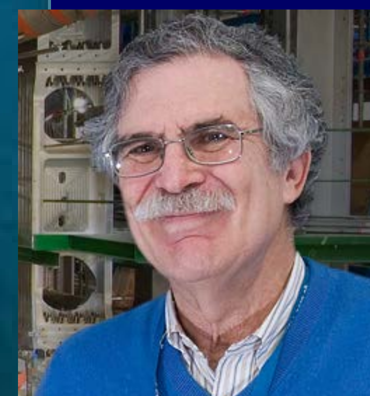
Adventures with Howard

In search of the perfect experiment



- AGS Expt 686 (charm search)
- D0 at Tevatron

P. Grannis, Oct 2, 2017

AGS Experiment 686*

The discovery of the J/ψ in the November 1974 revolution and the hypothesis that it was a bound state of charm-anticharm quarks set off a worldwide program to discover hadrons with open charm. Howard sought to find charm ($\pi^- p \rightarrow D^- \Lambda_c^+$) using semileptonic D^- decays at the AGS using the MultiParticle Spectrometer.

G. Donaldson #686
3/11/76
Draft

AGS Proposal

Subject: Search for Associated Charm Production and Study of Direct Electron Events

Experimenters: BNL: G. Donaldson, H. Gordon, R. Palmer and one additional staff member

University of Pennsylvania: L. Cormell, M. Dris, W. Kononenko and W. Selove

Virginia Polytechnic Institute and State University: G. Collins, W. P. Trower, J. R. Ficenec

Facility: MPS with Transition Radiation Detector and Shower Detector

Beam: 20 GeV/c π^- HEUB

Running Time: 500 hours plus test beam time

Spokesman: H. Gordon

I was on the review committee for several AGS charm proposals and liked Howard's choice of semileptonic charm decays. I had been working on anomalous prompt electron production so joined Howard and expanded the proposal to also seek anomalous e^+e^- pairs.

* Thanks to 686 students Jim Stekas and Steve Crandall for materials.

By 1978, evidence for charmed hadrons had been found in e^+e^-, νA, γp and high energy hadron interactions. But the exclusive cross sections for $D^- \Lambda_c$, $D^0 D^- p$ and $D^+ D^- n$ were still of interest. Cross section was expected to be small as one was just above threshold (~100 nb at $\sqrt{s}$ = 5.7 GeV). The final states, e.g.

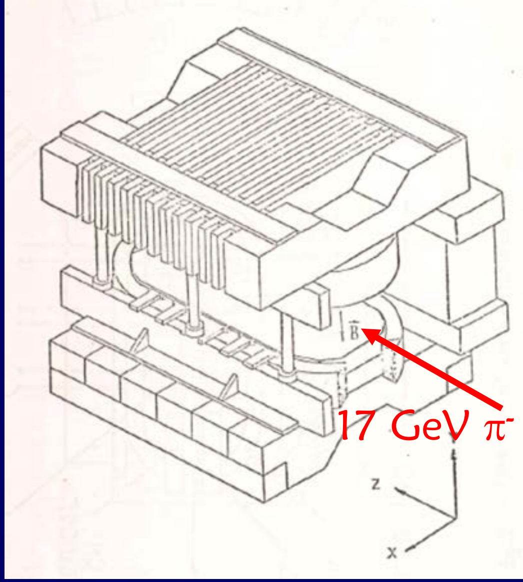
$$\pi^- p \rightarrow D^- \Lambda_c^+ \rightarrow (K_s\ (\pi^+\pi^-)\ e^-\ \nu)\ \ (p\ K^-\ \pi^+)$$

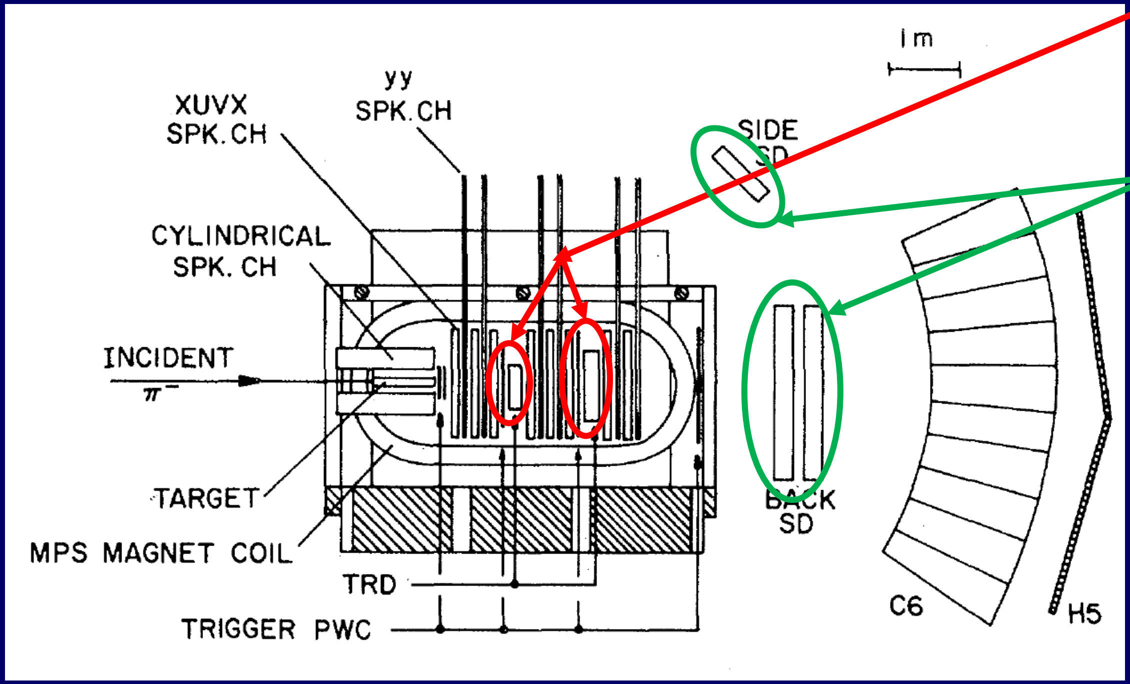
involve 6 tracks, including soft backward tracks from Λ_c. Expected sensitivity for E686 was ~10 nb.

Many experiments had detected an excess of single direct leptons (e or μ) at relatively large p_T. At high energies, a good fraction of these were explained after 1974 by semileptonic decays of charmed hadrons, and other meson decays. At lower energies below charm threshold, the anomaly still existed but had no clear explanation. Thus it was interesting to see if e^+e^- pairs were responsible, either from a new source, from new decays of known mesons such as η, ω, ρ etc., or from some other new mechanism.

The MPS facility was built by the Lindenbaum-Ozaki group with 44 planes of magnetostrictive spark chambers downstream of the LH_2 target in a 1 T dipole magnet, cylindrical spark chambers around the target for detecting backward particles, and PWCs for triggering.

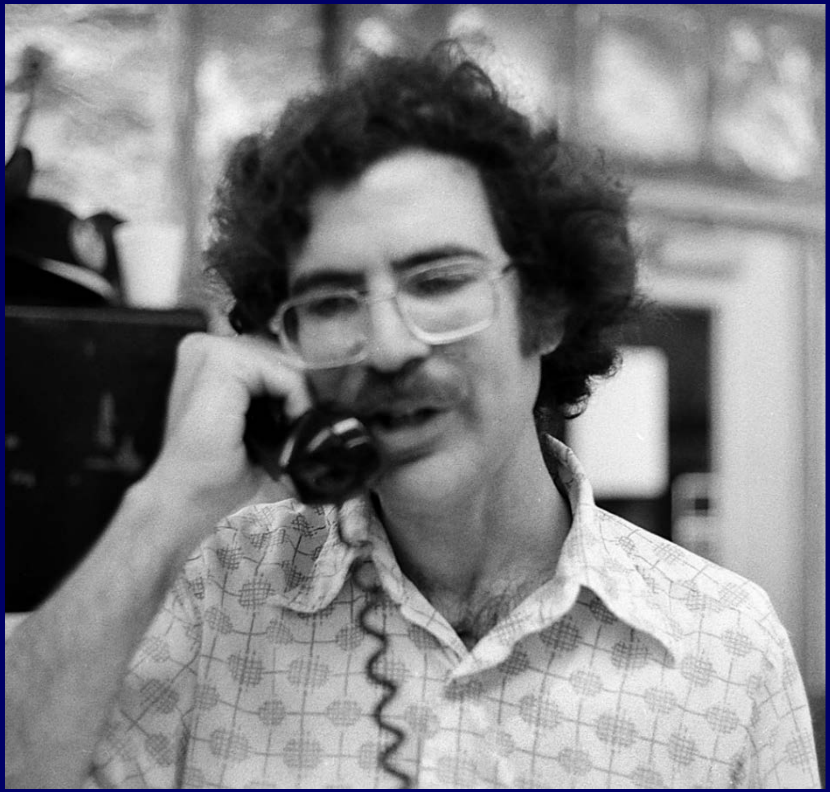
For the time, the MPS was a very impressive facility.

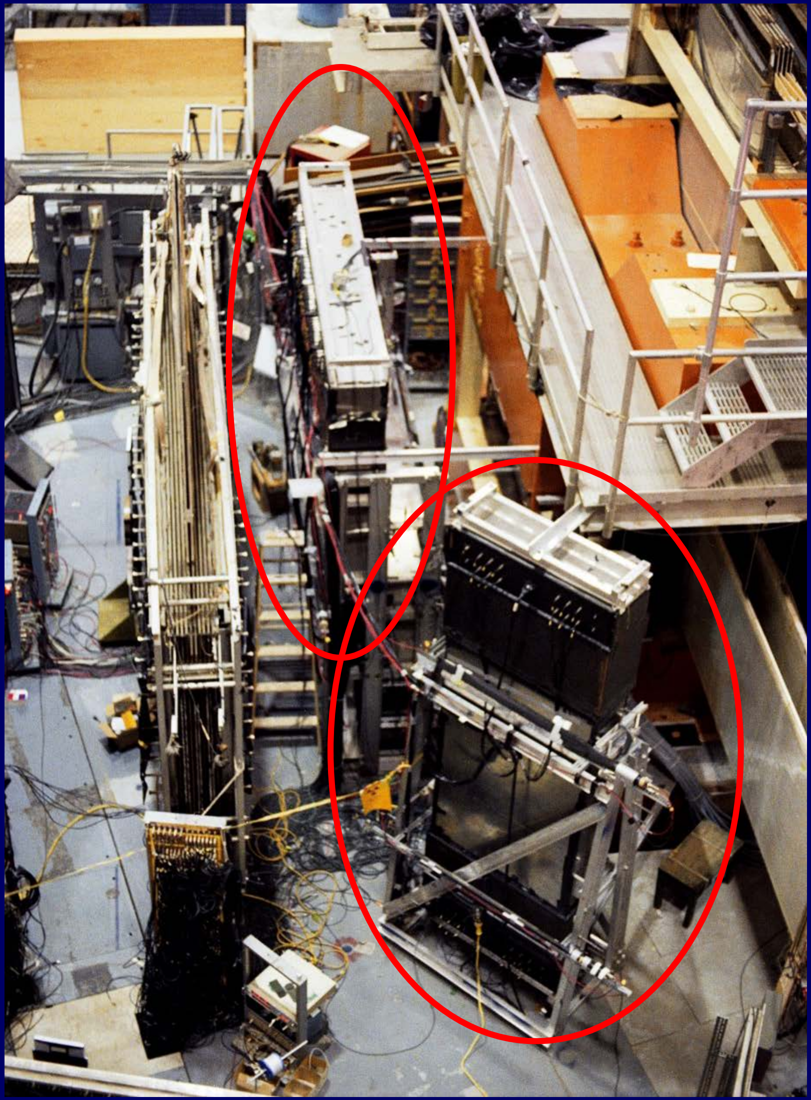




To this, E686 added 2 Li foil TRD's (recently pioneered by BNL for an ISR exp't) and 2 Pb Scint. EM calorimeters to trigger on and identify electrons. The trigger also used hit patterns in TRD and EM CAL roads to reject fake electron tracks. **A pretty sophisticated experiment for mid-70's.**

Data taking in 1978.





Rear and side shower detectors

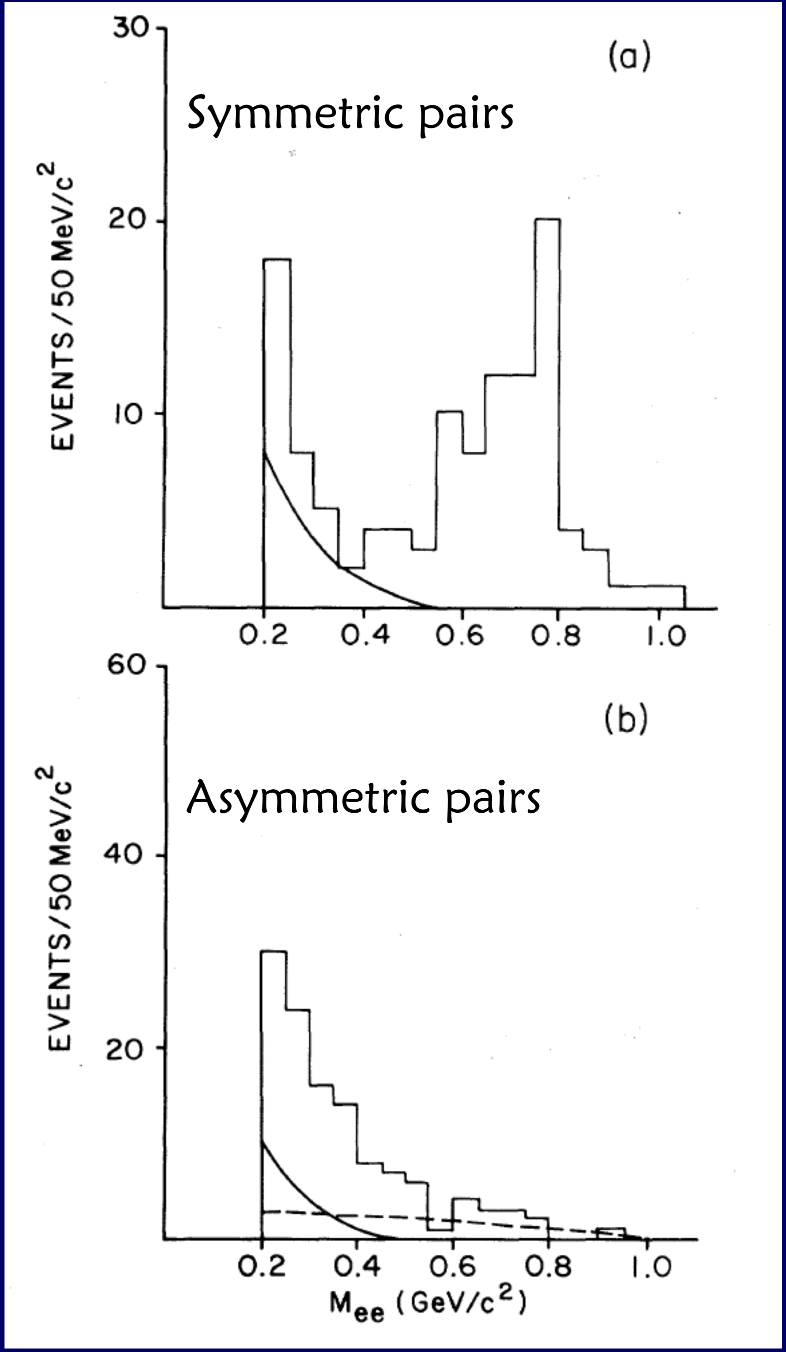
Ron Morris and Steve Crandall in control room

Anomalous e^+e^- pair results

For e^+e^- pairs in $0.2 < M_{ee} < 1.0$ GeV, expect direct decays (e.g. $\rho \rightarrow e^+e^-$), internal conversions ($\eta \rightarrow \gamma\, e^+e^-$, $\omega \rightarrow \pi^0\, e^+e^-$ etc.), and backgrounds (negligible). The experiment saw an anomalous excess over known sources.

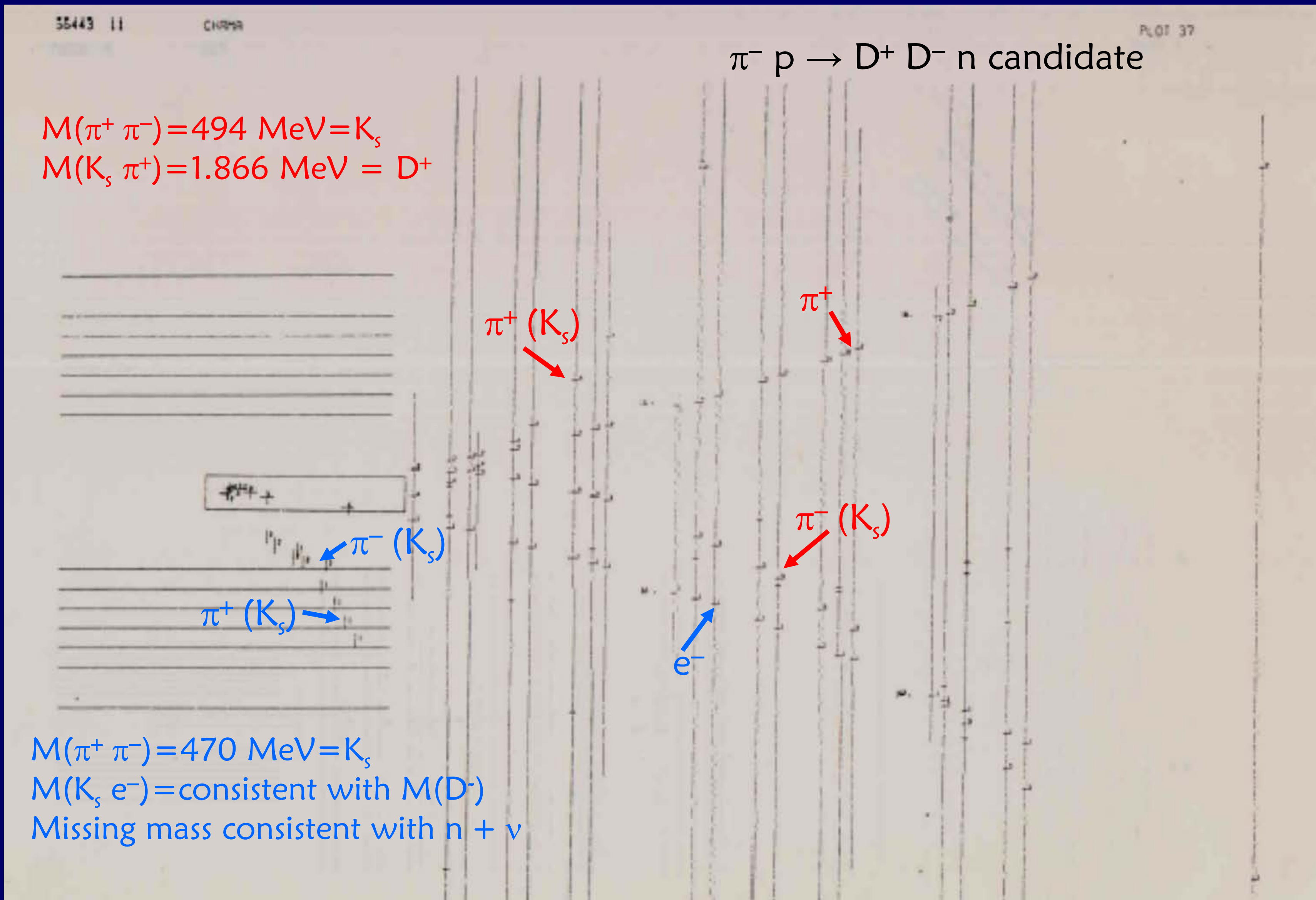
The anomalous pairs provided a good explanation of the anomalous single electron results at the same energy.

They were not associated with γ's, and were not decay produces of any known mesons. The production kinematics were similar to those for e^+e^- from η etc. A plausible source was radiation from final state quarks, in a sort of Ur Quark Gluon Plasma.



The charm search part of the experiment was tougher.

A typical (well, really the only good) charm candidate



Set limits $\sigma(D^- \Lambda_c^+) < 1.63\ \mu b$

$\sigma(D^0 D^- p) < 1.49\ \mu b$

$\sigma(D^+ D^- n) < 1.58\ \mu b$

(Large uncertainties due to lack of knowledge of BRs at the time)

Limits were considerably above the expected XS ~ 100 nb, largely due to low individual track efficiencies, particularly in the cylindrical chambers around the target, compounded by the high multiplicity, and low trigger efficiency for DDN events.

What did we learn?

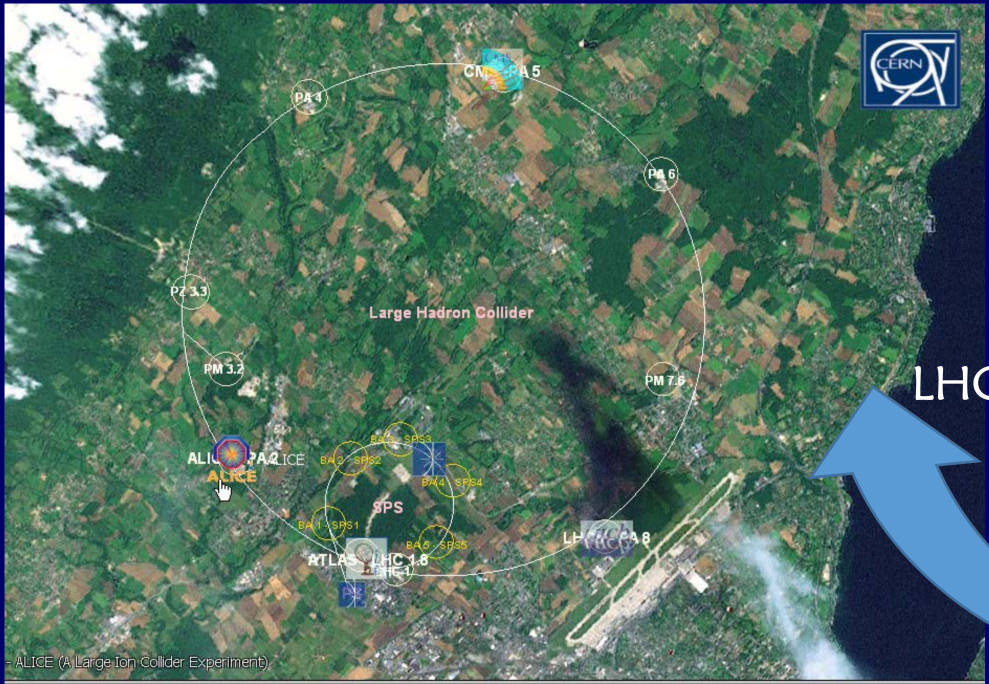
1. If you want to study heavy particles, high energy is a lot better than low energy (!)
2. If you want to look at all produced particles, working in the center of mass frame is better than the fixed target lab frame where many particles are low energy (low efficiency)
3. 4π coverage really helps
4. Sophisticated triggers are essential
5. TRDs and good EM calorimeters are very useful (!)

So what to do ??

Colliders anyone?

ISR

Tevatron

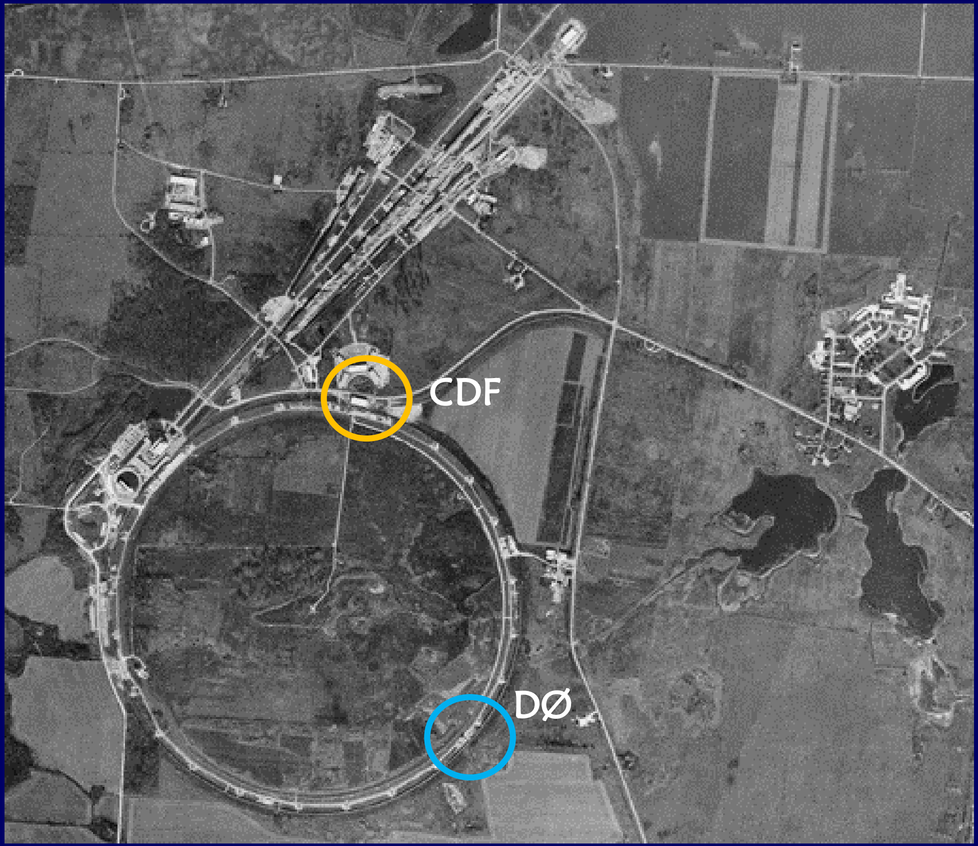


LHC

(SSC)

D0 Experiment at Tevatron

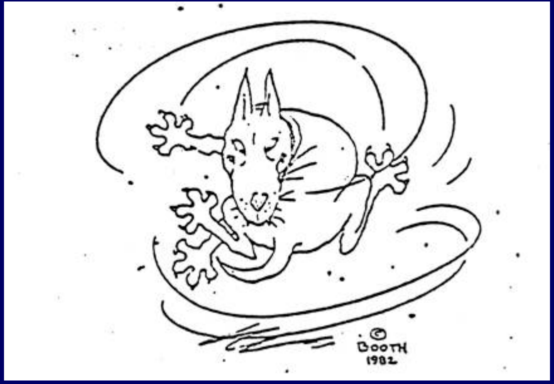
In 1981, the Tevatron collider was being built with CDF as its sole experiment. Director Leon Lederman called for proposals for a '**small** (9m cube), **simple and clever**' and **cheap** experiment at D0 IP for 2 years of operation starting in 1986 and offered $1M to build it. (Other IPs had RF, beam dumps, extraction systems etc. Even D0 had the kicker magnet for fixed target beams that would require rolling a detector on/off the beamline.)



19 proto-proposals, 12 survived for PAC evaluation.

In July, 1983, PAC conducted its own crazy experiment in experiment planning -- rejecting all proposals but giving carte blanche Stage I approval for a new one-person experiment that "should be <u>no worse</u> than those proposed".

The nascent D0 collaboration was seeded by the LAPDOG proposal originally formed for an ISABELLE experiment by BNL, Brown, Columbia and Stony Brook and a muon detection proposal from Fermilab. The collaboration included Sam Aronson, Bruce Gibbard and Peter Yamin from BNL (but <u>not</u> Howard).



Over the summer of 1983, the new D0 collaboration (now including people from other declined proposals) discussed a design based on an EM calorimeter using scintillating lead glass bars, a magnetized iron-slab hadron calorimeter/muon detector, and simple tracking system.

Unusually, at that time DOE had insufficient funds (and HEPAP said SLD had priority so as to beat LEP to the Z discovery).

The Fermilab management said that the Collision Hall could not exceed 12m height and that the main ring could not be lifted out of the hall as at CDF, so would have to penetrate the detector. Time was short as the first collider run was expected in ~1986-7.
So some self-restraint on the ambition in the design was needed.

Howard, though still not a member of the collaboration had joined our biweekly meetings to discuss the design. In August 1983, he wrote the famous D0 Note No. 1, **"On the Importance of Measuring Leptons Well at DØ"** in which he discussed the merits and shortcomings of existing detectors (UA1, UA2, CDF, ISR Dimuon Expt, & the L3 design) and addressed the **"Problems and Solutions with the Current D0 Design"**

DØ Note #1

8/23/83

On the Importance of Measuring Leptons Well at DØ

Howard A. Gordon

Physics Department

Brookhaven National Laboratory, Upton, New York 11973

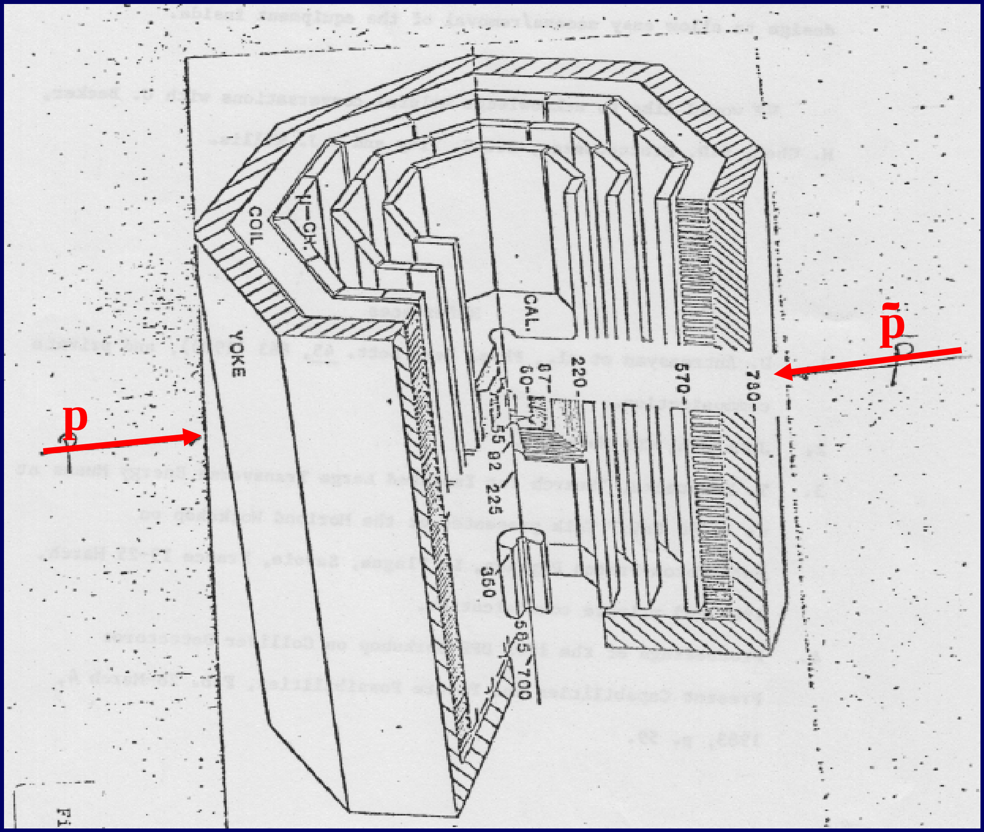
Introduction

Fermilab has made a commitment to the DØ experiment to provide first class luminosity and a reasonable by-pass. Therefore, as mentioned at the DØ collaboration meeting 8/17/83, a golden opportunity exists, to design a detector which would be competitive with the best detectors for hadron colliders: the upgraded UA1 and CDF detectors.

"Therefore … a golden opportunity exists, to design a detector which would be competitive with the best detectors for hadron colliders"

Howard's recommendations:

- Increase the segmentation of the EM calorimeter
- Add a TRD for improved *e*-ID (a recurrent theme)
- Increase the depth of the HCal to control punchthrough
- Add a central magnetic field
- Measure muon momentum in field outside calorimeter (!!)



He ended with a proposed design in which a solenoid was turned on its end to have $\vec{B}$ vertical (thus literally orthogonal to CDF!) allowing a focus on forward regions ("where most of the rapidity range is") with vertical drift chamber wires ("sagging not a problem")

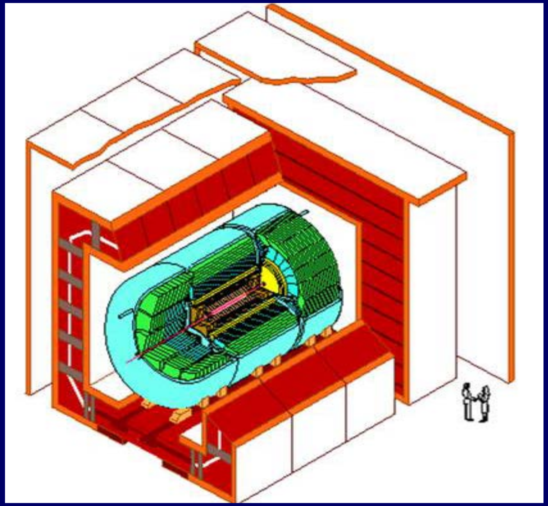
We did not buy all of Howard's arguments, but …

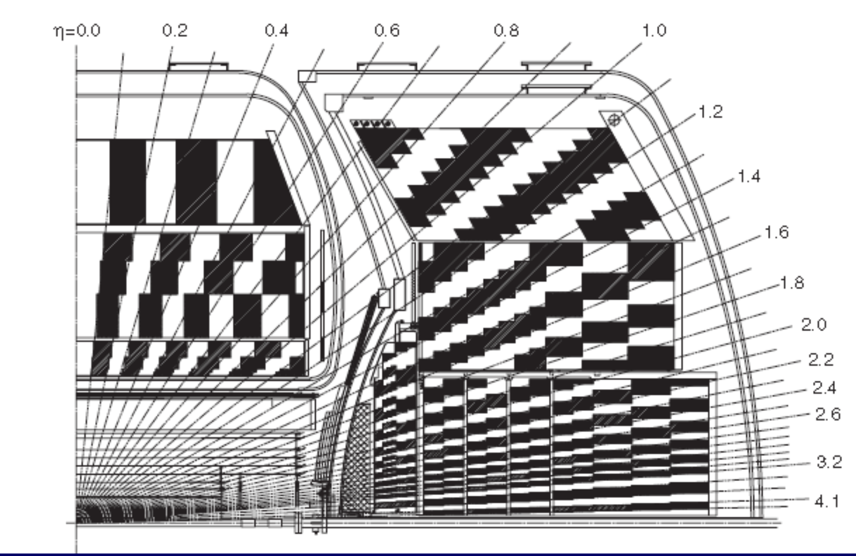
Perhaps not coincidentally, many arrived at the mid-September collaboration meeting with the conviction that the scintillating glass design would not work – poor segmentation, calibration difficulties, radiation damage…

We opted in that meeting for both EM and hadronic calorimeters to be based on uranium-LAr (equalize the electron and hadron responses, fine segmentation, unit gain, hermetic coverage). U/LAr had never been tried, so this decision by itself meant that a 1986 start was now out of sight.

Central & End TRDs were envisioned (central TRD realized when Saclay joined).

But still no central magnetic field (wait until Run II in 2001), and a small tracker.





Quadrant of D0 (in 1984 TDR)

And shortly thereafter, Howard (and Serban Protopopescu, Randy Johnson, Steve Kahn et al.) formally joined the D0 collaboration.

The D0 Conceptual Design Report was presented to Fermilab PAC in Dec. 1983 and was enthusiastically received (The PAC patted itself on the back for its successful experiment). Attention turned to the preparation of a Technical Design Report for the DOE Temple Review in Nov. 1984 (equivalent to today's CD1).

Howard became overall coordinator of calorimetry (Central cal work at BNL, End cal at Fermilab and Plug cal at Pennsylvania).

Let Howard's torrent of D0 Notes speak to his impact on the

DECIDING THE OPTIONS FOR THE DØ CALORIMETER

DØ83

Howard A. Gordon and Randy Johnson

This is a summary of the various decisions that must be made soon for the calorimeter. The path to these decisions requires three types of

"Deciding the options for the D0 Calorimeter"

Drs. William J. Willis and
Chris Fabjan
EP Division
CERN CH-1211
Geneva 23, Switzerland

Dear Bill and Chris,

At this workshop in Snowmass, one of the burning controversies is whether U/LA compensates! Otherwise for detectors it was an ode to transition radiation.

"Understand the physics of equalizing EM and hadronic response"

DØ Note #109

DØ CALORIMETER STATUS - AUGUST, 1984

DETECTOR TO BE COMPLETED AUGUST, 1988

1) CONCEPTUAL DESIGN IS BEING DONE BY COLLABORATION - H. GORDON

CENTERS: BNL - S. ARONSON

FERMILAB - B. COX

Summarize status, set goals and plans for Nov. 1984 TDR (see "Detector to be completed Aug. 1988"!)

DØ Note #169
H.A. Gordon
Jan. 28, 1985

Calorimeter Newsletter

The next general calorimeter meeting will be at LBL, Room 50B-4205 at 2 PM to 5 PM on February 14, 1985. We will continue to discuss how to get on with the design. Enclosed is a chart of the

Newsletter: Keep everyone informed, air all opinions openly

DØ Note #246.1
CAL

DATE: August 15, 1985

TO: DØ Calorimeter Group

FROM: H.A. Gordon HG

SUBJECT: Uranium meeting held at ANL 7/30/85

Manage the delicate DOE procurement of depleted and Nb alloy uranium from ORNL

M I N U T E S AND A G E N D A

DO CALORIMETER MEETING NOV 8,1985 FINAL

1. We discussed the work to be done in NW to prove that the coherent noise can be reduced to a negigible level. It was agreed that a very high priority should be placed on making improvements such as increasing the gain of the preamps, improving the grounding etc so that there will be no surprises in the final detector.

Establish goals for test beam campaigns (e.g. demonstrate control of coherent noise)

FILE COPY

DØ Note #312
CAL
H.A. Gordon

Evaluation of the Use of Transformers for DØ

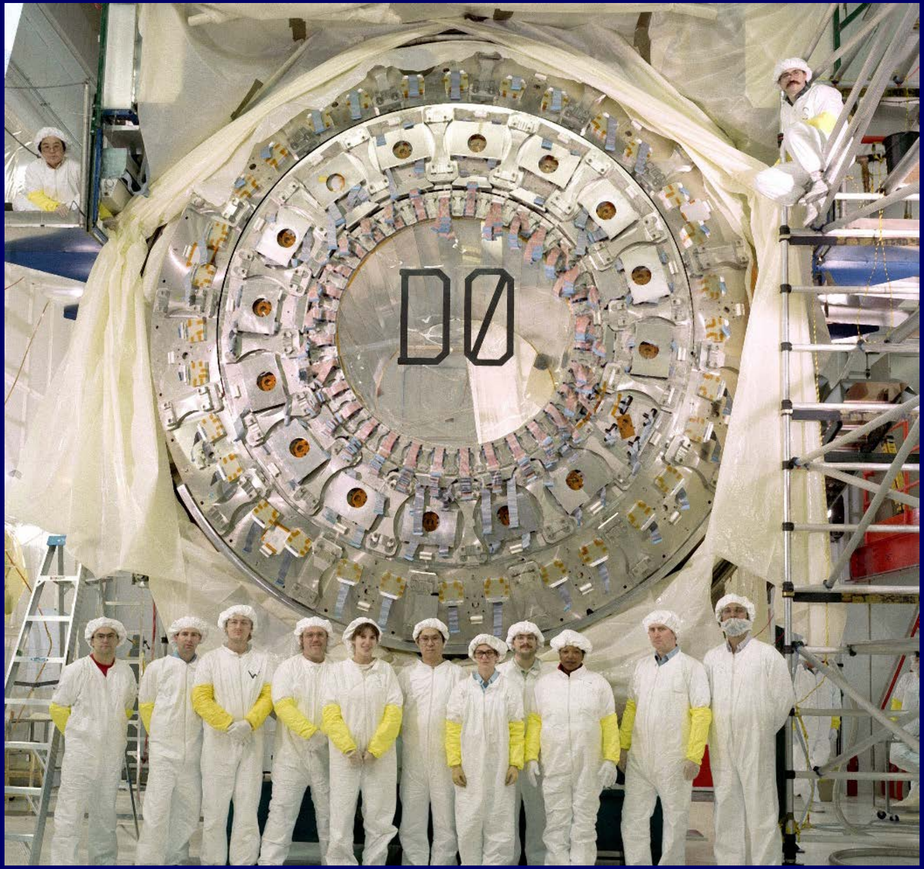
I have had a nagging feeling that we should use transformers to be able to reduce the sampling time in order to reduce the uranium noise. However it is necessary to go through the calculation in

Continue to refine the design – here considering adding transformers to reduce LAr sampling time (to reduce U noise (& pileup)

Howard shaped all aspects of the D0 calorimetry, which was at the heart of nearly all the physics we did.

CC EM module

First collisions May 1992



The central calorimeter built by BNL performed superbly for (almost) all of the 20 year run and 10 fb^{-1} accumulated data. (Though by the end, the EM calorimeters were gasping.)

Howard insisted on the best detector performance that could be achieved, but always with an focused eye on the physics.

Howard remained an author and helped shape the D0 Run I physics papers until 2004. His vision of superb lepton and jet detection with 4π coverage was key to the discovery of the top quark in 1995.

Although he did not play a major role in the upgrade of D0 for Run II (2001 – 2011), he was an author of the 1994 EoI to define the **upgrade for a potential subsequent Run III**.

An Expression of Interest(EoI) Based on the DØ Detector for Run 3 at the Fermilab Tevatron

M. Abolins[20], N. Amos[19], A. Baden[18], G. Blazey[10], A. Boehnlein[10], F. Borcherding[10], R. Brock[20], J. H. Christenson[10], D. Cutts[4], K. De[34], D. Denisov[10], H.E. Fisk[10], B. Gibbard[3], B. Gobbi[24], A. Goldschmidt[17], H. Gordon[3], N. Graf[3], P.D. Grannis[32], D.R. Green[10], P. Gutierrez[26], N.J. Hadley[18], D. Hedin[23], B. Hoeneisen[1], K. Johns[2], M. Johnson[10], S. Kwan[10], S. Kunori[18], R. Lipton[10], F. Luehring[15], S. Mani[5], C.S. Mishra[10], H.E. Montgomery[10], A. Para[10], D. Pellett[5], H. Piekarz[11], S. Protopopescu[3], J. Qian[19], R. Raja[10], M. Rijssenbeek[32], R. Ruchti[25], R.D. Schamberger[32], P.M. Tuts[7], H. Weerts[20], A. White[34], J.T. White[35], J. Womersley[10], D. Zieminska[15], A. Zieminski[15]

May 5, 1994

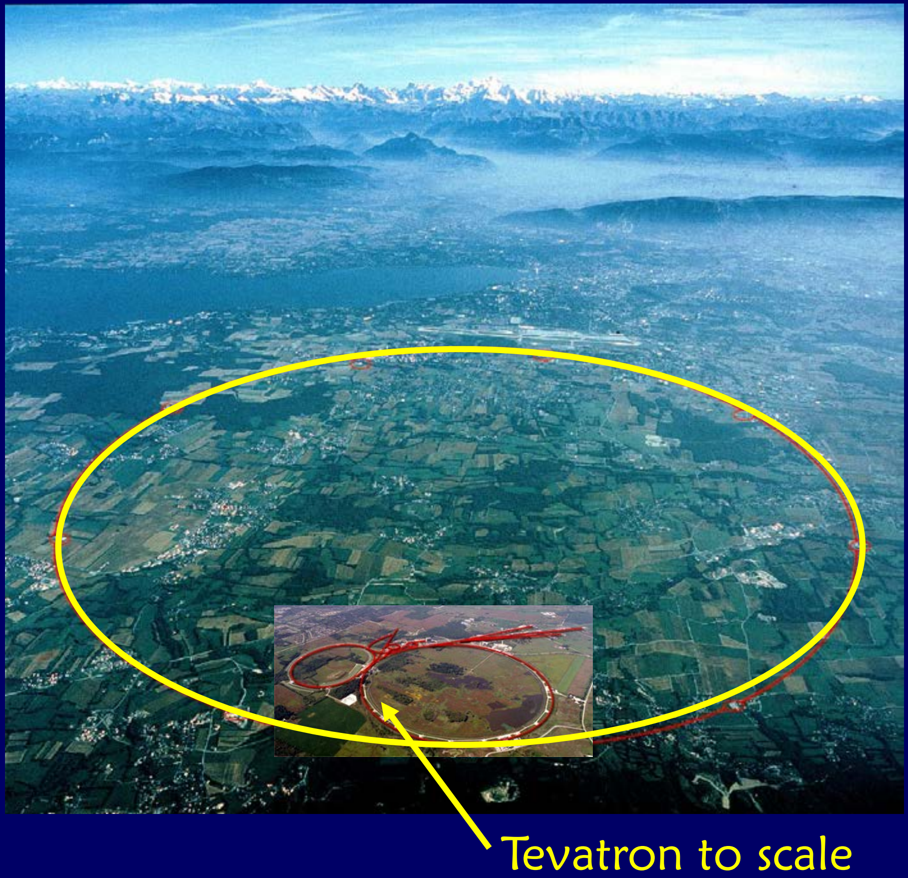
That document reviewed some of the physics that could be done in Run II (δM_{top} = 3 GeV (!!) , δM_W = 50 MeV (!!), search for Higgs above the LEP2 limit. It also looked to the further future studies of rare B decays, heavy flavor spectroscopy and properties. The Run III upgrade proposal included extensive silicon pixels, improved lepton triggering, new forward tracking, RICH particle ID, and DAQ upgrades.

But as good as the D0 experiment and the Tevatron promised to be, Howard could see the prospect for still better physics at higher energy.

Even before the start of D0 data taking in 1992, he heeded the siren call of the SSC, first with the EMPACT proposal, then with GEM, and we saw less of him.

Following the 1993 debacle cancelling the SSC, Howard gravitated to the LHC and we will hear that story from Peter Jenni.

But once again the quest for even more powerful detectors, still built upon transition radiation and powerful EM calorimetry, animated his search for the perfect experiment.



So, Howard, even if we did not fully achieve perfection in our experiments, you certainly steered us on that course.

Lynne frequently commented that we physicists like what we do so much that we will never stop – and would even do it for free.   And so, although Howard is retired, it may be hard for us to detect this!

And watch out – even as we speak, he is probably concocting the next “perfect” detector!

Congratulations, Howard, on a career well spent!

Michigan State Workshop 1984

Much of the real work was done at the annual D0 summer workshops

1985 workshop organized by Howard at BNL, with clambake at Smith Point

Brown 1986