From BEBC to DELPHI

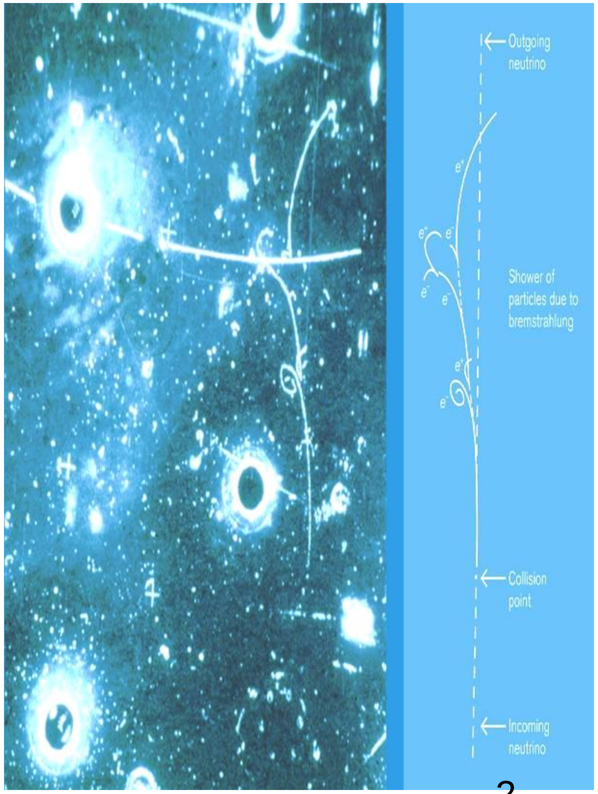
Some reminiscences and some physics highlights

Pre-BEBC

1964-1972: post-doc in Bristol (with Don Perkins) and CERN (Colin Ramm, Don Cundy) mostly on neutrino beams and on neutrino experiments in heavy liquid bubble chambers (Ramm's 1.2m, Gargamelle)

1971: met George in CERN, who recruited me to RAL still thinking of v expts, maybe in BEBC with a TST
RAL was the "home" of the TST (Colin Fisher)

1973-1974: At RAL on K^0 experiment (seeTrevor's talk), finishing GGM work, designing the SPS WB v beam, etc



BEBC proposal

1974: **The November J/Ψ Revolution :**

bare charmed hadrons should be produced

via $\nu_\mu \, d \rightarrow \mu^- \, c$ (then $c \rightarrow s$)

identifiable by single s-quark in final state

1975: Wrote BEBC+TST proposal using kinematic fits

plus detection of neutrals in Neon to find charm,

George helped round up collaborators

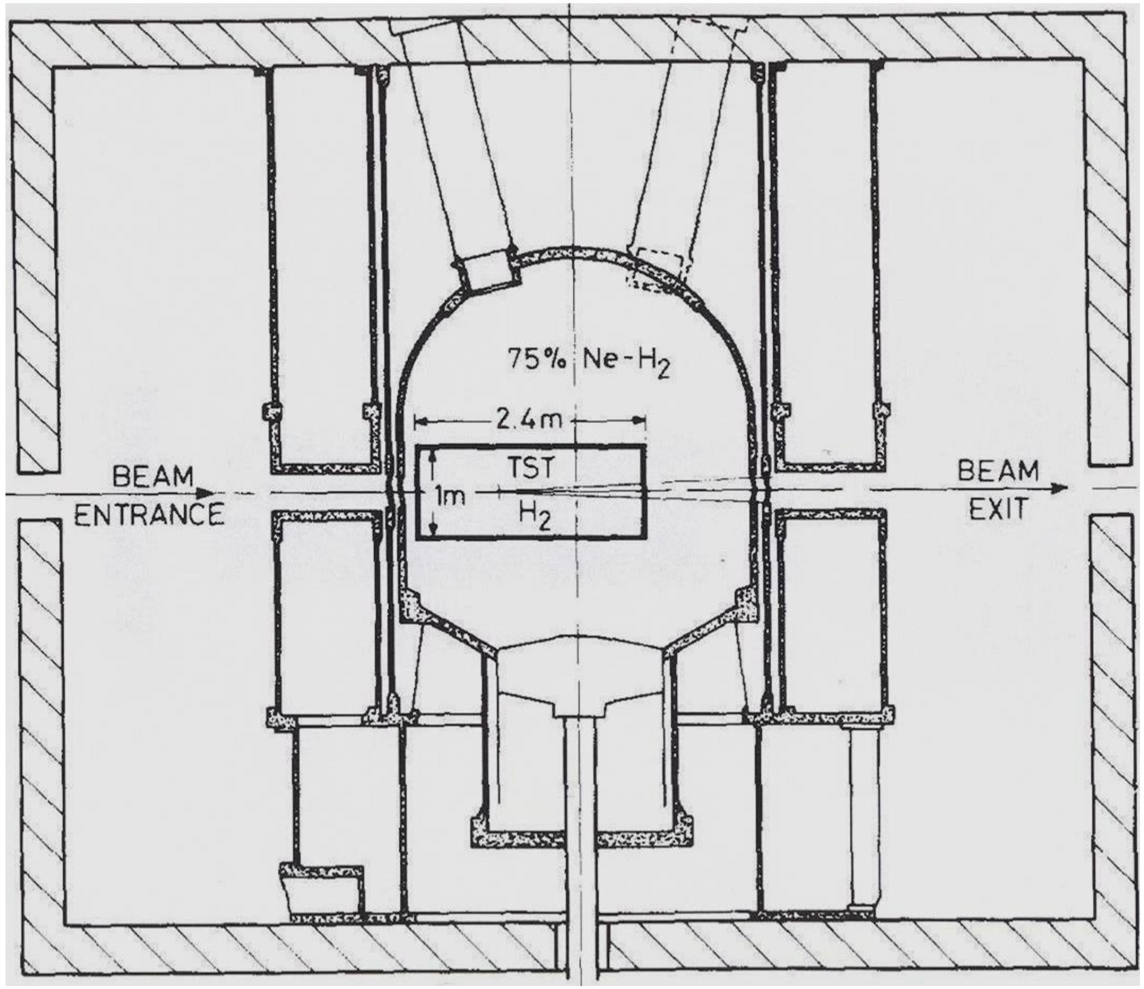
1976: TST built and tested, expt approved as WA24,

4-month sabbatical in FNAL (E180)

Track Sensitive Target

(TST)

inside BEBC

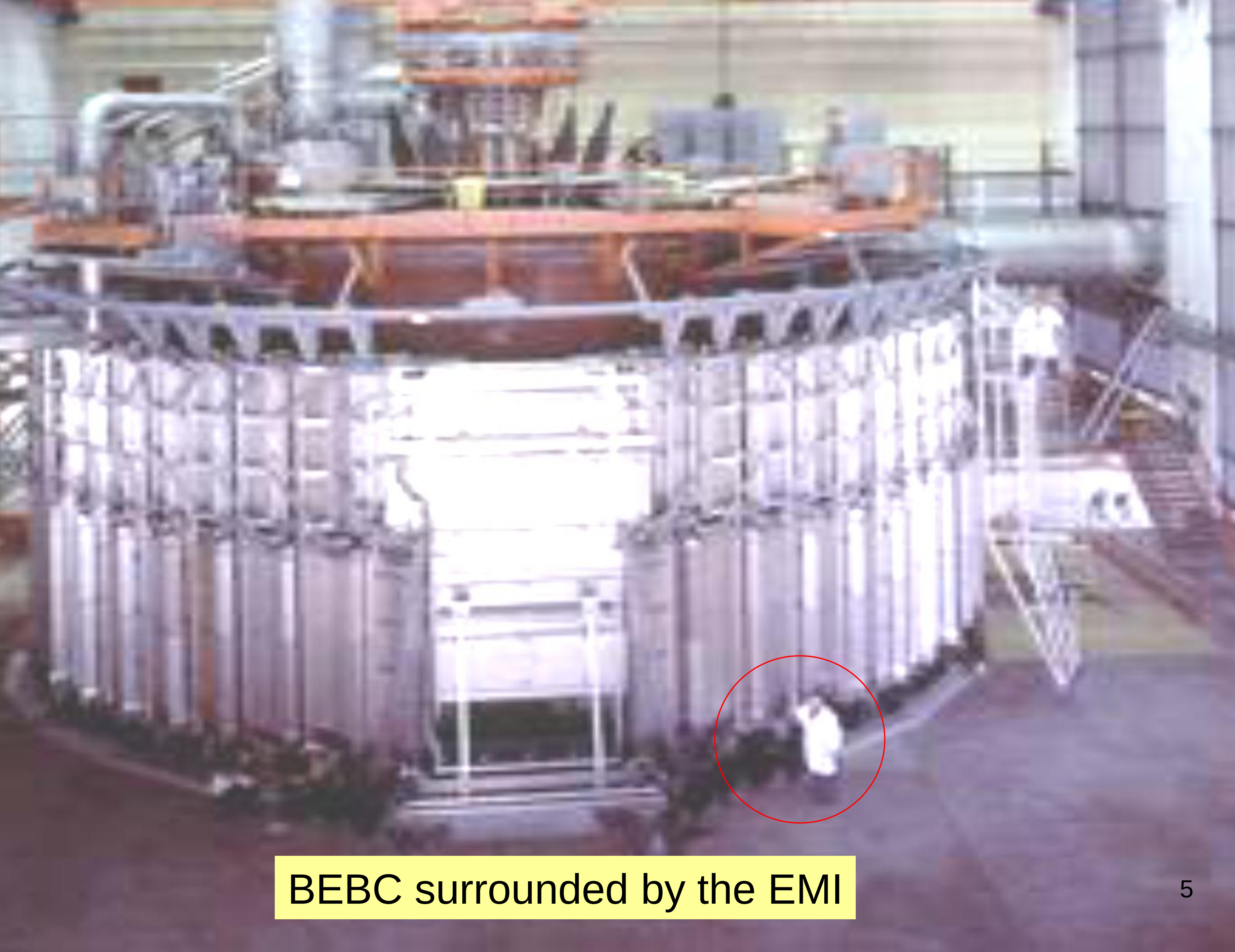


Pro: Interactions on free protons in hydrogen

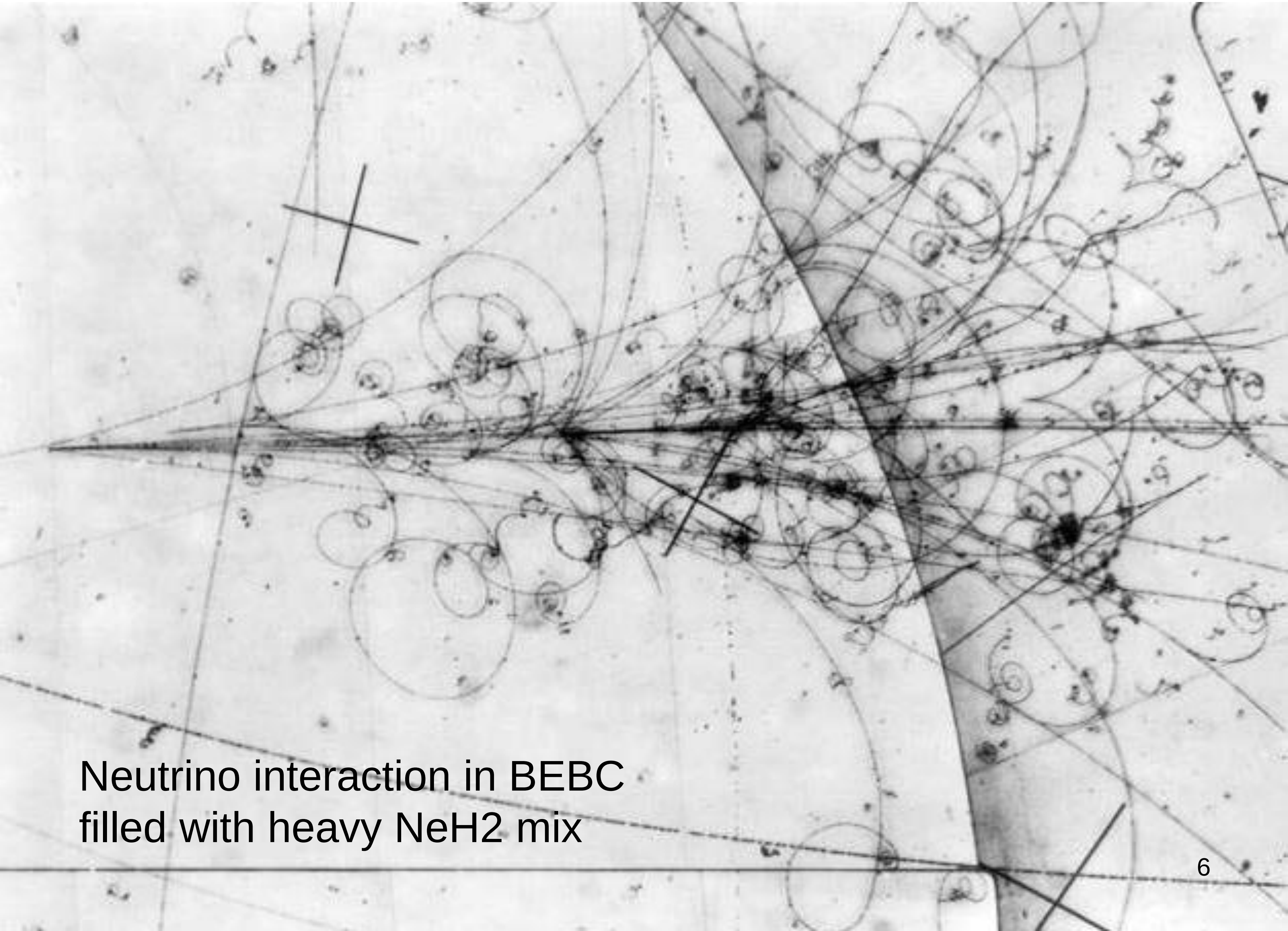
Detect neutrals / identify electrons in Ne-H2 mix (Rel. rise? Scint. light?)

Can compare H2 events and Ne events directly

Con: Much reduced event rate for neutrino beam



BEBC surrounded by the EMI



Neutrino interaction in BEBC
filled with heavy NeH2 mix

First data (WB test run)

1977: George learned WB test planned for April during weekend break in NB+Neon ν run

I proposed WA24 Collaboration take the data to check a rumoured excess of μe events seen elsewhere

Data taken with chamber hot, to get thin dense tracks (like in a TST) to maximise chance of seeing separate vertices

Sadly, no excess and no clearly separate vertices (just 1 "maybe" kink)

Result given at Hamburg Lepton-Photon Conf in July

(3-day run → 3 papers)

POSITRON PRODUCTION BY MUON NEUTRINOS IN BEBC FILLED WITH A NEON–HYDROGEN MIXTURE

Bari–Birmingham–Brussels–Ecole Polytechnique–Rutherford–Saclay–UCL Collaboration

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Istituto di Fisica dell'Università and Sezione INFN, Bari, Italy

D.C. COLLEY, G.T. JONES, L. LOWE, S. O'NEALE and S.J. SEWELL [2]
University of Birmingham, Birmingham, UK

G. BERTRAND-COREMANS, H. MULKENS [3], J. SACTON,
C. VANDER VELDE-WILQUET and J. WICKENS [3]
Inter-University Institute for High Energies, U.L.B., V.U.B., Brussels, Belgium

V. BRISSON and P. PETIAU
Ecole Polytechnique, Palaiseau, France

H.H. BINGHAM [4], W. CAMERON, D.J. CRENNELL, J.G. GUY,
G.E. KALMUS, A.G. MICHETTE, T. MYNDEL and W. VENUS
Rutherford Laboratory, Chilton, Didcot, UK

J. ALITTI, J.P. BATON, G. GERBIER, C. KOCHOWSKI and M. NEVEU
Dèpartement de Physique des Particules Elémentaires, CEN-Saclay, France

and

N.J. BAKKER [2], J.H. BARTLEY, F.W. BULLOCK, D.H. DAVIS and T.W. JONES
University College London, London, UK

Received 18 May 1978

For e^+ energy > 0.3 GeV and 10 GeV $<$ visible energy < 100 GeV we find that: (i) $f = (\nu_\mu + \mathrm{Ne} \to \mu^- e^+)/(\nu_\mu + \mathrm{Ne} \to \mu^-) = (0.41 \pm 0.15)\%$; (ii) 1.2 ± 0.5 neutral strange particles are produced per $\mu^- e^+$ event; (iii) the lifetime of possible positron-parent particles is $< 3 \times 10^{-12}$ s (90% C.L.); (iv) the cross section for direct e^+ production via the neutral current is < 0.2 times that via the charged current (90% C.L.); (v) the cross section for producing heavy leptons, L^+, decaying into e^+ ... is $< 0.7 \times 10^{-3}$ times that for μ^-production, implying $M(L^+) > 10$ GeV.

Implications for TST run of 17 μe events seen:

~0.4% μe rate → ~3% charm rate, so maybe 2% rate with p target

Charm flight paths unlikely to be visible

Revving up!

George proposed a companion experiment
looking for prompt electrons from charm
in 70 GeV/c π^- interactions in the TST

Runs with TST in BEBC scheduled for first half of 1978

Horst Wachsmuth and I started a Neutrino Beam Users Group
aimed at determining the Wide Band Beam neutrino spectrum.
This led to expt NA20 to measure $\pi^{\pm}$ and $K^{\pm}$ production spectra

George started a much more general BEBC Users Group
and chaired it for its first ~3 years

And George organised a Neutrino Conference in Oxford
for September 1978

Second data (TST run)

1978: Data taken March → July, double-pulsing

with 70 GeV/c π^- (George) or pbar (Jacques)
both looking for prompt electrons from charm

Operation was "very delicate" :

- TST piping began to leak (~2% of H-like events really on Ne, needed over-pressure led to 77 mole-% Ne dropping to 68 mole-% Ne over time)
- Dirt accumulated on top of TST (masks, masks, and more masks …)
- GGM's "Weinberg electrons" story (they proved not to be real)

But it was the most successful TST run ever

(over 1 million pictures taken, neutrinos + hadrons)

Physics highlight

Unique 3C-fit with a single s-quark (i.e. from c-decay !!) in final state

$\nu p \rightarrow \mu^- \Sigma_c^+ \pi^+$

With $\Sigma_c^+ \rightarrow \Lambda_c^+ \pi^0$

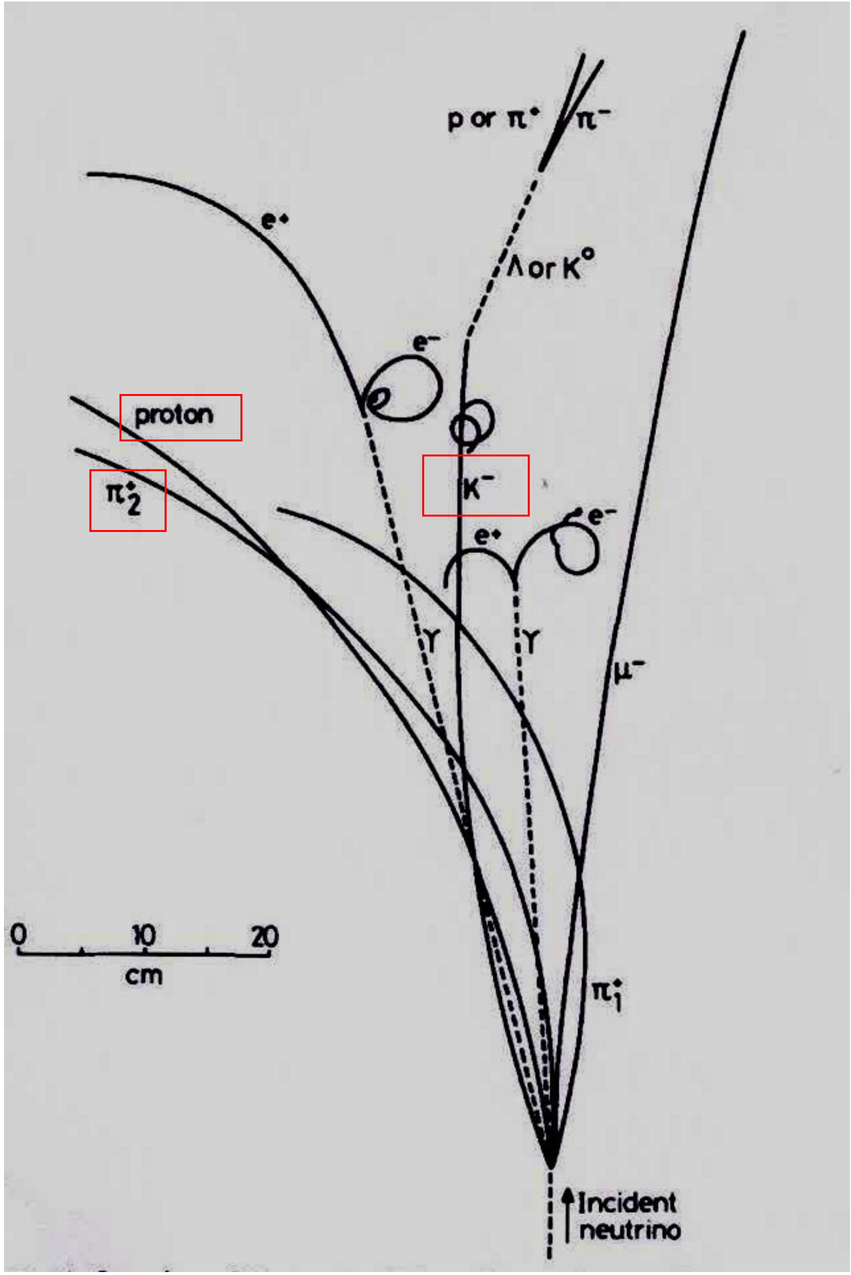
And $\Lambda_c^+ \rightarrow \pi^+ p K^-$

With $M(\Sigma_c^+) = 2457 \pm 4$ MeV

$M(\Lambda_c^+) = 2290 \pm 3$ MeV

$\Delta M = 168 \pm 3$ MeV

Still within ~1-σ of PDG values



FIRST OBSERVATION OF THE PRODUCTION AND DECAY OF THE Σ_c^+

BEBC TST Neutrino Collaboration

M. CALICCHIO, O. ERRIQUEZ, M.T. FOGLI-MUCIACCIA,
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R. BELUSEVIC, D.C. COLLEY, G.T. JONES, S. O'NEALE,
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Inter University Institute for High Energies ULB-VUB, Brussels, Belgium

H. LEUTZ and H. WENNINGER
CERN, Geneva, Switzerland

← the fathers of the TST!

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University College, London, England

Σ_c^+ was not seen again for 13 years
(then ~111 in CLEO2, now 660)

Received 28 April 1980

An event with the decay chain $\Sigma_c^+ \to \Lambda_c^+ + \pi^0$, $\Lambda_c^+ \to K^- + p + \pi^+$, has been observed in an exposure of BEBC, equipped with a track sensitive target, to the wide band neutrino beam from the SPS at CERN. The event has a unique three constraint kinematic fit to the $\Delta S = -\Delta Q$ reaction $\nu + p \to \mu^- + p + K^- + \pi^+ + \pi^+ + \pi^0$ with both gammas from the π^0 decay detected. The proton and other final state particles are identified. The masses are $M(\Lambda_c^+) = 2290 \pm 3$ MeV/c^2, $M(\Sigma_c^+) = 2457 \pm 4$ MeV/c^2 and $M(\Sigma_c^+) - M(\Lambda_c^+) = 168 \pm 3$ MeV/c^2. Including other data one obtains $M(\Sigma_c^{++}) - M(\Sigma_c^+) = 0 \pm 4$ MeV/c^2

Same again, or different?

Nice result, but ……..

- a lot of hard work, and run always on edge of collapse
- no other candidates convincing enough to publish
- SPEAR and others beginning to clean up charm area
- would need high resolution optics in 5^{th} camera port
- and would need to fix dirt problem first (but how?).

So, George → SLAC γp experiment (see Jim Brau's talk)
self → QCD studies (WA59)
with neutrinos/antineutrinos into BEBC with simple NeH2 mix

WA59 analysis in 3 phases

Phase 1, WA59 alone

Improved measurement of Λ_{QCD} inc. higher twist effects

At low Q^2, weak current really does behave like a hadron:

---- coherent production, shadowing

Many studies of particle production etc etc

Phase 2, compare WA59 data with WA21 and WA25 data

Compare with H_2 data (WA21) → structure fns,

formation lengths, …

Compare with D_2 data (WA25) → EMC effect

Phase 3, combine all combinable data sets:

From 1988 all data shared with Russian ex-E180 groups (ITEP, IHEP)

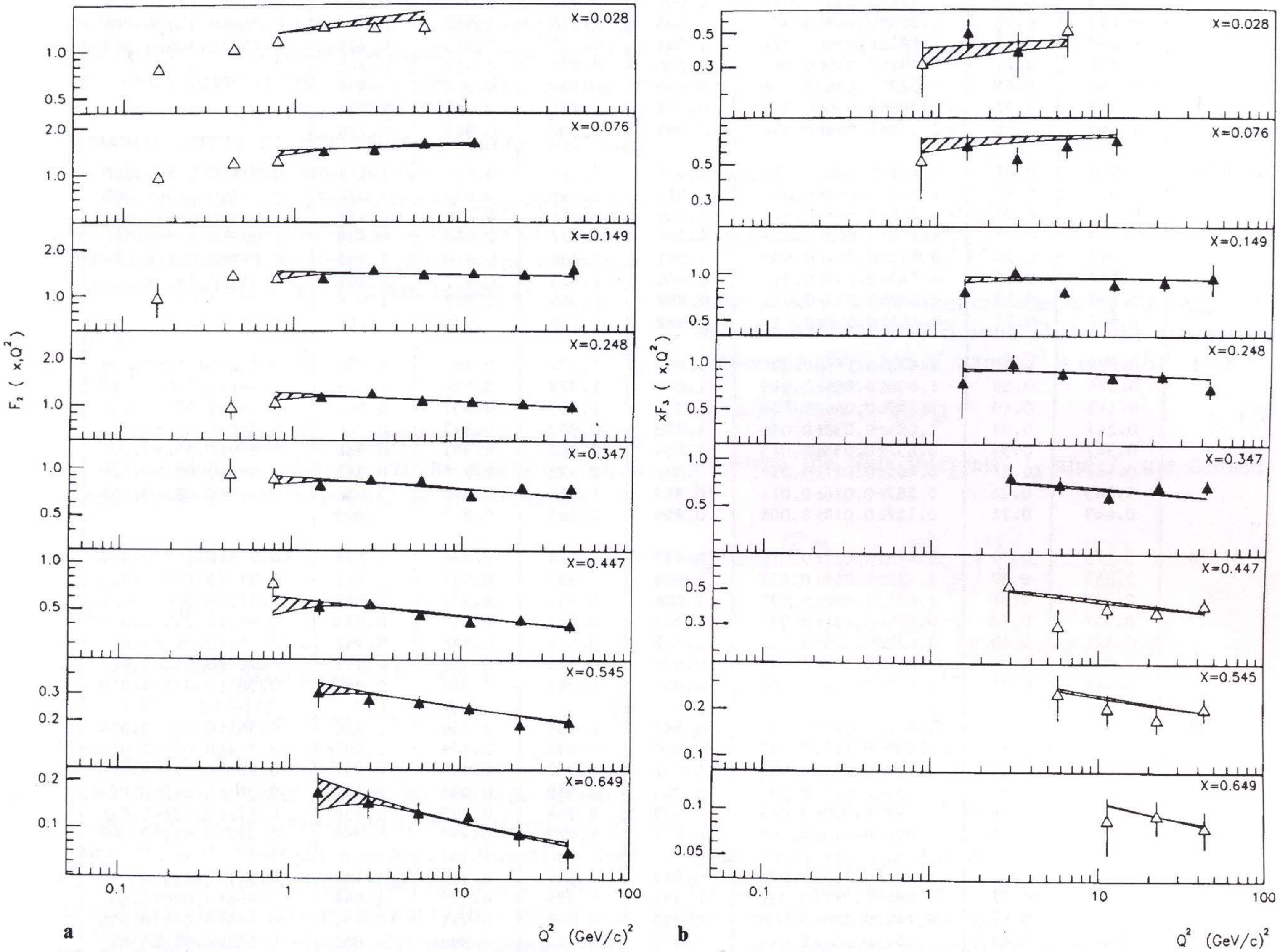
→ analyses with WA21 + WA25 + WA59 + E180 + E632 data

(BEC, diffractive F*, ….)

(and many interesting insights into Soviet Union and its collapse!)

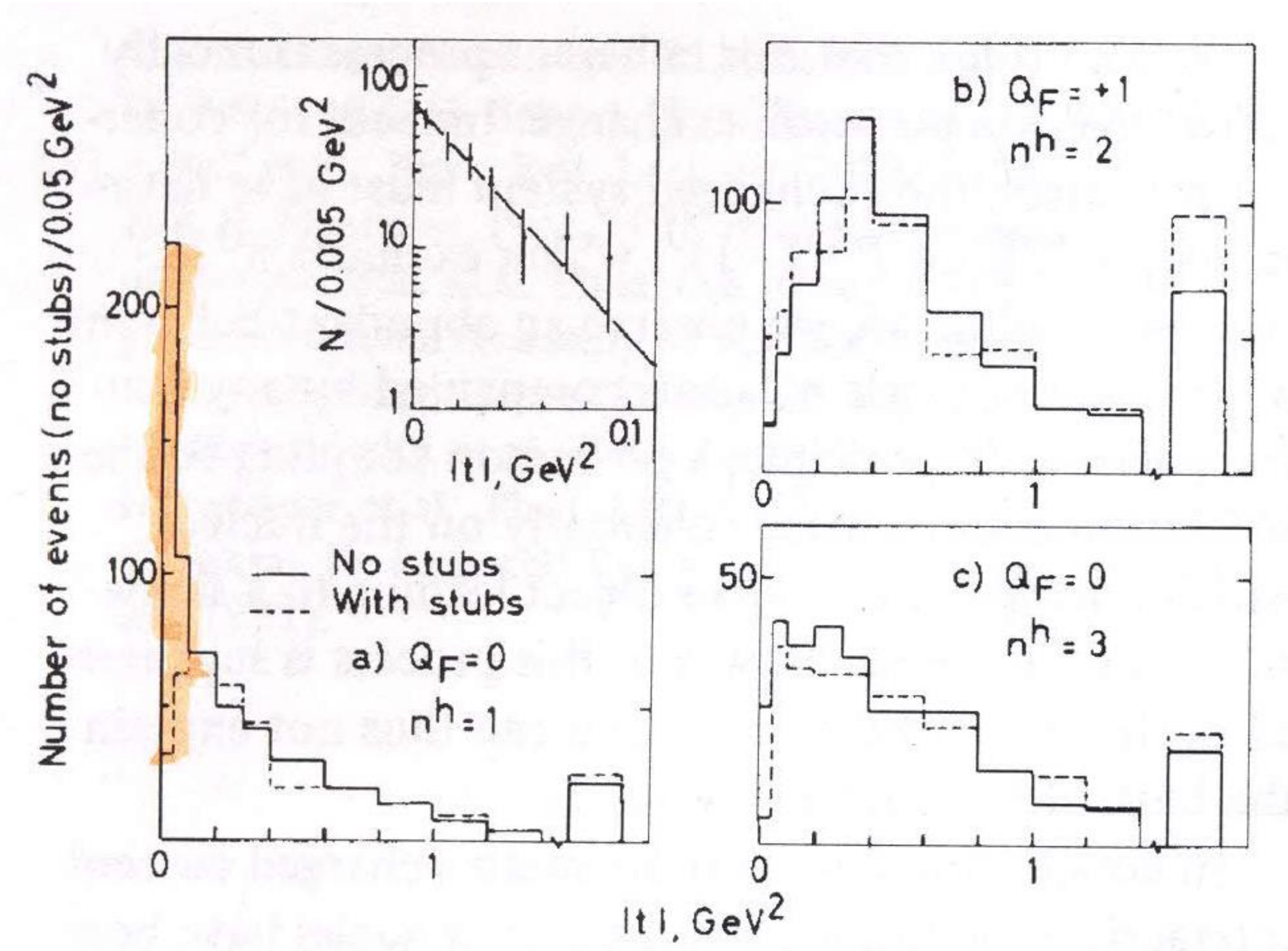
10 day run, 33 papers

Highlight 1 from Phase 1: The WA59 structure functions



25k CC events, 4 times previous stats, small ΛQCD~100 MeV, 2-3 σ –ve HT

Highlight 2 from Phase 1: The inclusive ooherent production signal



1 percent of CC events are coherent production off Neon nucleus

COLLEPS

“Collaboration for LEP Studies”

One day in summer 1980 I met Ugo in CERN more or less by chance
(we knew each other from BEBC and CHARM and an ep study group)

“Why not join our LEP study group?” said he.

I discussed with George, we discussed with the group
“LEP will be only game in town in 10 years, so why not?”

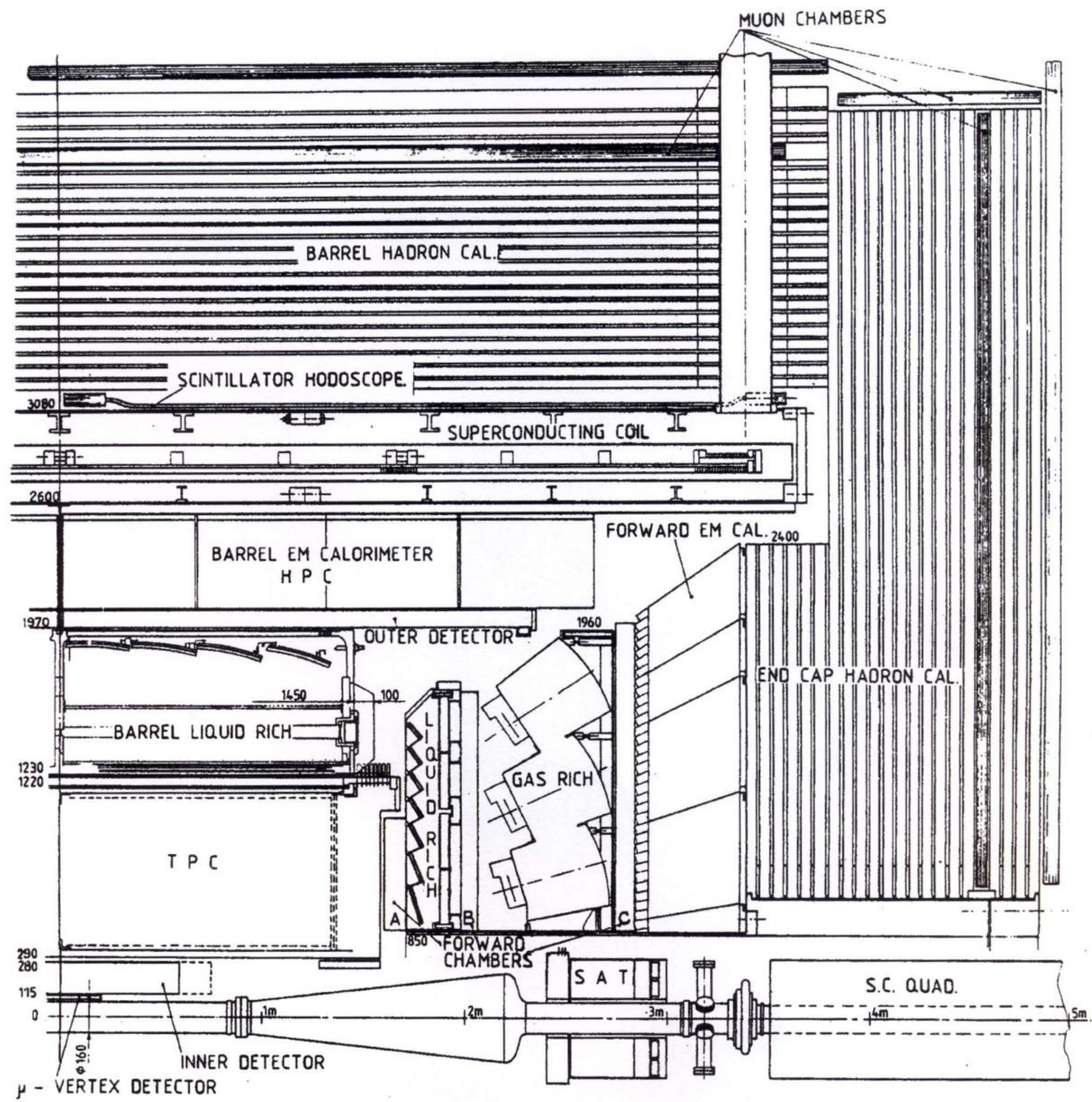
So we did.

In summer/autumn 1981, COLLEPS morphed into DELPHI

DELPHI

"Risky" items:

SC magnet
TPC
HPC
Barrel RICH
Fwd RICH



Letter of Intent Jan 1982, Technical Report mid 1983, approved as the "very ambitious" LEP experiment

DELPHI

A DETECTOR WITH LEPTON, PHOTON AND HADRON IDENTIFICATION

Letter of intent for an experimental program at LEP

DELPHI COLLABORATION

GROUP	CONTACTMAN
Collège de France	M. Crozon
Ecole Polytechnique	M. Urban
Orsay	F. Richard
Paris – LPNHE	M. Baubillier
Saclay	G. Smadja
Strasbourg	A. Degré
Karlsruhe	J. Engler
Wuppertal	J. Drees
Oxford	G. Myatt
Rutherford	W. Venus
Athens – Nat. Tech. Univ.	T. Filippas
Athens	L. Resvanis
NIKHEF – Amsterdam	F. Udo
INFN – Bologna	L. Monari
INFN – Genoa	M. Bozzo
INFN – Milan	A. Pullia
INFN – Padua	L. Ventura
INFN – Rome – Sanità	C. Bosio
Bergen	E. Lillestøl
Oslo	T. Buran
Cracow	K. Rybicki
Lund	G. Jarlskog
Stockholm	G. Ekspong
Uppsala	S. Kullander
CERN	J.V. Allaby

Soon after the Letter of Intent, I largely went back to neutrinos for ~5 years (including joining WA66)

But George remained very involved in DELPHI, leading RAL and UK groups and being a founder-member of DEC, DELPHI's 8-man Executive Committee, & Chairman of the Collaboration Board (see Ugo's talk).

1986: John Thresher went to CERN as Research Director, George agreed to "keep his seat warm" for 3 years, I did the same for George

When John eventually returned in 1992 he declined to retake his seat, and so George ~reluctantly became full time PPD leader

DELPHI

We discussed what our bubble chamber group should do in DELPHI

Strong on software skills → online and offline software
was a **massive** effort for the whole of the next (nearly) 20 years

Mike regained an interest in hardware → he and Bob Ely worked with PAG
on design parameters and construction of the HPC ("a lead-filled TPC")
Their ideas were in the Technical Proposal but weren't finally adopted

George was enthused by the physics potential of the Micro-vertex detector
which had been proposed by Weilhammer's group
(and was already in the Technical Proposal)

Mike too, so Mike became our silicon expert working initially with Micron
and later went out to CERN and became Project Leader
(and the rest is history …)

From the Technical Proposal

Reality:
2-layer at start of 1990
3-layer at start of 1991
with smaller beam-pipe,
Z read-out throughout
added at start of 1994
with double-sided
detectors

PART II: DETECTOR COMPONENTS AND PERFORMANCE

2.1 MICROVERTEX DETECTOR

Development work is in progress for the construction of a high resolution silicon vertex detector [2.1.1].

It is intended to arrange it as concentric cylinders of silicon wafers, (fig. 2.1.1(a,b)). A cylinder will be built up from an assembly of 30 unit cells, each consisting of a thin quartz frame onto which detectors and readout electronics are glued (fig. 2.1.2). The angular range covered will be 2π in azimuth, and down to 32° away from the beam axis. About 9% of each surface will be inactive. It is proposed to install 1 cylinder initially. However, if the development proceeds well, the inner detector can be modified to allow the insertion of three cylinders.

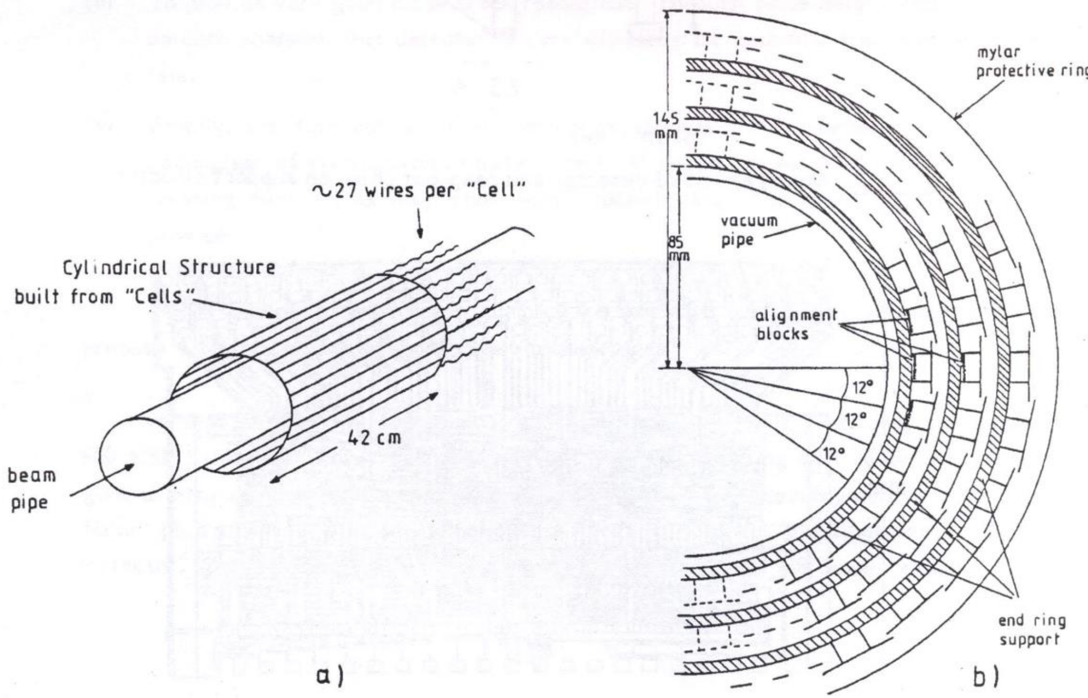
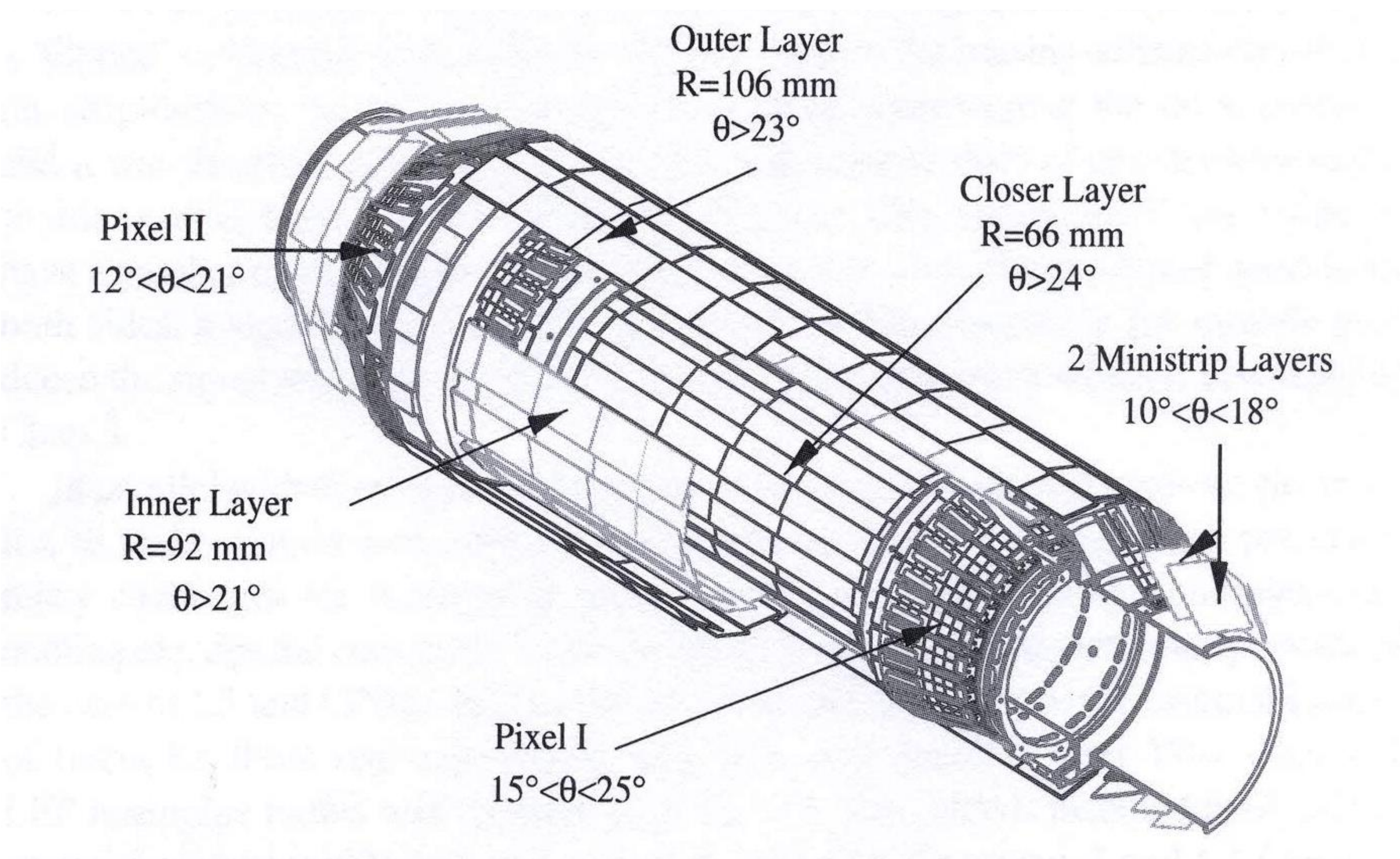


Fig. 2.1.1 Microvertex detector. (a) General view, (b) transversal cross section

Such a good idea that all 4 LEP experiments had vertex detectors by 1994 :

	ALEPH	DELPHI	L3	OPAL
Reference	[4]	[5]	[6]	[7]
No. layers	2	3	2	2
$\langle r_1 \rangle$ (cm)	6.3	6.3	6.1	6.2
$\langle r_2 \rangle$ (cm)	–	9.0	–	–
$\langle r_3 \rangle$ (cm)	10.7	10.9	7.8	7.7
No. detectors	96	288	96	75
Silicon area (m^2)	0.25	0.42	0.30	0.15
Readout-chip	CAMEX64A (3.5 μm CMOS)	MX3/MX6 (3 μm CMOS)	SVXD (3 μm CMOS)	MX5/MX7 (1.5 μm CMOS)
Signal/noise	17	13–16	–	22

Delphi's for LEP2 Higgs search, installed in 1997, with pixelled end-caps, was really (for its time) quite an impressive monster：



Physics Highlights?

Too many to describe them all

For example:

- B^0_s Λ_b Ξ_b observations & lifetimes, b,τ lifetime, B^0_d oscillations (and nearly B^{0s} oscillations), etc
- Very many QCD studies
- etc etc

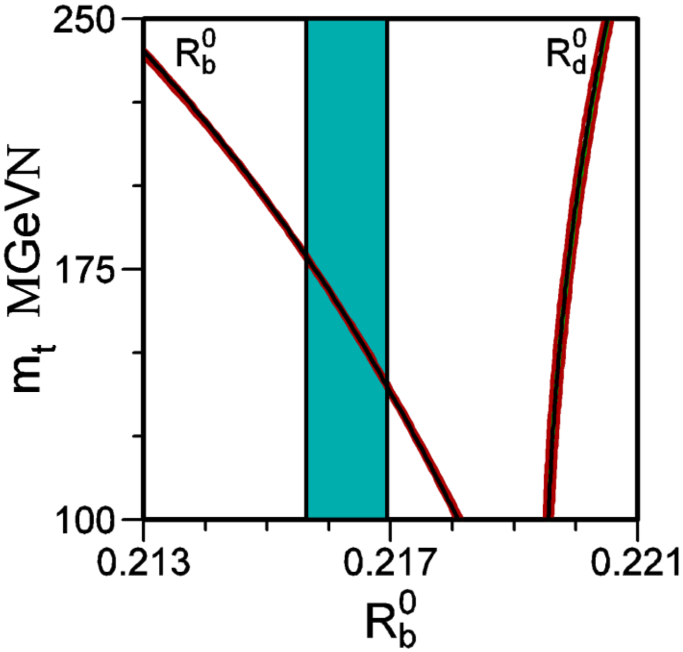
But my choice: precise Standard Model tests

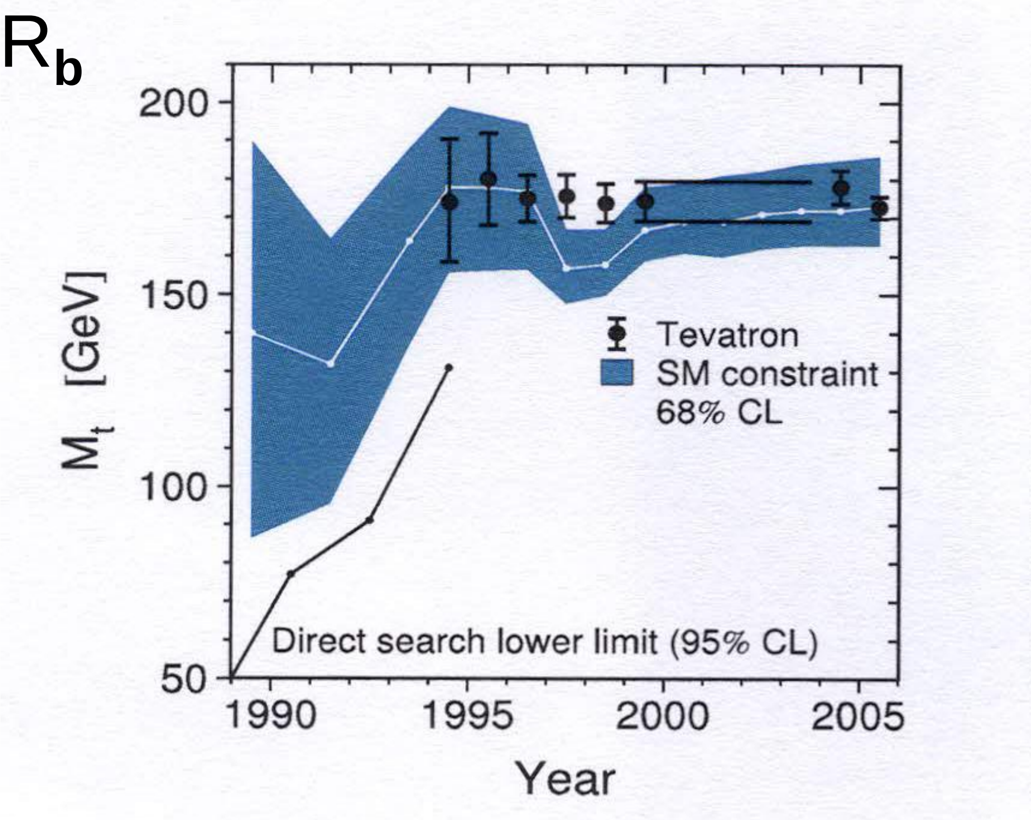
Hadronic b-bbar fraction R_b

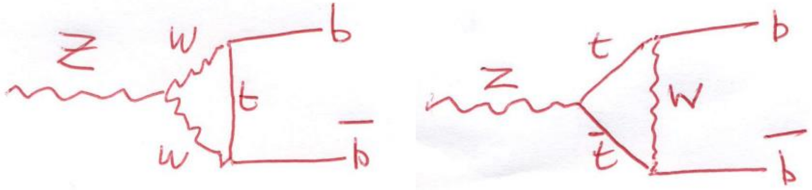
Purely from b-tag

Early R_b measts showed
b ≠ iso-singlet so t exists

LEP's best predictor of m_t



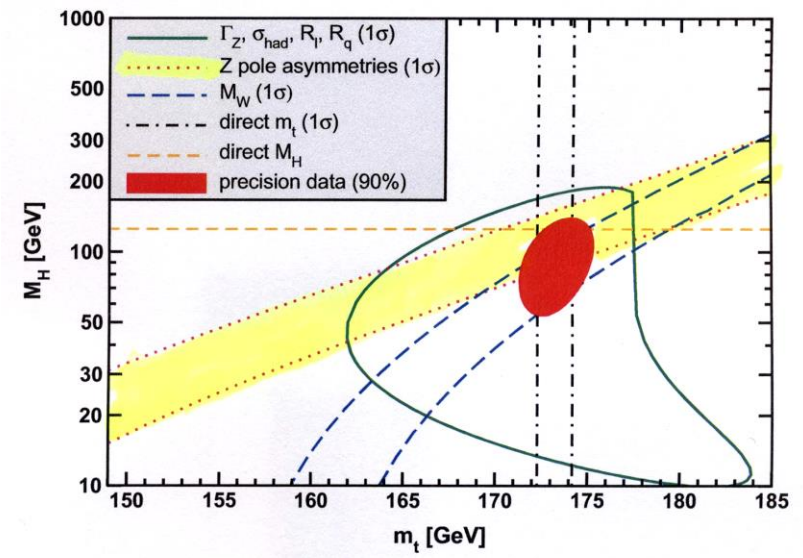


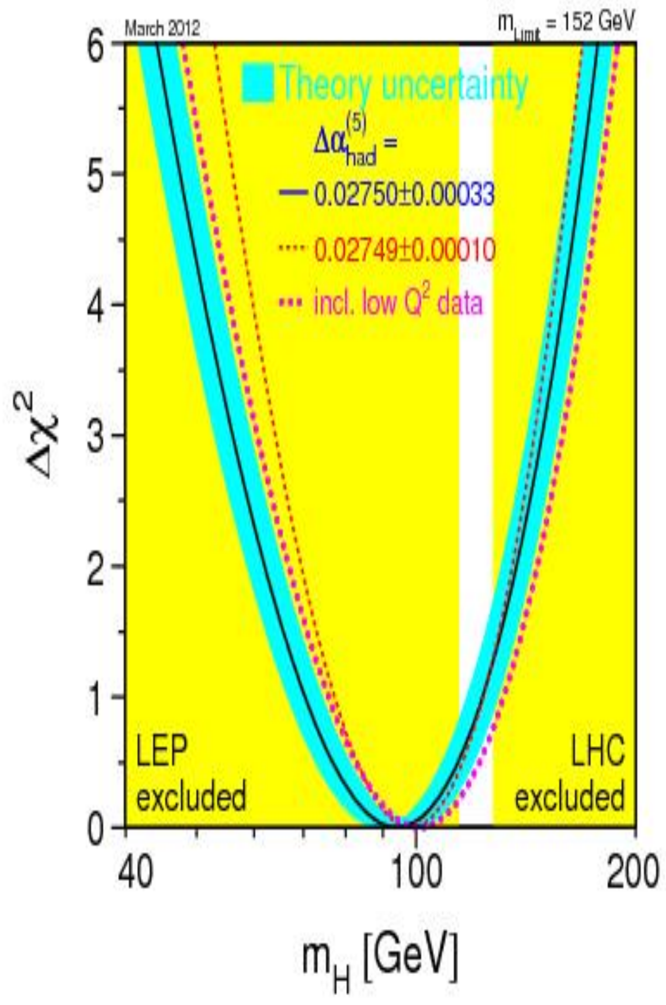


Forward-backward b-bbar asymmetry

~ half from b-tag + jet-charge
~ half from high-pt leptons

With m_W
LEP's best
measure of
$\sin^2\theta_W$
and hence
in SM
of m_H too
once m_t
Is known





	- 1 - all Z-pole data	- 2 - all Z-pole data plus m_t	- 3 - all Z-pole data plus m_W, Γ_W	- 4 - all Z-pole data plus m_t, m_W, Γ_W
m_t [GeV]	173^{+13}_{-10}	$173.2^{+0.9}_{-0.9}$	$178.1^{+10.9}_{-7.8}$	$173.3^{+0.9}_{-0.9}$
m_H [GeV]	118^{+203}_{-64}	122^{+59}_{-41}	148^{+237}_{-81}	94^{+29}_{-24}
$\log_{10}(m_H/\text{GeV})$	$2.07^{+0.43}_{-0.34}$	$2.09^{+0.17}_{-0.18}$	$2.17^{+0.41}_{-0.35}$	$1.97^{+0.12}_{-0.13}$
$\alpha_S(m_Z^2)$	0.1190 ± 0.0027	0.1191 ± 0.0027	0.1190 ± 0.0028	0.1185 ± 0.0026
χ^2/dof (P)	16.0/10 (9.9%)	16.0/11 (14%)	16.5/12 (17%)	16.9/13 (21%)

EWWG2

The successful predictions of the
top mass (red boxes) and Higgs mass (green box)
seem to me to confirm the Standard Model
quite astonishingly!

Cf Global PDG fit (includes e.g. g_μ-2) :

$m_t = 177.0 \pm 2.1$ GeV

$m_H = 89^{+22}_{-18}$ GeV

We didn’t find the Higgs

But only because LEP’s energy fell 5% short

If LHC had (most unfortunately) not been approved and resources had therefore been applied instead to maximising LEP’s energy for the Higgs search

there was room for 80-100 more SC RF cavities
needing more cooling but no expensive civil engineering

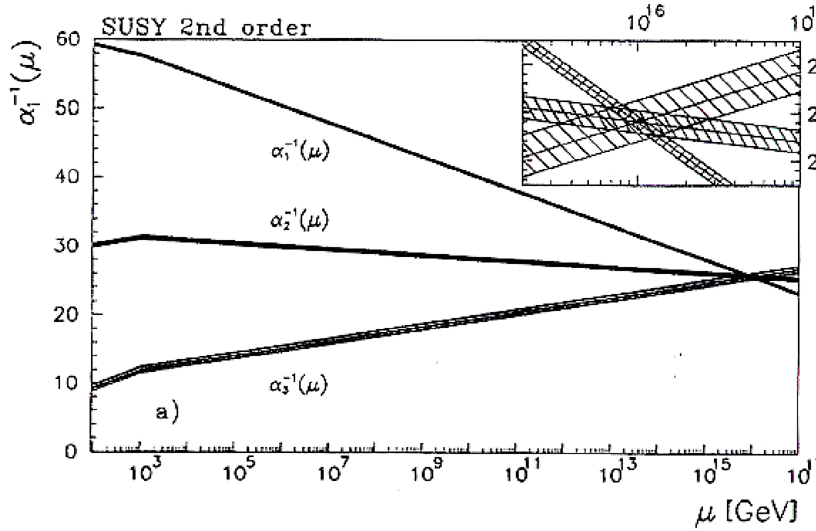
Bill would this afternoon have been talking about the Higgs discovery at LEP circa 2003

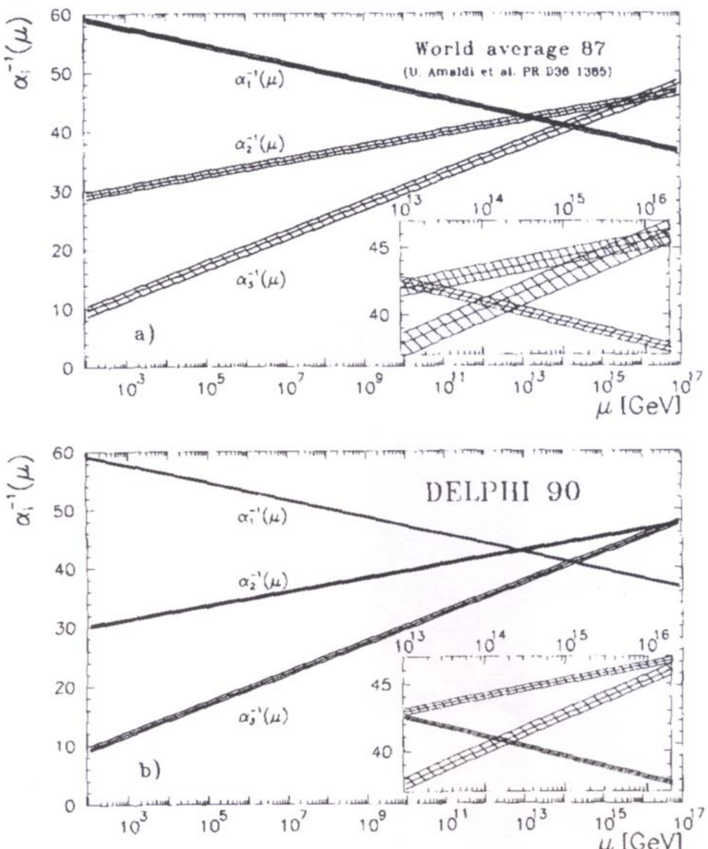
60 more cavities would have done it

And LEP3 would probably now be running as a Higgs factory

Can’t not mention
The “Grand Desert” & the SUSY scale

A TopTen HEP paper for many years!





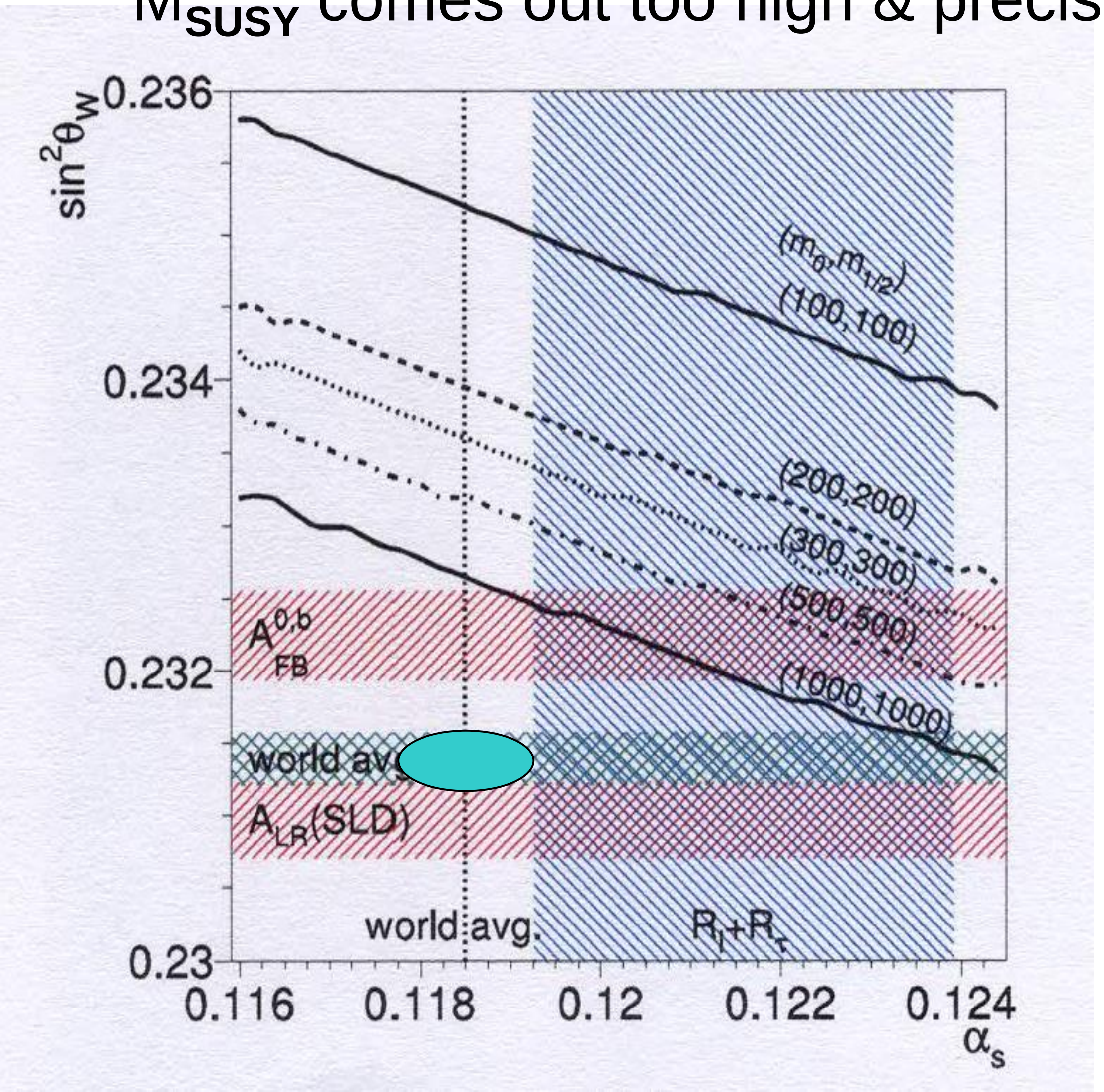
Amaldi, De Boer, Furstenau!

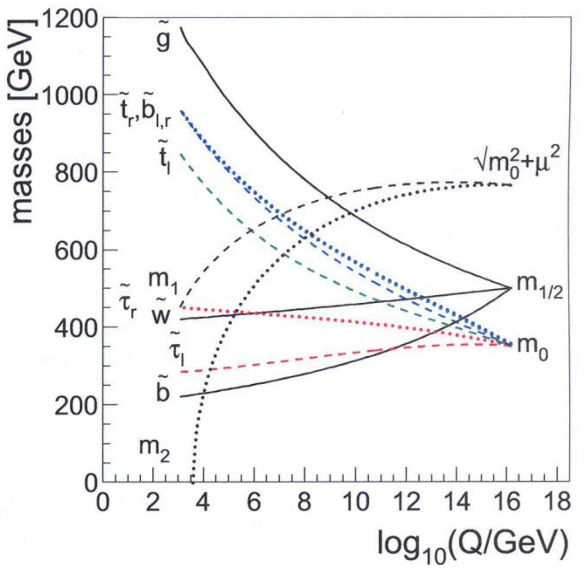
Strong, weak and EM couplings no longer unified in Standard Model, but could unify with SUSY

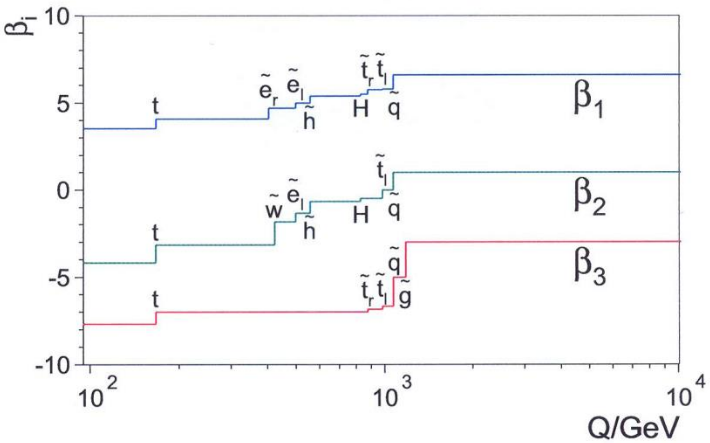
In 1990: $M_{SUSY} = 10^{(3.0 \pm 1.0)}$ GeV

But now the PDG says all the inputs (M_Z, $\sin^2\theta_W$, α_s) are all at least ×10 better known!
So are there now very tight constraints on SUSY models??

M_{SUSY} comes out too high & precise for general comfort!







PDG: $\alpha_s = 0.1185 \pm 0.0006$

$\sin^2\theta_W = 0.23126 \pm 0.00005$

$m_0 = 350$ MeV

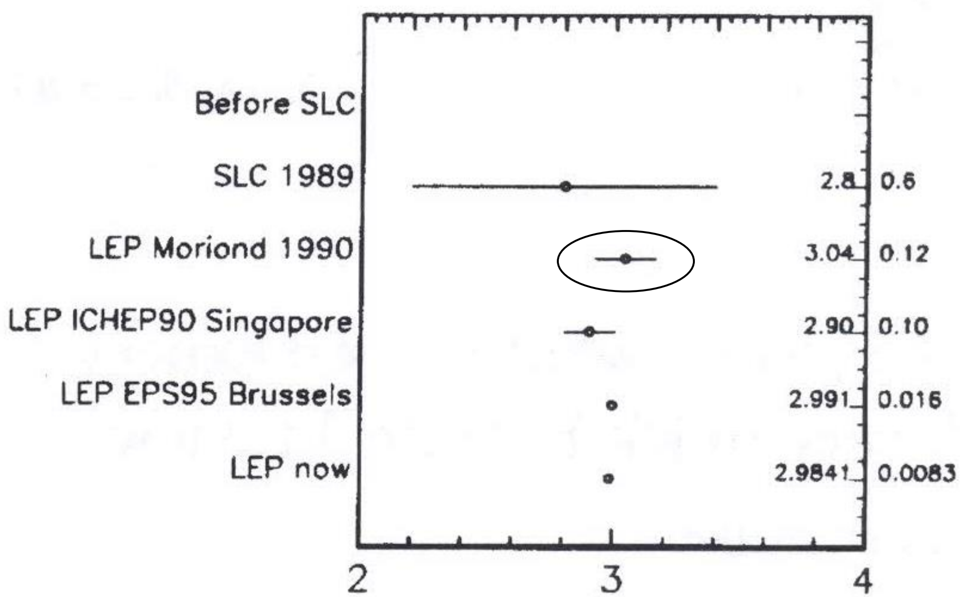
$m_{1/2} = 500$ MeV

Tan $\beta = 50$

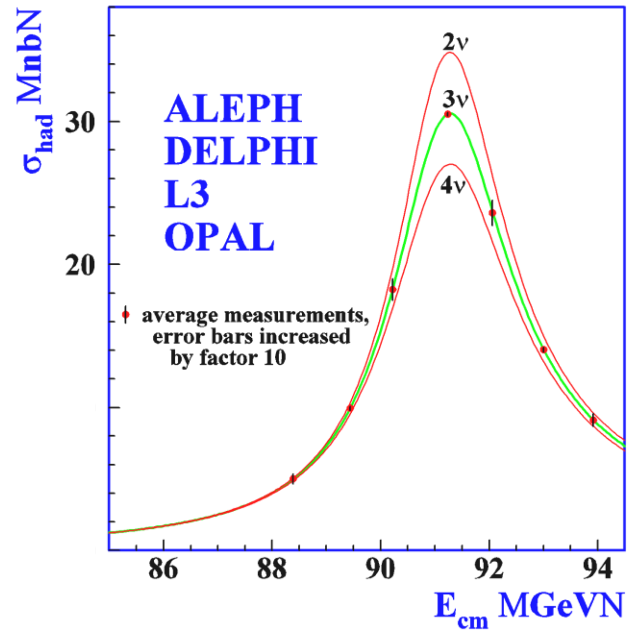
And can't not mention this:

Only 3 light neutrinos:

A major discovery made jointly by the 4 LEP collaborations within weeks of start-up



Latest EWWG version

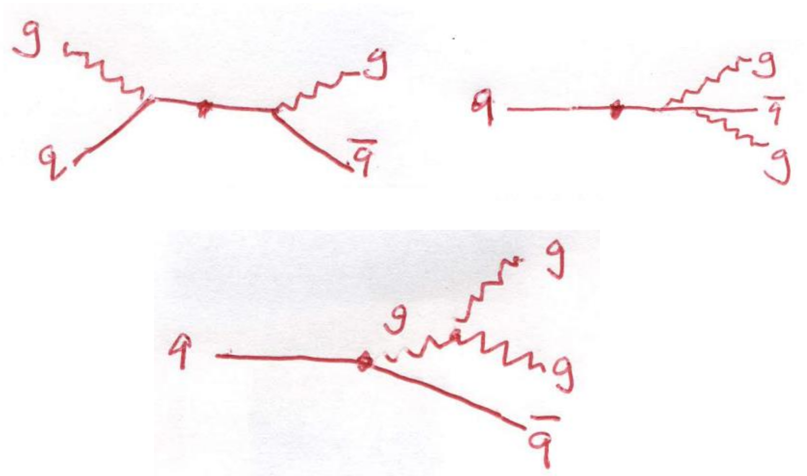


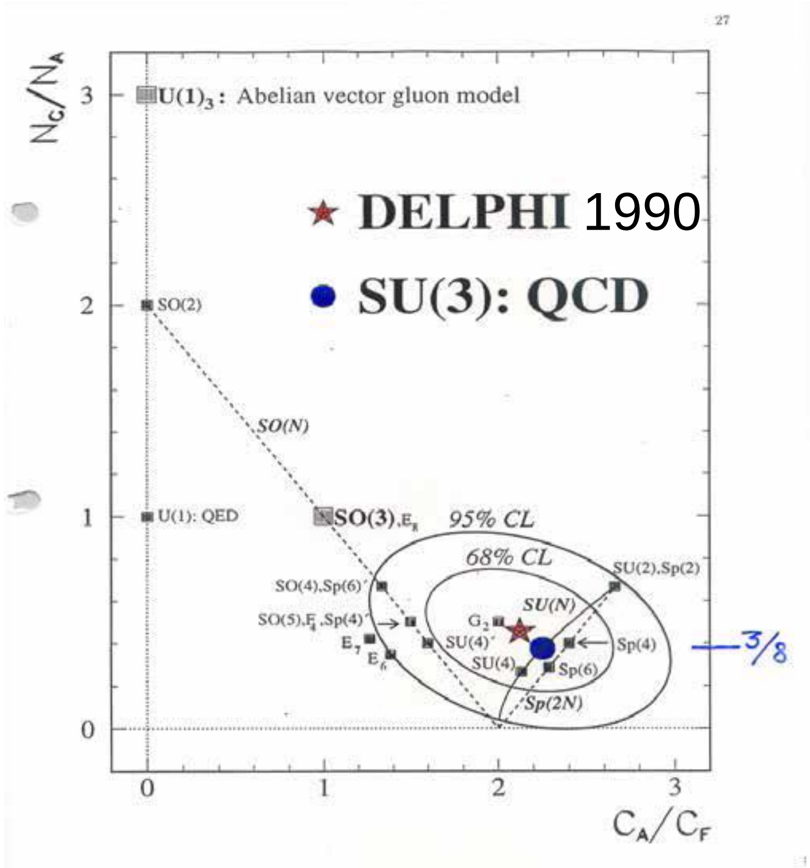
now N_ν = 2.9842 ± 0.0082 (EWWG fit)

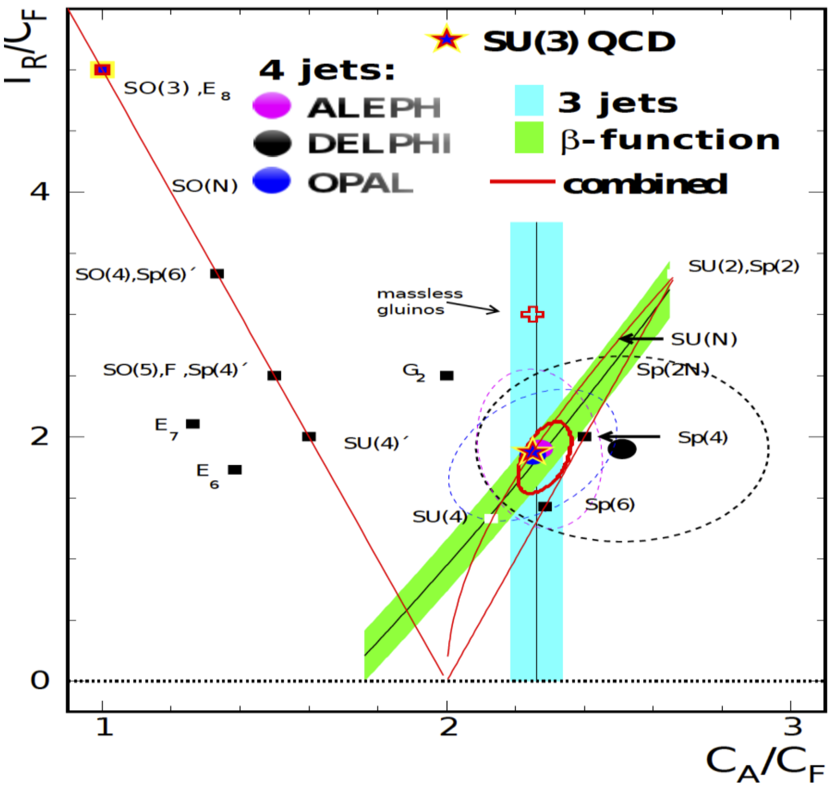
or 2.990 ± 0.007 (PDG global fit, Erler & Freitas)

And not this either:

Proof of triple-gluon coupling,
the reason why the strong coupling runs from confinement to asymptotic freedom
from structure of 4-jet events







That’s all folks!

Just one more thing to add …..

Happy Birthday, George
And thanks for everything!

Errata/Addenda

WV 21/9/15

SLIDE 3: In fact the TST proposal was received by the SPSC on 20/11/74 and its writing over the preceding year owed nothing to the "November revolution" (the joint announcement of the J/ψ discovery was on 11/11/74). It was based purely on Gargamelle's 1973 discovery of Neutral Currents (NC), despite the well-known absence of Flavour Changing Neutral Currents (FCNC) in K decays etc, and the proposed GIM mechanism cancelling FCNC with diagrams involving new charmed quarks of mass not far above existing limits.

SLIDES 29-30: As shown in slide 30, the order-of-magnitude improvement in the precision of the inputs (the most important one, in α_s, remarkably coming not from experiment but from lattice gauge theory) naively implies such a high and precise M_{SUSY} value that SUSY would very likely not be seen at LHC in either Run 1 or Run 2 and would not provide a natural solution to the hierarchy problem. The calculation shown (De Boer and Sander, 2004) assumes point-like gauge coupling unification, which corresponds to a degenerate mass spectrum at the GUT-scale, so in principle this conclusion could be avoided by widely separating the GUT scale masses. But, as Bagger, Matchev and Pierce (1995) found, the particles whose mass should be reduced mediate nucleon decay, so this solution is forbidden. Instead they proposed replacing minimal SUSY SU(5) at the GUT scale by an extension, the missing doublet model, thus reducing the predicted α_s for fixed other inputs by ≈ 4% or ≈0.005. **Unfortunately**, the proximity of the GUT scale to the Planck scale leads to an uncertainty in the predicted α_s proportional to M_{GUT}/M_{Planck}, due to unknown gravitationally-induced non-renormalisable operators (NRO's); for a SUSY GUT, it could be as high as ±0.006 (Langacker and Polonsky,1993,1995) but it could also be much smaller in some models. More theoretical work was needed because, while this ±0.006 estimate remained the best available, one could not significantly improve on the uncertainties of the 1990-91 analysis.