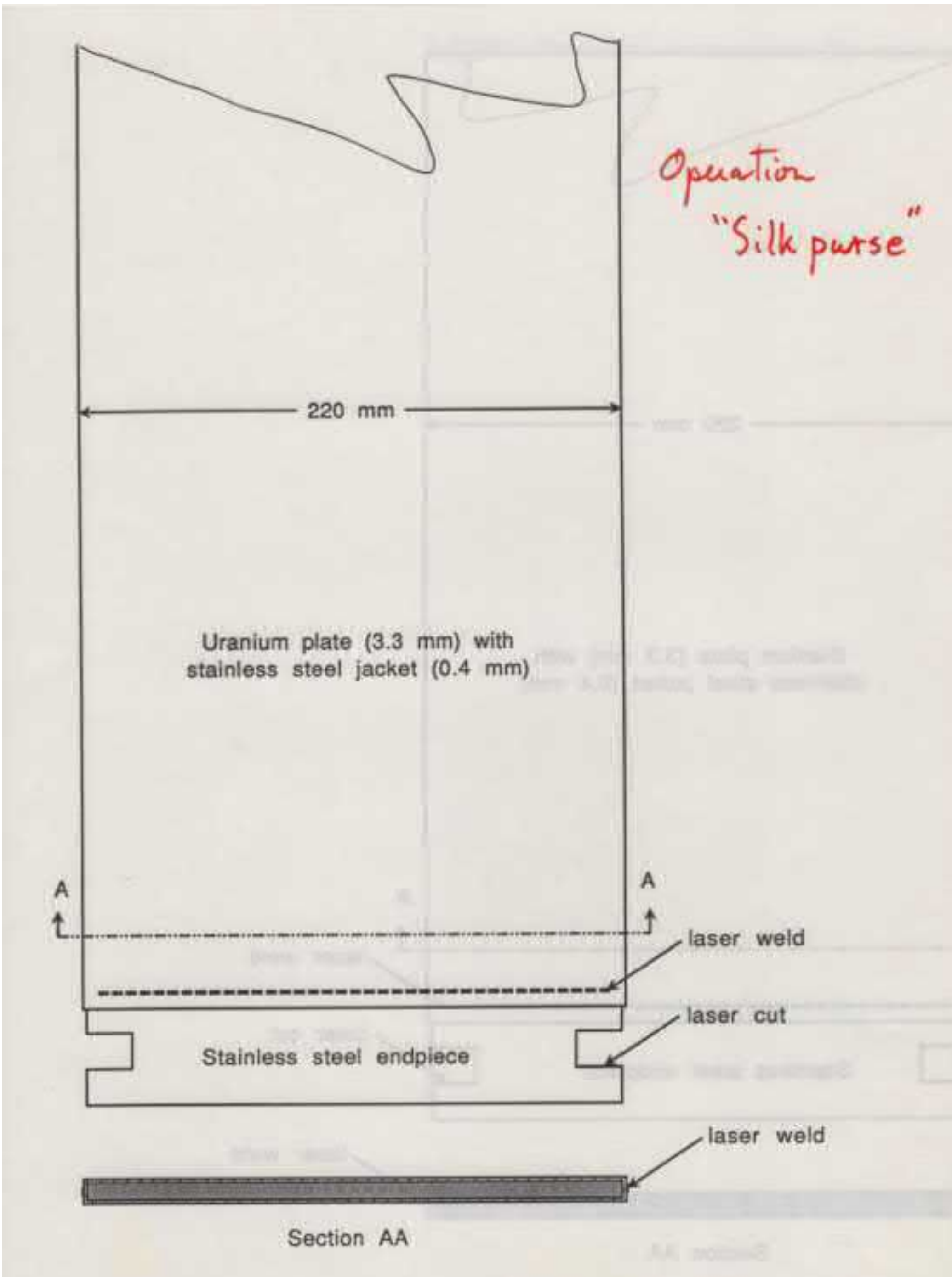


November 1987. Bare hall. No crane. First ourchase = 15 foot rolling ladder.
Second purchase = paint and brushes.

The rented site at Markham was just bare space. First, we bought floor and wall paint and a rolling ladder, but then we had to make a crane. Our chief engineer (Gavin) had been working with particle physicists for about 4 years. He could design and build anything. He had cut his teeth on building computers, but for us he also had to design heavy steel bridge structures as well as electronic computer control gear. Gavin first taught the young techs the theory and practice of welding and then had them build a heavy mobile crane. For our effective performance in the international calorimeter design wars, he also taught them to make their drawings of module design conform to European engineering conventions. As you will see, the equipment we built was then painted either yellow or dark red.

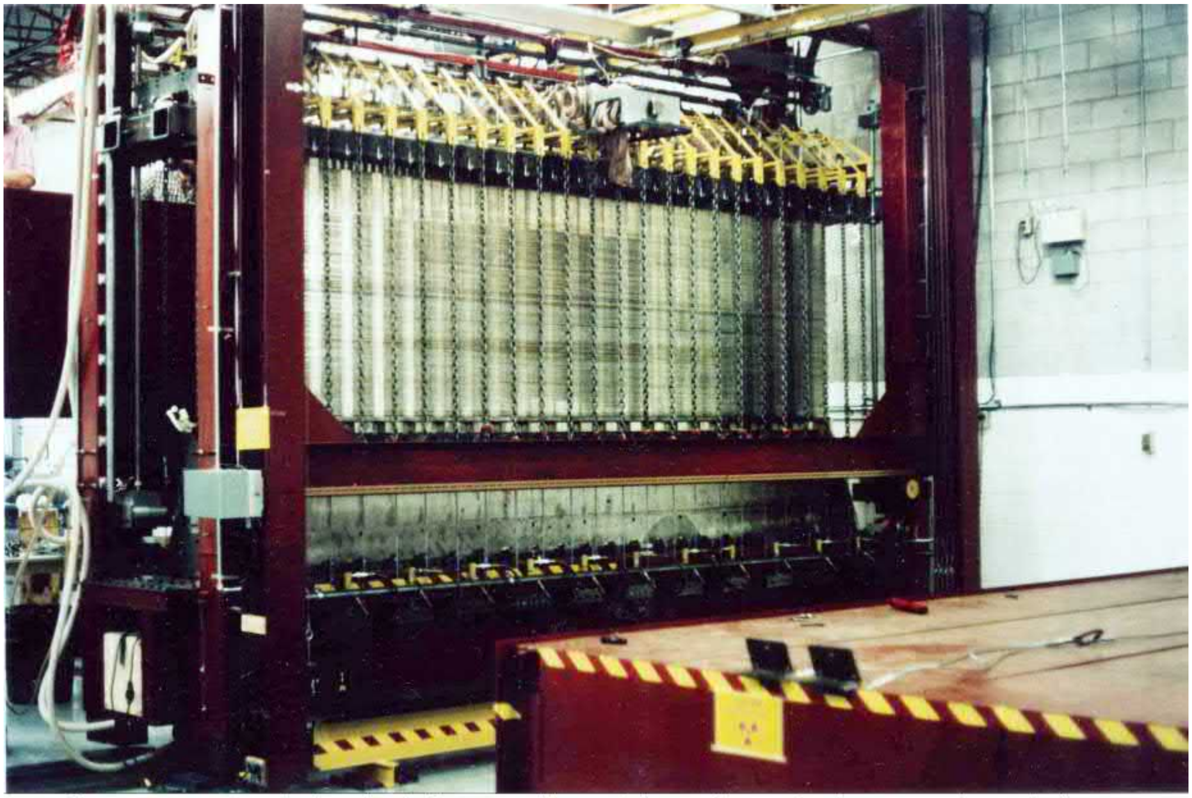
Next, we constructed 2 quality assurance presses to measure and record thickness variations in the delivered DU plates. One press was shorter and simpler, to be shipped to MSC in Tennesee to check the 10-foot long bare plates as they emerged from their rolling mill. We used the longer one at our Markham factory to make a detailed thickness variation inventory of the final plates after being laser end-welded, cut to length and clad in stainless steel foil by CRNL. The photo shows both QA presses just after construction at our factory, with our factory's longer one under test and the shorter one ready for shipment to MSC.

The importance of CRNL's adherance to specifications of plate construction is revealed in the following schematic, showing the assembly and subsequent laser machining of a plate. Both our stacker and our QA device first positioned a plate precisely by the endpiece notches and then determined the DU thickness at the 46 spacer locations.



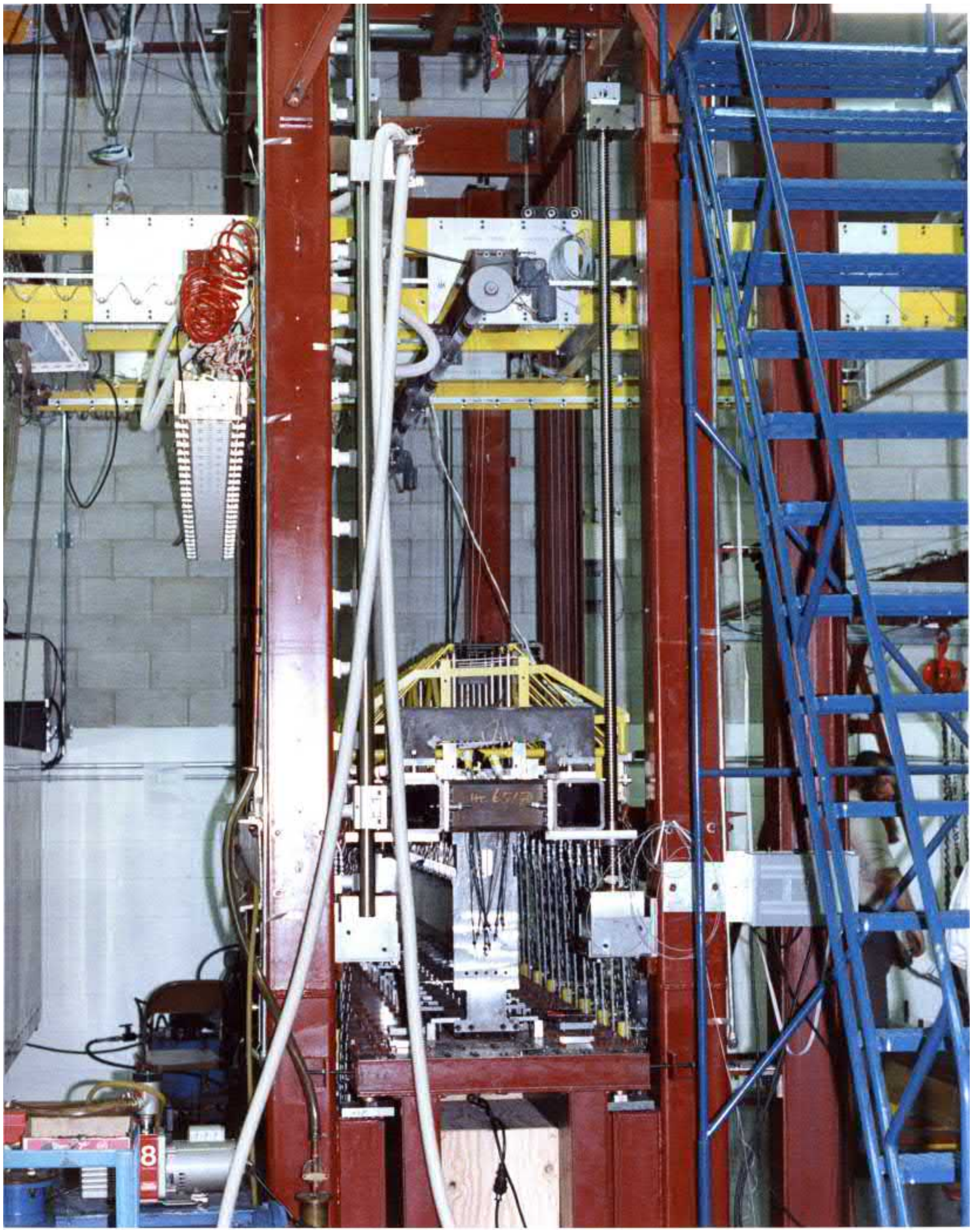
Here is the factory almost ready. Its layout (photographer perched on the rolling ladder placed near one of the site's rear exit bays) shows the stacker under construction in the far distance, with the QA press in the foreground and the large steel DU plate storage box in between. So, the sequence: delivery of DU through the bay behind the photographer, QA, storage, then module assembly. The left-hand side of the factory will soon be occupied by stations for installing the optical readout system and finally packing into the module's special (still to be invented) shipping frame (see p.59).

Then onwards! Shown below is the full-sized calorimeter module assembly press. This huge device, under computer control, is shown completing assembly of the 185-layer sandwich of depleted uranium (DU) plates and plastic scintillator elements, precisely aligned so that the calorimeter's electronically read-out light signal would tell not only how extensive (and so, how energetic) a shower was, but also indicate where that shower's initiating high energy particle had entered the calorimeter. The stack was to be held together by its stretched skin of stainless-steel strips: stretched tightly enough to withstand transatlantic shipping to CERN and land transport to DESY. The compressive stress was passed from plate to plate by small carbide spacers, to avoid stressing the scintillator. The photo shows a finished module fully compressed, just before installation of the its stainless-steel straps.



The computer controlled stacker built at our ad hoc calorimeter factory. In the compression mode (note chains) every 15 layers. Spacers were adjusted to keep the stack dimensions within specifications, so as to fit wavelength shifter assemblies. John Martin and Burk Burrow in the shielded floating command bridge.

Before that 185-layer stack could be started, slight surface variations of the bare steel back-beam had to be determined. Below, an end-view shows our computer-controlled stacker in its compression mode, measuring the initial height of all points on the 20cm grid, exactly where carbide spacers will be placed. The carbide spacers will hold the 20cm square tiles of scintillator in place, and when the stainless straps are finally tensioned, will keep the compressive force off the scintillator tiles. To keep stack height variations within specifications the operators would make compression-mode stack height measurements every 15 layers during assembly of the module and modify the carbide spacer thickness for the next 15 layers appropriately. Thickness precision was important, so our stack would precisely fit the array of thin light-read-out elements to be fitted on the side of the module, just under its stretched skin and in optical contact with the scintillator plates. These read-out arrays were ready and waiting, already constructed and delivered by our European colleagues: precision fit was our responsibility.



The large yellow frame near the top of the stacker was our computer-controlled genie, which could deliver three wishes. Its central part could supply the compression frame as shown. The left-hand part could pick up (by many little vacuum ports) a complete array of 22 scintillator tiles and the accompanying 46 carbide spacers from a precision-milled table (not shown), trundle them over to the central stacker position and lower them exactly into place on the top layer. The right-hand part could reach down and vacuum lift the next clad DU plate, trundle to centre, a lower it exactly into position. The two Thomson bearings (on the left column, one at each end) clearly played an essential alignment role.

To hold the stretched stainless-steel straps firmly for many years, Gavin invented what I used to call the SQN ("Sine qua non"), a small bracket that was the secret to holding (and changing) the strips of stretched stainless steel skin that held the module together during all its stressful life.

The next photo shows UofT grad student Burk Burrow preparing the back beam for our second 14 tonne module, installing the many SQNs that will hold the module's straps firmly in place.

Just in time too. At the far end of the factory floor the first module rides out of the press on its compressed air-pad sliding system. Look Ma, no crane! We had bought a large (but cheap) broken-down air compressor, whose fractured connecting rod was copied and rebuilt by Frank Jarvis et al in York's Petrie Science workshop. The air-pad sliding system allowed us to move finished modules around the factory floor for instrumentation installation and shipping.

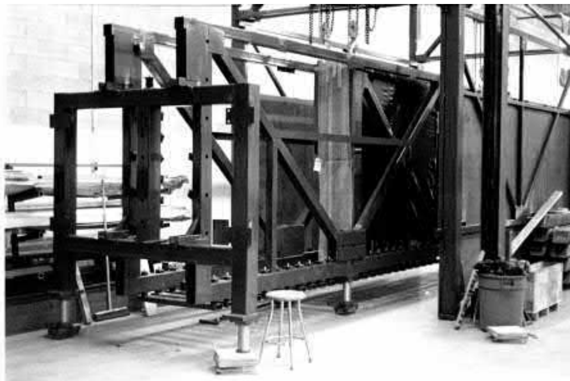
First ZEUS FCAL Module, with crew. Doug Hasell (York) missing (photographer, see inset).

David Hanna McGill, Tony Kiang Toronto, Gavin Stairs Toronto, Rob Snihur York, Bill Frisken York, Rod Abarca York, Sergio Iaccino York, Peter Greenwood York, Cathy Farrow York, Burk Burrow Toronto, John Martin Toronto, Helen Fawcett York, Kerry York, Angel Cerrato York

That first module was a cause for celebration, and here we see all available members of the crew assembled (some missing - see p 31). The SQN brackets are just out of sight behind the crew: 24 on each side. The photographer, Doug Hasell (inset, upper right) managed the site whenever I was called away to lecture at York (10 Km west), and John Martin, IPP RS and Professor from UofT, was almost always there, directing and inspiring the UofT contingent, and in particular driving the car-less Gavin out to our rather remote location almost every day.

The module was designed to be stable with only half of the stretched skin’s stainless strips in place, to allow installation (and later, service) of the scintillation light readout system. Below, John Martin shows our tech Sergio how to install this optical readout system. The plywood carrier delivers the thin assembly of wavelength shifter panels. The whole module had to be made light tight, to avoid stray ambient light contamination of the scintillation signal, so the next step will be to cover the gaps between the stainless strips with bands of aluminum tape covered by black photographic tape (see p.60).

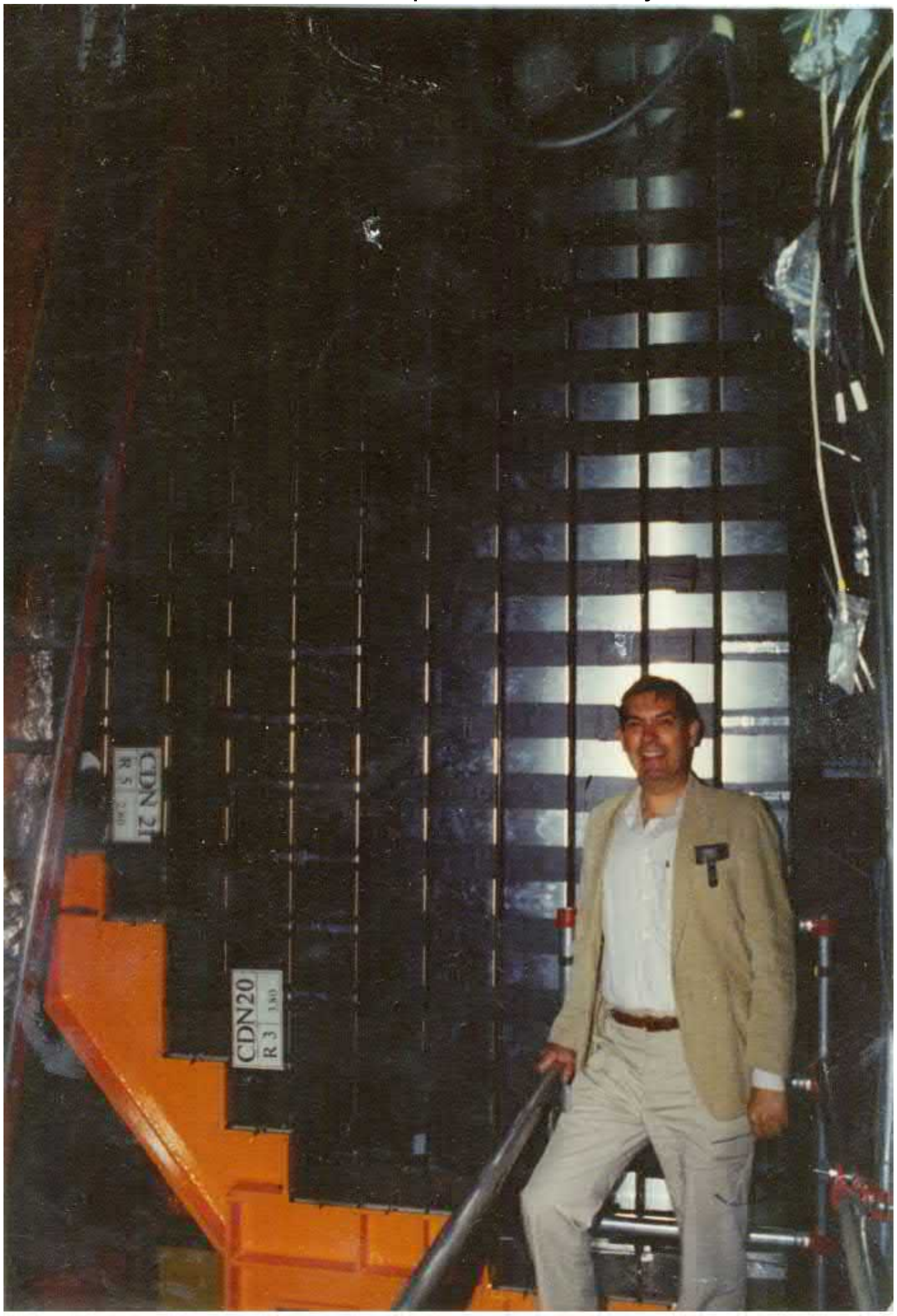
One 14-tonne module would have a tarpaulin-covered 16-foot-long shipping container all to itself and to protect our module against all the shocks of land/sea transport we designed a custom-made shipping frame. This massive frame had up-down/sideways elastomer-spring "railway bumpers", of which the up-down cylindrical bumpers can be seen in the left-hand photo below. Fortunately, these up-down legs had a draw-down feature, so we could collapse the tarp to allow exit through our loading bay door onto the waiting truck.




That first module was shipped to CERN for testing and calibration in spring of 1989 and the last (and smallest) of our 26 modules was shipped in October 1990.This photo of the last module (the smallest of our 26) includes three more familiar faces: front row in blue stiped shirt, Garry Levman (UofT senior postdoc), sitting behind Garry is UofT grad student Milos Brkic (with hair) and at our left behind him, McGill grad student Laurel Sinclair.

My only photo of the assembled FCAL at DESY has someone (me) standing in the way one year later in October 1991. Incidentally, this photo records that the

Canadian and Dutch modules were placed alternately.

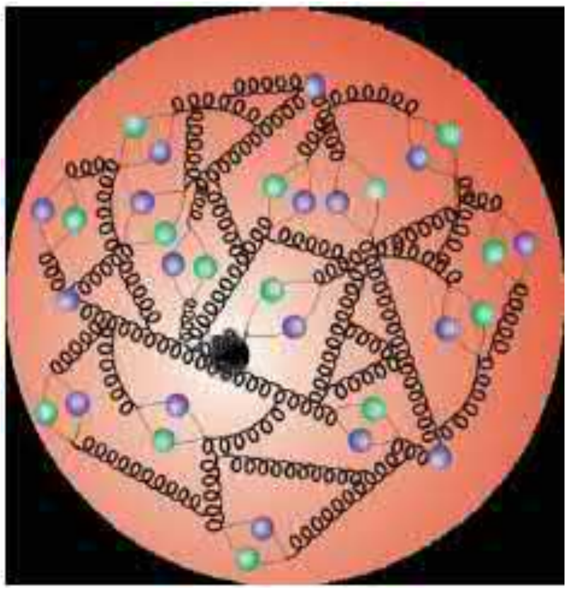


August 1991. Half of the RCAL/FCAL modules were made in NIKHEF, half in our Markham factory.

Calorimeter module construction was finished in 1990, and that calorimeter ran continuously until 2007, with several hundred tonnes of that delicate stuff being trundelled aside once or twice a day to avoid radiation damage (to the scintillator) during the regular electron storage ring refill process.

A most important result from the ZEUS e-p scattering experiments was the observation that both electrons and quarks showed no spatial extent down to the limit of our spatial resolution: their little radii were less than a billionth of a billionth of a metre (0.4x10**-18 metres), about 1/2000 of the known size of a proton. They appear to be just mass concentrated at a mathematical point. These results began to emerge at startup in the early 1990s and have been continually further refined ever since by further data collection (up to final de-commissioning in 2007) and even now by further ongoing analysis.

2009 model of the proton



Beware of distortion! Quarks have been magnified x40 relative to the proton so you can see them!

Fortunately during this period my teaching responsibilities had been limited to small upper division and grad courses, which required very little administration, and were mostly in areas where I needed very little preparation: classical mechanics (Goldstein), electricity and magnetism (Jackson), and of course nuclear and particle physics. Then as the ZEUS experiment got assembled and ready to run in its data-taking mode, York acquired a second professor of experimental particle physics (Sampa Bhadra) and even some graduate students. Our department began to change rapidly and as I write (2021) now has several active professors of experimental particle physics.

A good technical discussion of the construction and performance of our modules is available in a presentation I made to the Symposium on Detector Research and Development for the Superconducting Super Collider held in Fort Worth Texas in October 1990 https://inspirehep.net/conferences/967297 . What supercollider, you might ask. Read on, but first here is my front page.

FULL-SIZED MODULES FOR THE ZEUS HIGH RESOLUTION FORWARD AND REAR CALORIMETERS

Presented by Bill Frisken, York University
for the ZEUS Calorimeter Group

Abstract

Production of the ZEUS high resolution forward and rear calorimeter (FCAL and RCAL) modules was undertaken in the Netherlands and in Canada. The Barrel Calorimeter is of the same conceptual design, but will not be discussed here. Most of the massive modules were shipped directly to DESY, but several went first to CERN for beam calibration tests. This paper discusses special features of the design and construction technique, and test beam performance.

Introduction

ZEUS is an electron-quark collider experiment for HERA. It must measure deep inelastic scattering events both for neutral current and charged current processes. The latter have a scattered neutrino in the final state, and must be measured by the quark jet alone. Other physics measurements accessible at HERA require detection of missing transverse energy, and/or extra electrons within the quark jet. The ZEUS calorimeter is designed to have the best possible capability for the measurement of jets, with a particular view to hermeticity, energy resolution, angular resolution and hadron electron separation. The design process proceeded hand in hand with an vigorous testbeam program.[1,2]

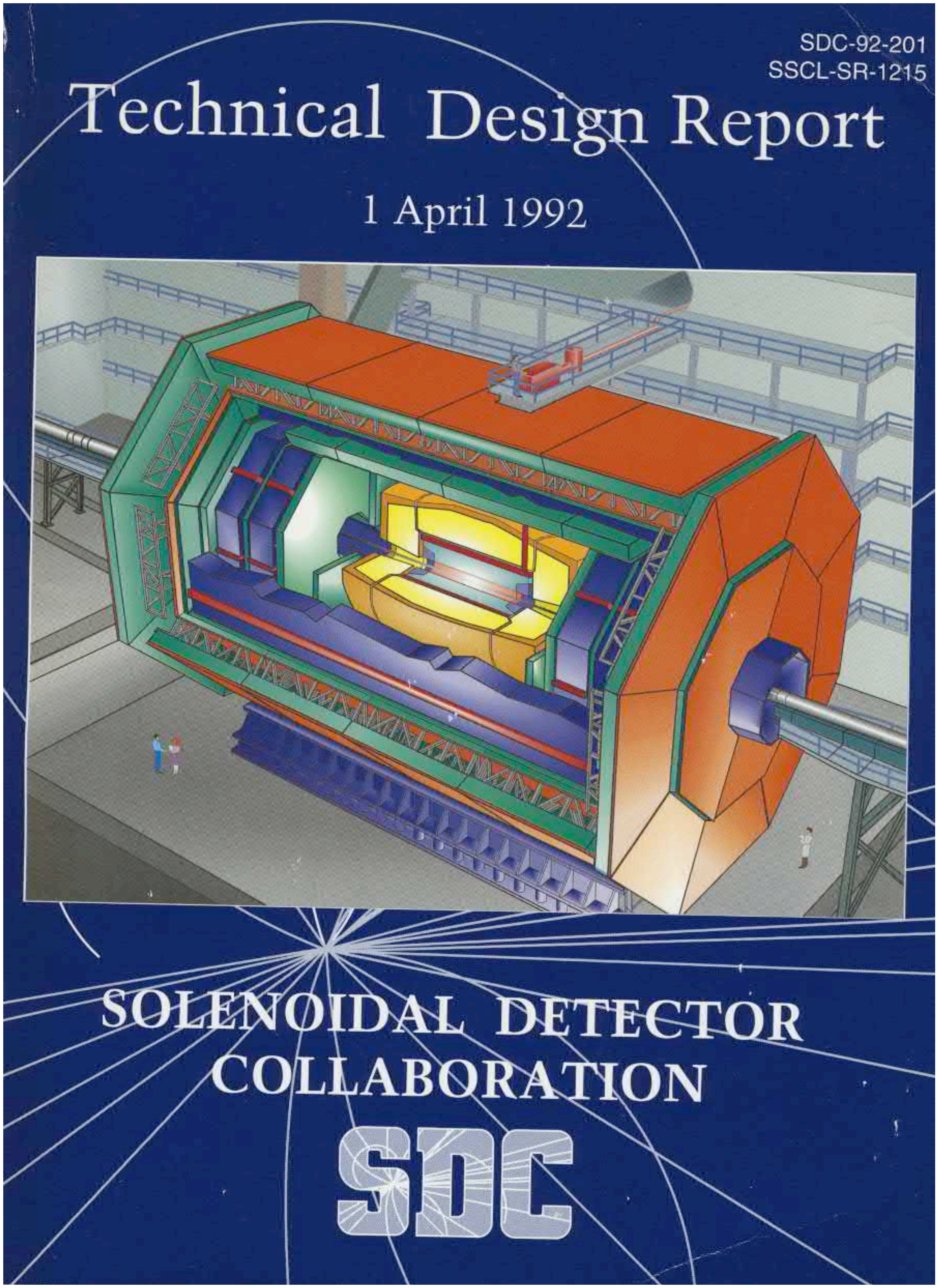
Design Features of the ZEUS FCAL/RCAL

Energy Resolution. In tests with preprototype Uranium-Scintillator calorimeters, the ZEUS Calorimeter Group had obtained hadron energy resolution equal to 35%/sqrt{E} using 3.3mm thick (1 radiation length) plates of Uranium, sampled by 2.6mm thick tiles of SCSN-38 plastic scintillator (see reference 2 and previous work cited therein). This layer modularity became the basis of the ZEUS design.
Angular Resolution. FCAL/RCAL hadronic calorimeter (HAC) towers are 20cm square, transverse to the beam direction, and the electromagnetic (EMC) towers are 5cm high by 20cm wide (10x20 in RCAL). The towers are read out by wavelength shifter plates (WLS) at both sides, which allows the EMC shower centroids to be located to within 5x5cm^2. The front plate of FCAL is located at 2.2m from the interaction point and that of RCAL at 1.5m.
Prototypes: Four prototype modules, each comprising four full HAC towers and 16 EMC towers, were designed and assembled at York University, and shipped to CERN late in 1987. Some of the parts and components were provided by other members of the ZEUS collaboration. These modules followed the intended final design very closely, both physically, and mechanically. The resulting four module array of sixteen complete HAC towers and 64 EMC towers has been subjected to detailed test beam investigation at CERN for more than two years.[1,2] The excellent performance of these prototypes not only proved that the basic layer modularity was correct, but also gave us a detailed prediction of the behaviour to be expected of the ZEUS calorimeter. This led to changes in the design of the final modules (lightguides changed from clear PMMA to wavelength shifter material, Uranium plate spacers in the EMC changed from WC to the lower Z material, TiC), and to the introduction of a 3mm sheet of Pb between modules when assembling them to form the ZEUS calorimeter.
It taught us a great deal about fabrication logistics, and it also proved that the unusual mechanical design could withstand the rigors and hazards of transatlantic shipping.
Hermeticity: The hermeticity requirement was concerned with loss of energy due to particles escaping through gaps in the calorimeter, loss of energy due to jet punchthrough, and also loss of energy deposited in inert structural members of the calorimeter.
-Minimal Intermodule Cracks. The number of boundaries is minimised by making the modules large, typically comprising 20 supertowers (20x20cm^2). They were also required to be geometrically precise, so that they fit well together, and to be strong enough and stiff enough that this precision survives installation into ZEUS. The tower geometry is intentionally rectangular rather than pointing, and our design allows us to have the most forward intermodule crack aimed just 50mr from the interaction point.
-Punchthrough. The calorimeter is deep enough (7 lambda deep in the FCAL) for 95% containment for 90% of the showers. Events with excessive punchthrough are detected by a backing calorimeter formed by instrumenting the iron of the magnet return yoke, which surrounds the ZEUS calorimeter.
-Inert structural elements are kept outside the active volume of the calorimeter. A stretched steel skin binds the module to the outside steel structure.
Hadron Electron Separation: The sampling structure of the ZEUS calorimeter is uniform in depth, and consists of 185 one radiation length layers for the deepest FCAL modules. Hadron electron separation to the 1% level is achieved by segmenting the readout longitudinally into a 25 layer EMC, followed by two Hadronic Calorimeter segments (HAC1 and HAC2) of up to 80 layers per section. The separating power is enhanced by two orders of magnitude through the introduction of two special layers near shower max in the EMC (only one in RCAL) tiled with an array of 3x3cm^2 Silicon Diodes in addition to their normal scintillators.
Built-in Calibration/Monitoring: Uranium-Scintillator calorimeters can be self calibrated, and self monitored by the

SSC - Superconducting Super Collider (what supercollider?)

My responsibilities for ZEUS had changed as the project entered its data analysis phase, featuring scores of post docs and grad students staring at their computer screens, running their "bump hunting" software as they searched for indications of new physics, basically anything that wasn't expected. That left me with two options: I could lead such an analysis group working in a particular direction like using ZEUS data to look for substructure of electron and quark, or to search for signs of new physics (e.g. the Higgs boson, the top quark, or some indication of Supersymmetry); or I could use my established position in the field to help particle physics build an even more incisive collider than HERA. For a while I worked on the quest for electron-quark substructure, but since I was not supervising a graduate student at the time, I was easily distracted by what promised to be the next step in the investigation of this "inner space": a large proton-proton collider called the SSC (Superconducting Super Collider) to be built near Dallas, Texas. The US government seemed agreeable, and an international collaboration quickly formed to design a detector that would be a huge exaggeration of ZEUS. This group, SDC (Solenoidal Detector Collaboration), formed under the leadership of Berkeley's Professor George Trilling, with notable input from Europe, Japan and a small Canadian group. I was made chair of SDC's Calorimeter group (comprising several international collaborators) which began the design of a massive part of this multi-story, hundred-thousand-ton detector. To facilitate this work, I spent my 1991-2 sabbatical at Berkeley, much of it on the airplane between Oakland and Dallas airports. Frances' and my sabbaticals were in sync, and she had colleagues at Berkeley too. We rented a lovely house on Grizzly peak, whose high balcony's view overlooked the Bay, with Alcatraz front and centre, in line with the Bay bridge: Telegraph Hill on the left and Mt. Tamalpais on the right.

SDC's 600-odd page report (cover shown below) in April 1992 showed the advanced state SDC design by the time we were heading home to Toronto. Look familiar? Note the scale though, the scientists standing on the floor beside the detector look like ants! That "little" yellow part was my group's contribution. The full report is available in my study for detailed examination by the curious.



The SSC was to have been an incredibly large hadron collider, and we were all convinced that any lower energy would never accomplish our aim of finding and studying the properties of basic structure entities called Bosons: you could find the bosons with less energy, but you couldn't find out much about them. A competitor proposal was raised at CERN (Geneva) and dubbed "Large Hadron Collider", but not as large as SSC and providing a much lower collision energy. At the time we all thought the CERN people were wasting their time, but in 1994 our project was finally scuppered, an event triggered in part by the aspirations of one (mid-west) American group, which challenged (you might say "Trumped") the design of the muon identification part of the detector by the Japanese group. The Michigan people pulled out all the political stops, and finally got their way, but when the insulted Japanese then withdrew from the SDC collaboration, Japan also removed their county's support for the construction of the SSC itself. Washington politics then closed in and cancelled the whole project, including the half-built SSC accelerator-collider. There had already been a lot of tension in Washington DC because so much was being invested in Texas. The withdrawal of the Japanese collaborators was the last straw, and the new President (Bill Clinton, a westerner: but Arkansas, not Texas) was able to get rid of this large Bush footprint speedily, but in the process completely wasting the $2 billion already spent on its construction.

This was an enormous blow to the American particle physics community, forcing many out of the field, and many out of research entirely. Some SDC collaborators opted to switch to CERN's "Large" Hadron Collider, but I felt its aspirations a bit beyond its capability. The basic problem was their compromise of choosing a lower energy for the colliding particles in order to keep down the cost, particularly the cost of the large amount of land required. Given a technical limit to the strength of magnetic field that could be used to turn the particle beams, the radius of curvature attainable will scale with the energy, and so the area (and cost) of land required with the square of the energy; twice the energy means 4 times the cost. If you want to make a pair of particles of mass 200 GeV in a collision of two protons of equal energy, travelling in opposite directions (so the event has a stationary centre of mass), you just need a bit more than 400 GeV of collision energy. You can achieve this "threshold energy" by providing each of the colliding protons with a little more than 200 GeV, so who needs more? Well, in the first place, with 400 GeV of collider energy you are making lots of other stuff, maybe very interesting a few years ago, but that now you already know about. Yesterday's physics has become today's background as usual: all that stuff keeps your counters, fast digital logic, computers etcetera too occupied to be bothered with the very rare events you seek. However, the occurrence rate of the events you seek just above the "threshold" will increase rapidly as you provide more energy, because more energy is available for the motion (the "phase space") of the produced particles. The LHC proponents figured they would be able to find and identify the Higg's Boson with 10 TeV of collision energy which they eventually managed to push to 13 TeV. The SSC proponents were aiming at 40 TeV of collision energy.

Chapter 5. Superconductivity, the Next Linear Collider and/or then...?

Why a linear collider? Well, to have interesting particle collisions, first you have to accelerate the particles to a high enough energy. A linear collider "simply" accelerates the particles in a single pass through a long straight vacuum pipe consisting of an end-to-end array of many accelerating cavities. If each 1 metre -long cavity can give a passing particle 25 MeV of energy, then to accelerate to 100 GeV you need 4000 cavities end-to-end for your very long (4 kilometres) accelerator.

The other choice is a storage-ring collider, which accelerates the particles by many repeated passes through just a few accelerating cavities spaced around a large circular ring vacuum pipe, the pipe at any point only a few centimetres in cross-section, but the ring a kilometer or more in diameter. A huge ring of rather small pipe. The accelerated particles would have to pass around the ring 1000 times, but you might only need four accelerating cavities, the particles gaining 100 MeV with every revolution. However the strength of the magnets required to keep the particles on this huge circular orbit (and inside the vacuum tube), as their momentum gradually increases, must be increased in synchronism with their energy gain, and such circular accelerators are called synchrotrons. When the particles have achieved the required energy, the magnetic field strength is leveled off and held constant, and the task of the "cavities" becomes just to restore any energy the stored particles may lose as they circulate around the entire ring time after time after time, every few hundredths of a millisecond all day and night.

Great. So why not just build a ring collider?

The physics of producing new particles out of pure energy in a collision is less cluttered if the colliding particles are themselves simpler. Electrons are much simpler (more elementary, Watson) than protons, and so electron-electron collisions would be easier to understand than proton-proton collisions. However, electrons present a problem when accelerated and stored in a ring collider because they are less massive: to have the same kinetic energy, an electron must travel much faster. Since it must travel much closer to the light speed than a heavier particle like a proton, when its path is curved it radiates a lot more "bremsstrahlung" (high energy x-rays) due to the centripetal acceleration involved by its continually-changing velocity direction. As the synchrotron raises the electron's energy, eventually it gets to the point where it is losing (radiating) energy as fast as the cavities can provide it. To study electron-electron collisions at interesting energies (high enough to produce Higg's Boson for example) we will need a linear accelerator. Instead of a synchrotron ring a few kilometers in diameter in which a few RF cavities accelerate the same circulating electron bunches time after time, we need a VERY LONG straight-line vacuum tube containing thousands of accelerating cavities, a linear accelerator through which each electron bunch passes only once.

We give kinetic energy to an electrically charged particle by exposing it to a strong electric field. The energy given to an electron (or any particle having the same electric charge as one electron) accelerated by a 1.5 volt flashlight battery is 1.5 electron-volts, or 1.5 eV. The rest mass (mc^2) of an electron in these energy units is about 0.5 MeV (million electron volts). Suppose we wish to study electron-positron pairs created in a head-on collision two (other) electrons. To produce enough mass energy to make such a pair out of kinetic energy, the incoming colliding particles would each have to have a kinetic energy of at least 0.5 MeV, so 330,000 batteries in a row for each! We did that experiment (not with batteries) in the 1940s and '50s. By the '60s we were producing proton-antiproton pairs with a 30 GeV accelerator (BNL's AGS, see Chapter 4 to read about my experiments with antiprotons), and now we want 40 TeV to study the Higg's Boson. (M for million or 10^6 eV, G for giga or 10^9 eV, T for trillion or 10^{12} eV).

However, since we are talking about zillions of volts, we can't just use something like an old TV cathode ray tube with 20 kilovolts. But even 20 Kilovolts can cause the air to break down, and we get lightning. The trick is to work in a very high vacuum (almost no air molecules present) and have the voltage on for only a VERY short time, a part of a nanosecond (billionth of a second, the time required for light to travel 30 cm = 1 foot). If the vacuum is good enough, any remaining air doesn't have time to break down and cause lightning. We do this inside an accelerating cavity in which the voltage from end to end can be made to oscillate between plus and minus several million volts so fast that it doesn't get a chance to spark inside from end to end.

Now suppose we are accelerating electrons. Once we get a steady stream of electrons almost up to the speed of light, we stream it into a cavity about 10 cm long in which an electric voltage is oscillating between plus and minus 5 megavolts every nanosecond. The average electron in the stream will gain about 3 MeV of energy, but the steady stream will become bunched, some electrons arriving at just the right moment to gain more than 3 MeV of energy and be sped up to join the bunch ahead of them, while those arriving slightly later will lose a little and be slowed down enough to join the following bunch. If we have a very long sequence of regularly spaced cavities (all enclosed in the same vacuum system), the electrons will emerge from the end of the long array of cavities each having gained almost exactly the same energy, but they will emerge in stable bunches about a tenth of a nanosecond (3 cm) long, and about 10 times that distance (and time) apart. This bunching tendency is called phase stability.

Of course driving this process requires high frequency electric currents to flow (back and forth) in the inside walls of each accelerating cavity. Running at the high powers we require will make the cavity very hot, unless its metal wall has almost no electrical resistance. A metal with a low enough resistance to support the currents we are talking about here is called a superconductor. Electrical resistance involves motion of atoms and this motion ceases at a temperature called absolute zero (-460 degrees Fahrenheit or -273 degrees C). At absolute zero the electrons can slip right on past the metal's atoms without heating up the metal. Absolute zero is difficult to achieve (well, maybe not in interstellar space) but some metals (eg Niobium) exhibit superconductivity at (just barely) achievable temperatures slightly above absolute zero. The high frequency (radiofrequency or rf-) cavities used to accelerate particles tend to be made of Niobium, or of Niobium-plated copper. The superconducting supercollider was not only designed with superconducting radio frequency (SRF) accelerating cavities, but it was to be a proton-proton collider, and its basic accelerator was to be a proton synchrotron, with its huge ring of bending magnets having superconducting current coils. (Actually the ring was a proton-antiproton collider, with the bunches of protons and antiprotons travelling in opposite directions around the ring. Similarly, electron-electron interactions can be studied in electron-positron ring collider.) There was nothing much new in the SSC design, which was basically an extension of technologies already in use in proton accelerators at Fermilab and at CERN.

But the SSC was "too expensive" (too much money being spent in Texas). As some of us had expected, the lower energy LHC (at CERN) proved to be not energetic enough because the proton-proton collisions they both were to provide would prove too complicated. With 3 quarks colliding on 3 other quarks, what would you expect anyway? The next step should be an electron-positron linear collider.

The NLC (Next Linear Collider) became a focal point for many of us, beginning in the 1990s: workshops and design studies were held, experiments proposed and so on, but nothing yet as of 2020. (2019: CERN is proceeding with a larger proton collider, but may be defeated by event complexity as mentioned above.) One of the principal costs of a linear collider involves the accelerating cavities themselves, which have to be superconducting, providing very high frequency power. Lou Hand at Cornell and I spent several of our early retirement years performing laboratory studies of the SRF properties of Niobium, our last publication on the subject being in 2005.

Cavities like the TESLA cavity (below) have niobium metal walls 3 mm thick. The niobium must be very pure, and is expensive, so many cavities are actually made of copper, with a thick film (a tenth or so of a millimetre) of niobium plated on the entire inner surface. This works because it is the nature of high frequency electric and magnetic fields to stay on or near the metal surface, and in a cavity, on the inner surface. However, this surface conduction tendency presents problems too.

Niobium is a "getter", and when heated it absorbs gases like air. In the old days, getters were used to improve a vacuum. A flash of power to vaporize a niobium (or more likely titanium) filament in a vacuum tube (like a CRT) improves the vacuum by absorbing oxygen and nitrogen (and various impurities) into the crystal structure of the first few atomic layers of the getter's surface. Even when stored in the laboratory at room temperature Niobium will gradually absorb oxygen, hydrogen (from humidity) and various lab hydrocarbons used for cleaning. So cavities have to be "cleansed" immediately before use, typically by coating the inner niobium surface with titanium (a better getter) and then baking briefly in a vacuum oven. The absorbed gases diffuse from the niobium into the titanium layer, which must then be etched off chemically before the cavities are used. Especially with a film cavity, this process can only be performed a few times, because some of the niobium is inevitably lost in each etching process.

Here is a 1 metre long 9-cavity resonator typical of a modern accelerator, capable of imparting 25 MeV of energy. To give an electron 500 GeV of kinetic energy we need 20 Km of such cavities.

TESLA cavity

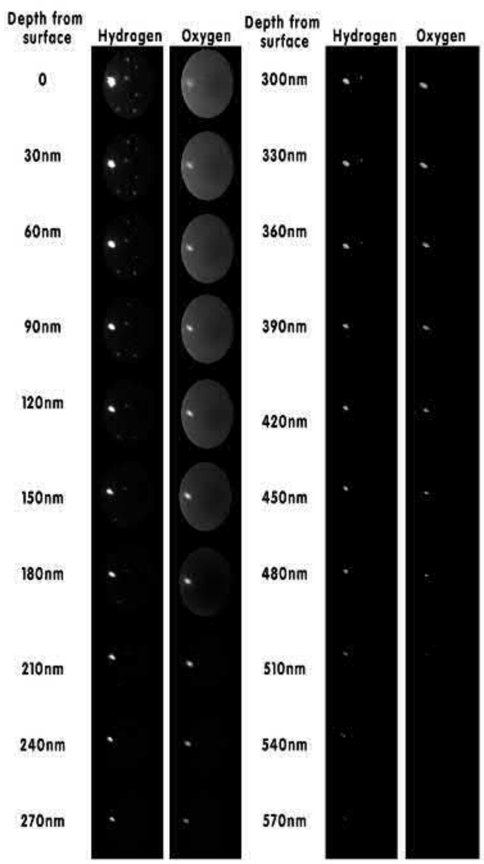
In April 2018 I found some old emails saved from my old Silicon Graphics computers "blueflag" and "Wildflag". Many of these emails date from 1996, and throw some light on my research activities in my immediate post-retirement years. This work started with me spending some time at DESY (Hamburg) working with members of DESY's Accelerator Division testing superconducting radio frequency (SRF) accelerator cavities. The emails show I received remuneration from DESY for travel/living expenses for work done both at DESY, and abroad on DESY's behalf. I remember promising Bjoern Wiik (DESY Director and an old friend) that I would continue this work in 1999. But then Bjoern died suddenly, leaving me with both an interest and an obligation. I interacted during this time with Maury Tigner (Director of the Synchrotron Lab at Cornell) but he soon retired, passing my Cornell connection to Hasan Padamsee and his (Maury's former-) 2 grad students Tom Hays and Jens Knoblauch. This work used a lot of my life for my first ten retirement years. Beginning as early as 1996, recovered emails contain an increasing frequency of detailed discussions with my old Cornell particle physics colleague Lou Hand about forming, treating and observing niobium films. DESY was supporting me to work with Tigner and eventually Padamsee, but these men were being gradually forced to reduce their development work on superconductivity due to pressure from the west coast groups. SLAC's director (Pief Panofsky) wanted federal funding to focus on his warm accelerator cavities, despite the inherent limitation to accelerators of higher intensity but not-so-high-energy electrons: or else to support existing technology for superconducting lower energy colliders like Fermilab's Tevatron. Basically this meant using collisions of higher intensity beams of lower energy particles; more signal but MUCH more background than using higher energies. Producing much more research activity and so lots of publications and lots of PhD theses, but less discovery.

This change in emphasis in the USA removed DESY's interest in and financial support for my Cornell connection, but led me into a period of more detailed study with Lou Hand of contamination of niobium surfaces and associated SRF accelerator cavity breakdown at high power levels. Lou and I managed to pursue this interest for several years, with almost no external financial support (but because we could wrangle the free use of facilities belonging to friends and acquaintances at the relevant laboratories).

For niobium film-coated copper cavities a central question becomes how thick a film is needed, and how many times can it be purified by titanium "gettering" followed by etching. Lou Hand and I were mainly studying the nature of the contamination of surface layers of niobium films, which we deposited by various techniques, and the efficacy of the titanium "gettering"/etching process. Attempts to make cavities capable of providing stronger electric fields (to allow construction of linear accelerators with fewer kilometers of cavities) had seemed to run into a brick wall at 25 MeV per meter, and we were trying to understand how to get past this wall.

It seemed clear that breaking through the wall would require a much better understanding of superconductivity. This field had been pursued with what struck me as theoretical generalizations (like statistical mechanics) and I felt that understanding had to be achieved at a more fundamental level. The particle physicist in me knew that a superconducting RF cavity didn't break down because of difficulties with a "grand canonical ensemble", but it broke down at a very particular spot, probably due to a very specific departure from the average behaviour, not because of the average behaviour. Jens Knoblauch's 1997 PhD thesis had been based on the discovery that when an SRF cavity broke down, it broke down at a particular place, leaving visible scars. Jens' detailed examination of these scars could find little apparent cause for the breakdown, except correlation with a local oxygen contamination of the surface, that may have preceded the breakdown. Lou and I decided to undertake a detailed examination of the way oxygen penetrates into a niobium surface.

But Hasan's principal responsibility was to keep the Cornell storage ring collider running: messing with future accelerator development was not encouraged, so we were on our own. Lou could use some lab facilities at Cornell both to make Niobium test films, and to clean them with the gettering/etching process. I was able to examine the subsequent behaviour of these films under various conditions using the SIMS (Secondary Ion Mass Spectroscopy) at U. Western Ontario. Our studies found that a surface of niobium film, just freshly cleaned, would first absorb hydrogen onto a tiny spot on its surface layer, soon to be followed by oxygen. The hydrogen would penetrate as a narrow column all the way through 100 nanometers of the film, with the oxygen following most of the way. Exposure to oxygen was known to enhance the likelihood of breakdown of a cavity when the RF power was turned on. Our results were published in the proceedings of several international conferences and workshops, the last in 2005 at Jefferson Lab in Virginia.



Pairs of hydrogen and oxygen SIMS images 150 microns in diameter recorded as function of depth as the primary ion beam etches away the surface of the film.

The 2 micron niobium film was sputtered onto an oriented single crystal of MgO , and has a cap layer of 180 nanometers titanium.

Repair of breakdown sites was also on the agenda, and I was able to interest some people in Engineering Physics at Simon Fraser University in some laser annealing tests. They helped with the annealing (I really just watched) and then I examined the annealed surfaces using an electron microscope in the biology lab back at York.

However the next step promised to be much more difficult: first locating oxygen-contaminated surface spots in real cavities, and then observing them become breakdown sites under RF power. After a few years we ran out of both our financial support and our youthful enthusiasm.

Addendum. Nigel Lockyer, BSc Physics 1975.

A student I taught but never employed.

Nigel was a Physics undergrad at York in the early '70s, and ran an undistinguished C-average in his first three years. Then in the fall semester of his final year he appeared in my small Nuclear Physics class of half a dozen students, in which his performance suddenly showed a startling uptick well into the Bs. For High Energy Particle Physics in the spring semester, the class size shrank to 3, and Nigel's performance shot into the high As! What was happening? Then I learned that Nigel had been a full-time employee of the Ontario government for all his undergrad years, merely studying physics for his batchelor degree, so he could follow with an MBA to improve his career in the civil service.

I advised Nigel to instead consider a career in Particle Physics. His C-average would automatically prevent his acceptance to a Canadian grad program, but in the USA, college grades were variously reliable and grad programs depended heavily on personal recommendations. I arranged a choice of acceptance (and financial support) by my former colleagues at two different universities: Case Western and Ohio State. The latter project involved a (then) cutting-edge experiment on Lambda Beta-decay, so Nigel chose OSU, to work with Professor Tom Romanowski (also Research Director for Particle Physics at Argonne Nat. Lab, Chicago). This work made them both famous, leading to an interesting postdoc for Nigel with Prof Jaros in California, working with high precision wire chambers at SLAC, followed by an Assistant Professorship at U. Pennsylvania. After many successful years at Penn, Nigel was appointed Director of TRIUMF in Vancouver in 2007, where

“Nigel has had a profound impact on TRIUMF,” said David B. MacFarlane, chair of the National Research Council’s Advisory Committee on TRIUMF and Associate Laboratory Director at the U.S. SLAC National Accelerator Laboratory. “He articulated an ambitious new vision for the laboratory and energetically set it upon a path toward an exciting world-class program in rare-isotope beams and subatomic-physics research......"

In 2013, Nigel Lockyer was appointed Director of Fermilab (US. National Accelerator Lab) in Chicago. David MacFarlane had been just one of Nigel's fellow Canadians supporting him for this appointment.

Nigel has made contributions particle physics in Canada since his departure to OSU. In the early 1980s he attended a design workshop I held at York for the design of our vertex wire chamber for the ARGUS experiment in Hamburg. And then all his contributions as TRIUMF Director. More recently (as Fermilab Director) he set up a joint appointment for a Fermilab scientist as Professor of Physics at York. This brought us Professor Deborah Harris (Fermilab Senior Scientist), now an important part of York's impressive group of experimental particle physicists.