P. Grannis

Comments at end of Tevatron operations, Sept. 30, 2011

(numbers refer to incidental slides at the end)

1 I was spokesperson from 1983, when D0 began, until 1993. In that year, Mont joined me as co-spokes until 1986. You should realize that of my 13 years, 9 were spent building the experiment and just 4 while we were taking data, so my recollections tend to focus on the travails of bringing D0 into existence.

D0 started on July 1, 1983 with an invitation from the Director Leon Lederman to form an experiment after all proposals for the D0 region were found wanting. The first collaboration meeting was in July in Stony Brook with people from some of the failed proposals. Among those attending then, here tonight, were Maris Abolins, Dave Cutts, Dean Schamberger, Dave Hedin, Chip Brock, Dan Green and me. Our original idea was to build a detector based on a scintillating glass bar calorimeter, and a surrounding muon detector. After two months of trying to make this concept work, in early September everyone came to the then-weekly collaboration meeting independently convinced this would never work. The bars of scintillating glass were simply too coarsely segmented and insufficiently pixellated,. Indeed had we built that experiment we would have lasted only the couple of years that Lederman had originally suggested for the D0 experiment. We decided instead to go for Uranium LAr, which none of us knew anything at all about, as a grand push to do something more ambitious. 2 By the end of November we completed a conceptual design report that outlined the liquid argon plus muon detector design with a small non-magnetic tracker, and a physics program (which we simulated on an abacus). How did we write the CDR in just 2 months? Easy! Tom Ferbel was not yet part of the collaboration!

D0 was approved in late 1983 to run for perhaps 2-3 years. At that time we had great difficulty in finding collaborators to join. 3 Our first accomplishment was in getting some additional collaborators from Fermilab and Brookhaven (e.g. Alan Jonckheere, Bill Cooper, Howard Gordon, Serban Protopopescu et al.) and a new LBL group (Mark Strovink, Stu Loken, Ron Madaras et al.). But the real shot in the arm was when Saclay came in under the leadership of Yves Ducros and Armand Zylberstejn, giving us some international cachet. 4 Around 1986, Saclay delivered an ultimatum: "if we don't run in 1988, we must leave". Alas we did not run in 1988, but fortunately Saclay stayed, and eventually many other groups from Russia, Brazil, India, Colombia, Ecuador, Argentina, Korea, Mexico, France, the UK, Germany, the Czech Republic, the Netherlands, Sweden, China, Ireland, Vietnam, Canada and the Ukraine joined to make D0 the force that it is today. (5 Actually we did run in DØ in 1988! The central drift chamber pre-module recorded collisions in November.)

In February 1984, there was a HEPAP meeting shootout where the question was whether to push forward SLD or D0, as there were not funds to do both. D0 made a more compelling case (in my opinion), but SLD had a strong voice on HEPAP (its co-spokesman) and SLD carried the day on the argument that the US had to push aggressively to compete with LEP to exploit the Z pole physics. It seemed like a death knell that we were relegated to lower status, and I was almost ready to throw in the towel. In the end, of course, LEP did superbly while the SLC machine initially foundered. In the end D0 caught up with CDF and the 5 year head start they had was erased. 6 (The real reason for this

was that the Tevatron worked better than we ever thought it would and CDF's first 4 pb^{-1} turned out to be negligible compared to what we eventually got in Run I, never mind Run II).

I became convinced that it is important to try for something truly adventurous in detector technology when proposing a new detector, but that trying for more than one miracle is too risky. The Run I miracle was the uranium liquid argon calorimetry. Pulling it off nearly sunk us. The Run II miracle was the fiber tracking with VLPC readout. There were skeptics galore who said tracking could not be done with only eight layers and that the VLPCs would self-destruct at high luminosity. **7** Making miracles happen require superheros and we had more than I can mention, other than to say that without Delmar Miller to put it all together, we would not have made it.

Concerning the Run I U/LAr adventure: We were babes in the woods, as none of us had any experience. The uranium came 'free' of charge from the depleted Uranium – Niobium alloy stockpiled at Oak Ridge Lab, but we had to pay for the rolling and machining. For the uranium plates, a company was set up outside the Oak Ridge gates to roll the uranium into sheets and they knew bubkes (forgive the Yiddish expression) as much as we did. **8** A parallel adventure was learning how to make signal readout boards on a truly large scale and to cover them with resistive epoxy with 40 MegOhms/square in a reliable fashion. Bill Cooper and Harry Weerts were the heros in that saga. Once we had the plates in an assembled module, we had to contend with the fact that uranium flakes and rusts. Joan Guida undertook the cleaning of the CC plates in a makeshift clean room in DAB, with all of us taking shifts under her demanding control. It involved long handled brushes, wires and vacuum cleaners and for some time it seemed that the rust came back faster than we could remove it. **9** In the end it got good enough that we thought we could live with the modules but we still worried. Then Sandor Feher invented a Rube Goldberg device whereby he pumped down a large vessel to as low pressure as it could reach, then opened a valve from the vessel to a CC module with the resulting hurricane of wind sucking up most of the rest of the remaining uranium dust. Has anyone had problems recently with discharges in the calorimeters? If not, thank Joan and Sandor!

10 We were ecstatic when we saw first collisions on spring 1992. That was a real high point of my life as spokesman. The celebration of first collisions was at midnight since the DOE Tiger teams were on site and we could not let them see us hoisting champagne. Our coming out party was at the 1992 ICHEP in Dallas where we reported the observation of W and Z events, and the successes in operating the various detector systems.

11 Before D0 ever ran we had already submitted (in 1990) a proposal for the Run II upgrade. The PAC was mightily resistant of an experiment wanting to upgrade before proving that the first incarnation worked. The project leaders, Mike Tuts and Mont and I tried over and over to convince the PAC that a magnet was a good thing and that fiber tracking in a confined space and with sparse sampling and VLPCs could be made to work. **12** There was a certain amount of resistance from CDF folks that a D0 upgrade would be a good thing, perhaps fearing that what might happen was what did happen – D0 would successfully compete for dominance in the Tevatron sweepstakes. The final approval of the upgrade only occurred some 5 years later, long after the successful initial running of D0.

13 The first physics paper was a search for first generation leptoquarks. We did not see leptoquarks then, just as succeeding generations of D0 analyses and now CMS and Atlas analyses as well, have not seen them.

One of our early papers was on W+jets production as a function of jet p_T with the aim of extracting the strong coupling constant. We were too dumb to realize at first that two things happen – the cross section changes as alpha_s varies, but so also the PDFs depend on alpha_s. Our ex-UA2 colleagues, Darien Wood and Stefan Grunendahl, helped us understand how this physics worked and ultimately we published a reasonably correct paper (but did not get the running of alpha_s until much later).

Mont may also comment on the top quark saga which happened in our joint tenure. In 1994, we published our second paper which set the lower-ever limit on the top quark mass (131 GeV). **14** That was interesting and important, but the salient point in that paper was the report of Event 417 – an event with an electron (100 GeV), muon (perhaps 200 GeV), MET (100 GeV) and two moderately robust jets (and a third mini-jet). This event was truly spectacular. I'm not sure we have had another so striking. We had quite a debate on how to report this event in the limit paper. In the end we said "this event is kinematically consistent with ttbar production over the mass range 100 to 200 GeV" with a peak likelihood at about 145 GeV.

Shortly after, CDF published their evidence for top which was a real tour de force, but was dependent on an upward fluctuation in the yield. D0 then had mild (2 sigma) evidence, but consistent with the SM cross section prediction. In another six months, we (and CDF) realized that we had enough data to make the discovery and in an incredible six week period, put together the discovery analysis. I wish I could give credit to all who made this possible, but then, and now, I find that it depended on so many people that I can do no better than to say "Thanks to the D0 collaboration".

15 The organizers asked us for high points during our tenure as spokes. That is an easy assignment. The best that happened to me in my years was the dedicated, inventive, collaborative efforts of so many people who designed and built the hardware, made the software systems, and fashioned the analysis tools on which we made an experiment that has truly provided a lasting legacy. It was a humbling experience to have helped shepherd this great collaboration of 1500 talented individuals toward the achievements that it has made over the years, and it is a delight to see so many old friends here tonight who made it possible. I also appreciate that the flow of inventive young people into D0 never stopped, and salute the young people who now carry us forward.

And now, let me introduce the real after-dinner speaker – Hugh Elliot Montgomery, Director of Jefferson Laboratory and D0's second spokesman.

1

Been scratching those fleas for 28 years

2

THE DØ COLLABORATION

B. Pifer
University of Arizona

L. Ahrens, S. Aronson, F. Connolly, B. Gibbard, H. Gordon, R. Johnson, S. Kahn, M. Month, M. Murtagh, S. Protopopescu, S. Terada, D. Weygand, D. H. White, and P. Yamin
Brookhaven National Laboratory

D. Cutts, J. Hoftun, R. Lanou, and T. Shinkawa
Brown University

P. Franzini, D. Son, P. M. Tuts, and S. Youssef
Columbia University

C. Brown, B. Cox, C. Crawford, R. Dixon, H. Fenker, D. Finley, D. Green, H. Haggerty, M. Harrison, H. Jostlein, E. Malamud, P. Martin, P. Mazur, J. McCarthy, and R. Yamada
Fermi National Accelerator Laboratory

H. Goldman
Florida State University

S. Kunori and P. Rapp
University of Maryland

M. Abolins, R. Brock, D. Edmonds, D. Owen, B. Pope, S. Stampke, and H. Weerts
Michigan State University

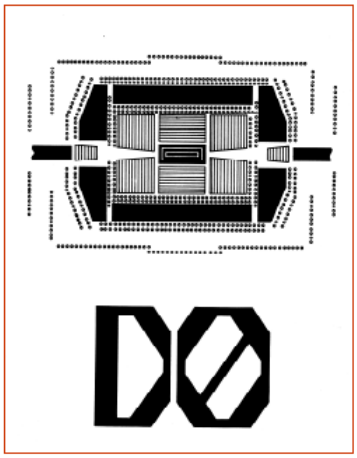
D. Buchholtz and B. Gobbi
Northwestern University

E. Gardella, W. Konovenko, W. Selove, G. Theodosiou, and R. Van Berg
University of Pennsylvania

M. Adams, R. Butz, R. Engelmann, G. Finocchiaro, L. Godfrey, P. Grannis, D. Hedin, J. Horstkotte, J. Kirz, J. Lee-Franzini, S. Linn, D. Lloyd-Owen, M. Marx, R. McCarthy, L. Romero, R. D. Schamberger, and H. Weisberg
State University of New York at Stony Brook

and

J. Ficenec
Virginia Polytechnic Institute



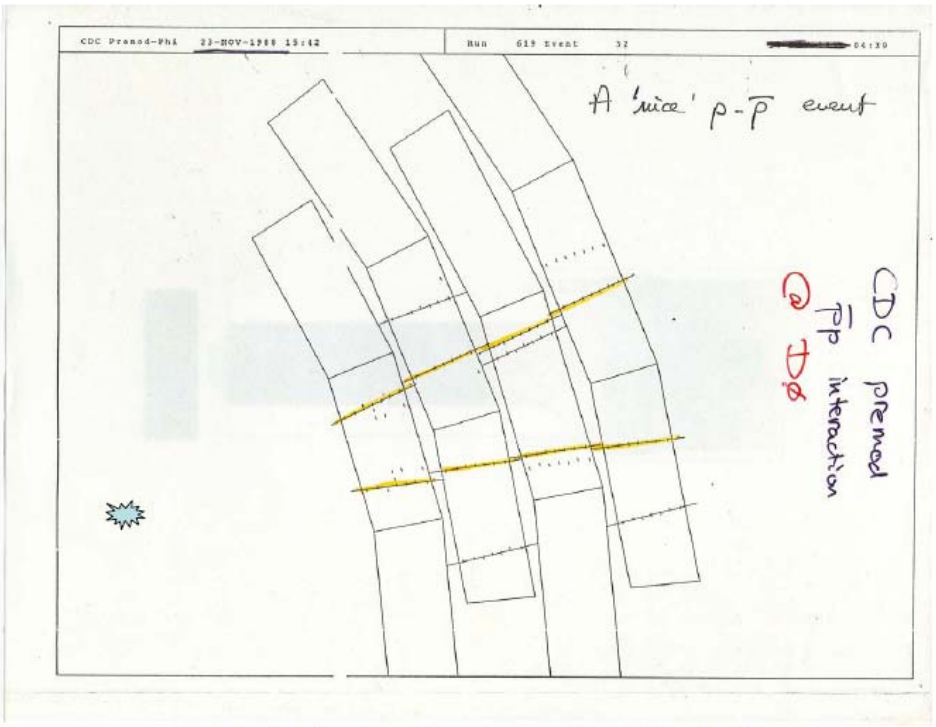
1983 Design Report

8 of 71 still left standing (on author list)

First DØ workshop at MSU in 1984 – working towards the DOE review TDR

Collaboration meeting 1985: "1988 or bust"

5



Central Drift Chamber premod in DØ in 1988

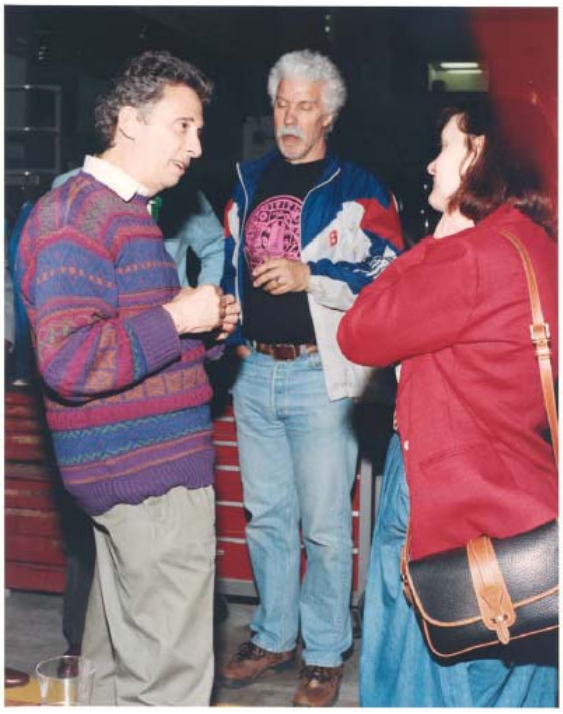
6

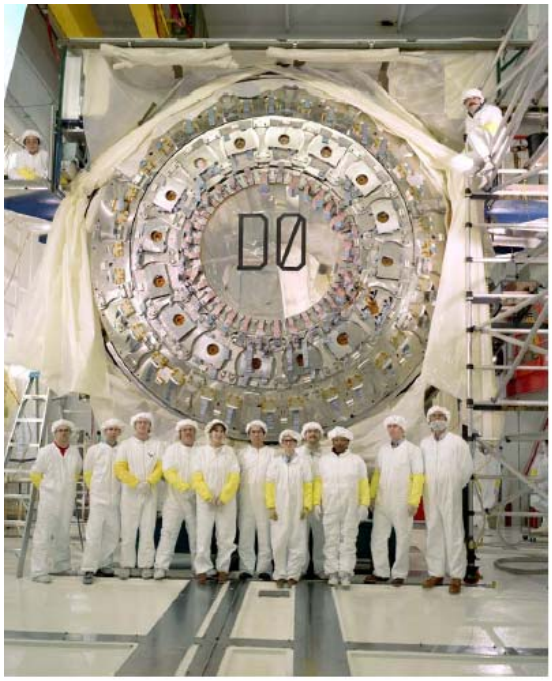
DØ in fall 1985 just after first collisions in CDF

Delmar Miller and uranium plates

Jim Christenson with John Peoples

Harry Weerts and ECEM signal board



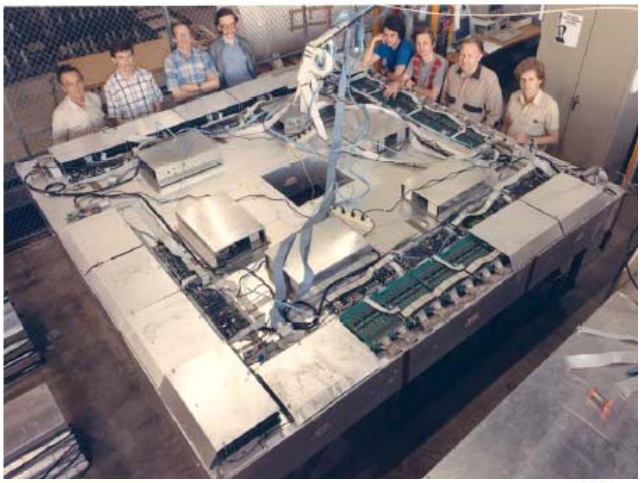


CC assembly

ECS assembly

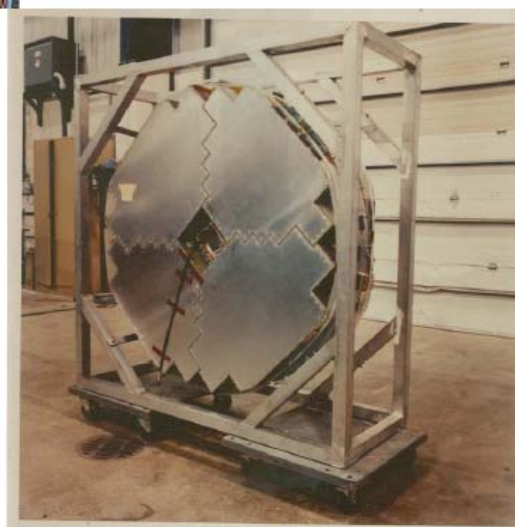
In the control room waiting for first collisions, April 1992

CF toroids and PDTs
with Gene Fisk



Protvino group and
SAMUS

Run I central drift chamber
(Ties Behnke and Guido
Finocchiaro)

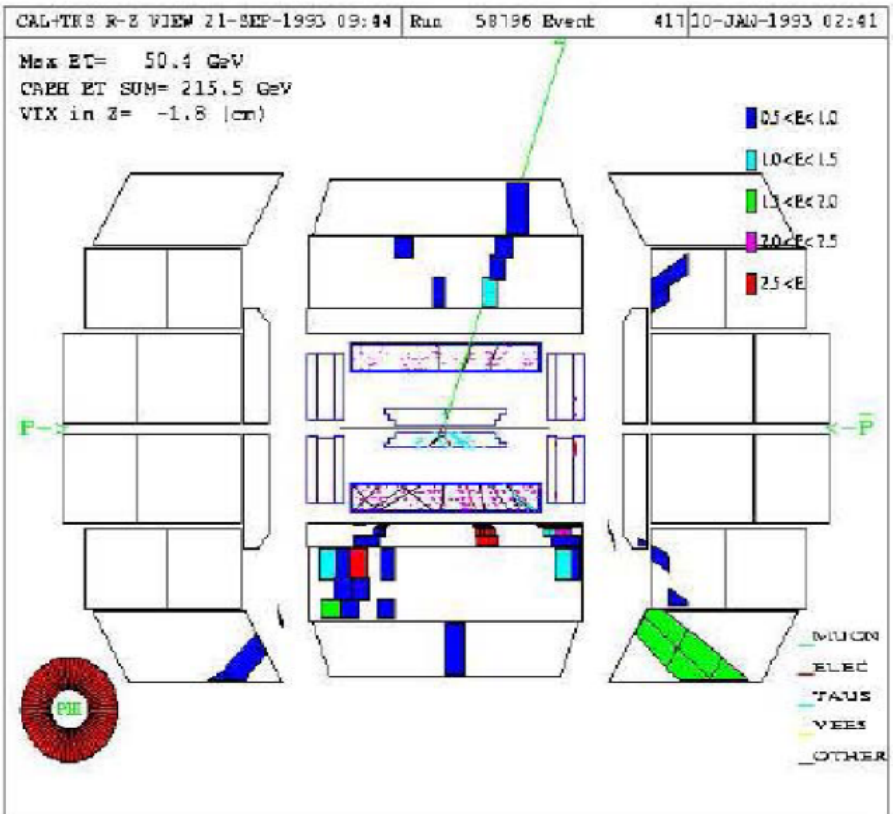


Run I forward drift chamber

13

Run I physics group leaders (and D0 admin group)

14



Event 417

DØ Collaboration
in 1995