

THE ALEPH 'EXPERIENCE'

25 years of memories

Editors Claus Grupen, Ian Hughes, Jim Lynch, Ron Settles

ISBN 92-9083-233-9

FOREWORD February 2005

Dear friends and colleagues,

The Aleph papers are scientific 'works of art', while this is a collection of 'memories', involving an undefined method for figuring out how to proceed and deciding what to include. We have been feeling our way for the last couple of years, trying to get a collection of stories that samples all facets of the happenings.

This whole thing started in 1998, when Hans Taureg sent an email to the Steering Committee asking for contributions for such a book. Dieter Schlatter, Peter Schilly, Ariella Mazzari and others were involved and later joined by Ian Hughes† who did most of the editing work in the following three years. The main reason it has taken so long is that everyone always has so much higher priority stuff on their plates, and the first attempts were just too early => our experiment was still running...

The stories that form 'The Aleph 'Experience' are a not-completely-random collection of **personal** impressions, and as such are more-often-than-not seen differently by others. Also, the names quoted in the stories will often do injustice to some who feel that they have been overlooked. So we, as editors, had to decide what to do about these 'biases', and the decision was to do NOTHING. What we suggest to those who think that any of these stories lacks the right perspective is that they should have submitted their own contributions to put the record straight, there were plenty of opportunities.

There can be no claim that this book gives a complete picture of what took place in Aleph, which, like any other large experiment, is so complex that a couple of hundred pages cannot do real justice to its history. As said above, what we have tried to do is to come up with a representative cross section of interesting stories, mostly pleasant ones, but as anywhere there were also less pleasant ones, a few of which are included.

We appreciate the help of Ariella Mazarri and Valérie Brunner of the Aleph Secretariat as well as the technical advice and assistance of Mick Draper, Susan Leech O'Neale and Christine Vanoli of the CERN IT/UDS group in putting together this 'scrapbook'. Thanks are also due to all our friends in Aleph who contributed articles without which this book would not exist!

Happy reading!

The (final) editors: Claus.Grupen@cern.ch, Jim.Lynch@cern.ch or Ron.Settles@cern.ch

[†] Unfortunately Ian Hughes had to give up his editorial work in 2001 owing to ill health and, sadly, did not survive to see the result of his efforts in print. Ian died in 2003.

Contents

EARLY DAYS	1
DETECTOR CONCEPT Jack Steinberger	3
ORGANIZATION	7
STRUCTURES AND PROCEDURES	8
THE LETTER OF INTENT	12
THE TECHNICAL REPORT	14
INSTRUMENT OF UNDERSTANDING Adolph Minten	16
SPOKESPERSONS	17
RUN CO-ORDINATION Claus Grupen/Adolf Minten	27
ECHENEVEX GROUP Olivier Callot	31
EDITORIAL BOARD Jean-François Grivaz	36
SPEAKERS BUREAU Jacques Boucrot	37
SECRETARIAT Ariella Mazzari/Valérie Brunner	38
SUBDETECTOR/HARDWARE STORIES	41
TRACKING	43
VDET1 Lorenzo Foà/Ron Settles	44
VDET2 John Carr/Paschal Coyle	48

ITC Peter Dornan	50
TPC Ron Settles	53
TRACKING ALIGNMENT Alain Bonissent	59
THE TRACKING UPGRADE Dave Casper	62
CALORIMETERS, COIL, MUON	65
ECAL BARREL Jacques Lefrançois	66
END-CAP MODULES (RAL) Mike Edwards/John Thompson	69
END-CAP MODULES (GLASGOW) Jim Lynch/Ian Hughes	72
THE SOLENOID Marcel Jacquemet	75
THE MAGNET LEAK (Open heart surgery) <i>Pierre Lazeyras</i>	80
INCEPTION OF HCAL Lorenzo Foà	83
HCAL (Down to earth) <i>Giampaolo Mannocchi</i>	85
THE MUON SYSTEM <i>Weimin Wu</i>	87
LUMI DETECTORS, TRIGGERS, DAQ, FALCON, LEP	89
SATR/SAMBA Claus Grupen	90
SICAL John Rander	93
LCAL Peter Hansen	94
BCAL/BCAL++ Enrique Fernández	96
LEVEL1 TRIGGER Paul Hanke/Eike-Erik Kluge	98
LEVEL2 TRIGGER John Strong	100
LEVEL3 TRIGGER Günther Lütjens/Beat Jost	101

THE ALEPH ONLINE PROJECT (Personal recollections) <i>John Harvey</i>	102
FALCON (Reconstruction farm) <i>Dieter Schlatter</i>	106
BEGINNINGS OF FALCON Enrique Fernández	108
THE ALEPH–LEP CONNECTION Maria Girone/Bolek Pietrzyk	110
OFFLINE STORIES	113
DALI (<u>D</u> isplay of <u>AL</u> eph <u>I</u> nteractions) <i>Hans Drevermann</i>	114
JULIA Jürgen Knobloch	116
ALEPH COMPUTING Jürgen Knobloch	118
SOME EARLY COMPUTING STATISTICS Dieter Schlatter/Jürgen Knobloch	120
MORE ON ALEPH COMPUTING Marco Cattaneo	121
LUMINOSITY Peter Hansen	124
ALPHA (The Aleph offline analysis program) <i>Jacques Boucrot</i>	126
ALEPH BOOKKEEPING AND SCANBOOK Jacques Boucrot	128
ALEPH WEEKS OUTSIDE CERN	129
ABOUT THE ALEPH WEEKS Ron Settles	130
PISA '83 Roberto Tenchini	131
MARSEILLE '84 Jean-Jacques Aubert	132
LONDON (IC) '85 Peter Dornan	133
MUNICH '86 Ron Settles	135

COPENHAGEN '87 Peter Hansen	137	
BARCELONA '88 Enrique Fernández	139	
ATHENS '89 Anna Vayaki	140	
FRASCATI '90 Giorgio Capon	141	
PARIS '91 Claus Grupen	142	
GLASGOW '92 Jim Lynch	144	
INNSBRUCK '93 Dietmar Kuhn	146	
HEIDELBERG '94 <i>Eike-Erik Kluge</i>	148	
BARI '95 Giorgio Maggi/Mauro de Palma	149	
CLERMONT '96 Bernard Michel	150	
OXFORD '97 John Thompson	151	
MAINZ '98 <i>Sascha Schmeling</i>	153	
SIENA '99 Roberto Tenchini	154	
AIX-EN-PROVENCE '00 Paschal Coyle	156	
LEUKERBAD '01 Fabiola Gianotti	158	
SOME PHYSICS HAPPENINGS	161	
THE FIRST Z EVENT Dieter Schlatter	162	
FIRST ALEPH EXPERIMENT Alain Blondel	164	
FIRST EXPERIMENT (The other side…) <i>Francesco Fidecaro/Fabrizio Palla/Monica Pepe-Altarelli</i>	167	
FIRST PRECISE R _b MEASUREMENT Dave Brown	170	

viii

RESOLVING 'THE R _b CRISIS' (The five-tags) <i>Fabrizio Palla</i>	172
THE UPPER LIMIT ON THE ν _τ MASS <i>Fabio Cerutti</i>	174
SEARCHES AT LEP1 <i>Mike Green</i>	176
THE QCD MEGAPAPER Glen Cowan	178
BEAUTIFUL OSCILLATIONS <i>Roger Forty</i>	180
B _s AND Λ _b DISCOVERY Vivek Sharma	183
AMAZING TALE OF THE TAU <i>Michel Davier</i>	189
GAMMA-GAMMA Alex Finch/Claus Grupen	196
THE LLV STORY Ioana Videau/Patrick Janot	198
THE 4JETS SAGA Patrick Janot/Peter Dornan	199
SEARCHES AT LEP2 Fabio Cerutti	201
THE HIGGS (and all that jazz) <i>Patrick Janot</i>	202
(like) ENERGY FLOW <i>Patrick Janot</i>	204
HIGGS STORY (The cuts-stream perspective) Gavin Davies/Pedro Teixeira-Dias	208
HIGGS STORY (The Wisconsin perspective) <i>Steve Armstrong/Peter McNamara/Sau Lan Wu</i>	213
THE W MASS STORY (The professor's perspective) <i>John Thompson</i>	221
THE W MASS STORY (The student's perspective) <i>Thomas Ziegler</i>	224
ALEPH GOES COSMIC (CosmoLEP and CosmoALEPH) <i>Horst Wachsmuth</i>	226

OTHER INTERESTING STORIES	229
EARLY ALEPH (Personal reminiscences) <i>Peter Norton</i>	230
THE CHOICE OF EXPERIMENTAL ZONE <i>Pierre Lazeyras</i>	232
THE LEP INAUGURATION (Unofficial part) <i>Hans Taureg</i>	234
ALEPH FULL-SCALE MODEL Karl-Heinz Steinberg	235
THE 1:20 SCALE MODEL OF ALEPH AND CAVERN <i>Jean-Claude Dusseux</i>	238
ALEPH COOLING <i>Ron Settles</i>	240
ALEPH BEAMPIPE STORY <i>Patrick Lepeule</i>	241
CHAMBERS OR TUBES? Claus Grupen	243
THE CASE OF THE STRANGE DOWEL PINS <i>Claus Grupen</i>	244
ALEPH TPC GAS (To seed or not to seed?) <i>Ken Ledingham</i>	246
B-FIELD BLUES <i>Ron Settles</i>	248
HCAL VISUALIZATION Giuseppe Zito	250
HCAL (as seen by an 'outsider') <i>Pierre Lazeyras</i>	251
BIG BROTHER (and a fast fix from the Ile d'Yeu) <i>Olivier Callot</i>	253
SUMMER STUDENT SABOTAGE Giacomo Sguazzoni	254
SHUTDOWNS Peter Schilly	256
THE 'MUR TYMPAN' (an interesting geological problem) <i>Peter Schilly</i>	258
SAFETY INSPECTIONS <i>Hans Taureg</i>	259

VIP VISITS TO ALEPH Jim Lynch	261
OPEN DAYS AT ECHENEVEX Marco Cattaneo	266
ALEPH AND THE BARCELONA GROUP Enrique Fernández	268
BEIJING PARTICIPATION IN ALEPH Weimin Wu	270
FIRE IN BEIJING Pierre Lazeyras	273
REMEMBRANCES <i>Xie Yigang</i>	274
FOND RECOLLECTIONS Dave Cinabro	275
PHOTOGRAPHIC MEMORIES Dave Casper	276
SKETCHES AT AIX Franco Ligabue	278
THE ANNUAL ALLONDON FRISBEE CHALLENGE Roger Forty/Jim Lynch	280
RUNNING THE LEP MACHINE <i>Ron Settles</i>	282
RUNNING THE LEP MACHINE (Recollections of friends) <i>Steve Myers/Mike Lamont/John Poole/Helmut Burkhardt</i>	284
'DID YOU FIND THE HIGGS?' Steve Wasserbaech	294
'OR DID THE RUN CO-ORDINATOR FIND YOU?' <i>Mike Green</i>	295
THE LAST DAY AT ECHENEVEX Peter Dornan	297
ALEPH—WHERE IS IT NOW? <i>Jim Lynch</i>	301
A POEM Anna Vayaki	303
ALEPH PUBLICATIONS	304
'ETERNAL' ALEPH AUTHOR LIST	315

Early Days...

INSTITUT FÜR HOCHENERGIEPHYSIK DER UNIVERSITÄT HEIDELBERG

F. Dydak

C. Jeveniper

[~~

L

69Heidelberg. 3 July, 1980 Albort-Überte Str 2 Teles: 481597 phythe d Telefon: Vorwahi 08221 Vermittlung 561 Sekretariat 569 333 Durchwahi 569

1

Dear Sir,

this is to confirm the place and the time of our first discussion meeting on LEP physics:

~

٦

Wednesday 9 July 1980, 10.30 h a.m. CERN, J. Steinbergers office (building 2, 1-029).

Representatives from the following institutions agreed to take part in this meeting: CERN (May, Steinberger, Wahl), Dortmund (Eisele, Kleinknecht), Heidelberg (Dydak, Geweniger, Kluge, Tittel), MPI München (Blum, Lorenz), Orsay (Davier, Lefrançois) Pisa (Bellettini, Foa), and Saclay (Rander, Turlay).

Yours sincerely,

F. Dydak



The 'young' Aleph Collaboration in 1986.

'Experiment is the interpreter of nature. Experiments never deceive. It is our judgment that sometimes deceives itself, because it expects results that experiment refuses. We must consult experiment, varying the circumstances, until we have deduced general rules, for experiment alone can furnish reliable rules.'

LEONARDO DA VINCI

1980-1989

DETECTOR CONCEPT

Jack Steinberger



Jack at the LEPC.

My recollections of the Aleph beginnings are faint and full of holes. 20 years have gone by, and we remember what is convenient to remember. When it became more and more likely, early in 1980, that an electron-positron collider, energetic enough to produce the as yet undiscovered Z boson, would be constructed at CERN, some of us got together to initiate discussions on a possible experiment. Some of us who collaborated in the CDHS neutrino experiment were joined by colleagues from Orsay, Pisa, Munich (Max Planck) and Rutherford Labs. The first question we asked ourselves was: 'Can we think of a focused experiment, requiring a specialized rather than general-purpose detector?' The answer was a clear no, and in fact, no specialpurpose detector was ever built at LEP. So we started to think of a general-purpose, 4π detector, such as had been developed at the DESY Petra and the SLAC PEP colliders, but clearly more ambitious in all aspects: tracking resolution, angular coverage, calorimetry, and particle identification.

I remember our design and construction period, 1980 to 1988, with pleasure. We worked together constructively, with a minimum of conflict, and the common aim of producing something with which we might do some good physics. Decisions with an impact on the physics, such as detector technologies, were taken in plenum. We had great luck in getting Pierre Lazeyras to take over as technical director, to fully take in hand the planning, co-ordination and financing all through the design and construction period, as well as long after that.

Soon we were more than a hundred physicists, with the addition of groups at Glasgow, Ecole Polytechnique, Wisconsin and IHEP (Beijing). One of the less brilliant early ideas I believe was mine: 'The big sphere', a detector concept trying to reflect the fact that annihilation physics is spherical, since the centre-of-mass system is the lab. But it was no good, because the magnetic field cannot be spherically symmetric. So, even though Guido Petrucci managed to design a spherical magnet (with field in the beam direction, of course), we chose the superconducting solenoid proposed by Saclay. Jacques Lefrançois was the author of one of the key considerations underlying the Aleph design, the realization that for the electromagnetic calorimeter, high granularity should take precedence over energy resolution. This was readily accepted by all of us. One of the tougher debates in 1981 was on ECAL technology, proportional wire sandwich vs. liquid argon. The final Aleph ECAL, although very difficult to construct, was surely one of the jewels of Aleph. Our choice of the new TPC technology over drift chambers was also not without debate, but we could convince ourselves that, although the only previous attempt, the PEP TPC, had great difficulties with drift distortions, this could be overcome and that the good zresolution warranted facing the TPC challenge.

By the end of 1981 a preliminary design had crystallized, the basis of our Letter of Intent to the newly formed LEP Committee, dated 25.3.82. The detector we proposed (see 'Technical Report' below) looks remarkably like Aleph. The name Aleph itself was proposed by Lorenzo Foà, in response to a request by Sau Lan Wu that the name begin with the first letter of the alphabet.

One of the most beautiful episodes in Aleph, for me personally, was the evolution in our understanding of the TPC. This began with a question by Günter Wolf, LEPC chairman, concerning the expected resolution. Somehow Wolf was aware of the poor performance of a small TPC at Triumf in Vancouver, especially on the wire chamber readout end. Already this comment by an experiment committee member was a pleasure; it is not often that one can expect a useful contribution from a committee member. The question was pursued seriously by Francesco Ragusa and Gigi Rolandi. Within a few weeks they had come with a note, in which they quite clearly had achieved a clear understanding of the resolution errors associated with the drift of the electrons in the crossed electric and magnetic fields of the TPC, as well as the errors associated with the production of the signals on the pads of the wire chamber planes. A realistic test TPC chamber, the TPC 90 (90 cm in diameter) was built in Munich and in 1983 was ready for tests. The resolution could be studied as function of many of the important parameters, especially also the strength of the magnetic field. One of the important outcomes of the Rolandi and Ragusa

study had been the realization of the importance of the magnetic field as a stabilizer of the drift paths. I think that the happiest moment of my life in Aleph was listening, at our plenary meeting in November 1983, to the report by Julia Sedgbeer on these measurements, in which one after the other of these predictions were quantitatively confirmed.

Another pleasure was watching Barcelona. Young Enrique Fernández had come back from SLAC to Barcelona, where there was no previous experience in particle physics, and nevertheless managed to assemble a group of remarkably talented and theoretically educated young colleagues who contributed, despite the limited laboratory resources in Barcelona, a forward luminosity monitor, and introduced work stations to Aleph in the form of Falcon, the online event reconstructor, which at the time was a very challenging problem. They also helped us a great deal to understand electroweak radiative corrections and QCD.

On the whole, I think we were all pretty happy with the detector which we produced. It was not the worst of the four, and allowed us to measure the events produced in LEP annihilations about as well as one could hope to. Because of insufficient financial resources promised to us, the committee asked us to stage (postpone) the construction of the silicon vertex detector as well as part of the TPC electronics. As it turned out, we did manage to complete the TPC electronics before LEP turnon in 1989, but the vertex chamber had to wait a year or two, after Pisa had been joined by Munich, to make the first microstrip device which could measure both z and φ coordinates simultaneously. We also made mistakes; I cite here one: we developed different microchip readout boards, separately in three labs, one for the TPC, one for the electromagnetic calorimeter, and one for the data acquisition system. Instead this could and should have been a unified, commercially based technique, and some years later in fact all of it had to be replaced.

One of the most difficult challenges turned out to be the combined readout of the data from the subdetectors, but when LEP turned on, we were there, even with the TPC. (See the story 'The First Z Event' below which displays it.) The first challenge was the measurement of the number of neutrino families. There we can be proud. We realized early enough that here we should concentrate on the absolute cross-section of the resonance peak, rather than on the width. Consequently we made a special effort to understand the measurement of the luminosity, and in the fall of 1989, with the first results, the Aleph value for the number of neutrino families had about half the error of that of each of the three other experiments, for this allimportant result.



No comment.

Organization



1980-1982

STRUCTURES AND PROCEDURES

1980-1982

Formation of the Aleph Collaboration

DECEMBER 1980

First Plenary meeting

JUNE 1981

First Steering Committee meeting

MARCH 1982

Letter of Intent to LEPC

NOVEMBER 1982

Aleph recommended by LEPC and approved by the Research Board

THE COLLABORATION

The Aleph Collaboration formed during the years 1980–82. At the time of the Letter of Intent (CERN/ LEPC/82-3) dated 25.3.82, the Collaboration consisted of 19 founding Institutes with 275 signing members. The so-called 'Instrument of Understanding' with all legal details was dated 18.4.1984. There was no list of names on the Letter of Intent, only Bari, CERN, Demokritos Athens, Dortmund, Ecole Polytechnique Palaiseau, Edinburgh, Glasgow, Heidelberg, Lancaster, MPI-München, Orsay, Pisa, Rutherford, Sheffield, Torino, Trieste, Westfield College London, Wisconsin.

With the passage of time:

- Westfield College became Royal Holloway
- Dortmund transferred to Mainz
- Torino transferred to Frascati
- 14 new institutes joined...

By 1989, the Aleph Collaboration had evolved to 32 institutes with 360 members signing the publications:

Laboratoire de Physique des Particules (LAPP), IN2P3-CNRS, 74019 Annecy-le-Vieux Cedex, France

Institut de Fisica d'Altes Energies, Universitat Autònoma de Barcelona, 08193 Bellaterra (Barcelona), Spain

Dipartimento di Fisica, INFN Sezione di Bari, 70126 Bari, Italy

Institute of High-Energy Physics, Academia Sinica, Beijing, China

European Laboratory for Particle Physics (CERN), CH–1211 Geneva 23, Switzerland

Laboratoire de Physique Corpusculaire, Univ. Blaise Pascal, IN2P3-CNRS, Clermont-Ferrand, F–63177 Aubière, France

Niels Bohr Institute, 2100 Copenhagen, Denmark

Nuclear Research Center Demokritos (NRCD), GR–15310 Attiki, Greece

Laboratoire de Physique Nucléaire et des Hautes Energies, Ecole Polytechnique, IN2P3-CNRS, F–91128 Palaiseau Cedex, France

Dipartimento di Fisica, Università di Firenze, INFN Sezione di Firenze, 50125 Firenze, Italy

Supercomputer Computations Research Institute, Florida State University, Tallahassee, FL 32306-4052, USA

Laboratori Nazionali dell'INFN (LNF-INFN), I–00044 Frascati, Italy

Department of Physics and Astronomy, University of Glasgow, Glasgow G12 8QQ, UK

Institut für Hochenergiephysik, Universität Heidelberg, D-69120 Heidelberg, Germany

Department of Physics, Imperial College, London SW7 2BZ UK

Institut für Experimentalphysik, Universität Innsbruck, A–6020 Innsbruck, Austria Department of Physics, University of Lancaster, Lancaster LA1 4YB, UK

Institut für Physik, Universität Mainz, D–55099 Mainz, Germany

Centre de Physique des Particules, Faculté des Sciences de Luminy, IN2P3-CNRS, F–13288 Marseille, France

Dipartimento di Fisica, Università di Milano e INFN Sezione di Milano, I–20133 Milano, Italy

Max-Planck-Institut für Physik, Werner-Heisenberg-Institut, D–80805 München, Germany

Laboratoire de l'Accélérateur Linéaire, Université de Paris-Sud, IN2P3-CNRS, F–91898 Orsay Cedex, France

Dipartimento di Fisica dell'Univervità, INFN Sezione di Pisa, e Scuola Normale Superiore, I–56010 Pisa,Italy

Department of Physics, Royal Holloway & Bedford New College, University of London, Surrey TW20 OEX, UK

Particle Physics Dept., Rutherford Appleton Laboratory, Chilton, Didcot, Oxon OX11 OQX, UK

CEA, DAPNIA Service de Physique des Particules, CE-Saclay, F–91191 Gif-sur-Yvette Cedex, France

Institute for Particle Physics, University of California at Santa Cruz, Santa Cruz, CA 95064, USA

Department of Physics, University of Sheffield, Sheffield S3 7RH, UK

Fachbereich Physik, Universität Siegen, D–57068 Siegen, Germany

Dipartimento di Fisica, Università di Trieste e INFN Sezione di Trieste, I–34127 Trieste,Italy

Experimental Elementary Particle Physics, University of Washington, Seattle, WA 98195, USA

Department of Physics, University of Wisconsin, Madison, WI 53706, USA

STRUCTURES

Although the Collaboration works essentially on the basis of consensus, good will and bona fides, there are some formal structures with defined authority:

- a) The Instrument of Understanding is the 'Constitution' of the Collaboration. This text defines the distribution of responsibilities between the participating Institutes and their Funding Agencies, and the responsibility of CERN, with respect to their contribution to the detector construction and operation. organizational, It contains managerial and financial guides. The Instrument of Understanding is followed up by the Finance Review Committee (FRC) where Funding Agencies are represented. The Chair is held by the CERN Director of Research. The Aleph Collaboration participates with the Chairman of the Steering Committee, the Spokesperson and the Technical Co-ordinator. The original Instrument of Understanding is dated 18 April 1984. It is updated when a new Institute joins the Collaboration. The FRC meets twice in a year.
- b) The Steering Committee: The Collaboration is governed by its Steering Committee, composed of one representative from each participating Institute. The Steering Committee appoints a Chairman charged with the direction of the committee, and a Spokesman charged to represent and manage the Collaboration, subject to decisions of the Steering Committee.

In more detail, the composition of the Steering Committee is as follows.

- Ordinary members representing an Institute participating in the Collaboration. Only ordinary members have voting rights.
- *Ex officio* members: the Chairperson, the Spokesperson, the Technical Co-ordinator and the secretary.
- Ad persona members: ex-spokes/chairmen.

In addition the project leaders of (major) subdetectors and subsystems (ON/OFFLINE) attended the meetings. In 1995 this representation was changed. There was still one voting member per Institute and there were also the *ex officio* and *ad persona* members. Present would be the LEPC representative, the organizers of Aleph Weeks and Thursday meetings, the co-ordinators of ON/OFFLINE, Echenevex, Physics Tools and Data Quality, the Chairpersons of the Speakers Bureau and Editorial Board.

The Steering Committee normally meets four times a year during the Aleph Week.

c) The Technical Co-ordinator: The Spokesman appoints a Technical Co-ordinator subject to approval by the Steering Committee. He is expected to be someone who can be resident at CERN (Instrument of Understanding).

From the Instrument of Understanding the Technical Co-ordinator has a double responsibility, technical and administrative. He has to make sure that the many projects in different Institutes converge and integrate into the detector as a whole. He administers the Common Fund financed by the Collaboration in order to finance common parts of the detector. The most obvious example for a common component is the superconducting magnet and its cryogenics.

In the case of Aleph the Technical Co-ordinator accepted three more responsibilities:

- By mutual agreement the parties shall appoint a group leader in matters of safety (GLIMOS) to co-ordinate all matters concerning the safety of the Collaboration. He shall be responsible to CERN on behalf of the Collaboration for all safety matters concerning the experiment and its staff. The GLIMOS function was given to the Technical Co-ordinator.
- The Technical Co-ordinator is Group Leader of the technical team (ALD) from EP Division.
- The Technical Co-ordinator is administrator of the budgets of the CERN groups.

MEETINGS

- The Aleph Week: four per year, one of them at a collaborating Institute outside CERN.
- The Plenary meeting: two half-day meetings of general interest during Aleph Week.
- The Steering Committee: usually four per year during the Aleph Week.
- The Thursday (Tuesday) meeting: discusses and approves experimental programme and run conditions (trigger), discusses and approves conference contributions and publications, meets about twice a month.
- The Echenevex Schedule meeting: discusses detector status and activities and short-term run conditions, weekly during shutdown, daily during running.
- The Editorial Board: discusses and approves texts of publications, meeting about once a month.
- The Speakers Bureau: nominates conference speakers.

1982-1983

THE LETTER OF INTENT

ALECH participing motitutes Italy Demokritos GREECE Dortmund Heidel berg MPI Munchen GERMANY E dimbrugh U. K. Rutherford field College E. P. Palaiseon Or say Saclay France Wiscmain I U.S.A. CERN

Recommendation by the LEPC

Following the letter of intent in March 1982 (on the left is the first transparency of Jacques' presentation to the LEPC), Aleph was recommended by the LEPC at its meeting on 16 November 1982 (see the DG's letter opposite).

The LEPC required a technical report to be produced by 25 April 1983 where a number of technical and financial issues had to be addressed and specified the following:

Milestones:

- TPC: a prototype of at least a 1.5 m long drift length, with magnetic field (later known as TPC 90)
- Shower counter: a prototype should be built with a mini tower geometry, large enough to contain a full shower of >10 GeV energy. It should be tested in a magnetic field and the energy and spatial resolution determined. And a prototype of a full size barrel section should be built and tested

Staging:

At the turn on, the magnet, the TPC, the shower counter and one layer of 11 HCAL chambers should be ready. Monte Carlo simulations were also requested.

Interesting remark:

ALEPH, OPAL and DELPHI were recommended (and ELECTRA and LOGIC not); 'if four experiments were to be approved, L3 would be considered as a candidate for the fourth experiment'.

Approval by the Research Board

In reality the final approval was given only in June 1983, after publication of the technical report (see next article below).

- 1. Milestones: as recommended by the LEPC (see above).
- 2. The DG decides that in view of the amount of money involved, written commitments must be done by the bodies responsible for financing giving the maximum of guarantee that they will fulfil their commitments

(NB: Later it turned out to be more difficult for CERN to respect its commitments than the other participants!) This LOI was followed in May 1983 by a memo from the Collaboration to E. Gabathuler in connection with the choice of the experimental area, 4 or 8, foreseen for Aleph or Delphi. (The choice of 2 for L3 and 6 for Opal was dictated by the electrical power possibilities for Opal and necessity for cost reason for L3 to be as close as possible to the surface.) In this memo a general planning was shown where Aleph would be ready for live tests by the end of April 1987 and that zone 8 should therefore go to Aleph. The argument for the experimental zone 8 was the fact that, in the LEP civil engineering planning, zone 4 was supposed to be ready 10 months later than zone 8 and therefore the installation and commissioning of the detector would be too late for the start-up of LEP (foreseen in early 1989 at that time). The closest to truth is the guess for assembly in 18 months, as it took about 21, the rest was Dichtung und Wahrheit.

(Editor's note-RS: as everyone knows, in the end Aleph got pit 4—see CHOICE OF EXPERIMENTAL ZONE by Pierre Lazeyrus below—which turned out to be the best.)



EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH

European Laboratory for Particle Physics

DIRECTOR GENERAL

CERN CH-1211 GENEVA 23 SWITZERLAND

DG/944-82

Professor Jack STEINBERGER Spokesman LEP experiment ALEPH

EP Division CERN

Geneva, 25 November 1982

Dear Professor Steinberger,

I am pleased to inform you that the Research Board has given conditional approval to your experiment, which has been recommended by the LEP Experiments Committee. This conditional approval will require:

- A full technical proposal to be submitted for approval by 25 April, 1983.
- The necessary financial and technical support to be defined by signed contracts between CERN and the collaborating Institutes in the first half of 1983.
- The approval of the LEPC to the milestones which have been defined by them in their recommendations to the Research Board.
- The detector to meet the CERN Safety Rules, in particular to the use of flammable gases in underground areas.

A more detailed paper on this item will follow from Erwin Gabathuler.

I should like to take this opportunity to thank you and your colleagues for all the detailed work which has been carried out in order to achieve this approval, and to encourage you to have your detector operational by the end of 1987 when we expect to have first collisions in LEP.

Yours sincerely,

Hervig Schopper

Telephone: GENEVA 1022) 83 23 00 - Telex: 2 36 98 CH - Telegram: CERNLAB-GENEVA

1983

THE TECHNICAL REPORT



The Technical Report (CERN/LEPC/83-2, LEPC/ P1, 15 May 1983) gave a fair description of what was planned to be built and the final product was in general not far from this description for the hardware part.

In the LOI (see preceding article) the control rooms were located in the cavern, both sides of the lift, as in UA2. In the Technical Report, this idea has been abandoned and the control room is at the surface.

This was followed a year later by the Status Report (CERN/LEPC/84-15, 10 September 1984) in which the designs of all subdetectors had essentially converged.





Details of the TPC support.

	Cost estimates		
Item	Total cost (MCHF)	Staged version (MCHF)	
Iron structure	9.8	9.8	
Coil	9.7	9.7	
Refrigerator	2.0	2.0	
Main	1.2	1.2	
computer			
Infrastructure	3.45	3.45	
Counting	3.55	3.55	
room			
Inner chamber	0.90	0.90	
Trigger	1.20	1.20	
Luminosity c.	0.65	0.65	
DAQ	0.95	0.95	
Offline soft	2.50	0	
TPC	11.40	11.40	
ECAL	11.35	11.35	
HCAL	6.90	5.0	
Minivertex	1.25	0	
Installation	6	0	
Total	72.80	61.15	

Installation, running-in and commissioning were considered as contingencies. It was not expected to occur in terms of cost before 1988, and we expected to have other resources available at that time!

Share of the work

Magnet: CERN for the iron and refrigerator, and Saclay for the coil

TPC: MPI, CERN, Wisconsin, Dortmund, Glasgow, Pisa, Trieste, Edinburgh + a financial participation of Heidelberg and Siegen

Electromagnetic Calorimeter: End caps: all UK participants (except IC London), Barrel: All French institutes

Hadron Calorimeter, inner muon chambers: Barrel: Frascati, End caps: Pisa and Bari

Other muon chambers: Beijing

Inner Chamber: Imperial College London

Luminosity monitors: Siegen and Copenhagen

Minivertex detector: Pisa

Trigger: Heidelberg and RAL

DAQ: CERN



Resources (in MCHF)		
CERN	13.6	Including 1983 inflation
Denmark	0.5	
Germany	9.5	For 3 years, extrapolated for following 5 years
IN2P3	7.0	Including 1983 inflation
INFN	12.0	Including 1983 inflation
Saclay	4.0	Including 1983 inflation
UK	10.6	Including 1983 inflation
Wisconsin	3.0	
Total	60.2	

The estimated cost was 72.8 MCHF, but as the foreseen budget was only 60 MCHF, including 7 MCHF for infrastructure, and counting rooms, so part of the detector was staged:

- Minivertex
- second layer of muon chambers
- offline

- part of the electronics for Hadron calorimeter.

At this point we considered that the difference between the staged costs and the budget of about 1 MCHF was smaller than the errors bars. Perhaps we will be lucky with some items or get extra resources to cover this 1 MCHF.

This 1983 Technical Report and the similar 'Design Report' in 1984 were approved by the LEPC.

1984-1988

INSTRUMENT OF UNDERSTANDING Adolph Minten

In the Instrument of Understanding, the Aleph maintenance and repair was mentioned, but it was foreseen that it would be an addendum to the IOU to be agreed upon at a later time.

A first discussion took place at the Finance Review in November 1985, where the basic principles were defined, at least on responsibility, and stating that the cost should be somehow shared by the whole Collaboration and not only by the builders of the various parts.

At the following meeting a more detailed discussion took place, where some members of the FRC proposed that CERN should take care of all the maintenance costs *for the parts coming to CERN from the Member States institutions.* In November 1986 we had a proposal that the contribution of each Institute should be proportional to its contribution to the construction. The total budget had two parts, one for detector maintenance, the other for the common operation, gas, magnetic tapes, magnet, etc. The money foreseen for detector maintenance was kept 'at home'; the rest was paid to a common pot. Long discussions took place concerning the sharing of the cost between Institutions, depending on the contribution to the detector construction, number of physicists or anything else.

In May 1988, the Director of Research was in a position to announce to the FRC that all participants but China had signed the document concerning maintenance and operation. China was anyhow a special case.

(Editor's note-RS: This way of managing the maintenance and operation budget was very successful, with Aleph receiving enough money to finance the operations and having some surplus to make several important upgrades, e.g. replace the Fastbus ROCs with VME, buy more powerful computers for online and offline.)



SPOKESPERSONS



SELECTION OF SPOKESPERSON

The Spokesperson is nominated by the Steering Committee, represents the Collaboration towards the LEPC and the CERN management, accelerators and research and towards the HEP community. Inside the Collaboration he directs, animates and mediates within the guidelines given to him.

Until 1994 the nomination was prepared by a restricted search group of seven persons, representing different Institutes. The group found a consensus on the candidate and presented their proposal to the Steering Committee. Since 1994 a formal election procedure has been followed, and the Spokesperson has been elected from among several candidates.

During the lifetime of Aleph, the Spokesmen were

1980 -> 1990	Jack Steinberger
1990 -> 1993	Jacques Lefrançois
1993 -> 1994	Lorenzo Foà
1994 -> 1997	Gigi Rolandi
1997 -> 2000	Peter Dornan
2000 -> 2001	Dieter Schlatter
2001 -> 2005	Roberto Tenchini

REMINISCENCES OF SPOKESMEN

Each spokesman was asked to submit a couple of sentences for this book about his period as spokesman. Here are the responses.

Jack Steinberger:

See Jack's article above entitled 'Detector Concept'.

Jacques Lefrançois:

'I think the two moments I can remember, which to me are most representative of how I think of my spokesman's job, are just before the start and just at the end of my mandate. The memory before my election is linked to a document I found again recently (cleaning old papers!). Young physicists based at CERN wanted, in 1990, a discussion with the prospective spokesman in order to present their views on organizational matters for Aleph. Many points they raised were quite sensible but, in the middle of the exchange, I was asked what my future programme was! I was taken by surprise. I could not think of a better answer than to say: 'Aleph has been doing very well as collaboration. I can think of nothing better than being available and giving my effort to help it continue as well as before'... my lack of imagination undoubtedly!

Well, three years later, after many drinks at the end-of-mandate ceremony (where I had received a beautiful windsurfing board), came the moment for my speech of thanks. I remember noting that succeeding the nine years of Jack's guidance could seem to be a frightening perspective, but that I had found that the 'team spirit' he had inspired in the collaboration and the fact that only the best of our efforts were good enough for Aleph finally made the job not so difficult. So at least I was constant in my view of Aleph throughout the years.

Even in the most difficult moments, when Aleph was divided on how and whether to publish the observation of llV events, I think that it was this team spirit which finally took over and allowed us to publish a paper (presented to CERN auditorium by Lorenzo) and signed by all Aleph members. This was not trivial since there are many examples of collaborations which were definitely split by such an occurrence. Instead of trying to select the memory of one especially wonderful moment during these three years, I prefer to remember all the exciting times, when we discussed physics together and shared the pleasure of understanding. This could be in large groups as in our traditional 'Gigi' physics meetings, in smaller specialized physics meetings, or even with a few others as internal referee of one of our papers for the Editorial Boards.

I think there were two important facts which allowed these fascinating discussions to be rational and clear (well most of the time!). On one hand our apparatus was well designed and well understood, but also most important I think was the quality of the software. I realized through the years how crucial this was; either for the big building blocks but also simple beautiful ideas like the EDIRs or the simplicity of ALPHA or the beautiful event display of Drevermann. All this I think helped enormously non-specialists (like the spokesman!) to share the understanding of an analysis and contribute to the final result by relevant questions. My only hope for the future of particle physics is that this will still be possible in the LHC years.'

Lorenzo Foà:

'It is difficult to describe how pleasant and easy it was to be spokesman of Aleph. Jacques handed me a collaboration in very good state and my only duty was not to damage it too much during my term. The major happening in this period was the leak in the magnet (see Pierre's article) and I could not do anything but support the fancy idea of Pierre to install a full workshop with milling machine up in the air in the underground cavern. It was a risk—but it was a major success!

I had to intervene also in the quality control (as we say today) of the various editions of the Monte Carlo simulations, because too often a version was released with something missing and I gave this responsibility to a small group led by Roberto. Since then the problem disappeared. For the rest, I had to spend time looking into events, following various competing analyses. Learning a lot of physics (most of which I have now forgotten!), reading papers and encouraging a lot of young physicists to work also at night. What could I dream better? It was only too short.'

Gigi Rolandi:

'I have written a few sentences:

It took me three months to understand that the role of spokesman is to take decisions. When I started my period—after Jack, Jacques and Lorenzo—I was so worried about making mistakes that I was not taking decisions... and the experiment was really going into stall.

I still remember the Thursday meeting where the 4-Jet paper was approved. It was in the EP conference room in Building 13 and it was difficult for me. I cut the discussion short and I made many people unhappy... but the paper went through.

However, in order to inspire myself, for these few sentences, I was reading the speech that Ioana gave for my farewell party and I have copied it (see below). Copying was far easier than writing the two sentences above. I do not remember many details. I have only a big feeling of 'nostalgia' for that very nice time of my life.'

Ioana's speech at Gigi's farewell party as spokesman in 1997:

From this period of three years I will certainly remember the preparation and running of the first high-energy period of LEP in November 1995 and the enthusiasm you succeeded to communicate to every single person in Aleph. It was not really surprising to me who has watched you during the pilot run in 1989. You stayed awake for something like 50 hours because it was absolutely crucial for you to see the first Z in Aleph. We will also remember, all of us, the 4-jet saga. It is not yet finished; you leave the task to Peter, our new spokesman, I wish him a lot of luck in this and other matters and I really hope that we will get real answers to the 4-jet puzzle as soon as possible. What we appreciated a lot in this whole business was the transparent way it was handled at the risk of information leaking out of Aleph. I believe strongly that this was the right thing to do.

Another thing I want to say is, that during your spokesmanship, you encouraged some very bright young physicists in Aleph to take responsibilities in this new phase of LEP. This proved to be a very clever move in my opinion. I said that you served Aleph for the last three years. In reality you have dedicated your energy to Aleph for a much longer time... but before you only got back pleasure and enjoyment (without responsibility)...'

Peter Dornan

It was truly a baptism of fire, specifically a fire in the SPS control area which prevented the run in 1997 starting on time. There were fears that the 1997 run may have to be abandoned but, after the kind of superhuman effort we grew to recognize as normal from the machine people, the first major run of LEP above the WW threshold began in early August. The next years would allow new tests of the precision of the Standard Model and searches for predicted states in regions where hopes could be high.

Over the next three years the machine's performance just became better and better, both luminosity and energy records were constantly passed, 64 pb^{-1} at 183 GeV in 1997, 196 pb⁻¹ at 189 GeV in 1998 and 249 pb⁻¹ at energies between 192 and 202 GeV in 1999. It was a feast, always better data pushing the Standard Model, always the hope of the great discovery as we explored uncharted areas.

The four jets

There was also a fiery start on the physics front. The famous, or infamous, four-jet signal. At the end of the running in 1995 LEP had made its first step towards LEP2 with short runs at 130 and 136 GeV. These were used to test the emerging analyses for the Higgs boson search. Surprisingly a small excess of four-jet events appeared at 130 GeV and remarkably a similar one at 136 GeV. There was no rational explanation and the statistical significance, even together, was inadequate to make great claims. The other experiments claimed nothing but when their data was combined there was again a small effect.

What should we do? Few believed it to be more than a statistical oddity but history is littered with cases of experiments missing important results. Consequently in his last presentation to the LEPC, Gigi had suggested a new LEP run at 130–136 GeV to clear up the mystery.

It was not a universally popular request, the other experiments were lukewarm, some theoretical colleagues were incredulous—after all there was no theoretical explanation and so it must be wrong. Nevertheless, the wish to see this settled was endorsed at an Aleph Plenary meeting and so, at my first LEPC in September 1997, I argued the case for another run. The request was for twice the original luminosity but we came to an agreement that if no excess was observed with the original luminosity when the experiments were combined, the run would be stopped. It went ahead, no excess was seen with the original luminosity, Aleph even had a small deficit, so the run stopped and the four-jet saga came to an end. The theoreticians could feel exonerated.

Building the LEP2 Infrastructure

The move to LEP2 had not been without difficulty for the machine. Small sets of data had been taken during 1995 and there had been two short test runs above the WW threshold at 161 and 172 GeV in 1996. The slow progress to LEP2 had discouraged some and the increasing attractions and pressures of the LHC programme had caused others, including many who had been very active during LEP1, to move away from Aleph or at best remain peripherally involved. This drift had to be stopped and the infrastructure rebuilt, in particular it was necessary to engage the younger members of the collaboration more positively. There had been some complaints that Aleph had been lagging behind the other experiments in this regard.

The first task was to fill the senior positions. Dieter took over as chair of the Steering Committee and Ron would handle the collaboration meetings. Both Dieter and Ron understood the collaboration very well, Dieter's advice and appreciation of the CERN system was invaluable and Ron ensured our meetings went smoothly.

The other major position was that of physics co-ordinator, now to be given its correct title in place of the enigmatic Thursday meeting co-ordinator. With the main changes planned in the physics analysis areas, this position would prove to be the most critical and it was a pleasure for me that Alain agreed. During the next years we had to work closely together, particularly during the occasional crisis, and it became a very positive relationship. Two changes to the physics groups were made in order to engage more—and more younger—members in the organization. The first was to rotate the group co-ordinators on a regular basis and the second was to have two co-ordinators per group. This allowed younger members to work with more experienced colleagues and share the organization. Despite some teething troubles and occasional differences between the co-ordinators, which required Alain to arbitrate, the system ultimately operated successfully.

Fortunately most of the experts for producing the data from LEP1 remained. Olivier continued as Lord of Echenevex; in the years ahead his team would constantly and successfully strive for higher and higher efficiency and, if my memory serves me well, legitimately achieved a recorded efficiency greater than 100% on one short run! The online group under John and Beat always ensured the Aleph DAQ took priority as they moved their attention to the challenges of LHCb. Data quality was vital and new procedures ensured faults were tracked with speed. John (Carr) continued to maintain a smooth software operation so that the data were swiftly available, a tradition continued with equal success by Jacques when John moved to take charge of ANTARES. Brigitte continued as queen of luminosity and Monte Carlo. Bolek, Joe and Maria would handle our contact with LEP and Maria would charm the LEP machine crew to ensure we received appropriate luminosity. All institutes continued to support their subdetectors, the Echenevex meetings took place every morning of data taking where Jim, as well as organizing the social activities, would ensure we never missed the latest sporting news, particularly on the very rare occasions when Scotland did well. The foundations of the Aleph success were in safe hands.

LEP2 Physics

There were three major physics areas at LEP2 and each attracted a different persona, the WW area for those who felt unfulfilled if nothing was measured, the Higgs area for those with aspirations to Scandinavia, and the SUSY area for those trusting theoreticians and seeking nirvana.

The WW area continued the electroweak activity. The big prize would be the W mass, one of the main parameters of the Standard Model. With the 1997 and following years' data the luminosity would be adequate to achieve a precision significantly better than that from the Tevatron and an important input to the Standard Model fits. However, determining an effective mass of ≈ 80 GeV to 40 MeV demanded that, even at this late stage, it would be necessary to embark upon even greater studies of the systematics of the detector, particularly the calorimeters. Small, even very small, corrections could easily change the measured mass by 20–30 MeV. There were also theoretical difficulties. The procedures adopted relied heavily on the simulation, which
did not include effects such as the hypothesized colour reconnection. This began to cause difficulty when the statistical accuracy became better than 100 MeV. This is still not fully resolved and, although values have been published, the final W mass result remains outstanding in 2004. The WW area also allowed investigations of gauge couplings inaccessible at LEP1 but the most surprising result concerned the W-pair production cross-section. In 1997 there were a number of consistent predictions for this and so we were apprehensive when we consistently measured a value about 2% too low. After extensive checks we bravely announced this result; the theoreticians had another look, included more terms and reduced the prediction by... 2%. It was a small but satisfying victory.

The Higgs area was the most competitive of the physics areas. To find the Higgs boson was, and still is, a dream for most particle physicists, particularly those working at LEP. It is the great unknown, the great mystery that should give the final indication of the degree of the validity of the Standard Model and quite probably an indication of what is beyond. The SUSY area, in particular, has a very rich Higgs sector. The Higgs group was large, the number of channels many and we had enough people to support different analyses for each of the various final states. Usually these were totally independent, neural nets or cuts, and both had to be consistent before we would give a result. There would also be directly competing analyses and these caused degrees of excitement and conflict, particularly from those schooled in the alternative ways of Descartes and Confucius. Conflicts can be detrimental but they can also be invigorating; the conflicts we experienced in the Higgs area had both consequences.

On the positive side Aleph established a procedure for looking for the Higgs which was second to none. To ensure no bias was caused by the actual data, a procedure was established to optimize the analysis on the previous year's data and then freeze it before data taking began for that year's running. It worked extremely well and, as the analysis could not be changed, it could be run immediately the data had passed quality control. Thus new results could and did appear on a daily basis and with a smart bit of programming by members of the Wisconsin group (Editor's note-RS: see the Wisconsin Higgs story below which explains the 'Behold' tool) the latest results were made available on the Web for the whole collaboration, usually by the following day. It was a great innovation. It allowed everyone, including those away from CERN, to join the excitement, to have hopes raised when the statistics went our way, to share the disappointment when they returned to normal. No other experiment was prepared to show similar confidence in its procedures. Unfortunately during my time no signal materialized, although perhaps it did the following year.

The SUSY group did not have the conflicts there had been in the Higgs group. SUSY has so many different varieties that no matter how many people one had there would always be another channel, another way of looking for things and so very few overlapping analyses. This did not mean that there was lack of innovation and imagination. It was a very fertile area for new ideas, in one case even modifying the simulation for the interactions of a SUSY state as it passed through the detector. The SUSY group ran extremely smoothly and frequently set the standard for the LEP results. With so many channels, there were bound to be exciting statistical fluctuations and these did occur, causing temporary excitement but invariably, as we took more data, the statistics calmed things down and regrettably no real evidence for SUSY was found. Nevertheless, important steps were taken in restricting SUSY parameter space under many assumptions, gravity mediated, gauge mediated, R-parity conserving, *R*-parity violating etc. and a major result was a robust mass limit on the lightest neutralino, still the favoured candidate for dark matter.

LEP1 Physics

The main focus was naturally producing results as quickly as possible with the new LEP2 data. Nevertheless, there remained much to be completed from LEP1. The final Z line shape results had to be published, analysis of the tau polarization needed to be completed and in the heavy quark and QCD areas, there were still significant opportunities to extend results.

During the LEP1 period the reconstruction programs had been steadily improved and so we knew it would be beneficial to reprocess all the LEP1 data. This was a massive task as not only the data but also the Monte Carlo would need to be regenerated and reprocessed. Would it be worth it? It was one of the first decisions which had to be taken. Predictions of the gains, particularly in the heavy flavour area, were substantial and so we decided to proceed. John masterminded the operation and many put in extraordinary efforts so that the whole process was completed in less than one year. This gave a uniform data set with our best quality reconstruction on which the final LEP1 results could be based. During the following years the final paper on the line shape and lepton asymmetries at the Z would be published, a 100 page effort by very many people which received much critical acclaim and, after finding an arbitrator for our two Paris teams, the best LEP results on the tau polarization would appear.

The new data set led to a revival in heavy flavour activities, which had stalled somewhat in the interim between LEP1 and LEP2. A new analysis group was formed under two new young co-ordinators and new results appeared, including the world's best limit on the B_s mass difference, Δm_s , and an observed decay asymmetry which was indicative of the CP violation results to follow in later years from the B factories. Throughout this time the QCD group continued; they of course could benefit from the high energy and so were able to extend the analyses into the new energy realm. The higher energy also stimulated an expansion of the two-photon efforts. Consequently by 1999 the thriving physics activity was on a similar scale as it had been in the LEP1 peak.

Relations with the LEP machine group

In the first days of Aleph relations between the experiments and the machine group had often been tense, however, over the years we had begun to appreciate their problems and they had begun to appreciate ours. Regular talks by Steve, Roger and other members of the machine group at Aleph plenary meetings became the norm, Patrick became LEP co-ordinator and mutual suspicions died as all realised we were on the same side. All we wanted was higher energy, higher luminosity—and the discovery of the Higgs! The efforts and ingenuity with which our LEP colleagues responded to the first two were remarkable, and time will tell if we just managed the third.

Beer in the Coop

The LEP experiments were still CERN's flagship data-producing experiments although the evolving LHC, and in particular its financial difficulties, increasingly occupied the mind of the management. Probably as a result of this there were times when it was felt that management, and sometimes the LEPC, were making decisions without appreciating the impact they may have on the LEP experiments. This led to more collaboration between the experiments and during one difficult period we decided the only course was a common response. The spokesmen would have to meet and evolve a united position. This was a significant new departure as there was still healthy rivalry. The first question was where could we meet?—it would have to be neutral territory—and so the decision was a beer in the Coop at 5.30 on Wednesday. Thus the following Wednesday Bob Clare, Rolf Heuer, Wylbur Venus and I sat down with a beer and planned our riposte to management. Bizarrely I cannot now remember what the issue was but that day we decided that such a meeting was a positive step and we would continue to do so every month. Although competition between the experiments continued, serious friction stopped and the management realised that we had to be consulted about changes. Relations with the LEPC similarly went through highs and lows although Peter Zerwas, when chair, was very supportive.

Support and Sadness

As everyone knew Aleph ran sweetly, not because of the physicists, but because of the excellent support personnel. It is impossible to mention all from CERN and the Institutes but, without Ariella, Valérie and Monique, being an effective spokesman would have been impossible. Also I would like to add a personal thanks to Joel who rescued me, or rather my PC, very late one night before a crucial LEPC.

Times were mostly happy but there were sorrows. Shortly after I took over we were saddened to hear that Ronald Hagelburg had been killed in a climbing accident. He had been a mainstay of the software infrastructure. Then, later, we all had a great shock when Elizabeth Bishop Martin, perhaps one of the most loved and iconoclastic members of the collaboration, died suddenly. They were a great loss.

Being Aleph spokesman was a great privilege, a highlight I never imagined the day I asked Jack if Imperial could join after the demise of Electra. There were difficult times, both internally and externally, but such is to be expected with ambitious colleagues and the world waiting for our results. In general the teamwork, expertise, and comradeship within the collaboration were extraordinarily positive, productive and a joy to experience.'

Dieter Schlatter

'The most dramatic occurrence during my time as spokesman was the Higgs saga, so here are my reminiscences of those times and events leading up to them.

The search for new physics was always high up on the agenda of Aleph, starting at LEP1 in preparing the tools and it culminated at LEP2 during the last year of running at LEP in 2000. We had created in 1987 the Higgs Task Force to make sure that we would concentrate all our forces to beat the competition. Our excellent detector together with the most advanced algorithms—from energy flow to neural networks—should have enabled us to either find the Higgs or at least allow us to set the best limits. We had plenty of improved methods, and most of the time the non-expert had difficulties seeing the difference in the performance of method A versus method B. But for the experts of course, they were worlds apart!

By 1999 our analyses were well advanced, so that we decided to introduce an online selection and analysis program, a system which was called BEHOLD (Editor's note-RS: see the Wisconsin story below for more details). It would calculate confidence levels online and display the results on the Aleph Web pages. People would regularly click on the BEHOLD page to see if any sign of a signal was building up or not. One major advantage of this program was that it excluded the temptation to 'improve' the methods while data was coming in. During August 2000, while running at the highest energies the accelerator could achieve, the confidence level plot of BEHOLD suddenly started to develop a peak as if a Higgs particle together with the associated Z particle would have been produced in our detector. Each particle decayed into a pair of quark jets, giving a signature of four jets. By September, an effect compatible with the production of a Higgs particle with a mass of 114 GeV was about 3 standard deviations above the background expectation. Aleph was electrified! The Higgs Task Force meetings became Plenary meetings.

The LEP accelerator was supposed to terminate by end September 2000 unless a significant scientific result would require a continuation. On 5 September the LEPC meeting should decide, if this was the case. Aleph made the statement that we 'can neither claim nor rule out that the excess observed in the Higgs search is a first sign for the production of a Standard Model Higgs'. We did request an extension of running to double the statistics at the highest energy. This was supported by all the other LEP experiments. The result was a two months extension!

Meanwhile, the four LEP experiments had decided to break with the tradition of mutual secrecy and actually exchanged and combined the Higgs search results from all four experiments as fast as possible.

On 5 November the LEPC met again and since no additional candidates were recorded by any of the four experiments, the verdict was predictable: the LEP era is terminated! Emotions were strong, not only in Aleph, and it took some time to accept that we have to wait for FNAL or the LHC to get the final answer on what we recorded during the summer of 2000 in the Aleph detector...'

Roberto Tenchini

'As one of my predecessors said, 'every good experiment comes to an end', so I was asked to be the last spokesman of Aleph...! One of the highlights of my 'shift' actually started just before I was asked to do this job in Aix-en-Provence and I was still physics co-ordinator. We were collecting the last data at the highest energy and we saw three beautiful Higgs candidates. Days and nights, nights and days, (of many of us) were spent trying to answer physics questions from our LEP and non-LEP colleagues, physics and non-physics questions from journalists (I had my share, mostly Italians...). This continued until very recent times; any possible effect was reviewed many times. Well, after all, the three events are still there, with very similar background probabilities. Maybe in ten years from now we will know what happened!

Another refrain of my period was the struggle to complete the final publications, without losing the traditional high Aleph standards. Not easy at all, with more and more people with growing commitments in new experiments. For searches it was rather fast (well, unfortunately no new physics, folks!) but putting the word 'end' to precise measurements proved more difficult than expected. Systematic errors are naughty guys!

Let me conclude by saying thanks to all of you!!!'

RUN CO-ORDINATION Claus Grupen/Adolf Minten

When the construction period neared completion, the question of how to run such a complicated experiment consisting of so many different subdetectors was discussed extensively in the collaboration. It was not too difficult to agree on various event triggers, but how to collect the data online in a co-ordinated fashion required a very careful planning.

Initially an 'Online' and an 'Operations group' were in charge of handling the running of the experiment. The online group, which was responsible for data acquisition, building events, storing data on disk, and controlling the data-taking runs, was headed by Wolfgang von Rüden. They worked intensively to make sure that the more than 100 front-end microprocessors were configured correctly and received their commands to send the data up to the larger computers in the right sequence. It became a custom to start the configuring of a run as much as 24 hours before beam was expected, to make sure that all the components would be in working order when collisions were finally available.

The operations group under the leadership of Jörg Wotschack was responsible for all aspects of data monitoring: event displays, histogram plotting, alarm handling, and error logging. In the first year of data taking, and not only then, the event display was extremely useful. The father of this pioneering work of graphically representing the properties and performance of the detector providing a first look at the physics was Joe Rothberg. The operations group was also responsible for recording and monitoring of machine-related backgrounds which were expected to be a major problem. Also in this case Joe Rothberg acted very successfully as LEP contact person. The basic tools prepared for looking at the data and verifying its integrity were ready in time for the pilot run in August 1989.

A practice of holding daily meetings at 9.00 each morning was already instituted in 1989 by Horst Wachsmuth. A representative of each subdetector and trigger system was present along with people responsible for offline analysis. At these meetings all subsystem problems were mentioned and a plan and timetable for solution was formulated. These meetings continued (although with slightly reduced attendance) to the end of Aleph running and played a major role in maintaining the good working order and spectacular efficiency of Aleph running.

From the group of the online experts two people, Wolfgang von Rüden and Jörg Wotschack, pioneered the task of data acquisition and they prepared the ground for the successful running. The problems of day-to-day running were handled by Olivier Callot, the data quality monitoring and run management was mainly in the hands of Bill Cameron, assisted by Mike Green, Lothar Bauerdick, and Ramon Miquel. Liz Veitch and Horst Wachsmuth were responsible for the shift organization.

In the first years there was a kind of rush to participate in the data taking by going on shift. Later some encouragement was necessary to find enough people for the important daily duty.

THE IDEA OF RUN CO-ORDINATORS APPEARS

This was all very fine for the early days, but soon it was recognized that the task of running the experiment could not rest on the shoulders of a few dedicated individuals but rather had to be distributed. Sharing responsibilities implies creation of a structure to organize the running. It became immediately clear that, certainly for the initial periods, one expert responsible for each subdetector should be present in the control room or at least permanently available. This presented a conditio sine qua non. But as in an orchestra it is not sufficient that all the instruments be properly tuned, they must also harmonize. Therefore a demand for a kind of conductor arose who would co-ordinate the efforts of the subdetector experts. Such a person must not necessarily be able to play all the instruments but rather should have an overview of what was at stake. Therefore it was only too natural that very soon the idea of having run co-ordinators responsible for the whole of the experiment took shape. The first run co-ordinators were installed at the end of 1989. This job had to be shared in such a way that one run co-ordinator should have a term of two or three weeks after which-being tired from being occasionally called in the middle of the night—he had to be replaced by a fresh one.

Members of the Aleph experiment qualified as run co-ordinators if they were not engaged with a special subdetector, but rather responsible for the whole of Aleph. The initial crew of run coordinators consisted of Peter Dornan, Friedrich Dydak, Lorenzo Foà, Adolf Minten, Frederico Ruggieri, Ron Settles, Dieter Schlatter, Ken Smith, Klaus Tittel, John Thomson, Ioana Videau, and Horst Wachsmuth. Some of these run coordinators of the first days were later replaced or joined by Michael (Mike) Green, James (Jim) Lynch, Roberto Tenchini, William (Bill) Cameron, Claus Grupen, Francesco Ragusa, and John Rander. Pierre Lazeyras and later Jean-Paul Fabre acted as technical co-ordinators and were responsible during shutdown times. Although introduced by the needs of the early data-taking periods, in later years there have been rumours that the institution of run co-ordinators was created as a model of how to care for elderly physicists.

A conductor is, of course, completely at a loss if he does not have gifted people who know how to play the instruments. Apart from the subdetector co-ordinators it was absolutely necessary to have someone who would continue and co-ordinate the work of the primordial operations and online expert group. The online activity was taken over by Ioana Videau and later by John Harvey. They harmonized with Olivier Callot who—as the soul of the Echenevex group—played a dominant role in data-taking activities over the years.

LEP CONTACTS AND DAY-TO-DAY RUNNING

To guarantee high-quality data at high luminosity it was also important to know what was going on in the LEP control room. In particular, the background conditions were considered to be crucial for the experiments. The lifetime of sensitive subdetectors like the TPC or VDET could be in danger if large beam losses should occur. The co-ordination between LEP and Aleph activities required some experts who acted as link persons between the accelerator people and data takers. Joe Rothberg pioneered these important machine contacts. Initially this was done by Joe Rothberg and then by Thomas Lohse and Jordan Nash and more recently by Bolek Pietrzyk and Maria Girone. In later years dedicated background experts were installed, especially when LEP was upgraded and higher backgrounds due to increased synchrotron radiation were expected.

The problems of day-to-day running were handled by the Echenevex group. The people on shift (including a person from each subdetector, at the start) were kept busy watching for glitches in the data acquisition and for anomalies in the gas, voltages, and cooling systems. At the outset many subdetector experts had to spend time in the 'pit' to check electronics and other systems. As time went on, more computer-based monitoring of data was installed and three 'pit tours' per day were sufficient to verify that electronics, gas systems, etc. were working correctly. By the end of a few months of running, many automated checks on data quality and detector performance were in operation. Eventually elaborate 'expert systems' were developed to respond quickly to data acquisition failures and to automatically report discrepancies in detector response.

THE PILOT RUN

After LEP did final beam tuning including orbit corrections, beam-size squeezing, and injection optimization, the beams were finally put into collision on Sunday 13 August 1989 at about 11 p.m. The beam collimators were still open and backgrounds were very high so the TPC could not be turned on to full voltage. Just before midnight Opal claimed to have seen the first Z^0 in their calorimeter. About fifty people crowded into the control room were looking at every event to try to identify a Z^0 with the detectors that could be turned on. The offline analysis team was standing by to process the data as soon as tapes were written.

By about two o'clock in the morning collimators were set to the inward position and the TPC could be ramped up to its normal voltage. Backgrounds were low and the tracks in the TPC were surprisingly clean. The first clear Z^0 was not observed until about 13:15 the next afternoon but a careful look at the data taken earlier revealed a Z^0 signature in the calorimeters that was somehow missed by the crowd. Electron-pair events were seen soon after and even a three-prong tau decay was observed by late afternoon on 14 August. The detector and the data acquisition were in good shape and events were much cleaner than anyone had hoped for.

Z events could be recognized in real time by simply looking at tracks and at the energy deposited in the calorimeters.

By the evening of the 14th it was already clear that Aleph and LEP were a big success and that data would eventually be plentiful. Asymptotically a systematic structure for the run organization developed:

- The Subdetector Co-ordinators on duty must be present at CERN and reachable for possible problems of their subdetector. In the first year of data taking the permanent presence of all subdetector experts in the control room was necessary, but later it would have presented a burden in many respects.
- 2) One of the two LEP Contact Persons, representing Aleph, must go to the regular meetings at Prévessin and report at the nine o'clock meeting on the interface between LEP and Aleph. On the Monday and Friday scheduling meetings at Prévessin they were normally accompanied by the run co-ordinator on duty.
- 3) The Run Co-ordinator, chairing the daily nine o'clock meeting, takes short-term decisions, and reports to the Thursday and/or Plenary Meetings. Later the delivery of croissants at the Sunday ten o'clock meetings was introduced and became another important duty.
- The two Shift Persons, the Shift Leader/SLIMOS and the Data Manager were responsible for the data taking. This system was introduced in the second year of data taking.
- 5) The Echenevex group had to solve every-day problems at the pit that no one else could or was willing to deal with. One important job, namely the training of the shift people, was also in the hands of the Echenevex group.

This system of shared responsibilities survived over the years and guaranteed harmonious running and data-quality management up to the last round of data taking in the year 2000.

AN OUTSTANDING EVENT

The run co-ordinator, chairing the daily nine o'clock meeting, takes short term decisions and is responsible for the reliable running and the safety of the experiment. Even though there have been many spurious alarms, only one real safety alarm occurred: a fire in a TPC power supply. There was no real danger. This alarm was handled exactly as had been anticipated in the training for several years. The fire brigade came, the run co-ordinator happened to arrive when the fire started, as if he had smelled it, and all subdetector co-ordinators came within a quarter of an hour. The fire though was a little disappointing. There were no real flames, just a lot of smoke. The fire brigade were not impressed. The power supply was soon replaced and data taking could continue. The only deviation from the expected behaviour of the safety precautions was that the power was not cut in the rack which was on fire.

BASIC DUTIES OF THE RUN CO-ORDINATORS AT THE DAILY MEETINGS

The daily meetings chaired by the run co-ordinator on duty were held in the seminar room next to the Aleph control room. In the pioneering years these meetings could sometimes be as long as one hour. The present record in the year 2000 stands at three minutes—frequently achieved during this final year of data taking.

One of the important duties of the run co-ordinators was to deliver fresh croissants to the participants at the Sunday ten o'clock meeting, for some subdetector co-ordinators a very good reason to look after the performance of their detector at the pit. Another custom was to celebrate outstanding events by offering a few bottles of champagne to the attendants at the nine o'clock meeting and also to the LEP crew at the Prévessin meetings. Reasons for such occasions could be a very high integrated luminosity over the last 24 hours, a new energy record, or anything else that could be used as an excuse to have a drink. On one occasion a run co-ordinator justified the champagne by arguing that according to recent findings the Universe was 15 billion years old, and this was certainly a very good reason for a celebration. These daily meetings have become so customary that it will be very difficult in the future not to steer your car to Echenevex every day, including Sundays.

(Editor's note-JL: See photo at the end of 'Echenevex Group' article.)

ECHENEVEX GROUP

Olivier Callot

In the early days of Aleph, up to 1989, there was an 'Operations group', in charge of handling the running of the experiment. This group produced many tools, like the Presenter, Event Display, communication software with LEP, Run database and also some technical software. However, a severe conflict developed between this group and the Online group, in such a way that the Operations group was dissolved at the end of 1989, and its work left to be done on a goodwill basis.

For a few years, the handling of operation-related tasks was performed by a few individuals, Bill Cameron for the Data Quality, Olivier Callot for day-to-day running, Liz Veitch for shift organization. The software was maintained by the Online group. In 1992 it became clear that the good will wasn't sufficient any more, and that a group had to be formed, with a clear mandate to avoid a similar clash with the Online group. This new structure was approved by the collaboration in February 1993. The main function of the group was to support the shift crew in operating the experiment. This implies shift organization, training, documentation, definition of the tools to monitor the data (Presenter, Event Display, etc.) and the performance.

SHIFTS AND TRAINING

A shift crew of two people runs Aleph. Beginners have three shifts as number 3 during physics to complete their training. Some shifts statistics are given in the table below. DM and SL are the usual abbreviations for Data Manager and Shift Leader.

Year	Shifts	DM (New)	SL	People with the highest number of shifts			
1992	1353			33:	Marco Cattaneo		
1993	1236			31:	Marcello Maggi		
1994	1421			27:	Markus Schmidt, Adel Trabelsi, Stan Thompson, Andrea Venturi		
1995	1476	52 (34)	29	33:	Olivier Callot		
1996	1262	40 (28)	23	26:	Andrew Betteridge		
1997	1185	36 (21)	23	27:	Franco Ligabue, Peter Van Gemmeren		
1998	1339	47 (29)	26	33:	Franco Ligabue, Marc Swynghedauw		
1999	1394	41 (18)	32	39:	Tommaso Boccali		
2000	1548	49 (24)	28	39:	Tommaso Boccali again		

As can be seen, some people like shifts. In the early years, shift registration was like a gold rush, the registration was closed after a few weeks, for example three weeks in 1993. But, due to ageing or decrease of enthusiasm, the registration never closed in recent years... The record for having the greatest number of Echenevex duties at the same time is held by Jim Lynch: On 26 July 1998, he was simultaneously Shift Leader, ECAL, LCAL, SiCAL, Echenevex and Shift co-ordinator!

Training was one of the heaviest duties. Each crew member had to follow a mandatory Safety training, and a course adapted to his function, SL or DM. The format of the training evolved with the years, with the introduction of dedicated courses for beginners, and a new introduction to the DAQ and Slow Control. The training was even converted to colour with PowerPoint for the 2000 training season! One still missing feature was to explain how to get to Echenevex, as several beginners in the last years did not know where the experiment was!

DOCUMENTATION

Another heavy load! The first big task was to produce various manuals. The second task was to maintain and update them during all those years, with all changes in the system and in the LEP operating mode. Chris Bowdery wrote an initial manual using DECWrite.

A completely new implementation was then produced by David Casper using DECDocument, this is the famous 'ONLINE 101'. This '101' puzzled me for several years, until I decided to confess my ignorance to Dave. He explained that in US universities, the first course you have to register for always has this number, so you should read it as 'Introduction to the Online world'. Manuals for Subdetector and Run Co-ordinators were also produced. One of the most famous sections is the description of the duty of the Run Co-ordinator to bring croissants for the Sunday meeting!

ALEPH PARTIES

This was not originally in the official list of duties to be fulfilled by the Echenevex group.

However it became a solid tradition to have a party during most Aleph Weeks, with labels 'start of run', 'end of run', 'middle of run' or any other pretext, in fact having fun together was the only real motivation.

The running of the parties was, like the rest of the system, almost completely automated, and documented on the Web.



Olivier celebrating at the grill.

Some famous moments:

- The Italian band called 'The Leaning Towers' animating a change-of-spokesperson party.

The band was made	up of
Duccio Abbaneo	Piano
Alberto Messini	Sax
Alessandro Cardini	Guitar
Roberto Tenchini	Bass Guitar
Cesarone Da Bari	Drums
Franco Ligabue	Vocals

- Dave Casper performing his famous 'Blues Solo' with 'The Leaning Towers' at Echenevex and at Martina Franca
- Football and Frisbee games on the ground in front of La Chenaille, and the subsequent search for the ball in the evacuation pipe, in fact a pipe-line where the new (lost) ball pushed out an old one, lost at a previous party.
- French 'andouillettes' or Scottish Haggis?

PERFORMANCES

The famous 'Big Brother' screen was introduced in the middle of 1991, to display in front of the SL the performances of the current fill. Integrated over the years, the inefficiencies (in %) are listed below. The guideline for the Echenevex group was 'Everything that can be automated should be automated', and this is how improvements were made in the global performances. The biggest improvement is due to the Online group, as the DAQ inefficiency has been almost irrelevant since 1996.

TOOLS

The most famous tool was CIA introduced in 1994. Another American name, as you can guess... It replaced the work done by permanent 'Data Managers'. (Editor's note-JL: 'CIA', Compile, Interpret and Archive, was a tool to enable subdetector cordinators to update a database of hardware problems and to archive them when they were understood and solved.) The Daily Report was another great invention, allowing each SD co-ordinator to get a global view of the performances and problems of his detector when entering the control room for the nine o'clock meeting. This daily meeting produced a lot of minutes, which were taken by the Echenevex 'piquet' from 1994. However, a program was written which produced a template with the statistics lines, the shift list, and included the names of the co-ordinators, so producing the minutes online was reasonably easy.

Year	Operation	DAQ	Dead time	Total	
1991	10.89	11.10	2.60	22.8	
1992	≈4	≈6	≈6	≈13	
1993	3.93	6.60	3.29	13.22	
1994	4.39	4.79	3.49	12.15	
1995	5.14	2.80	2.13	9.76	
1996	5.21	0.64	1.30	7.04	
1997	3.95	0.74	1.50	6.09	
1998	3.33	0.92	3.05	7.14	
1999	2.43	0.34	2.50	5.18	
2000	1.59	0.80	2.08	4.40	

MEMBERSHIP

The Echenevex group was regularly renewed, with a small core of people working there for several years. In 1993, the group was not responsible for taking the minutes, but included 'Data Managers' (dark block) and a specialist in documentation, Chris Bowdery.

There was also some help from time to time by past Echenevex group members, mainly in the last two years when the group was reduced to only four members, one of them teaching and commuting from Scotland.

	1993	1994	1995	1996	1997	1998	1999	2000
Paolo Azzurri								
Chris Bowdery	Doc							
Olivier Callot								
Bill Cameron								
David Casper								
Marco Cattaneo								
Fabio Cerutti								
Alessandro Giassi								
Maria Girone								
Corinne Goy								
David Hutchcroft								
Cal Loomis								
Gerd Lutters			1 week					
Jim Lynch								
Elizabeth Martin								
Fabrizio Murtas								
Bolek Pietrzyk								
Philippe Rosnet								
Sascha Schmeling								
Ingrid TenHave								
Lee Thompson								
Edwige Tournefier								
Jeff Turk			1 week					
Andrea Venturi								
Alison Wright								

INCIDENTS AT ECHENEVEX

Everyone has his own memory of special events during the past 10 years.

- Two data managers were doing the pit tour when a thunderstorm triggered the usual fast magnet discharge. The magnet's UPS wasn't working, all valves opened, and the helium was released into the cavern, with a big bang and a nice cloud. The pit tour came back very fast, and with white faces! One ran faster than the other: 'The strong should not risk their safety to help the weak—in case of emergency save yourself!'
- One night, the pit tour never came back, as the lift stopped in the middle. The fire brigade had to rescue them.
- During another pit tour, a Glasgow student wanted to switch on the lights in the gas building, but pressed the wrong button, the emergency stop... Since then, the lights are left permanently on in this area.

- Only one real safety alarm took place, a fire in a TPC power supply. No real danger; in fact all occurred as had been indicated in the training for several years. Except that the power was not cut in the rack which had the fire!
- The cleaner once decided to clean the nice red button, the Emergency Stop in the computer room, which is protected to avoid exactly what occurred immediately—an abrupt power cut on the computer and network equipment.
- We always liked to drink champagne (even if it's not that great at 9 am), and the collection of empty bottles was filling the shelves. However, a complete cleanup had to be performed for the open day in 1994. Some safety official thought it was not appropriate to show that we have fun at work!



There were always plenty of occasions to celebrate at Echenevex! (Editor's note-JL: Before the 'safety' cleanup!)

EDITORIAL BOARD

Jean-François Grivaz

Within a few months after the beginning of Aleph operation, it became clear that some procedure had to be set up in order to make sure that the whole collaboration could feel collectively responsible for its rapidly increasing number of publications. While the scientific relevance of the results was examined in the physics groups and ultimately approved in Thursday meetings, there was no specific body primarily in charge of the editorial quality. An 'Editorial Board' was therefore appointed for that purpose, comprising a small number of admittedly wise senior physicists. Ultimately, the membership was extended to younger (but hopefully equally wise) participants, and it currently contains fifteen members.

The procedure, which leads a paper to the stage where it is submitted for publication, is the following. After approval of the analysis in a Thursday meeting, a first draft is produced and submitted to the collaboration. The chairman of the Editorial Board appoints two referees who will scrutinize the details of the analysis, collect the comments, and help the authors to produce a new draft. When the referees are content with the status of the draft, the Editorial Board meets and examines all aspects of the paper, from the most formal ones ("You should not instruct the reader with 'Note that' ...") to questions on analysis issues. In some, fortunately very rare instances, serious errors were discovered at this ultimate level of investigation.

Strict editing rules were never set down by the board, but the fact that the membership extended over an arbitrarily long period of time (indeed until resignation, and a few of the members have resisted resigning for more than ten years!) guaranteed some continuity in the Aleph style. There nevertheless remains a difference between two classes of papers: those which had the privilege of being 'Steved' and those which did not. (In the first category, for instance, a 'b quark' will always remain roman and unhyphenated.) (Editor's note-RS: 'Steved' means having been carefully read and commented on by Steve Wasserbaech.)

The Board was, however, confronted once with a problem which almost triggered a religious war. It all started with a PPE referee who suggested that 'systematic errors' should be avoided in favour of 'systematic uncertainties'. Oxford and Webster, among others, were called for help, but this was not sufficient to reach a consensus. After a number of sometimes humorous (but not always) mail exchanges, it was finally decided not to decide anything... As a result, the subsequent Aleph publications show a fair balance between 'errors' and 'uncertainties', used in an essentially random fashion.

The typical duration of the Editorial Board meetings was two to three hours per paper, but a twelve-hour record was established on a Sunday in November 2000, when the paper reporting the 'Observation of an excess in the Higgs boson search' was discussed in a Board extended for this occasion to include a few invited experts. The modifications suggested were implemented essentially on the spot (actually overnight), and the paper was ready for submission the next morning.

SPEAKERS BUREAU

Jacques Boucrot

As soon as the first physics results arrived in 1989, the Aleph management decided to create an internal committee to choose and propose Aleph physicists as speakers at international conferences and workshops. Known as the 'Aleph Speakers Bureau', this body meets 5 to 7 times a year, mostly just before the Summer and Winter conferences. There are about 12 members, the spokesperson and the Thursday meeting convener being *ex* officio members. Other members are appointed for two years and are in general the conveners of the physics analysis groups in Aleph.

The Bureau is in charge of looking for conferences or workshops where Aleph talks can be given. A member of the Bureau is given the task of contacting the organizers and proposing topics for talks. Then, after discussions, the Bureau proposes the speakers. This may imply several iterations, especially for the large international conferences (ICHEP and EPS) where long negotiations (with parallel session organizers and other LEP experiments for the sharing of talks) are unavoidable.

A list of possible speakers is regularly updated following suggestions sent to the spokesperson mostly by group leaders in the Homelabs or by Physics group conveners.

Since the beginning, a strong preference has been adopted to give talk opportunities to young postdocs or to PhD students just before or after their thesis, with the condition that they have worked at least two years in Aleph. For more senior physicists another rule has been applied: They have to wait at least two years before becoming eligible for selection to give a new talk from Aleph. In the last years these rules have become somewhat loosened for some subjects (e.g. Heavy Flavours or QCD) where the number of talks proposed by Aleph was greater than the number of possible speakers!

Being the only body in Aleph where discussions and judgements are made on individuals, the discussions in the Bureau are strictly confidential and the members of the Bureau are expressly asked not to disclose any detail of the internal discussions. The minutes and the list of possible speakers are not public; they can be consulted at the Secretariat by group leaders only. The only public outcomes are the list of forthcoming talks and speakers already approved, and of course the list of all the talks already given.

The work of the Bureau grew with time, owing to the ever-increasing number of conferences and workshops. This increase allowed the Bureau to offer more and more talks in recent years, with the drawback that some of these conferences were not always of a great scientific level. More than 600 talks have been organized by the Speakers Bureau since 1989: 50 per year on average, with a record of 68 in 1999.

SECRETARIAT Ariella Mazzari/Valérie Brunner

In October 1982, the Aleph Secretariat was created from the CDHS Secretariat, and both Aleph and NA31 shared the services of Ariella, two part-time ladies (Janet and Suzanne) and one full-time (Suzy). Suzanne left the group shortly after and Gabriella arrived. We poor ladies had to fight hard in order to get a room and nicer furniture but, thanks to Pierre Lazeyras, we finally got it and, for a long time the Aleph Secretariat was regarded as a 'model secretariat' by the other secretaries: We all felt very proud of it.

Ronnie became part of the team at the end of 1988, when Gabriella left, and shortly after, Janet also left to join the Secretariat of the SPSC. Ronnie stayed with us for about three years, working part-time, and then decided to take up a full-time job with the Wisconsin group... but this proved to be too hard for her and she had to give up after about one year.



The 'model secretariat'.



Gabriella, Ronnie, Susy and Ariella.

In October 1994 Valérie joined the team and, in February 1995, Suzy retired, though she had already stopped working a couple of months before for health reasons. Shortly before Christmas 1995, Suzy left us finally, leaving behind a sense of emptiness and of 'ce n'est pas juste' with all those who had benefited by her kindness, her good will and her cheerfulness. In the meantime Yvette had arrived and so we continued for four years until the retirement of Yvette. Valérie has become the 'good fairy' of the secretariat (efficiency and cheerfulness are the two pillars of her character) and more recently Monique joined the team, helping the now mixed Aleph/LHCb Secretariat.

In December 2001, Ariella also retired. Fortunately she did so 'softly' allowing us time to take over her work. Monique and Valérie did their best to take care of the Aleph Secretariat and also help Nathalie Grüb with LHCb matters. The strength of the team lay in the fact that there was always a very good atmosphere in the office and all the ladies have very fond memories of working together. A few months after Ariella's departure, Valérie was offered a position in the EP Division Secretariat. Since the Aleph work was slowing down (as the decreasing number attending Aleph parties proves!), she accepted and left the team in January 2003. Sandrine came to help Monique in her task but she also had to leave after a few months. Now Monique and Nathalie are the last 'survivors' and they have the pleasure of continuing to help Aleph and LHCb physicists.



Valérie, Ariella and Monique.



Valérie, Nathalie, Monique, Ariella and Sandrine (in December 2002).

Subdetector/Hardware Stories



Tracking



VDET1 Lorenzo Foà/Ron Settles

THE ALEPH VERTEX DETECTOR FOR LEP1

Behind the backdrop around the Aleph Vertex Detector, VDET, lurks another dramatic story. The VDET was originally proposed in 1982 by Pisa (led by Marcello Giorgi) on the basis of the pioneering experience gained in the NA1 experiment on charm photoproduction at the SPS. But in those days, the proposal foresaw only two layers of single-sided Si-strip detectors. This was clearly a shortcoming and was mainly due to the technology for microelectronics still being in its infancy, so that the limitation was due to the number of channels which could be instrumented. (CCD technology was around but, when it was studied a few years later as an option for Aleph, the CCD option was deemed too slow for the LEP environment.) Because of this limitation and because of the lack of money and manpower, the VDET was staged by the Aleph collaboration to be installed in a 'second phase' (whatever that meant!).

MPI-Munich joined the VDET game a couple of years later, as it also had a leading tradition in Si detectors for charm physics at the SPS. In 1984 Gerhard Lutz and RS visited Lorenzo Foà in his office late one evening where they discussed the possibility of upgrading the original VDET design to a version with two layers of double-sided strips and the necessary microelectronics (for which MPI-Munich could also ask for additional money). Lorenzo received the suggestion with enthusiasm and they decided to write a note to the Aleph collaboration proposing to build this improved detector and to 'unstage' the VDET!

This proposal was accepted by Aleph, and Pisa and MPI-Munich joined forces to build it. Pisa had already done a lot of the groundwork and Marcello Giorgi was chosen to lead the VDET team. Pisa and MPI-Munich both applied for and got additional money and they started work. The going was slow in 1985–1988 because it took time for the technology to ripen and because of limited manpower (these were the years in which all the rest of Aleph was built and installed!).

Now followed a very sensitive phase, and the next part of this story is told from my (RS) point of view at MPI-Munich. (*Editor's note-RS: My point of view* was not remembered in the same way by everyone at Pisa or MPI. For example, in an email exchange with Andreas Schwarz about this, he saw the story from another perspective, and this is told below, in italics.)

Tension arose between Pisa and MPI-Munich as it became clear that VDET would be late. (It's always that way with things that aren't going too well!) Our task distribution foresaw Pisa being responsible for the wafers and mechanics and MPI-Munich for the microelectronics. Since the latter was really the place where the technology needed time to develop, it put MPI-Munich in a tough spot. I (RS), as MPI-Munich group leader, wrote a memo to Lorenzo, Marcello, Jack, Ettore and a couple of others, expressing this unhappiness and requesting that Pisa help out on the electronics and MPI-Munich would help out on the other things. Many people were upset with me for having written such a memo, and, in the end, it had good effects and bad effects. One good effect was that Pisa started helping on the electronics and MPI-Munich got into the mechanics and both of these changes helped the project to succeed. A bad effect was that MPI-Munich also built wafers which they thought should go in and this created new tensions between Pisa and Munich! The Munich wafers finally turned out somewhat less advantageous than those from Pisa which were the ones finally installed.



So we did manage to get in a couple of prototype faces for the first LEP running in 1989 and then again in 1990. They didn't work very well (if at all!), but we learned a lot. Finally in 1990–91 we got our act together, built better wafers/faces, had better electronics (Siroccos from Delphi) plus MPI onwafer chips, new mechanics (carbon-fibre support structure and better installation methods), and succeeded to get a full coverage VDET installed at the last minute (if not the last second!). There was an Aleph Plenary meeting in April 1991 where there was a progress report by Hans-Günther Moser on VDET and the collaboration asked exactly what the steps were for installation. Some weak 'steps' were mumbled (by Hans-Günther) while, in fact, the steps were being invented on a minuteby-minute basis. When we mentioned using black Scotch tape to fasten down some pieces for the aircooling being installed, Jack hit the ceiling, saying it was 'below Aleph's 'niveau' to allow the use of Scotch tape'... little did he suspect, because we had a ton of Scotch tape in there...!

And, much to everyone's surprise, the thing (like many things in Aleph) 'worked like a dream'. Many people will remember an Aleph Plenary meeting around mid 1991 where the first results using VDET1 were shown and they were flabbergasting! Everyone is used to its performance now, but to see this unfolding then, where it was working to expectations (and hope!) was unbelievable, especially in the light of all the trials and tribulations leading up to it. The whole VDET team was finally vindicated by this success and peace settled again over Aleph...

Andreas Schwarz took over from Marcello as team leader and then went on to DESY after a few years. (*Editor's note-RS: Now to the email exchange with Andreas—he wrote back:*

Hi Ron,

I just read the VDET stuff. God, what times those were... It will sure be interesting if and what other colleagues will write. I am sure it will be very hard to do everybody justice...everybody will remember things differently. I, for one, have rather a different set of reminiscences compared to yours... The way you write it, things look much less problematic and traumatic than they really were... For what it's worth: in my recollection, we had really reached an impasse with our Italian colleagues as both groups pushed ahead on their own in a very competitive way. Both Munich and Pisa had solved the double-sided problems, Munich had built the readout electronics, designed the hybrids and new mechanics. In 1989 ('1st' attempt) we installed 3 modules of varying origin and in 1990 ('2nd') we installed 1.5 layers of mixed

origin which would die because of various problems (power supplies, AFACs, e.g.) and the performance was shitty because of (a) this, (b) the pin holes created in radiation accidents and (c) the TPDs (8 bits, which were not enough, and no common mode subtraction). Around that time we enlarged the group by outside people (Santa Cruz, Wisconsin, Marseille and CERN). This helped considerably in diffusing the problem between Pisa and Munich. I remember the tough decision by the Munich group to move to the Pisa detectors for the sake of physics! It was a breakthrough, but a tough one. In this new ('3rd') effort we changed many things: another set of new modules, now all with Pisa detectors, yet another set of mechanics, replacement of the AFAC and TPD systems with new stuff (12-bit digitizers from Delphi), a radiation protection and dump system (Santa Cruz)... and indeed on May 8th, 1991 did we (Hans-Günther and myself were on long term shift) observe the first beautiful event online... This was a happy moment for all of us, Pisa, Munich and the rest, and resulted in the 'ovations' mentioned by you... Cheers, Andreas)

Andreas was followed by Luciano Bosiso, then Ettore Focardi and later on by Paschal Coyle. We continued to maintain and improve VDET1 over the next few years of LEP1 and produced an amazing abundance of 'beautiful' physics at the Z peak.

VDET1, which was so successful for LEP1 physics, had two drawbacks. The polar-angle acceptance was limited (to 45°) and the faces were rather thick (1.5% of a radiation length). The planning for an upgraded version to ameliorate these points for LEP2 started rather soon in 1992.

VDET1 was then replaced by VDET2 in the fall of 1995 for running at LEP2. For VDET2 new collaborators had joined: Florence, Marseille, Rutherford, Imperial and Glasgow. Munich reduced its role to being responsible for the installation and for some of the mechanics where the general philosophy, similar to VDET1, was adapted for VDET2, the story of which follows next...



Reconstruction of displaced vertices by VDET.





Logbook entry by Andreas Schwarz on the occasion of the detection of the first hadronic decays of the Z in VDET.

VDET2 John Carr/Paschal Coyle

Motivated by the desire to increase the efficiency for the detection of the Higgs decay into pairs of b quarks, a new vertex detector was proposed and constructed for the LEP2 data taking. In fact, the VDET2 was really 'born' in a pub during the 1992 Aleph Plenary meeting in Glasgow. During the meeting various options for its design had been presented but no clear consensus had been reached. Andreas Schwarz finally guided us to the right choice over a few drinks but almost immediately left us on our own to complete the task, preferring to seek his fortune as spokesman for HERAb!

Compared to the original VDET1, the VDET2 design, as the name implies, was twice as good; the main differences being that it was twice as long, half as thick and radiation hard. Based on the results from test beams during the summers of 1993 and 1994, the various technological choices were made. One important choice was whether to use Viking or MX7 readout chips. This led to friendly competition between the UK and Italian groups, culminating in the construction of two prototype modules. The many power supplies needed to power the detectors were affectionately nicknamed the 'Tower of Pisa' and the 'Tower of London'!

The production of the modules for the final detector proceeded relatively smoothly under the leadership of Luciano Bosisio, who used his considerable experience to design detectors, which proved to be exceedingly fault free. The modules were fabricated by CSEM in Neuchâtel. Readout hybrids were provided by Glasgow and the Kapton for the z-readout supplied by Florence.

The wire bonding was performed at Pisa, Bari and Rutherford. Testing of the glued modules was made at Imperial College, Glasgow, and Pisa. In Marseille pairs of modules were glued together onto an 'omega' support beam and finally shipped to CERN for mounting onto the carbon fibre support structure and a final test.

The schedule for the production and assembly was rather tight and, for the first LEP2 run at ≈ 133 GeV, a total of 19 of the 24 faces were installed. The detector operated very successfully and its data was rapidly incorporated into the standard Aleph offline analyses. The only small problem was the observation that the Kevlar support beams were sensitive to humidity variations and thus, during the winter shutdown, extra carbon omega beams were added.

The running of the detector during the subsequent five years, co-co-ordinated originally by Ettore Focardi and subsequently by Paschal Coyle, was fairly painless, although nearly every year the detector was removed to fix small problems in one or two modules. These problems turned out to be due to faulty connections or cracks in the capacitors of the hybrids or, in one case, due to real radiation damage caused by a LEP beam loss in the detector. The removal and installation of the detector, supervised by Hans Dietl, was always a rather nerve-racking experience. The sight of Heini Fischer carrying the detector up and down the precarious scaffolding will be forever etched in the minds of many nervous spectators! This should be a lesson to future designers of vertex detectors => don't make the detector too easy to remove!!

(Editor's note-RS: I couldn't disagree more; Aleph profited from the easy maintainability of VDET, for which we worked hard to achieve. To solve the 'problem' with Heini, it would have been easy enough to design a system using the crane in the pit to transport the VDET up to the middle of Aleph. Had the VDET been larger/heavier, this would have been a necessary addition.)

Near the end of VDET2 operation and after only eight years of trying, the VDET group was very proud to finally win a long-standing bet with Mike Green. Mike had promised two bottles of champagne if, during one of his periods as Run Co-ordinator, the VDET power supplies did not give a problem. Mike was true to his word and the champagne was drunk during a VDET meeting, which took place in a café in the Piazza del Campo during the 1999 Aleph Plenary meeting in Sienna.

The VDET2 now lives out its retirement, sitting pride of place, in the CPP Marseille entrance hall.

(Editor's note-JL: Part of VDET2 managed to find its way to Glasgow, where it is proudly displayed in the Department of Physics and Astronomy.)



Some VDET2 builders.

ITC Peter Dornan

I was returning from vacation via CERN, waiting in the Coop to pay for breakfast when the spokesman-designate of ELECTRA approached me. 'They have turned us down'. It had always been a possibility; five experiments had been proposed for LEP; we knew there would be no more than four, maybe only three. Plan B had to come into effect.

At Imperial we had worked hard for ELECTRA but realism had caused us to examine the other experiments should there be disappointment. Which of them had the same basic physics goals, which had a niche where our expertise would be valuable? Aleph satisfied both goals and the niche we saw was the tracking area close to the interaction point, building on the two wire chambers we had produced for TASSO.

The TPC, which Aleph had bravely chosen, was without doubt the best tracking device for a LEP

experiment, although experience from SLAC had not been totally encouraging. However, it posed two major difficulties. As the drift time would be >50 μ secs it could not supply the tracking for the first-level trigger and, to prevent space charge build up, it actually needed a fast ($\approx 2 \mu$ sec) trigger to open the gate and become sensitive. Secondly, also to prevent space charge effects, it was considered inadvisable to have the TPC too close to the beam. A further inner tracking device was thus necessary to provide tracking from the beam pipe to the first TPC pad at about 30 cm radius.

The inner chamber needed to satisfy these requirements had therefore to produce a fast track trigger and accurate $(r-\phi)$ tracking inside the TPC. The latter would be used in the first instance to locate displaced vertices and later, after the introduction of the silicon vertex detector, to link tracks from the vertex detector to the TPC.

Different ideas abounded concerning how the track trigger should be provided. For those from a none⁺e⁻ background it was obvious—with a solenoidal field tracks were straight in the r–z projection and so this was the projection for the trigger. However, for those from an e⁺e⁻ background this was outweighed by the difficulty with a cylindrical wire chamber of providing accurate points in the z-coordinate.



The ITC at Imperial College.

Discussions were warm and led to a compromise; there would be two trigger processors, one in $r-\phi$, the other in r-z! But this did not finish the argument. How would the z coordinate be measured? Cathode bands were the chosen form of the non-e⁺e⁻ fraternity, anything but for the e+e- one. The alternatives were charge sharing or the much less used time-difference approach. Charge sharing required 2 m long resistive wires and this would degrade the signal and thus the $r-\phi$ resolution. Thus time difference was preferred and a prototype was constructed with both cathode hoops and time-difference electronics. Tests in a beam at CERN showed that whilst both could work, time difference had the advantage as it enabled a much more robust chamber to be constructed. At a meeting late in 1985 the collaboration backed our choice of a chamber with rigid endplates, simple, small drift cells and a z readout using time difference.

Despite all the discussion over the z readout much more critical for the performance of Aleph was the r- ϕ processor. This used programmable masks on the hit wires to search for tracks with transverse momentum above a threshold of $\approx 1 \text{ GeV}/c$. This information was then used to predict which trigger sectors of the calorimeters the track could reach. A positive signal from the ITC r- ϕ processor became a necessary component of all triggers which depended upon final states with charged tracks, including Aleph's two main triggers, charged electromagnetic (ITC and ECAL) and hadronic/ muon (ITC and HCAL/muon chambers). Programmability meant that a dead wire could easily be masked so that the efficiency did not



The ITC in the centre of Aleph.

suffer; it also allowed special triggers to be formed such as a simple back-to-back one, which enabled the efficiency for the triggering on Bhabhas and dimuons to be evaluated. With the exception of purely neutral final states, the ITC r- ϕ trigger fired for virtually every interaction used in Aleph results. In a specific survey for τ -pair production the failure rate was found to be less than 4×10^{-6} . A final irony was that, as the outer radius of the chamber was ≈ 30 cm and the fast decision time needed to open the TPC gate meant that the drift time could not be used, all tracks with transverse momentum greater than 1 GeV/*c* were essentially straight lines passing through the ITC.

Time difference enabled us to design a novel processor, the space point processor, for the r-z trajectory in which the time difference for the signal to reach the two ends of the wire was expanded by a factor proportional to the radius so that tracks from the origin would line up in expanded time difference. The processor worked as predicted but its efficiency depended upon the time-difference resolution, which depended critically upon the gain in the chamber. As the performance of the ITC was to become critical, it was decided to run at modest gain; this had only a small effect on the $r-\phi$ resolution but increased the z resolution from the 1.5 cm obtained in the test beam to ≈ 5 cm. The space point processor became a valuable tool to improve the trigger selectivity for interactions with weak signatures such as two-photon processes.

Worries that there would be insufficient test beam time in 1988 demanded that the chamber be ready for final tests in 1987; and with a major effort this was achieved and the chamber comfortably passed its beam tests with performance slightly better than expectations. Two years were to pass before the chamber next saw beam with the start of LEP in summer 1989. From then until the end of running in 2000 the chamber and its processors worked admirably. A few modifications were made during the running: the gas was changed from 50:50 argon–ethane to a more conservative 80:20 argon–carbon dioxide and when the chamber occasionally showed signs of depression a little alcohol was added. The high-voltage system was also totally replaced in 1993 to one with greater granularity. Otherwise there were few problems, Aleph never ran without the ITC and at the end of running just 15 of the 960 sense wires were dead.

Many were responsible for the success of the ITC but for the construction Mike MacDermott and Derek Miller pulled out all the stops, ably assisted in various areas by Geoff Barber, Ray Beuselinck, David Binnie, Marco Cattaneo, Dave Clarke, Roger Forty, Dave Gentry, Ann Heinson, Dave Price, Julia Sedgbeer, Les Toudup, and Andy White. Many more took their share of ITC shifts and checking performance. However, most of the credit for the fact that no more than 10 days of Aleph running were lost over twelve years as a result of ITC problems goes to Bill Cameron. During the whole running period Bill kept the ITC in excellent condition (in fact Bill and the ITC appeared to develop a mystical bond). One year, after a perfect period of running, Bill (or was it Carol?) decided he must go on vacation. Within hours of leaving the control room three wires began to draw large currents and it was at this point we decided the ITC needed alcohol to overcome the trauma. It worked, no more wires died that year-but the recalcitrant three could not be consoled until Bill returned and nursed them back to health.

The ITC still looks beautiful. Its outer carbon fibre shell has been replaced by a transparent perspex one, the gold wires still glisten and it holds centre stage in the lobby of the High Energy group area of the Blackett Laboratory. But now the sharpeyed will notice that there is one broken wire; this happened when it was being prepared for display—never in operation.



Julia checking the ITC.

TPC Ron Settles

The Time Projection Chamber had a turbulent history, which is probably the same for most subdetectors in the collider experiments of this world. But the TPC went the gamut, oscillating between failure and success over several dramatic years.

When some us of proposed a TPC as central tracker around 1981, there were two main obstacles: (1) the original Pep-4 TPC built by LBL wasn't working yet, and (2) there was a counter-proposal in Aleph for a 'big sphere' detector concept with main tracking being done using a standard drift chamber. The idea behind the big sphere was that, since the e^+e^- physics is spherically symmetric, the detector should be too, and a 'big-sphere' study was published 12 June 1981.

Some of us pushed ahead (Walter Blum, Jürgen May, Robert Richter, RS, Ulrich Stierlin and Henri Videau), writing our ideas down in a note at the time of the Villars workshop (in the spring of 1981 I think it was) recalling the principles and advantages of a TPC. This evolved into (what later became the Aleph Notes) the note entitled 'Report of the TPC Working Group' (with the aforementioned colleagues plus Heinrich Wahl) dated 19 July 1981. A bit later Gigi Rolandi, Francesco Ragusa (see Jack Steinberger's story above on the 'Detector Concept' for Gigi's and Francesco's important roles in the game) and others joined the fun, Jürgen May was 'elected' leader of the TPC group and at some point Wolfgang Richter got into the game. Meanwhile we started building small prototypes, to 'learn the ropes', so to say...

We weren't out of the woods yet because the Big Sphere was still a serious competitor. Peter Norton's article below called 'Early Aleph (Personal reminiscences)' (see also Jack's story) tells the happenings very nicely and is repeated here (for your convenience, to not have to thumb through these pages too much).

"The choice between the 'Big Sphere' and a Superconducting Solenoid. My recollection is that the motivation for the sphere was the uniformity of calorimetry and the reduction of the cost of the iron by tapering the magnet ends. The calorimetry at the time was assumed to be scintillators with wavelength-shifting fibres, which would be integrated with the iron and coil of the Big Sphere. The Solenoid Group produced a thick report by the middle of 1981 (amazingly quickly)—it was a feasible project. The Big Sphere, for all of Petrucci's efforts, was too complicated and it was abandoned in September 1981.

The decision between a **TPC** and an **Axial-Wire Chamber.** This decision was quite difficult at the time, as I recall, simply because there was no properly working TPC in existence. Reports were produced from both proponents. There were worries for the TPC about field uniformities, complexity of electronics, space charge effects, etc. Despite these the TPC was declared 'our preferred solution' in October 1981, although I recall that the wire chamber solution appeared as a back-up in the Letter of Intent." I visited LBL for two months during the following year to gain some experience with the TPC which was just starting to take data at PEP4. Milestones which arose from the Letter of Intent in 1982 (see above) were only related to the TPC, so that apparently the LEPC, through its chairman Günther Wolf from DESY, had already decided between the options for our central tracker, and I recall that the Aleph collaboration made the decision formally at a Plenary meeting which I couldn't attend. But by the Technical Report in 1983 the TPC design was well under way; the groups were (copied from the Technical Report article above)

"TPC: MPI, CERN, Wisconsin, Dortmund, Glasgow, Pisa, Trieste, Edinburgh + a financial participation of Heidelberg and Siegen"

which started joining forces to build the gadget. Later Dortmund 'drifted' to Mainz, while Heidelberg and Siegen took on other tasks (trigger and background monitor, respectively).

Then, by the 'Status Report' a year later, the design had essentially converged. Jack Steinberger had introduced his long radial pads and his nowfamous sector geometry with a zig-zag in the outer sectors to improve our hermeticity. The division of tasks amongst institutes, in addition to financial support, was (very roughly, from my alwayshas-been-so-isn't-related-to-age-hopefully dim memory): CERN—Fastbus-electronics, field cage, gas system, daq system, infrastructure; Glasgow and Mainz-laser and gas studies; MPI-Munichsectors and preamps/shaper-amps; Pisa-Fastbuselectronics; Trieste-mechanical support system (the 'wheel'), daq system; Wisconsin-calibration systems, daq system, performance. I won't try to cite all of the names here for reasons of space and time and because it is difficult not to miss someone, but the lists in our two handbooks, THE ALEPH HANDBOOK 1989, ALEPH 89-77/Note 89-03/28 April 1989, ed. W. Blum, and THE ALEPH HANDBOOK 1995, ISBN 92-9083-072-7, ed. Chris Bowdry, give a reasonable approximation but don't do justice to the students and technical personnel of all the institutes. The photos below are samples of the early days at the TPC90 test setup:

A story: the whole TPC90 phase took longer than hoped because Jürgen and the Wisconsin students spent too much time playing horseshoes.



Meetings, meetings, meetings...



...achieving technologal understanding of the FADC (cartoon by Mike Binder).

And another Wisconsin student Mike Binder was doing an in-depth study of the FADCs.

OK, so much for generalities; the complete history could go on for hundreds of pages. I shall wind up the rest of this article with happenings mostly from the sector-fabrication-and-operation points of view, since they're the parts I know best. All of the TPC collaborating institutes were enthusiastic and produced great results in an extremely pleasant atmosphere, and more stories for this book by other TPC colleagues would have been highly welcome. Well, here's another story. As you know, we ended up with a design with three types of sectors M, W and K—Mann, Weib (sorry) and Kind:

I remember a meeting with Jack, in 1984 roughly, in which I was explaining to him how much extra difficulty was caused by the little corners ('earlobes', we called them—'Ohrwaschl' in Bavarian) on the very inside radius of the K sectors (see pictures below), hoping Jack would say, 'Well, they are not really that important...' so that we could remove them from the design and make life easier.

However, Jack's response was, 'I'm not worried about the amount of work you guys at MPI have to do...' which ended the discussion and the design was kept as it was in the drawing below left (one thing nice about many discussions with Jack was that they were short and to the point) and turned out to look like the right-hand photo below (but I doubt if the 'ear-lobes' really made much difference to the performance), which ain't bad for beginners.

There are zillions of stories, but to not go on forever here is a chronicle of some of the happenings.

 1983–1984: Build TPC90 test set-up, build and test a series of two prototype sectors using the Berkeley Pep4-TPC electronics.



The three TPC sector types.

- 1984–1985: Finalize design of all TPC components, start production. (Another story: sitting in the TPC meetings where the party-line was often proclaimed that the sectors should be constructed within two years, I thought to myself, 'this is blauäugig'. Fortunately everything else also took about a year longer including the LEP machine, so I was able to finish on time, that is, within three years.)
- 1985–1987: Manufacture of all components. (For the sectors, we had two big disasters, along with several small ones, during production: [1] One morning I came into the assembly area and found the already-glued wires on about the third production sector drooping and almost touching the pad plane. We changed the design so that the wires were glued with a small wedge for further mechanical fixation-which eliminated the possibility in future. At the same time we did dozens of tests to find out why, but we never really did understand because we couldn't reproduce the effect because all testgluings were solid. The only logical explanation was that the two-component glue had been inadequately mixed. [2] When we started the high-voltage testing, the first sectors wouldn't



hold voltage beyond a few hundred volts! The culprit turned out to be a milling process during the manufacture which caused deposition of debris [small metallic chips] where the wires were attached and the cleaning procedure did not yet eliminate all of them. The problem was solved by beefing up the cleaning via thorough inspection/removal of chips and high-pressure washing with alcohol. Of course it took a few months to find/implement solutions to these two problems [Charlie Ackermann and the MPI team did a great job!], so that it ended up taking 3 years to manufacture the 36 sectors and 6 spares.)

– 1987–1988: Assemble all components in the TPC hall at CERN and commission the gadget. (Another story: When I was HVtesting the sectors at MPI, they would draw a couple of nanoamps of dark current after being broken-in; however, the first K sector I mounted in the big TPC at CERN started drawing ≈100 nanoamps when it was turned on, and I said to myself, 'that's funny', not really a very bright thought. As the TPC team started commissioning the electronics with the big chamber, huge distortions of the

tracks became evident and Jürgen May became very depressed. After a week of frustration a few of us were discussing one evening that maybe the gating, which had been dormant up to then, should be commissioned. When this was done the distortions disappeared and my K sector drew only a couple of nanoamps again. So what was happening was that with no B-field and no gating, the positive ions flowing back into the drift region were creating a large positive E-field which sucked in the electrons from tracks outside the region of that K sector and amplifying the effect even more. With the gating in operation, the tracks were straight, and everybody heaved a big sigh of relief! And we had a party, as usual...)

Ready to go!!

– 1989: Move to Pit 4, install and commission the full detector. (Well, you can imagine what this was like! There was one episode where Jürgen discovered an error in the readout chain, in the TPDs I think, then organized the CERN /Wisconsin groups to remove each of the units from the already-completely-installed electronics, modify it and reinstall it. There were 800 TPDS, but they managed to do it in a few days.) The TPC soon started working.



A. Sakharov signs a cosmic-ray muon event.

1989–1995: Take LEP1 data. (The biggest event here was the near disaster with the carbon fibres. During a repair job on the field cage at the end of 1991, a large number of carbon-fibres (≈1 cm long, ≈200 micron diameter) were inadvertently introduced onto the field cage, not exactly a healthy step for a region of large E-field. The carbon fibres caused shorts in the field cage which introduced distortions. Werner Witzeling had taken over from Jürgen

and set about, along with the technicians, to develop ways to find and remove them. Since we were taking 1992 data, there were distortions in the production data, and Werner Wiedenmann, Michael Schmelling [who also did a lot of good stuff on the TPC monitoring] and others developed mathematical techniques for locating the shorts in the field cage from the data and correcting the data. The carbon fibres would wander around [they were very light] due to the changing background conditions at LEP, so that after opening, removing one or two and closing again, a new short could appear a few weeks later after a beam loss. The TPC was opened about five times in 1992 to remove carbon fibres and a few times more in the following years, as one might rear its ugly head after another beam loss. Two reasons why this incident was not fatal to our detector were first, that the TPC was designed to be easily accessible for maintenance so that we could open, fix and close again within a few days, and second, that Werner Wiedenmann's techniques of correcting the data worked so well that the performance for the user was as good as if nothing had happened at all! Being completely transparent for the user meant that these two aspects, the design of the TPC and the mathematical techniques for correcting the data, are not well-known or appreciated by many of our colleagues. Around 1993 Pere Mato replaced Werner Witzeling, implemented the upgrade to VME, continued the battle with the carbon fibres and introduced many valuable ameliorations to the TPC monitoring tools.)

– 1995–2000: Take LEP2 data. (Except for occasional beam losses and carbon-fibre appearances, things went very smoothly from here on in, since the techniques existed for recovering from these incidents fairly easily were well understood. An interesting observation was that the higher the LEP energy and luminosity, the lower our backgrounds became: at LEP1 it was not unusual to run at ≈100 nanoamps/ sector, while at the end of LEP2 we were running with only a few nanoamps/sector.)



N.B. Design your detector to be easily accessible!

So much for the chronicle. The last thing to do is to make a few suggestions for the future. A TPC for the next e⁺e⁻-linear-collider (LC) generation should be designed to be easily maintainable: this property saved the life of the Aleph TPC after the carbon-fibre incident. The backgrounds will be higher at the LC, so that's another reason to want to be able to get in and fix it quickly. The TPC should have as high a granularity as possible to be robust against backgrounds (Aleph had about 20 million pads × time-buckets, while the LC TPC could have a factor of at least 100 times more) and be limited only by gas-diffusion granularity (which would be the LC case with that factor) and not limited by electronic granularity (which Aleph was). With higher backgrounds, (1) provision must be made for thorough alignment and correction techniques (similar to what Werner Wiedenmann did for Aleph-but we know ways to improve and these will be needed since the momentum-measuring precision at the LC will be 10 times better than at LEP), and (2) the readout chambers must be able operate well in such conditions. The Aleph TPC worked well up to about 100-200 nanoamps per sector, but we don't know exactly what the limit would have been, but the LC TPC should be able to handle about 10 times more. (In fact the LC backgrounds have been calculated to be about 50 times less than that, i.e., not so different from LEP, but the LC machine technology is new, and one should be ready for surprises.) These two conditions should make sure the LC chamber is robust in all situations.
TRACKING ALIGNMENT

Alain Bonissent

The Aleph tracking system is composed of three different subdetectors: the vertex detector (VDET), the inner track chamber (ITC), and the time projection chamber (TPC). Among these, only the ITC is constructed as a monolithic object. The TPC is composed of 36 independent sectors, and the vertex detector involves a total of 24 faces, with 6 or 8 wafers each (the first version of VDET had a smaller coverage in θ).

The geometry of each TPC module was carefully checked during the construction, and a survey was performed after the final assembly. But this is not sufficient to guarantee the required accuracy, of better than 100 microns, on the respective position of each sector. For VDET, we need to know the position of each silicon wafer with an accuracy of a few microns. This is far beyond the intrinsic precision to which the support structure can be built, given its typical size of some 10×40 cm. The ITC was built and its geometry was monitored with an accuracy of 10 to 20 microns, but its absolute positioning with respect to the rest of Aleph could certainly not meet this precision. Finally, the whole tracking system lies in a magnetic field, which could be measured only once, in 1989, when the first magnetic field map was established. In addition each subdetector establishes its own configuration of electric field, which can be described theoretically only to a certain extent.

For the various reasons mentioned above, we knew from the beginning that we would have to make use of real tracks in order to reach an acceptable degree of precision. From a conceptual point of view, all the procedure is based on redundancy: any track with more than three coordinates offers an over determined system of equations. Minimizing the χ^2 provides information on the respective positions of the hits on the track.

At this point, it should be noted that during the preparation of the experiment the final performances of the vertex detectors were difficult to anticipate.

In an historic perspective, we shall review independently each subdetector since each one had its own problems.

Compared to what they are now, the expected performances of the TPC in 1989 were rather modest. This is why the original alignment procedure was not very elaborate. The sectors were aligned only in the $r-\phi$ view, using the $\mu^+\,\mu^-$ events fitted to a single helix, and the drift velocity was determined from laser events. When VDET became operational in 1991, it became necessary to determine more precisely the drift velocity, so that VDET hits could be associated with TPC tracks. This was done with the socalled PASS0 method, which matches the primary vertex determined separately in the positive and negative z part of the detector. Then, in 1995, when the tracking procedures were revisited and the z measurements from pads and wires were combined for a higher precision, we discovered that the z resolution did not improve as much as expected. The investigation of this new problem resulted in a revision of the magnetic field map, which was now constrained to fulfil Maxwell's

equations, and a full 3-dimensional alignment of the sectors. At this point, the intrinsic resolution of the Aleph TPC was close to the design in the $r-\phi$ view. In the z view, it was still some 1.4 times too large. Further investigations, during the LEP2 era, proved that this was related to a timing problem in the readout chain. The effect was corrected partly by using VDET as an absolute reference.

The alignment of the vertex detector is a challenging problem, owing to the large number of parameters involved and the required accuracy of a few microns, well beyond the possibilities of any survey system. The original VDET alignment procedure was designed for the LEP1 run, where lots of Z⁰ data were available. The hardware design guarantees some overlap between neighbouring wafers. This provides a sample of tracks with redundancy within the vertex detector, independent of the outer tracking. Another such sample is the dimuon events, which are fitted as a single helix with four hits and a known curvature. However, preliminary studies proved that these two families of tracks were not enough to pinpoint completely all possible degrees of freedom, and a small number of hadronic tracks constrained by TPC coordinates were included in the fit. The χ^2 minimization was handled by Minuit. For the LEP2 running period a new vertex detector was built. The main goal was to increase the angular coverage, and this resulted in a larger number of wafers, and thus 1.5 times more alignment parameters. At the same time, a much lower luminosity was anticipated. In fact, in practice, only the short 'calibration runs' at the Z peak provided useful data for alignment, with only some 10⁵ Z⁰'s per year. The first consequence was that dimuon events would be in very short supply. Moreover, the new running conditions did not even guarantee that the two muons would be back to back. This is why we decided not to use them anymore, but to replace them by the common vertex constraint on hadronic events: all tracks are forced to emerge from a common point. A new fitting procedure was developed, based on a direct matrix inversion in the space of (864) parameters. The first and second derivatives of the χ^2 with respect to the alignment parameters were computed analytically. As an extra bonus, the new procedure enabled us to take into account the constraints resulting

from the optical measurements of the faces, prior to the final assemblage. With present-day computers, the full VDET alignment procedure uses about 10 hours of CPU time. During normal running conditions, the global position of the vertex detector is monitored with a laser system. Movements of order 20 microns maximum have been observed.

Usually, the complete procedure starts with the internal alignment of the vertex detector. To first order, this is not affected by the alignment of the other detectors. In the same step, the global position of VDET with respect to the outer tracking is also determined. Then, the ITC expert determines the preferred position of his chamber, using hadronic tracks. Finally the TPC sector alignment is based on dimuons constrained to ITC and VDET. If large displacements are observed, it may be necessary to iterate over the three detectors. When convergence is reached, some internal ITC constants can be adjusted. They correct for drift velocity and the non-uniform field shape around the wires. These corrections do not affect the overall alignment. It may then be useful to adjust the effective drift velocity and T₀ of the TPC for a better positional and angular matching with VDET.

In the absence of any unexpected problem, the complete procedure takes approximately a week after a sizeable sample of Z^0 events is available. It is usually revised at the end of the data-taking period, for time-dependent effects. Among others, a special procedure has been developed to correct for electric field distortions related to beam losses in the TPC, with a typical time constant of a few weeks.

Could we have done better?

With our present knowledge of the detector, the alignment procedure is close to optimal. However, there are a few things which we could have done better. The most obvious is the implementation of the alignment transformations. It would have been highly advisable to develop one alignment package for all three detectors, which have an intrinsic cylindrical geometry. This was not done, and even worse, we used a mixture of Euler angles and small-angle approximation matrices, with various sign conventions. This does not affect the quality of the final results, but makes the code more obscure to non-experts. For the TPC, the current limitations to the performances are related to our incomplete knowledge of the magnetic field map and of the timing in the TPDs. The magnetic field measurements were made in a very short period during the mounting of Aleph, and the experimental conditions were not ideal. After the complete assembly, such measurements could never be repeated, so that this will remain forever as an uncertainty. The alteration of the z coordinates by some extra delay in the TPC readout chain is another thing which could have been measured during the fabrication, if such a problem had been anticipated. But at this time the performances equalled or exceeded the specifications. In fact, the problem was not discovered until 1998.

The following names should be associated to the alignment: Dave Brown, Bill Cameron, Dominique Fouchez, Lorenzo Moneta, Anne Moutoussi, Fabrizio Palla, David Rousseau, Manoj Thulasidas, Ian Tomalin, Werner Wiedenmann.

THE TRACKING UPGRADE

In my opinion, the Aleph tracking system, and the TPC in particular, is one of the great success stories of the experiment. For this, enormous credit must go to the designers and builders of the TPC, and to the relative handful of people whose names you find in the comments of the original TPC reconstruction software—Robert Johnson, Dieter Schlatter and Mike Mermikides being some I remember seeing literally everywhere.

In 1993–94, with the VDET fully integrated into the tracking system, Dave Brown first suggested that a comprehensive upgrade to the tracking software might be in order. Some understandably raised their eyebrows at this suggestion, since Dave seemed to be recommending that we 'fix' something that clearly wasn't 'broken'. However, Dave knew that the other experiments, Delphi for instance, who were behind Aleph in tracking performance, were working hard on catching up, and he argued that 'good' might not be 'good enough' in a couple of years. The general outline of what became the tracking upgrade was written by Dave in a list of projects on a whiteboard during the first meeting he called, and in the end, nearly all the points listed were in fact followed up. Dave Brown's opinion about tracking carried considerable weight, since he had written the most widely used B-vertex fitter, and done extensive studies on its performance. Unfortunately for us, not long after the tracking upgrade work actually began, Dave left Aleph to work on BaBar. The departure of experienced people at watershed moments would be a recurring theme in the tracking upgrade.

The tracking upgrade took years, but early on we had clear evidence that there were large performance gains to be had if we persevered. One striking demonstration of that was the earliest study of TPC wire z information by Ian Tomalin. Ian showed that the wire data were potentially very useful, but subject to large, angle-dependent systematic effects which nobody understood. Also, pad dE/dx information showed great promise, but the task of creating a reliable universal calibration would be long and hard. This was another recurring (and somewhat frustrating) theme-as we refined the tracking performance, we found new systematic effects which had previously gone unnoticed, and had to first explain and then remove them before the algorithmic improvements would have any real value in analysis.

Those of us pursuing the project were lucky to have a spokesman, then Gigi Rolandi, who not only provided strong encouragement, but being a tracking expert himself, was also able to offer extremely valuable technical advice. For example, two papers Gigi co-authored while the TPC was being developed anticipated the ability (which we eventually exploited) to use pulseheight information from the wires to correct pad coordinates for Landau fluctuations along the track. Francesco Ragusa was another strong advocate who helped encourage and co-ordinate our work during the most crucial early period. Many others who had built the relevant hardware also patiently answered our questions over the years. For me probably the most memorable moment was when Pere Mato and one of his students helped me look at the response of the TPC front-end electronics on an oscilloscope. This was probably the turning point in mastering the TPC wire zcoordinates. What the scope showed was that the stretched pulse, which would be digitized, distorted from its nice Gaussian as the input pulse lengthened. For pulses long enough to correspond to tracks at large dip angles, the pulse resembled a square box more than a Gaussian. This clearly indicated that the apparent systematic shifts in the wire coordinates were actually caused by the pad coordinates, which we had been using to compare with the wires. The z-information from the wires (where pulses were short even for tracks at large angles) was actually more reliable than that of the pads, and we could therefore use it to quantify and calibrate away the pulse-shape distortions seen on the scope. We had finally gone from the abstract software level to looking at a pulse on an oscilloscope; after that point, what we were doing somehow seemed a lot more 'real' to me.

One important task identified by Dave Brown at the inception of the project was to improve the association of VDET hits to tracks by treating the assignment problem 'globally'. Since VDET hits had such high resolution, an incorrectly assigned VDET hit seriously compromised the quality of a track's fit. This idea was successfully put into practice by Paul Rensing and Jean-François Pusztaszeri. The technique they eventually adopted involved a sophisticated linear algebra method which avoided the time-consuming combinatorial nightmare that a 'brute force' approach would have led to. Because the method involved mathematics which was hard to explain, it was viewed with scepticism by some, but nevertheless they made it work. Unfortunately (again) Paul and Jean-François both left the experiment before the global pattern recognition was fully integrated into the evolving 'Julia 3.00', causing delays as others less experienced had to complete the final coding.

We also wanted to improve the reconstruction of V⁰'s, and identify kinks and nuclear interactions whose secondary products confused both the VDET pattern recognition and energy flow. Starting with code developed for the τ analysis by Gigi and Anna Gregorio, we were able to identify a large number of secondary interactions in beam pipe and tracking detector walls. Paul Rensing also developed code which found most track kinks by using the known kinematics of hyperons which could decay in flight. Absolutely vital to this effort was the enthusiastic support of Hans Drevermann, who regularly incorporated new features into the DALI event display so that we could visualize what was going on. It is gratifying to see these secondary vertices on Aleph event displays which I see from time to time even today. Improvements in V⁰ reconstruction were realized by Paolo Spagnolo and by incorporating energy loss in the TPC gas into the Kalman filter track model. As the list of modifications grew and extended to virtually every corner of the offline software, Marco Cattaneo and Florence Ranjard patiently (in most cases...) helped incorporate them and ensured that the final product would meet the expected standards of reliability and software quality.

Because previously unnoticed systematic effects were appearing at each stage as we pushed harder on the tracking performance, it was eventually realized that parallel improvements in the quality of alignment would be necessary. Anne Moutoussi led the effort in the ITC, while Alain Bonissent and Manoj Thulasidas tackled the VDET. Werner Wiedenmann attacked the most difficult aspect of the alignment, the complex interplay between geometry and electric and magnetic fields in the TPC. In a truly beautiful piece of work, he started from first principles and essentially solved Maxwell's equations subject to the complicated and non-uniform boundary conditions of the real TPC. With improvements to the algorithms and alignment all in place, we could finally see the system as a whole at work. The z-coordinate resolution in the TPC improved by a factor of five for the large dip-angle tracks which were previously the worst reconstructed. Secondary tracks were no longer associated visibly to the VDET pattern recognition, and after global assignment the incidence of incorrectly assigned VDET hits decreased by a factor of two. The large shift in the reconstructed mass of K⁰'s was eliminated. The dE/ dx information from the pads was now available even for tracks buried in jets.

As preparations got under way to reprocess all the LEP1 data, your reporter, last of the original tracking upgrade people, also left Aleph. The job of making the tracking improvements available to physics analyses fell to John Carr, who took over co-ordination of the tracking upgrade from Francesco later in the project. This by itself was a daunting task, even apart from the effort already invested in getting the programs ready. Working on the tracking upgrade and having the opportunity to understand the inner workings of a marvelous device like Aleph was a wonderful learning experience for me. Having joined after the experiment was already working and extremely successful, I was glad to feel I had contributed to making it better, rather than simply taking advantage of the labours of others. I'm also grateful for having had so many hard-working and talented people to collaborate with while doing it.



The tracking upgrade could detect an interaction in the wall of the TPC field cage, as seen in this example (which is a scanned event display since the original was no longer available).

Calorimeters, Coil, Muon



ECAL BARREL Jacques Lefrançois

This short story of the ECAL barrel will actually describe mostly the birth of the idea of the small cell calorimeter in the early years.

During the first brainstorming discussions involving mainly John Rander, René Turlay, Jean-Jacques Veillet and me, the initial idea in 1980 included a lead scintillator sandwich, which had been built or decided for UA1, UA2 and CDF (and NA3 my previous experiment). Another idea that shower localization could be done by MWPC and with pads (not by an ambiguous XYUV system) was also present.

When Ioana and Henri Videau joined Aleph in early 1981, Henri brought with him the idea of a system where the sampling system would be wire chambers, as had been done in the PEP4 experiment from which he had just returned. This was much more elegant than the use of scintillators and photomultipliers (in a magnetic field!!!) and was adopted rapidly. Actually there is an internal note in April 1981 by Henri and me which describes rather well the final detector with 12 modules inside the solenoid and chambers made of aluminium extrusion; even the idea of a fuse on each wire is mentioned, and all the advantages compared to a scintillator solution are explained.

The first design shown in the LOI (March 1982) included large towers (about $15 \text{ cm} \times 12 \text{ cm}$ I believe); in a few planes there was an ambiguous-free readout by delay-lines with a resolution of about 3×3 cm. The limitation, which necessitated these large towers at the time, was that we were afraid of the cost of the low-noise precise analog electronics needed to read out the detector. Later

in 1982 (around July) a new design was invented were the localization, still only in a few layers, could be done with cheap electronics (CCD type or bucket brigade device!) giving high granularity corresponding to roughly the size of a shower. These devices were, however, too imprecise and could not be well enough calibrated to be used to measure the full energy. The measurement was still being done using the big towers.

Somewhat earlier (Christmas 1981), we had decided to test a first prototype to measure the energy resolution of a calorimeter made of lead plates $(0.4 X_0)$ and wire chambers. Actually various modes were tested for the chambers: Geiger mode, streamer mode and MWPC, but only the last one turned out to be linear enough. A prototype of about 200 kg was therefore built, and, looking for a test beam in winter time (during CERN beamoff period), we decided to test the prototype at SLAC. This could only be affordable because of the link of Ecole Polytechnique with the defence ministry: the transport could be organized 'for free', by a French army flight from Paris to Mururoa which was stopping in Los Angeles. (I suppose you understand why our calorimeter was light compared to their usual transport to Mururoa!) Unluckily, part of the test apparatus and gas system was stolen in the customs zone at the airport, and the technicians from Ecole Polytechnique, A. Busata and P. Poilleux, had to perform miracles to reconstruct part of the system at SLAC. Finally the physicists on shift, Ioana and Henri Videau, Jean Jacques Veillet, Denis Bernard, one of their students, and I could measure the linearity and the energy resolution (about 16%/ \sqrt{E}) and we were rather happy and confident.

Then came a test in September 1982 where the test calorimeter was put in a big magnet in a CERN test beam, and as some pessimists had suspected the resolution degraded to $22\%/\sqrt{E!}$ This was due to a few infrequent delta-ray electrons of about 10-50 keV captured by the magnetic field in a spiral around the wires thus giving occasionally very large energy releases. A quick brainstorming session followed and I think it was Jack who suggested the use of xenon as the main gas instead of the usual argon: its high Z throws the electrons out of the spiral by multiple scattering. In time for the presentation to the LEPC of our progress on ECAL about two weeks later, I did a simulation program following the path of the 10-50 keV electrons in gas tubes which showed that xenon should save us. (I remember the program since it was the last sizeable piece of code I wrote in my life!) A test in December at CERN showed experimentally that we could obtain $18\%/\sqrt{E}$ (including an effect when the lead plates are crossed at an angle).

Around that time, happened what to me has been the most important breakthrough in the ECAL conception. We owe this to Bob Chase who was taking part in most of the brainstorming sessions on ECAL but also of other parts of Aleph. He convinced us that, with the evolution of commercial amplifier chips, he could design and build a multiplexed low-noise high-accuracy electronics for such a low price that we could afford to have 210 000 independent accurate readout channels. This made possible the ECAL as we know it with its small towers. This idea must have happened around September 1982, since in the September presentation to the LEPC it was mentioned as a possible option being studied, but not decided, and the large-tower design was also described at the same level! Of course Bob's beautiful idea was adopted rapidly soon after. I remember also the important role of Ian Corbett from Rutherford in those discussions on the electronics.

In parallel with the main option of MWPC plus lead sheet, another calorimeter option had been pursued by Daniel Fournier, using liquid argon. Daniel had not yet invented his beautiful accordion idea, and for mechanical and electronics reasons (the electronics signals were smaller and therefore the electronics more expensive) the liquid argon calorimeter design had a coarser granularity (about 12×12 cm). However, the energy resolution was almost twice as good, about $10\%/\sqrt{E}$. Aleph had to choose between the relative importance of fine granularity and of energy resolution. With the help of Monte Carlo simulation on electron identification and photon detection (most of them done by A-M Lutz), we could show the advantage of the fine granularity over energy resolution, and convince Jack and the collaboration (it was done in that order!) by the end of 1982.

I remember that by May 1983 John Rander and I produced a note, under the prodding of Jack, to show how we could build a calorimeter which was uniform by construction. A uniformity of 1% was aimed for and about 2% was achieved and had to be corrected... well one can't always win!

We had, in 1983, to share the work of construction. The French labs concentrated on the Barrel while Rutherford and Glasgow took the responsibility of the end-caps construction. Ecole Polytechnique, Saclay and Orsay had been joined in 1981–82 by Marseille (J.J. Aubert) who took the job of the gas system and its monitoring and by Clermont-Ferrand (B. Michel) who took the responsibility of the high-voltage boards, the wires readout and the high-voltage system. Clermont converted the idea of the fuse on each wire from a cute gadget to a professional and practical system which turned out to be very useful—the famous *Fusibleur*.

Ecole Polytechnique, Orsay and Saclay, with Henri Videau, J.L. & J.J. Veillet and John Rander and a team of engineers, shared work in a close-knit organization for the module construction with weekly meetings, to co-ordinate the movement of apparatus from one lab to the other. The frontend electronics was done by J.J. Veillet and Bob Chase at Orsay while the ADC boards were done by Jean-François Renardy at Saclay and the DAQ under the responsibility of Ioana Videau at Ecole Polytechnique.

It is difficult to do justice to the large effort of the years of construction. In each of the labs we had to make a transition from conception of prototypes to mass production (540 planes of wire chambers and cathode pads!) and quality control. Nothing had been done before on this scale at Ecole Polytechnique or Orsay, we had therefore to recruit personnel for repetitive labour either part-time undergraduate students or unemployed (TUC). I think most important was the development of a large number of machines for production and quality control. The cathode planes were built at Ecole Polytechnique and the equivalent of a printed circuit (but much cheaper) was done by a machine cutting a copper foil to make pads and welding wires to transfer the signal to the edge. Other machines checked that all pads were connected. It was so difficult that, retrospectively, it is amazing that it worked so well (after a painful first module). The aluminium planes were built in Orsay after measuring the height of all cells to 10 microns! The absence of barbs of aluminium was checked with a high voltage system on each plane. The wires were soldered in Saclay on HV boards from Clermont-Ferrand and each plane was tested for wire tension. Finally the stacking of chambers and lead sheets (measured in thickness to better than 50 microns) was done in Saclay and Orsay. But now came the most painful exercise. We had to connect the 2 million pads to form the 48 000 towers. A wire from each pad had to be

soldered on a bus (three buses per tower for the three-story readout in depth). This work was done by two teams of 'câbleurs' (cablemen) at Saclay and Orsay. The quality of our calorimeter owes a lot to the dedicated work of these teams who accepted this delicate and tiring job. Bob Chase invented a system (by pulsing in turn each wire plane) which allowed, quasi online, each pad soldering to be verified by the person who did the work. This helped enormously the morale of the 'câbleurs' (compared to being told by others of their mistakes) so they could make it a point of honour to deliver good modules. An overall test was done by the physicists at the end of module cabling with a surprisingly small number of errors such as cold solderings or short circuits.

Well, after all these efforts we inserted the modules in the solenoid in 1988–89 and after some initial difficulties in August 1989 the modules worked very well for the next 12 years. We had foreseen a 13th module for eventual replacement which, however, was never used.



An ECAL barrel model emerging.

END-CAP MODULES (RAL) Mike Edwards/John Thompson

In early discussions with our ECAL collaborators in France, it was proposed that the UK groups would share construction of the 12 barrel modules with either the Saclay or the Orsay group. The 24 end-cap modules would then have been built in a French institute not committed to the barrel. Fortunately, this idea was quickly abandoned when it was realized how much tooling would have to be duplicated in the UK and France, and that all drawings would have to be in both languages! So, following approval by our funding authorities in 1983, the UK teams began preparations to construct the end-cap modules. Two-thirds were planned to be built at RAL and the rest at Glasgow with substantial technical support from Lancaster University, Royal Holloway College, and Sheffield University workshops. This was the largest project we had ever undertaken involving major design and construction effort in mechanical engineering at RAL and Glasgow. In addition, we agreed to design and build at RAL all 221 000 front-end analog multiplexer circuits required for the full detector.

MODULE MECHANICS

It took two years to build the first end-cap 'petal' module. There were 45 wire planes to build and test. The first step was a 'flare' test in air at 1.8 kV. The first six 'successful' planes were stacked between lead sheets in a 'tank' where air could be excluded and replaced by a $80:20 \text{ Ar/CO}_2$ gas mixture to simulate more closely the conditions of the detector in operation. They all failed to hold the

required high voltage! This was a nightmare since direct observation of breakdowns was impossible in the tank and mechanical manipulations of the heavy lead sheets time-consuming. Whilst we were away at one of our many ECAL meetings in Paris, Martin Morrissey and Roger Gray erected a simple transparent 'tent' enclosing an argon atmosphere over the individual planes which allowed sparks from misplaced HV wires to be seen and the wire placement to be corrected! Thus, this 'flushing' test became an essential part of our build process at RAL. We were never sure our Glasgow colleagues did this since they always claimed that their wireplacing machine was superior.

The cathode planes were fabricated from six large, double-sided, printed-circuit boards comprising 1024 pads. A 'bed-of-nails' test was devised to check the electrical connection of each pad to its track on the underside of the PCB and its insulation from all neighbouring pads. Perhaps surprisingly, this was not done by the manufacturers and could not be taken for granted. This was the first of many occasions requiring scheduled shifts for all participants involved to adhere to the deadlines! Some of our ex-students and professors still recall this duty with 'affection'. Each wire plane was then sandwiched between a lead sheet and its cathode plane. This was isolated from the opensided aluminium extrusions of the wire plane by a graphited Mylar layer. We had just enough graphited Mylar, which was just as well because the French factory responsible for its manufacture burned down! All 45 wire plane assemblies were stacked on to a precisely machined base plate with

more HV testing as each storey was completed. After consolidation of the full stack in a large press and yet more HV tests, the 'side-wiring' began, connecting all pads together to create the 1024 projective tower elements in the module. Finally, after capacitance measurements to check all the connections, the stack was enclosed in its aluminium case and held in place by PVC bags filled with epoxy under pressure. Only one bag has ever leaked. Thankfully, this was discovered during the filling process. The case was quickly stripped down and the offending bag removed before the wire planes were flooded with epoxy!

Meeting the deadline for the first completed module to be tested in the West Hall beam at CERN in 1985 was extremely challenging. At the last moment, the 'O' ring which was meant to seal the top plate to the sides of the petal box refused to co-operate. No grease was permitted to alleviate the situation as this was thought to cause long-term depositions on the chamber wires. In desperation, we resorted to epoxy-much to Jack Steinberger's disgust! It is still there and the module remains sealed. After the completion of the tests, the module was sealed off for the winter months. The following spring, we discovered that the internal pressure had fallen to 680 mbars-clear proof that it was leak tight and that the graphited layers were excellent absorbers of the xenon/CO2 mixture!

The multiple flat cable 'feed-throughs' bringing the cathode pad signals out to the front-end electronics boxes were sealed with epoxy which was more problematic than the PVC bags. Despite testing before and after installation, several of them leaked after the long journey of the modules to CERN. Patching them up externally was not satisfactory. Instead, in each case, a carefully adjusted volume of epoxy had to be introduced to flow under pressure through a drilled hole in between the cables to seal the leak. Twelve years later the modules remained gas tight—a tribute to their sound design and meticulous testing.

MODULE ELECTRONICS

At RAL, we embarked on two different solutions for the front-end multiplexers knowing that in the end only one would be adopted. In the Electronics Group, monolithic chips in CMOS technology were favoured being 'high' technology as opposed to the more conventional hybrids. The former had a clear advantage in power dissipation and the pedestals were smaller and more uniform. However, the pedestals of the first version were as sensitive to the power supply voltage as the hybrids much to the consternation of our colleagues at Orsay who had designed the basic circuit. After a redesign of the input amplifier and several frustrating delays at the manufacturers, wafers were obtained with excellent yields. A full box of circuits was made and tested on a module in the cosmic rig in December 1988 over the Christmas-New Year period! The monolithics performed as well as the hybrids but not better from the point of view of stability and noise. Meanwhile, the hybrid solution was in full production as our French colleagues had already decided to put their trust in them! Since time was running out and experience with them had been limited, we also decided to abandon the monolithics in favour of the hybrids. This was a great disappointment after so much effort and expense. Nevertheless, the experience was salutary and may have helped our French colleagues to devise an improved version which ultimately replaced all the hybrids three years later.

TESTS WITH COSMIC MUONS

It was clear that it was not feasible to test every module in beams at CERN. However, we wanted to measure the uniformity of response over the surface of each petal module to a precision of $\approx 1\%$, as achieved in the tests at CERN. The solution was to build a cosmic-ray station at RAL capable of reconstructing externally each incident muon track passing through the module and reading out the responses from the pad towers around it. This was an enterprise almost equivalent to a 1960's HEP experiment. Large-area scintillation counters and MWPCs, taken from the old NA4 muon scattering experiment at CERN, were installed in horizontal planes above and below the petal. At first they worked well even though they had been designed to operate in a vertical orientation! However, towards the end only a few survived the course and we had to use the petal data alone to confirm where the muons were!

It took three weeks per module to collect the statistics required beginning with the first in May 1987 and finishing with the last in late November 1988. Although the measurements achieved the precision required and matched those made in the test beam, we were unable to verify them by HV-pulsing the individual wire planes. The barrel pulsing studies had shown that their pad responses were related to variations in the vertical positions of the HV wires in their extrusion cells as expected. However, it appeared impossible for us to disentangle the effects of our non-rectangular geometry. The moral is to beware of trapezoidal detectors and to offer to build the rectangular ones instead!



ECAL 'PETAL' plane assembly at RAL.

END-CAP MODULES (GLASGOW) *Jim Lynch/Ian Hughes*

In 1983 the Technical Report was produced and the distribution of most of the responsibilities for construction and funding of the component parts of Aleph was agreed. The Glasgow group undertook two tasks: studies of laser ionization for the TPC calibration system (Editor's note-JL: See 'Aleph TPC Gas' (To seed or not to seed?) article by Ken Ledingham) and a major involvement in the construction of the end-cap 'petals' of the electromagnetic calorimeter, responsibility for which had been undertaken on behalf of UK groups by the Rutherford Laboratory. The collaboration with the Rutherford Laboratory to build the petals was very successful. At the outset Glasgow undertook to build 8 of the 25 petals $(2 \times 12 \text{ plus})$ 1 spare). In the end (although Jack Steinberger was initially uneasy about our capacity to do the job) 10 of the petals were built in Glasgow.

The late Colin Raine was primarily responsible for the successful construction of the Aleph ECAL end-cap 'petals' in Glasgow.



Bob O'Neil with his weaving machine.

A new clean area was built for assembly, and the whole complex for construction of the elements, assembly and testing was brought into action in 1986. Construction was completed, on schedule, early in 1988.

PETAL CONSTRUCTION

Wire Plane Assembly (R. O'Neil)

Bob O'Neil*, a technologist with the Glasgow group, designed and built a 'weaving machine' for stringing the wires in the assembled aluminium extrusions of the wedge-shaped plane.

(This machine was built from spare parts left over from the drive mechanisms of a set of radioactive source calibration systems, which Bob had built for the WA70, liquid scintillator electromagnetic calorimeter at CERN.) Bob was an expert craftsman and this weaving machine was a key element in the Glasgow part of the project. The machine automatically strung and soldered the gold plated tungsten wires in all the extrusion channels (which varied in length) of a petal plane under the correct tension with the minimum of wire wastage and considerable savings in cost (as would be expected for a true Scotsman).

*Bob will be remembered for having such a broad Scottish accent that even fellow Scots had difficulty understanding him. When Lynn Silverman came to Glasgow to record the petal construction work for the Aleph film, it was suggested that she use subtitles in the section of film where she interviewed Bob about his wonderful machine!

Wire Plane Testing (C. Raine, K. Smith, J. Lynch and I. Hughes)

Complete wire planes were then 'flare' tested. The plane was placed in a transparent gas-tight box, filled with inert gas and HV applied. A time exposure Polaroid photograph was then taken. Sparking and corona discharges seen on these photographs showed locations of problems in the wiring of the planes, which were corrected, and the test repeated.**

**This is equivalent to the 'flushing test' mentioned in the RAL article and confirms that Glasgow did perform the test despite the doubts of our RAL colleagues.



Colin Raine.

Stacking

(C. Raine, R. O'Neil and Glasgow technicians)

The tested wire planes were transported to 'Crane Hall' at the other end of the department for stacking with cathode planes, lead sheets, etc. This crane hall was situated above the site of the 300 MeV Glasgow Electron Synchrotron, which operated in the department in the 1950s and 1960s.

Side Wiring

(C. Raine, Glasgow technicians, etc.)

The side wiring of the stacked planes was performed by a team of technicians and temporary staff including Colin Raine's wife, Jane, and family members of other support staff were recruited for the duration!

Encapsulation

(C. Raine, R. O'Neil and technicians)

The completed stack was finally placed in an aluminium case, plastic bags filled with resin placed round the sides, and the case sealed with a top plate.

After vacuum testing the finished petal was ready to be transported to RAL for further testing (including tests in a cosmic-ray test rig) before transportation to CERN.

Testing, Commissioning and Installation at CERN

(J. Lynch, K. Smith and C. Raine)

During the period August 1988–March 1989, Jim Lynch was mainly responsible for the initial vacuum and electronic testing of petals in the East Hall at CERN where they were delivered from RAL.

I (JL) well remember the weekends spent in the East Hall checking the petals as they arrived from RAL (often accompanied by my 13-year-old son, Edward, who was looking for help with his school homework!)

- One Sunday afternoon a beautiful new red Ferrari arrived outside the East Hall. My son's eyes were like saucers! This was Pio Picchi arriving to work on the HCAL construction which was taking place in the East Hall adjacent to the ECAL petal test area. When Pio offered to take Edward for a spin in the car it really made his day!
- I also remember the difficulty in convincing Mike Edwards at RAL that the petals he was shipping out, all checked and tested, were leaking like sieves! It turned out that the multiway electrical feed-throughs were the source of the problem. These feed-throughs had been produced at RAL and had been vacuum tested in the petals before shipment to CERN. Mike eventually came to CERN himself and confirmed the problem. He also ingeniously cured the leaks by drilling through the feed-through, inserting a hypodermic needle and injecting just enough epoxy resin to cover the inside of the feed-through and seal the leaks.

In spring 1989 Ken Smith and Colin Raine joined Jim Lynch at CERN to help with the installation and commissioning of the petals on the Aleph detector in the cavern at Echenevex. Colin Raine was principally responsible for the commissioning of the readout electronics of the ECAL end-caps.

My (JL) memories of working in the cavern in early 1989 are many:

- One Friday evening, when I was working in the ECAL barrack, B1, in the Aleph cavern, the phone rang. 'Hello. This is Jack' said a voice. Thinking that this was some of my Glasgow colleagues pulling my leg, I replied 'Oh yeah!'... but it was Jack Steinberger! He was calling to inquire if there would be people working in the cavern the next morning as he was bringing Andrei Sakharov to visit Aleph. I had the honour of meeting Sakharov, the famous Russian physicist, that Saturday morning in the Aleph cavern.
- I remember the safety instructions—always wear a hard hat and an emergency breathing pack round the waist. In case of fire, activate the breathing pack and walk slowly to the emergency exit so that the pack would not overheat due to heavy breathing! Later on these breathing packs were housed in cupboards next to the exit in the cavern. In an emergency would anyone stop to collect a breathing pack when they were so close to the exit?

- I recall the Apple Macintosh PC I was using to check the gas system for the petals (on loan from the Marseille group) being stolen, only to be subsequently found stashed away on the stairs from the cavern awaiting later collection! After that, the PC was secured with a large chain and padlock and hidden behind an electronic rack. However, these precautions did not prevent it being stolen again and, despite climbing down both sets of stairs from the surface in search of it, it was never seen again!
- Working with Jeff Bizzell, RAL electronic technician, assembling and testing the frontend electronic cards for the ECAL petals and marvelling at how expertly Jeff could install the multi-pin hybrids into their sockets without bending or breaking the pins.
- Watching, day by day and week by week, the Aleph detector slowly take shape and, finally, the excitement when the Aleph end-caps were first closed successfully without any damage to the detectors.



Andrei Sahkarov (to Jack Steinberger's left) in the Aleph cavern.

THE SOLENOID

Marcel Jacquemet

Memories abound concerning the Aleph superconducting magnet project. The oldest of these memories, more than fifteen years later, resurface and with them come the faces, the comments, the 'well-tuned' phrases or the jokes, the arbitrary judgments, or the hesitations of those who participated in this project.

The day my boss, Mr Prugne, entrusted me with the responsibility for the solenoid project, a sunny morning in September 1982, I couldn't imagine at that moment the adventure I, or rather we, would experience, trying to act on the events. Acting, clearly, but mainly trying not to be driven by them!

A TEAM ADVENTURE

Like every team project, this one was scattered with challenges agreed in common, choices, and more or less bitter discussions. And 'more' was frequent. This adventure was conducted with enthusiasm, everyone realizing as the months passed, that he was participating in the conception and then the construction of one of the biggest superconducting magnets ever built, at that time obviously.

This adventure was marked with successes, but also with temporary difficulties to overcome, and with disappointments, hopefully always temporary, to be dealt with. During all that period I had the full support of my supervisors, the Director, J. Horowitz, the head of department, R. Turlay and the head of the service, P. Prugne.

THE TIMES OF CONCEPTION AND OF TECHNICAL CHOICES

My first memory is linked to discussion about the 'thermosiphon' which had never been used before to cool a superconducting magnet. However, this concept was attractive for its simplicity: The liquid, cooling the magnet, becomes hot, and vaporizes. Lighter, it rises and is replaced by heavier, therefore cooler, liquid, which in turn cools the magnet. The cycle is started! No need for a mechanical pump, consuming power and subject to failures. As you all know, the thermosiphon was chosen, opening the way for its usage on other big solenoids.

The second memory is linked to the choice of the winding process, inside a coil ('mandrin'), forming a kind of collar, the purpose of which is to contain the magnetic forces. After several months of discussions, and when we had to take the decision, we selected this method, common nowadays, but seldom used in the mid 1980s. This was not without difficulties: The quick discovery of local defects by the practised eyes of H. Desportes, J.C. Lottin and J. Lebars avoided many days of 'unwinding'.

The third memory I want to describe is the choice of conductor. Here also an innovative solution, suggested mainly by H. Desportes, was selected. H. Desportes was, with J.C. Lottin and a few others, the man behind the initial design of this solenoid. The superconducting cable is inserted in a sheath of very pure aluminium, obtained by co-extrusion. I remember questions which were impossible to answer, like '*Will enough aluminium fit among the superconducting wires*?' First, what was the meaning of 'enough'? Samples gave encouraging results, but longer samples would have been needed, and we were already late on the planning. Test samples were produced to verify our choice, which met the specifications, J. Lebars, high priestess of ultrasonic measurement, was then able to verify very accurately the good adherence of the aluminium to the superconductor.

Having evoked these memories linked to the period of technological choices, which gave rise to heated (this is a euphemism) discussions in which everyone felt he knew **The Truth**, I shall quickly pass over the winding and setting up phases.

THE CONSTRUCTION— A MARATHON WHICH BECAME A TEAM SPRINT

At that time, our chief planner, J.M. Garin, kept reminding us of the critical paths during our weekly meetings. He did so with such strength and care that the majority tried to make up for lost time or, in the worst cases, not to lose any more time. During these two years, three fundamental ideas really dominated our thoughts:

- Respect the planning, at all costs.
- Keep to the technical specifications, established at the beginning of the project. A question of future recognition, and of reputation.
- Do not go over the budget, another challenge.

As usual, this period was a period of haste. All margins in the schedule had been eaten away beforehand in order to select the best technologies. For a project leader, this period is only bearable with the help and support of 'trouble-shooters', prepared for a lot of efforts to get all things done on schedule. I was lucky enough to work on the Aleph project with two men and their teams: J. Heitzmann and J.C. Languillat. Two people, apparently so different, yet surprisingly so close in their approach to the task, based on 'serve at best', and who knew how to get the same spirit from their teams. Impossible to not mention the consistent Among the numerous tales of that period, I have selected two in particular. This is a difficult exercise, sorry for those who are not mentioned.

I remember a night in April, around one o'clock, the phone rang at home. Michel A. informed me that the glue used for blocking the conductors wasn't polymerizing. When driving to Saclay, I understood that the problem was not a fault anywhere, just the temperature was too cold and, as many Do-It-Yourself fans know, Araldite-type glue won't harden at all in this case. That's why afterwards the interior of the machine was heated, which led some people to wear 'extremely light' clothes. It must be said that the temperature inside the machine was almost tropical!

I can't forget the suspicions expressed by those who, seeing yellow deposits in the centre of Saclay, suspected cyanide or its derivative, which were being used to prepare the junction between two lengths of conductors. Fortunately, this yellow layer was only due to spring flowers being blown down by the gusty March showers.

THE TESTS IN SACLAY— A POSITIVE ANSWER

The next step, testing the magnet in Saclay, has also its share of memories. For example, the control racks started to move due to the magnetic field they were not sufficiently secured: A small 'detail' was forgotten, the absence of a return yoke!

The day the thermosiphon started was unforgettable. The same for the first current ramp to half-field, and the fast discharge performed in the middle of the night, so as to minimize the number of attendants. From that night, I still remember the noise at the opening of the fast valves, and the vision of a cold helium jet in the pale lights of the hall's neon. I still hear the sort of whispering noise, similar to the one produced by chips just poured into a deep fat fryer. But during the first seconds, I can assure you that one thinks more of a fault or failure, it's only afterwards that one is fully reassured! That very same night, champagne celebrated this first ramp, and the perfect behaviour of all the safety systems. A warm and sympathetic ceremony took place at Saclay, with the presentation of several plaques, one congratulating the whole team:

Our most sincere congratulations to the technical team of STIPE at the occasion of the completion of the building, and the success of the tests, of the superconducting coil of Aleph. With our admiration, the Aleph collaboration, April 30, 1987

THE TRANSPORTATION— A THREE-WEEK TRIP

It is now time for the finished and ready-to-work object to leave the place it was built. This journey from Saclay to Geneva had been prepared for a long time. The first question was the mode of transport, by road or by air? The big air carrier solution was not pursued, as the transportation date had to be fixed with high accuracy a year in advance. Also, a special road transport would have been anyway needed between Saclay and the airport, as (as you all know) there is no runway inside Saclay! The road option was studied from 1984, and a 5 cm thick report described the complete path, with picture and details, sometimes metre by metre.

Starting from Saclay, we tried to attract journalists to show our technological success: Building one of the biggest superconducting magnets in the world. No interest, apparently superconducting magnets don't sell newspapers. Many of us were upset that this technological marvel was not better recognized. However, a 53-metre-long truck, with a lot of wheels, crossing the neighbourhood; this is a nice subject for a local newspaper. In this way, we managed to attract the media, and then exhibit our technological successes.

The departure from Saclay in early May 1987 is still alive in our memory. First a family picture, where all those who worked, a week or a year, on the project were assembled. Not easy to gather everyone for such a picture. (See the 'Family picture' below.)



The Saclay team—the 'family' picture.

The peculiarities of such an operation are numerous; I haven't counted how many traffic lights, how many road signs, how many street lights were temporarily removed for the occasion. I don't know how many gendarmes contributed to the escort, nor the number of short or long stops. Just to give an idea, the average speed between Saclay and Geneva was around 5 km/h. I just want to cite a few specific problems, mainly concentrated in the last part of the journey.

First, I should mention the preparation and use of a special slip road of the A-40 motorway, to allow the crossing of a bridge near Nantua. In fact, the road haulier showed with pictures that the bridge over the N-84 near Saint-Martin-du-Fresne wasn't high enough. Instead of going under it, one had to go over the bridge, which means using the A-40 motorway for 300 metres. As this implied stopping the traffic on the motorway, this was impossible before, during or just after a weekend.

Crossing Frangy, a true bottleneck, seemed to be a miracle. Part of the roof of a house was extending over the road, 1 metre had to be removed along the whole length of the house, and put back afterwards.

Apparently it was not the first time, and the owner was used to negotiating compensation.

Then came the crossing of the railway, by night of course to avoid the train traffic. As a bridge was not high enough, the solenoid had to go over the bridge, after dismantling of the track, signals and catenaries, which all had to be put back afterwards. Crossing the Rhône was foreseen on the Carnot bridge. What wasn't foreseen is that the bridge had just been remade, and one had to install iron plates to distribute the load, the concrete not being cured enough to allow the heavy load. Or we would have had to wait a few weeks!

After crossing Farges and the roundabout in Saint-Genis, CERN is reached. It was on 2 June 1987 late afternoon, after crossing the border customs that the heavy convoy stopped behind the 'Main Building', attracting the eyes of those going to the canteen. This was an opportunity to inspect this 180-ton convoy.

The last step before the final installation was the test with full current. The heavy magnet yoke, foliated to receive detectors, had been prepared



The road to success.

by our CERN colleagues in the large BEBC hall. Sliding the solenoid inside the yoke, before suspending it with holding bars of impressive size, was a relatively easy operation. Cabling the instrumentation, connecting electric and cryogenic lines, and installing control racks took until the middle of August 1987. It was now time to start cooling, while our colleagues from MPI-Munich and from CERN worked together to prepare the magnetic field measuring device, which would later confirm that the field was homogeneous enough. One must specify that this very sophisticated device was impressive by its size and accuracy: 8 metres in length, 170 Hall and 7 NMR probes, the measuring arm was moving over a radius of 2.3 metres! (See 'B-field Blues' story by RS.)

After this cooling phase, with its usual quantity of problems, the first full ramp was, as usual, a very tense moment. When approaching the nominal current, a few hundred amps below the nominal 5000 A, the step size decreased... as one wanted to reach an asymptote, the nominal value. Once reached, everyone relaxed. The computer screens, affected by the magnetic field, were showing distorted tables.

Before keeping this current for days, to measure the field map, one had to try the fast discharge, which lasts a few minutes. This mode is mandatory and had to be tested for safety reasons: In case of problems with the magnet, the stored energy has to be extracted as quickly as possible. This fast discharge triggers a quench, a transition from superconductive to resistive states, and thus allows the correct behaviour of all the protective systems to be fully checked. Designing and building such systems is a real profession, requiring knowledge in many domains.

THE LAST STEPS IN THE ADVENTURE

After this successful test, the mapping of the field at various currents started. From that time on, the magnet's ownership could change: The real magnet was in conformity with the design specification, and CERN (more precisely the Aleph collaboration) could gain ownership.

So on 14 October 1987, R. Turlay and I gave, ceremonially and symbolically, the key of the solenoid to J. Steinberger. Our best wishes of success and big discoveries, thanks to this detector, accompanied the key. I remember with great emotion this day, as strongly as the day of the departure from Saclay. They both mark, in their own way, the end of an adventure.

On 9 June 1988, after a perfectly controlled descent underground, the magnet was again inserted into the yoke in the heart of the Aleph cavern. We could not resist, before the end of the work, to write a mark, for those who would have, one day, to dismantle this magnet. Proud of the successful job, we stood for the official picture. All technical problems, all troubles were forgotten, only the success and the good times will stay imprinted in our memory.

(Translated by Olivier Callot)

THE MAGNET LEAK (Open heart surgery)

Pierre Lazeyras

APRIL 1993

On 4 April 1993, after the annual shutdown, the cooling-down of the magnet began. The following day it was becoming evident that the screen circuit was leaking. The vacuum was poor but was improved when isolating the screen circuit from the refrigerator and evacuating it.

It was decided to try to cool down the magnet as much as possible without helium circulating in the screen circuit, and then try to find some solution to cool the screen, perhaps with nitrogen produced by evaporating liquid nitrogen from a dewar. After 5 days of cooling down, the coil temperature was about 100 K and the screen was at about 210 K, but the temperature was not decreasing any more.

At that moment, a pressure test was made, with the idea of measuring the rate of the leak, but to our surprise, no leak was detected so we reconnected the screen to the refrigerator and finished the cooling-down, hoping for the best.

The magnet was run with no more problems than usual during all of the 1993 running period of the LEP machine.

JANUARY 1994

At the end of the running period, the magnet was first equipped with more sophisticated items, namely a mass spectrometer online on the vacuum vessel and an adapted data acquisition system for all parameters during the warm up. Then the magnet was warmed up. Nothing particular was observed until the temperature of the coil reached about 175 K, then a helium signal became visible on the spectrometer; a pressure test was made on the screen circuit, which indicated a leak, strongly pressure dependent, as if the leak was opening up with the pressure.

At the beginning of January 1994, the coil was at room temperature and a last pressure test showed a very large leak, too large to be measured with the mass spectrometer. Some tests were made, by injecting short bursts of helium on the inlet and outlet sides of the screen circuit at the refrigerator level and measuring the transit time to the mass spectrometer. These tests indicated that the leak was close to the inlet, 10 m or so from it. This gave us the suspicion, or rather the hope, that the leak was in the cold box or in the transition tube between the cold box and magnet vacuum vessel, both parts having the advantage of being relatively easily accessible. We thus first opened the valve box, to find that two of the three supports of the 500 l helium vessel were broken; fortunately one was still intact and the piping was keeping the vessel in place quite well, but unfortunately, no leak: everything was perfectly vacuum tight.

The cryogenics chimney connecting the valve box to the vacuum vessel was open and found to be perfectly tight, but at this moment it was possible to hear the leak when pressurizing the screen pipe.

Then, God bless medical technology—and we have good mechanics. With the aid of an endoscope and a mirror it was possible to see the leak just at a piece called the bibraze. This bibraze makes the junction between the aluminium pipe on the screen and the stainless steel pipe coming from the manifold from which helium is distributed to the various circuits in parallel on the screens. The leak was close to the highest point of the screen, about 30 cm deep inside the vacuum vessel.

The leak being localized, access was needed to it. Thus it was decided to cut an aperture as large as possible through the flange, ≈ 90 mm thick, closing the vacuum vessel. We could make an aperture of about 330×700 mm. Calculations were made in order to make sure that the mechanical stability of the flange would not be affected and the stress level would remain acceptable. For the drilling a framework was built, able to support a milling machine with enough stability against the vibrations. This object was fixed on the magnet, on

the barrel on one side and on the end-cap on the other side. The milling machine was installed on this framework.

The drilling started on 15 February, went smoothly, with some precautions at the end to avoid chips falling into the vessel and was finished on 17 February. The super insulation was then cut and we could see exactly what had been happening. For some unknown reason, at the assembly, the stainless pipe had been blocked between two pipes; thus the flexible part supposed to take care of the contraction of the screen due to temperature could not play its role, and the result was an enormous tensile stress on the pipe, until it broke.

During this time, at Saclay, a piece was prepared and tested to replace the broken part. The idea was to have again a bibraze for the stainless-steelaluminium junction, terminated on the aluminium side by a conical thread to be screwed onto the screen pipe itself and at the other end a 700 mm long flexible pipe. It was made completely pressure tight with some glue. This was carefully and thoroughly tested at Saclay, including temperature cycling between 300 K and 77 K and pressure tests up to 10 bars. At this moment, having access, the damaged tube was removed, the new piece was again tested under pressure at room temperature and at 77 K and found to be tight. Finally on 8 March the new junction was put in place, glued on the screen pipe and then welded at the other end on the manifold. At 16.45 the repair work was over!



Access aperture cut in magnet vacuum vessel.

Twenty-four hours later, vacuum and pressure tests on the piping took place: the circuit was tight. A few hours later, the super insulation was again in place and the aperture was closed by a new flange. We were able to start pumping down the vacuum vessel on 21 March. Again pressure tests were made, without detecting any helium in the vacuum vessel.

Finally we started the cooling down of the coil, beginning 31 March and finished without incident 10 days later.

CONCLUSION

First of all we have been very lucky, because the leak was reachable without doing really major work on Aleph. The repair was made possible by the dedication and the skill of all persons, at CERN and at Saclay, mechanics, specialists in cryogenics, in vacuum techniques, etc. The direct reason for the leak is very obvious as we have seen. It is less obvious to understand how, at the assembly, when the piping was welded, it was possible to block the pipe as it was blocked; here probably the lack of space played a role.

What will never be understood is by what mystery the leak closed by itself during the 1993 coolingdown!

INCEPTION OF HCAL

Lorenzo Foà

During the preliminary discussions on the structure of the Aleph detector it quickly became clear that the hadron calorimeter should fulfil the following conditions:

- 1. To have a high granularity
- 2. To guarantee the best hermeticity compatible with the general structure of the magnet return yoke in which it had to be housed
- 3. To be as cheap as possible.

The request for the best energy resolution was not in the list since all charged particles had to be measured with high precision in the central tracker, while HCAL had to deal only with the few neutral hadrons produced in each event and to give a first and fast measurement of the global event energy.

The first attempt to design such a hadron calorimeter was based on iron-scintillator sandwiches detecting the light through wave-length shifters, following the design of the hadron calorimeter of CDF at that time under construction in Italy. However, it very soon became clear that such a technique was hardly compatible with the above conditions and such a design was quickly abandoned.

An alternative technology was offered by a gas detector based on plastic tubes as long as eight metres, working in streamer mode. This was a principle under development in those years by Iarocci and co-workers at Frascati to build a proton decay experiment to be installed in the Mont Blanc tunnel. The most important limitation of this detector, its limited rate capability, which was prohibitive at a hadron collider, was irrelevant in a low-rate, very clean machine such as LEP.

The signal of a streamer tube is independent of the energy released in its gas by the crossing particle. Therefore such a device did not seem, at first sight, the most adequate instrument to measure the energy released by a hadronic shower. But if a resolution of the order of $100\%/\sqrt{E}$ (GeV) is acceptable, a measurement of E based on counting the number of crossings of the secondary particles in the shower through the many layers of the detector is perfectly sufficient. Indeed the analogue sum of the signals induced on external electrodes arranged in projective towers satisfied very nicely the conditions described above, providing a resolution of 80%/ \sqrt{E} (GeV).

In addition, the really attractive and unique asset of this detector lay in its tracking capability, based on the digital readout of strips running parallel to the tubes, allowing a clear view of the path of the muons (and the hadrons) in the transverse plane, on their way towards the muon chambers. The price to pay for this precious performance was an enormous amount of rather sophisticated electronics for handling hundreds of strips, a job that required several years of work by the Bari group. Once this design of the hadron calorimeter was accepted we were not surprised when Jack Steinberger asked us also to build the two layers of muon chambers with the same technology, with the difference that the readout was done by means of two planes of strips in orthogonal directions and each plane of tubes was double in order to improve the efficiency from 90% to 100%. We agreed that this was a very reasonable way to proceed, but our resources were not sufficient to do all of the work.

Fortunately, we found that our Chinese colleagues of IHEP were interested in building the second layer of muon chambers using the same tools. We then began one of the first operations of 'technology transfer', in the sense that our Beijing friends, following the detailed drawings brought from Italy, built all the complicated pneumatic machines needed to reproduce in their laboratory, the factory for streamer tubes which we had developed in Frascati, Bari and Pisa. This operation was a success and the second muon layer was installed only a few months later than the rest of the detector.

One of the most attractive properties of the hadron calorimeter built with streamer tubes was its easy calibration, since the only quantities that had to be monitored continuously as a function of time were the gas mixture and the charge of the streamers, a procedure which had been done regularly for more than ten years, by means of a small telescope of external tubes with a radioactive source and by using the signals of the cosmic muons in the towers.

I still remember a very tense general meeting of Aleph in which I tried, with some success, to convince a very sceptical collaboration that we knew what we were doing, that the calorimeter was really self calibrating and that we did not need to expose every single tower to a test beam. Thus we began the construction of the tens of thousands of eightfold tubes, then to be tested and installed in the yoke of the magnet at CERN.

Even if the mechanics of the calorimeter was rather low tech, the overall construction required a lot of organization, a characteristic that was still largely lacking in the Italian teams. While our groups were full of brilliant young physicists and of talented and devoted technicians, we had no support of expert technical managers and engineers to follow the various steps of the construction. At the end, with a lot of help from Pierre Lazeyras and stretching his patience to the limit, the calorimeter and the muon chambers were successfully completed, but we had accumulated a few months of delay, which had a large impact on the enormous cabling work, particularly for the digital readout of the strips. (See HCAL (as seen by an outsider) by Pierre Lazeyras.)

As a result of all this I spent most of the year 1988 in the pit correcting connections and labels, missing a lot of the nice events that were happening on the surface and in the counting room. But finally the readout was correct and became an essential ingredient of the muon identification, which was one of the strong points of Aleph.

HCAL (Down to earth) *Giampaolo Mannocchi*

A hadron calorimeter is usually a simple object, the simplest detector that one could conceive. In the specific case of Aleph, HCAL 'simple' means 'very simple'. The same is true, of course, for the muon chambers. There are then no excuses in case of failures, being practically obvious that such devices must work. The choice to build the calorimeter set us in the same position as the young student who, upon announcing to his parents 'I passed my examination', gets the answer 'It was your duty'.

Once the choice was made, people from the INFN laboratories in Bari, Frascati and Pisa and from IHEP of Beijing were ready to stretch 200 000 wires, to graphite 25 000 PVC frames to assemble and test 25 000 tubes, and finally to select 20 000 of them.

We had the knowledge to do this, in particular those who participated in the construction of NUSEX, the most quoted among the first generation proton decay experiments. (Indeed we can say NUSEX was also the luckiest one, at least for those who, between 1979 and 1987, spent some months per year in garage 17 inside the Mont Blanc tunnel.)

All the fundamental choices had already been made during the construction of the 3.5 m cube of Iarocci tubes: for example the current INFN president made an accurate selection among all possible methods to spread the graphite on the PVC to obtain the required resistivity, and the result was dish cloths, but only the ones produced by a particular Spanish branch of a given factory XXX passed the selection.

Following the NUSEX experience we then started the work: the shifts to paint, stretch, assemble, and test were democratically shared among directors, senior physicists, technicians and students and everybody, independent of position, participated in all kinds of work.

The final mounting was then made at CERN where we made the selection for the final choice of the single elements, struggling with any tube subject to strange (almost supernatural) phenomena such as the 'poubelle' effect discovered by P. Picchi, more or less understood but never fully determined.

In Building 158, at BEBC and at Echenevex we mounted, dismounted, mounted again, re-dismounted and finally mounted the full calorimeter. Everything in-phase/out-ofphase with the other subdetectors, under the impeccable timing directions of the steadfast and intelligent guidance of Pierre Lazeyras, conductor of the orchestra; a 'maestro', fighting, fortunately successfully, against the second principal, Alberto Bechini, as well as Pio Picchi and me. In the meantime, the institute IHEP in Beijing was getting ready to start the construction of a part of the apparatus: in practice on our side we had to transfer to China our know-how. I had the luck, together with Carlo Bradaschia, of accomplishing this pleasant task. We went to China in November 1985, bringing with us, besides our knowledge, a few circuits and streamer tubes, slides and art books about Venice, Rome and Florence.

We were warmly welcomed by our Chinese friends: J. Lin, W. Wu, Y. Xie and W. Zhao did their utmost both at work in order to make our job easier, and out of work to show us their wonderful country. After the reception by the Academia Sinica President, held in the IHEP Institute with the participation of all the academic authorities, we were invited to write a sentence on the special guest logbook in which Carlo and I discovered that the only two previous occidental signatures belonged to W.K.H. Panofsky and J. Steinberger. We asked if we might write it after the visit to the laboratory so that we might have time to think of a sentence which could survive comparison with the previous ones! I do not remember what I saw during the visit, but I remember very well what we finally wrote on the logbook: 'As physicists we are proud to join, together with the Chinese friends, Aleph: a beautiful challenge of the mind. As Italians we are proud to refresh a collaboration between two civilizations that already met many centuries ago." I still think that it was a nice sentence.

I already mentioned what we brought to China. From China I brought back the shame of having caused an old man to fall down on the zebra crossing whilst bringing on his bicycle eight boxes in a pyramid structure, full of hens and chicks, sugar canes, cabbages and much other stuff. This happened because I was so presumptuous as to try to teach one particle of the extensive and continuous cyclist shower how to use the zebra crossing! Furthermore I made the mistake of ignoring the law of inertia. In partial extenuation, I can say that I helped the poor gentleman to collect all his stuff, which was spread around a large area (indeed not everything since a cock had run away...).

Furthermore, I brought back from China 1200 slides, a Red Army hat, a Red Army coat, a red booklet (it was already at that time an antique) and a strong wish to return.

THE MUON SYSTEM

Weimin Wu

THE 'MIDDLE-ANGLE' MUON CHAMBERS

The first stage was to assemble the muon chambers for the middle angle region of the inner layer, using materials provided from Italy. This was to be followed by construction of the whole second layer of muon chambers, with a full series of machines, plastic extrusion, coating with graphite, wiring and testing etc., locally in China.

For this goal, costs were estimated at half a million Chinese 'Yuan', which definitely was not easy (to give a scale: the average salary for a physicist was about 100 Yuan per month at that time). After many talks, meetings and reports (especially helped by the visit of Sau Lan Wu and Hans Taureg, to China)

the deputy president of the Chinese Academy, Qian, signed the agreement for IHEP participation in Aleph and the funding of 50 000 Yuan per year for 5 years. The Beijing/IHEP/Aleph group became responsible for 'the middle angle muon chamber and possibly the outer muon chamber' as it was written in the Aleph Technical Design Report of April 1983; a group of seven physicists formed the Aleph/ Beijing/IHEP contingent. Later on, additional funding, from the National Science Foundation of the People's Republic of China, was contributed to this task for the Aleph/ IHEP/Beijing group, after its vice president visited Aleph at CERN. A total of 4500, 8-fold tubes, ranging from 4 m to 7 m in length, were constructed and shipped to CERN, and from August 1987 to August 1991, these tubes were installed into a total of 66 large surface chambers. The work was completed on time, and the installation went smoothly. Lorenzo Foà wrote a letter to the Chairman of the Bureau of International Affairs of the Chinese Academy, in 1991. He wrote:

'When tested in the pit with real events, the chambers built in Beijing performed as well as those built in Italy'.



Aleph muon chamber production in Beijing.

This collaboration was included in the agreement between CERN and the Chinese Academy in 1984, and added into the existing agreement between INFN and the Chinese Academy.

Our success owed much to many Italian friends— P. Picchi, C. Bradaschia, G. Mannocchi, G. Maggi, P. Campana, P. Iaselli, M. De Palma, A. Bechini... who worked in our laboratory during days and nights and to the support of J. Steinberger, P. Lazeyras, D. Schlatter, S.L. Wu, H. Taureg and others. We would like to record our appreciation to these and all our friends in Aleph.

In May 1985, Jack Steinberger and Lorenzo Foà visited Beijing, on the occasion of Jack's 65th birthday, to celebrate the collaboration of Beijing/ Aleph. The vice-president of the Chinese 'congress' and the president of Academia Sinica met the delegation. From some points of view, the example of the Beijing/Aleph collaboration became a sort of 'model' for other collaborations later on with China. The new concept of 'in-kind contribution' to describe this kind of participation from developing countries, due to their cheaper labour and material, has been used again and again. As a result the value of these muon chambers in the Aleph detector was much more than the half million 'Yuan' which was totally funded for Aleph/Beijing/IHEP by the Academia Sinica and the Chinese National Science Foundation.

Lumi Detectors, Triggers, DAQ, Falcon, LEP



Cartoon by John Rander.

SATR/SAMBA Claus Grupen

The experience from PETRA at DESY in Hamburg had shown that accurate cross-sections can only be obtained if a precise luminosity measurement is available. With scintillation counters and calorimeters the PETRA experiments achieved accuracies of a few per cent. This was, of course, not good enough for Aleph. Even though the Copenhagen luminosity calorimeter was segmented, the feeling was that the impact point of the scattered electrons on the calorimeter face must be determined with a precision that only a tracking device could provide. Since the Bhabha crosssection has a very steep polar-angle dependence this argument was accepted, and consequently a Small Angle Tracking (SATR) system was designed and built by the Siegen group. Siegen, as the historical centre of iron-ore mining and steel processingthe famous character 'Wieland the Smith' had his sword made in Siegen-decided to build a robust and solid tracking device, because this instrument had to survive in the harsh background of LEP. For obvious reasons iron was not a good candidate for an instrument which had to be operated in a strong magnetic field, and Jürgen May carefully checked that not a single screw of iron was used for the installation. The SATR was eventually made of nine layers of brass tubes of quadratic crosssection, which were operated in the drift mode and provided the necessary spatial resolution.

The SATR/LCAL tandem improved the luminosity measurement well below the per cent level. Owing to the fine granularity of the electromagnetic calorimeter, and taking advantage of the nonuniformity of response over individual pads, the Copenhagen luminosity experts managed to reach a level of several per mille even without using the spatial information from the SATR. The general feeling was that one might be able to do even better than that by increasing the Bhabha rate by having an instrument which would measure at smaller angles thus benefiting from the increased count rate.

The collaboration decided in 1992 to replace the SATR by a high-precision silicon calorimeter which extended the polar acceptance to smaller angles. Eventually, a per mille accuracy for the luminosity was obtained by using LCAL together with the new SICAL.

Since the SATR had also provided some interesting data on the machine background, the idea came up that Siegen could build a simple instrument to measure the LEP background by some thin device sitting in front of SICAL. This triggered the birth of a dedicated instrument for which Alain Blondel coined the name SAMBA (Small Angle Monitor for Background). SAMBA was a small single-layer multiwire drift chamber which did its job in the coming years. It was able to tell off-momentum background from synchrotron radiation background by using time and amplitude information, thereby helping the shift crew to find out about the background conditions in LEP. SAMBA could even be turned on before 'physics' was declared by LEP, and the Figure Of Merit (FOM) from SAMBA was used to decide on whether the high voltage of such delicate instruments like the ITC or TPC could be ramped.

This was all very fine for LEP1 running. For LEP2 larger backgrounds were expected because of the increased synchrotron energy loss of the circulating beams. It was decided to add tungsten masks and tungsten shielding to the beam pipe for highenergy running. Unfortunately, SAMBA was in the way for the tungsten shield. There was no way to modify the existing SAMBA modules to make them fit in the new environment: a new system had to be built. This was the time (1995/96) when SAMBA II was born: a double-layer multiwire drift chamber with excellent timing and an appropriate spatial resolution. It was built on short notice, installed, and worked until the end of data taking in the year 2000. SATR, SAMBA I and SAMBA II provided valuable information, not only for Aleph, but also for the LEP machine, because—among others—these simple robust detectors could locate the origin of beam-related background thereby helping to improve the running conditions for Aleph. It also provided many Siegen diploma students with hardware experience, diploma and Ph.D. degrees.



Shining SAMBA I mounted around the beam pipe.



A fresh and proud ALEPH-Siegen Ph.D. student (Johannes Hess) with his examiner (CG).



The SAMBA subdetector co-ordinator enjoys a samba!





(Editor's note-CG: see also Peter Hansen's story on 'Luminosity'. A silicon-tungsten electromagnetic calorimeter (SICAL) was built by the Saclay group with the aim of achieving an experimental luminosity measurement with a precision at a level of better than 0.1%. The detector uses homogeneous construction to give full azimuthal acceptance for Bhabha scattering over polar angles from 24 to 58 mrad. Detailed information concerning shower development is obtained from zero-suppressed readout of the 12 288 pads of the detector. Trigger decisions are generated from a rapid flash-ADC system using programmable gate arrays. The anticipated precision was verified in the actual data taking at LEP1 thus significantly improving the luminosity measurement of LCAL and extending the acceptance to smaller polar angles thereby taking advantage of the increased Bhabha rate.)

LCAL Peter Hansen

The Copenhagen group had joined Aleph in 1984 and undertook to build the luminosity calorimeter, LCAL, intended to measure the energy of forwardscattered beam-particles ('Bhabhas') which were used to determine the luminosity of the colliding beams.

To keep things simple, it was decided to use the same internal design as for ECAL, although the shapes of the LCAL modules had to be quite different, fitting tightly around the beam-pipe, just down-stream of the focusing quadrupoles.

By the time of the Copenhagen Aleph Week, very little progress in the construction of these modules had been achieved and this was beginning to worry the collaboration. It helped a little that there was a first wire-plane on display in the workshop, and the Danes proudly announced that it had been sitting there on high voltage the whole night without a single spark. Some days later, a natural explanation was found by examining the fuses. They were all blown!

However, just before deadline the year after, all the pieces were miraculously assembled (with good help from CERN in making the tools for the assembly) and the four LCAL modules turned out to work very well. In all of its thirteen years of operation there was never a 'fusiblage', a shorted plane or a serious gas leak in LCAL. There were a few problems with the first-generation electronics, and a few operational blunders occurred, but in general this subdetector was just sitting there for a decade munching on its abundant signals and delivering valid results from its other end without the need for any interventions. Why was it then such a cliff-hanger in the construction phase? The main reason was the airtight aluminium containers, shaped as half-cylinders, enclosing the detector and the beam-pipe. It was afterwards suggested that it would have been cheaper and faster to buy a solid block of aluminium and carve out the interior, than to shape and weld the pieces together from



Half of one side of LCAL installed.
aluminium sheet. The lesson learned was that the welding of aluminium is not very compatible with a high target on tolerances—at least not when using present-day welding techniques.

Another lesson was learned immediately after taking the first data. LCAL was at first only supposed to measure energy. To measure particle positions, a system of brass drift-tubes (SATR) was placed in front of the calorimeter. However, the amount of material in this region, not least in the brass tubes themselves, excluded precise measurements of highenergy electrons because of early showering. In the calorimeter, however, particles could be located, due to the fine grained segmentation of the ECAL design, arranged so that the cathode towers pointed to the interaction vertex, and to the 'zero tolerance policy' implemented in the construction. Equally important, each particle location is automatically weighted in the calorimeter by the particle energy. It turned out that the energy imbalance across segmentation boundaries in the LCAL cathode towers provided a very precise definition of the polar angle acceptance for Bhabha scattering. This acceptance was one of the most important sources of error in the determination of the Z peak crosssection.

To minimize effects of placement precision, beam parameters and theoretical approximations, the acceptance was defined with asymmetric sizes on the two sides, the small and large acceptance changing sides at each event. As a result, Aleph could in 1990 present the world's most precise determination of the Z peak cross-sections and thus of the number of light neutrino species.

In the following years long discussions ensued (among people with sufficient stamina) about exactly how precise this determination was—with the Copenhageners (most of them called Hansen) taking a quite aggressive stance on the size of the systematic error. In the end, the aggressive error was more or less vindicated (it could, of course, be by accident) by the more precise determination of the Z peak cross-section using the SICAL luminosity which emerged in the second half of the 1992 run. This part of the story is beautifully illustrated by John Rander's drawing, which was presented at an 'end-of-LCAL' party at Echenevex. *(See cartoon on page 89.)*

During the LEP2 era, LCAL was revived again as the main luminosity provider, but the pressure for high precision was now considerably lighter. Because of the small rate of interesting processes at LEP2, all cross-sections were now dominated by statistical errors.

BCAL/BCAL++ Enrique Fernández

BARCELONA'S INVOLVEMENT IN BUILDING ALEPH

At the time we started with Aleph in 1985 the detector was already designed. Furthermore we were really a small group, consisting of Jose M. Crespo, Pere Mato, Salvador Orteu and myself. Ramon Miquel, Andreu Pacheco and Josep A. Perlas finished their undergraduate studies in 1985 and they also joined the group at the end of the academic year. Lluis Garrido and Manel Martínez came to Barcelona in October of 1986 and presented their theses on Mark J shortly afterwards. In April of 1987 Manuel Delfino came from SLAC, where he was a post-doc of the University of Wisconsin working in MAC (and where I had met him 5 years earlier). The same month Pere Mato became a CERN Fellow. That year we did not get any new students. In February of 1988 Lluis Garrido left for CERN as a Fellow and at the end of the summer Lluisa Mir and Eduard Tubau joined the group as doctoral students. At the end of the academic year 1988/1989, Ricard Alemany, Vicent Gaitan and Francisco Ariztizabal also finished their undergraduate studies and joined the group. We were not very small any more, although we had a very large ratio of students to doctors. We got a new doctor in September of 1989 (the first on Aleph), Ramon Miquel, but his advisor, Manel Martínez, also became a CERN Fellow a month later, and left for CERN.

Our decision on what to do in Aleph took place during the winter of 1985–1986, and ended up in what later were called BCAL and FALCON, the latter being described elsewhere in this book.

The idea of BCAL was not ours. The first person that I heard talking about a forward luminosity monitor for Aleph was Phil March, during one of the Aleph meetings in 1985. It interested me from the start, since I thought it was something that we could contribute to, not only on the construction but also on the design. Phil had many ideas about the monitor, and he and Hans Taureg had already thought of many of the practical problems of installing it in the crowded regions near the beam pipe, 7.8 metres from the interaction point. We had many discussions about it during that year and we eventually became responsible for its design and construction. My first presentation to Aleph on BCAL (which we called originally SALM, for Small Angle Luminosity Monitor) was in May of 1986. What we proposed was a sampling calorimeter made of layers of tungsten with scintillator and silicon as sampling media.

The name BCAL came almost by mandate, as there was the rule that all the Aleph subdetectors had to start with different letters. BCAL was also good as both Bhabha Calorimeter and Barcelona Calorimeter, whichever choice one would like to make. Although this detector is quite small by any Aleph standard, it was a big project for our group. We were alone in designing and building every aspect of it, from the mechanics to its inclusion in the trigger. It also used two technologies, phototubes (the only detector in Aleph to do so at the time) and silicon. For us it was a big effort to build this detector, which, I have the impression, was not very visible in Aleph. We in fact kept a low profile on this work, perhaps because we were conscious of being quite bold in taking this project entirely by ourselves, with the resources we had at hand. But the fact is that the detector was ready on 14 July 1989, and delivered from the start an online measurement of the relative luminosity. It was quite useful not only for Aleph, but also for the LEP machine people.

Most people in the Barcelona group worked at one time or another in BCAL. The main load of the work, up to 1989, was on Ll. Garrido, M. Martínez, Pere Mato, Lluisa Mir, Josep A. Perlas (who did his thesis on it) and myself. Shortly afterwards it was Ricard Alemany who took over the job of Perlas. For LEP2 a new luminosity monitor was built, BCAL++, and it was Mokhtar Chmeissani, who joined the group in December of 1992, who led the whole project from the beginning to the end.



BCAL.



BCAL++.

LEVEL1 TRIGGER Paul Hanke/Eike-Erik Kluge

The Heidelberg group was a member, from the very first hour, of what would become the Aleph Collaboration. The institute (then called 'Institut für Hochenergiephysik', later renamed 'Kirchhoff-Institut für Physik') had already been connected in earlier experiments with the founding collaborators.

After participation in the frequent 'brainstorming' sessions on nearly all principal aspects of the detector, Heidelberg chose to work on the Level1 Trigger and the 'Luminosity' detectors. For the latter, a lot of design and simulation studies were done before the task was taken over by Universität Siegen and NBI Copenhagen.

The work on the trigger appeared to be suitable for a university institute with a 'strong' electronics department and physicists who had gained experience in this field in earlier experiments on the proton-proton intersecting storage rings, on the proton-antiproton collider and on the DESY electron-positron collider PETRA. In fact, by studying data from PETRA, physicists at Heidelberg tried to estimate the background with which the trigger algorithm to be developed would have to cope at LEP. In retrospect suffice it to say that LEP turned out to be a fantastically background-free machine. On the other hand, based on PETRA experience, Aleph had a trigger system, which, even under the 'worst' LEP conditions, was never really fully challenged (i.e. no major problems were encountered over the many years of operation-neither hardware problems nor performance limitations).

Our institute was responsible for the design, construction, and operation of the Level1 trigger in the Aleph experiment at LEP. The final system consisted of approximately 130 Fastbus-size printed-circuit boards, about half of which followed the Fastbus form-factor for reasons of signal input/ output ('analog boards') while the other half were 'real' Fastbus boards with data-access.

Practically all detector elements in Alephcalorimeters as well as track detectors-delivered signals suitable for trigger purposes. Hence, the Level1 trigger was making use of them all (except the 'slower' TPC signals). A long study involved the formation of so-called 'super-segments' (i.e. overlapping segments in pseudo-rapidity and azimuth, to minimize trigger losses at segment boundaries). The conclusion in the end was that the complexity of the system and noise contributions outweighed the small gains on uniformity of trigger acceptance. Another subject of great fun in electronics are analog input signals without a 'zero' reference, especially when threshold setting is a mandatory task. This striking feature of the electromagnetic calorimeter led to the invention of the 'Rückstell-Motor', a sampling and feed-back circuit to keep the base-line of the signal constant. The principle can only work with very low 'signal occupation'—a requirement fortunately fulfilled at LEP.

For the Level1 logic scheme, a whole range of possibilities was studied like setting-up specific trigger conditions for specific physics channels, e.g. vetoing background tracks, establishing coincidences between spatial regions, or anticoincidences between others.

After all those studies, a remarkably simple approach was finally chosen: all electron–positron annihilation events should be accepted within the possibilities of data-acquisition even at highest luminosities and cross-sections. Hence, the presence of at least one single particle was required within the solid angle of nearly 4π , be it a tracksegment of predefined quality or an energy-cluster with a signal above a certain threshold or more restrictive combinations thereof. Background rejection was basically achieved with the help of a fully 'projective' geometry to the vertex-point at the centre of Aleph.

All signals used for triggering represented this 'directionality'—track segments as well as projective 'towers' in the calorimetry (72 adjacent segments in η - ϕ space). This feature, implemented in all subdetector systems, enabled Aleph to adopt the 'fully inclusive' approach for triggering with all its benefits: least bias with highest background rejectivity. With the addition of a few 'special' triggers, e.g. for Bhabha events and, later on, for two-photon exchange physics, this scheme persisted for the full duration of the experiment's data-taking (more than 10 years). Even though the trigger system was designed to use 'standardized' modules as far as possible, a variety of many different modules had to be developed and built (e.g. 'ECAL Tower Mixer' (see figure below), 'ECAL Wire Mixer', 'HCAL Tower Mixer', 'HCAL Wire Mixer', 'LCAL Tower Mixer', 'LCAL Wire Mixer', 'Standard Discriminator Bank', 'Trigger Segment Register', 'Trigger Segment Scaler', 'Trigger Segment Display', 'ECL Fanout' and the important 'Trigger Pattern Register'). Very few modules could be obtained commercially. The largest purchase comprised the 'ECLine Logic' to form correlations between trigger elements (e.g. calorimeter towers, track segments etc.). Of course, items like crates, Fastbus segment interconnects, and last but not least, the Aleph Event Builder for readout were contributions that did not have to originate in our electronics department.



The 'ECAL Tower Mixer' crate.

1984-2000

LEVEL2 TRIGGER John Strong

The original estimates for trigger rates in Aleph gave a first-level rate of up to 500 Hz mainly due to beam gas interactions. The TPC (or secondlevel) trigger was required to reduce this rate by about two orders of magnitude by looking at tracks in the TPC and selecting events for readout only where the tracks came from the interaction region. The system was built with two stages of processing to achieve the required reduction. The splendid performance of LEP resulted in a very low beamgas rate and the first-level trigger rarely exceeded 10 Hz. With the emphasis on very high efficiency rather than high rejection, the second stage of the TPC trigger processing was turned off after a couple of months and only the first stage used for the remaining twelve years of Aleph data taking.

The system ran with almost monotonous reliability, normally requiring only minor intervention after power trips. One year, however, following a successful checkout of the system prior to running, a loss of efficiency was reported for one of the processors. Tests on all modules failed to indicate any operating faults and only a detailed analysis of some event dumps showed the cause-two cables had been interchanged on the TPC during the recabling following the winter shutdown. Because these cables were connected to sectors with different pad configurations all the hits were being seen but were not being processed into tracks. Fortunately the solution did not involve opening Aleph but simply making the corresponding cable interchange in the barrack. Because of redundancy in the system, the effects on efficiency were marginal and the problem was sorted out in a reasonable time.

LEVEL3 TRIGGER

Günther Lütjens/Beat Jost

(Editor's note-RS: I'll mention what I know about this device, since it was designed by MPI-Munich, i.e. Günther. More details were added after discussing with Günther and Beat, so any errors in the following story are due to me...)

Although software triggers are standard in today's detectors, at the time Aleph was in planning, it was a rather novel thing. The concept had been used in a couple of fixed-target experiments-e.g. NA11-but the technique was still in its infancy. The idea was to pass the raw data through a software program after the readout which would throw out the junk so that it is not written onto tape (it was decided not to try a more aggressive version which could even stop the readout). To this end, Günther, with the help of Mauro Comin, developed an analysis framework of algorithms for examining the subdetectors' raw data to find objects from real particles. This framework was subsequently filled with contributions from the specialists in the subdetectors which performed the analysis under Günther's orchestration software. Conservatism was the name of the game at the beginning, since experience had to be gained with this sort of gadget and we didn't want to lose any good events: 'when in doubt, keep the data for that trigger'. The data-transfer software was developed by Beat and implemented initially on a system

based on MicroVax co-processor boards plugged into the main DAQ computer; this was done in the framework of a joint project with DEC (RIP!). So the programs were written, Wolfgang von Rüden found the money for the computer, and the Level3 subsystem was plugged into the DAQ system. After everything got working, the Level3 was switched from a passive debugging mode to an active 'rejection' mode and discarded about 30% of the triggers. As experience was gathered we were in a position to be more selective. We were ready in case the backgrounds became really bad as the luminosity of LEP increased, and actually it evolved into a rather powerful option as its sophistication improved. It was used for timing during multi-bunch operation, and for event-tagging, for example. One of its main jobs at LEP2 was to reduce the background from 2photon events while not cutting into the 2-photon physics being pushed by the Lancaster and Siegen groups (this led to some interesting discussions, as you can imagine!). However, in the end as time went on, the backgrounds got better and better as the understanding of the machine improved, and it turned out to be never necessary to resort to pushing the Level3 selection beyond the 30% level.

THE ALEPH ONLINE PROJECT (Personal recollections) John Harvey

My involvement in the online project started in September 1984 when my then-employer, RAL, transferred me to CERN from DESY, where I had been working on the TASSO experiment. At that time Wolfgang von Rüden was project leader and there were two other project members-André Lacourt was working on software and Sandro Marchioro on hardware. Some important decisions had already been taken before I arrived. The online computer configuration would be based on the VAX architecture and the operating system would be VMS, both provided by DEC. We were always very happy with this decision, but in retrospect it was perhaps rather a risk to have chosen a proprietary system. A new network technology, called Ethernet, appeared just about then and we decided to go with that. Fastbus was chosen as the instrumentation bus and Sandro had already started his design of the Aleph Event Builder.

I got involved in the software project and was soon joined by new colleagues, Jean Bourotte from Ecole Polytechnique, Christian Arnault from Orsay and David Botterill from RAL. We organized a VMS course that took place at DEC headquarters in Geneva and this was attended by several colleagues from the subdetector groups as well. In the years that followed we managed to assemble an impressive team relying very heavily on some excellent new recruits via the CERN Fellowship programme—some I remember include Richard McClatchey, Tim Charity, Martin Saich, Sarah Wheeler, Beat Jost, Pere Mato, and Alessandro Miotto. Fortunately many of these people made such an impact that they stayed on providing the continuity that we so badly needed. Several are still at CERN today and hold high-level responsibilities in the Organization.

Being confronted with a new project was particularly exciting for me. It was important to establish the architecture of the software and to build a culture for our software production activity. At CERN a DAQ system had been developed that ran on VMS for the UA2 experiment. I had used this on TASSO and had become convinced that a complete re-write would be necessary to cope with LEP requirements. My idea was therefore to develop something new from scratch. I toured the States to see what other experiments were doing and came back with some good ideas. I proposed to use the CDF Buffer Manager to organize the event store and started to talk in terms of writing 'producers' and 'consumers' of data. We followed closely the work of the offline team as well and participated in the SASD training programme set-up by Gottfried Kellner. Very soon we were all talking the same language.

Friedrich Dydak was carefully observing the way things were going and must have been aware that not everyone was in agreement with the line we were taking. He invited me to give a presentation in an Aleph Plenary meeting (a set-up). I remember to this day the intense debate that took place in the PS Auditorium in Building 6. It seemed that the whole audience were sitting on the edge of their seats, had a point of view and spoke up. Several Aleph collaborators made convincing arguments that Aleph should assemble a strong online team and take responsibility for developing and operating its online system (thank you Ioana). The end result was that the programme presented at the meeting was endorsed and from that point onward we never looked back.

The design of the software evolved very quickly. The buffer manager was soon complemented by a user interface package (UPI)-started by André and continued by Christian-that relied heavily on the SMG package of VMS. Several people were busy data modelling to provide configuration databases so that all components of the DAQ and Control systems were described. I started thinking in terms of Finite State Machines. Each process running in the DAQ was modelled around its own FSM that defined the states it could be in, the commands that caused it to move to new states, and the actions the process would perform on making these transitions. This turned out to provide a very convenient tool for modelling the behaviour of the system. I wrote the Run Controller which was the 'chef d'orchestre' issuing the commands and waiting to see that all its slaves had obeyed them before issuing the next set of commands. Starting a run seemed to take a long time in those very early days as everything had to do its job in the correct sequence. However, as all the problems were ironed out the speed of the run cycle improved. I made large posters of the state machines and stuck them on the walls in front of the shift crews. Status displays would record the state in which the tasks were sitting and would flash in inverse video if a transition failed. This way we could identify easily the offending component.

As soon as the Event Builders were delivered we realized that we would need to write some software to run on them. Of course everyone wanted to write their software in FORTRAN and Hans von der Schmidt, who was working on UA1 and then Opal, had developed a FORTRAN compiler. However, Wolfgang was very keen to use a commercial solution and chose the OS9 operating system from Microware as the software platform. Microware also provided a C compiler and after lots of discussion (big understatement) this was the direction we took. A team was hastily assembled comprising Tim, Pere, Alessandro and others to start developing the libraries so that we could extend our software architecture to run on the embedded processors. Some of the basic packages, such as the buffer manager and FSM were re-written in C and everything was brought together in a very short time (fortunately).

Meanwhile the Slow Control system was being developed by Martin Saich, Wolfgang Tejessy, Jenny Thomas and Sarah Wheeler. They developed a single task that dispatched slow control commands to the control devices and fielded error messages. This task was given the name BRIAN, the name of the snail in the Magic Roundabout, a childrens' TV programme in the UK (get it— SLOW control). It was probably inevitable that when the application to turn on/off the high voltage on the detectors was written it would get the name Zebedee, another character from the same programme. Oh yes and we also had a dedicated PC that counted the bubbles emanating from the gas detectors 'the Bubble Counter'!

The commissioning of the system with all subdetectors started at the beginning of 1989, rather later than we had hoped. We had introduced the concept of partitioning, so that detectors could be read out either together (e.g. during normal running) or independently (provided that the partitioning rules were obeyed). Features of the trigger distribution system (the FIO units) made this possible. We started the commissioning by including one detector at a time and then got more ambitious reading out two at once etc. Finally we managed to read out the whole detector-and it worked! Maybe it was 'mit Ach und Krach', but it worked! Unfortunately, my father died one week before the pilot run started so I wasn't present in the final days before first collisions. It was the first week of August 1989. I was at home in Cardiff listening carefully to the BBC for news and heard the announcement live one lunchtime.

The online team worked very closely with our subdetector contacts throughout this period and even though there were many problems to overcome I remember that we had a terrific 'esprit de corps' and also a lot of fun. Long days and nights were spent at the pit. We got to know and love a lot of restaurants in the area—in particular Enzo's Pizzeria. When Enzo moved his restaurant we followed him around the Pays de Gex. The following may bring back memories and raise a smile for some people:

- Beat to John: 'Why don't you use the spelling checker'. The result was that I lost the entire paper I was working on!
- 'The trigger has stopped and we are all suffering from it'—with time this actually appeared as a message on the shift leader's control panel.
- Overheard in a telephone conversation 'How exactly do you spell OS9?'
- Does anyone remember the 'Cactus' chip?
- Beat in a loud voice if something failed: 'Oh my god! 'Cancel!', 'Retry!', Reset!, Reset'!
- A note on the men's toilet door: 'If you find a problem with the toilets, take HCAL out of the partition'

Wolfgang had always insisted on building redundancy into the system and one day this was tested when we suddenly lost all power on the VAX8700, the main online computer. A new power supply had to be obtained from Zurich and would take 24 hours to replace. In the end we ran using the TPC VAX and very little data were lost. I don't think anyone thought it would work, but it did.

As each milestone was passed (e.g. the integration of a new subdetector in the overall system) we would celebrate with champagne. Token bottles were kept and labels with the names of their sponsors were stuck on. Soon whenever someone had anything to celebrate-new software release, a tricky bug found and removed-they would sponsor a bottle which was put up on the shelves. Soon it became a competition, everyone wanted to sponsor a bottle and have their name visible. Even the Run Co-ordinators got in on the act! With time we assembled a large collection of bottles and this soon became a talking point when showing visitors around the control room. (Editor's note-IL: See photograph at the end of 'Echenevex Group' article!)

Of course software is never finished and many upgrades were made right up until the experiment was finally stopped. The UPI package was rewritten by Christian (more than once?), the Aleph Message System was written by Beat etc. One addition proved to be particularly useful, DEXPERT-the online expert system. This was Pere's baby and he wrote it using 'new technology'. The 'tentacles' were implemented in C++, but the 'brain' was written in a language called OPS5. DEXPERT was connected to the error logger and after each error was reported it 'thought' for a bit exercising the rules in its knowledge-base and then started to issue the standard commands via the Run Controller. By the time the shift crew had realized there was a problem DEXPERT had fixed it and we were off running again. It became a real challenge to minimize the down-time as well as the dead-time. Olivier Callot was king of the control room and he watched these numbers like a hawk. Ramping up the high voltage as soon as 'stable beams' was declared was a particular challenge for the shift crew.

During running the online team took it in turns to be on call. In the beginning we tended to camp out in the control room, but soon things started to settle down. We got used to receiving telephone calls, often in the middle of the night. Some of these tested our patience, some were hilarious. We got into the habit of scrutinizing the shift list trying to anticipate whether we were likely to have a quiet night or not. I particularly remember a story told by Ioana who one evening received a call from Rick St. Denis-he was laughing his head off: 'Ioana, you'll never guess what I've just done-I've deleted the Aleph partition! Ha! Ha!' I'm sure Ioana would have reached down the telephone line and strangled him if she could have! And who was it that used to call us in the middle of the night just to say that 'everything is running smoothly, but what should I do if xxx happens?'. I forget. I also remember getting called by someone, who shall remain nameless, at a similarly unearthly hour, who informed me that an error message had come up to say that 'The Run Quality Program is not running'. This would have been fine, except that it was a shutdown and we weren't even taking data!

Perhaps the biggest upgrade we made to the system was replacing Aleph Event Builders (Fastbus modules) with commercial VME boards from CES. This happened during Ioana's reign as Online Co-ordinator and was managed by Beat. We worked closely with the people from CES, François Worm et al. Dave Casper was already an Aleph member and he decided to work with us on the project, writing a software package which would allow multi-user access to Fastbus from VME. Dave was a lot of fun—who will ever forget his rendition of the 'Blues Brothers' at an Aleph party in La Chenaille. By the time we approached the end of Aleph the system was running remarkably well:

We were able to report very high data taking efficiencies, just the odd fan-failure kept us from reaching perfection. By the late 1990s we were already thinking of the next project and most of the CERN group joined the LHCb experiment. But that is another story!



'It worked like a dream!' (and after the VME upgrade even better...!)



FALCON (Reconstruction farm) Dieter Schlatter

One of the early decisions for Aleph computing was to perform the reconstruction of the raw data and the production of the DST (later called POT for Production Output Tape) in quasi-realtime at the pit. With the expected data rate and reconstruction time, we estimated in 1985 that we would need 6 CERN units* of computing power for this task. At that time, commercial systems of that size would cost in the order of at least a million Swiss Francs. We considered for some time to use homebrewn 'emulators' which would have imposed constraints on the software and would have required in-house maintenance. Fortunately, the advent of powerful, relatively cheap workstations came just in time to provide an elegant solution. The first implementation of the reconstruction farm FALCON developed by the Barcelona team consisted of 12 VAX 3100 workstations each providing 0.5 CERN units of computing power.

The rapid availability of reconstructed data was not only a big advantage for the physics analysis but provided also a vital tool to monitor the proper performance of the Aleph subdetectors. A further element in the Aleph computing strategy was a powerful network between the pit and the CERN computing centre extending also to the analysis farm ALWS where the most popular data could be kept on disks. Another technology breakthrough that came handily at the time of Aleph commissioning was the availability of Gigabyte-Disks at 'affordable' prices.

After their heated meetings, the FALCON planning group and friends gathered in the Aleph Betting Club to predict expected events and to bet on them. The loser of past bets provided bottles of wine—normally from his country of origin. In the case of members from the United Kingdom, we also tasted English wine but gracefully accepted additional bottles from the 'colonies'.

The data production system for Aleph in 1989: The raw data are written by the online system onto shared disks with FALCON. After parallel reconstruction on the FALCON nodes, the POT is transferred to the CERN computer centre where data are written to cartridges in the robot and copies are produced for use in the homelabs; in addition a copy of the POTs was transferred to the disks of the offline farm ALWS. For reprocessing, raw data were served from the central Cray back to FALCON.

^{*} One CERN unit of computing power was defined to be equivalent to the power of one processor of an IBM 370/168 mainframe. At the time of this writing (1999) a PC costs about 2000 Swiss Francs and provides 100 CERN units of computing power.



BET between Peter Doman & Jean François forez 1 bottle of the wine (choice of the loser) By the Aleph meeting in September 1988 a multipuppose detector at the SLC will have observed and made public at least one maltihadromic of event from the decay of the Z? TRUE: Pote Duna. FALSE: JE Sing WITNESSES: M_IPH-S. Lewind Somand la Welter Blum. Senera 27.11.1987.

BEGINNINGS OF FALCON

Enrique Fernández

The other major contribution of Barcelona to the Aleph detector was FALCON (see also BCAL). We became involved in the offline computing from the start, during the summer of 1985. It was clear in Aleph that we were going to need considerable computing resources for the reconstruction of the data and that we had to take care of them by ourselves, since the CERN computing centre was not going to be able to cope with the LEP experiments.

But what was the foreseen computing capacity and data storage that we would need? What kind of computer facilities should we buy and when? Where should they be located? Which steps should we have from the data taking to the 'DST' etc? We created an Aleph working group on these issues and of course had many discussions on them (these topics are always controversial). I think it took quite a while to mature the ideas, which started to take shape at the time of the Aleph meeting in Munich, in September of 1986. I made a presentation on the work of the working group and proposed the name FALCON (Facility for Aleph Computing and Networking), which was adopted. (The name occurred to Salvador Orteu and me. We had converged on FALCO, which means 'falcon' in Catalan, but I was convinced that networking was an important part of the facility, and thus the N was added.) This was September of 1986, and what we had there was only a sketch, which had nevertheless some features of how FALCON ended up. At that time we were still thinking of using emulators, and in fact, we sent Pere Mato to CERN to learn and work with them early in 1986.

It was during the spring of 1987 that Wolfgang von Rüden and I visited the Digital Headquarters in Marlborough, Massachusetts. It was clear that they were going to put a lot of emphasis on developing very powerful workstations (and we saw that many people there had already 'personal workstations' on their desks, quite a luxury at that time!). But to come up with the needed computing power for the Aleph event reconstruction we would need something like 28 workstations, of a new soon-tobe-announced future powerful model. That seemed too many and too expensive at the time. But perhaps the idea of using workstations should be taken seriously. And in fact, Wolfgang and Manuel Delfino, who had just arrived in Barcelona, took it seriously. Their point was that perhaps by 1989 there would be workstations twice as powerful as those that were going to be announced and much less expensive. During the summer of 1987 Manuel Delfino and Andreu Pacheco did some measurements of the performance, in computing power and in data flow, of a cluster of workstations, which clearly showed that they were competitive in every respect with emulators (in addition to being commercially produced and maintained). So by the end of the year the decision was taken to go in that direction.

Most of the work on the actual implementation of FALCON was then done by Manuel and Andreu. Salvador Orteu also contributed substantially, as he had the experience of running the Aleph reconstruction software. They not only had many ideas on the overall architecture of the system itself and on the flow of data, but wrote a simple and powerful code to distribute the events to the different workstations and collect the outputs together, which worked beautifully and reliably from day one.



Manuel Delfino, Andreu Pacheco and Enrique Fernández in front of the boxes containing the FALCON workstations, as they arrived at CERN circa May 1989.



THE ALEPH-LEP CONNECTION Maria Girone/Bolek Pietrzyk

During the last years of Aleph data taking we acted as 'LEP contacts' i.e. we were a sort of interface between the Aleph experiment and the LEP machine. This entailed one of us visiting the LEP control room at Prévessin, first thing every morning in order to find out what had happened with the accelerator during the previous 24 hours. This information was then reported to the nine o'clock Aleph meeting at Echenevex.

The LEP machine co-ordinators were rotated weekly and we soon realized that different tactics were required when reporting Aleph problems, depending on the LEP co-ordinator and which of us was LEP contact. (Maria's reporting of such problems proved much more acceptable than Bolek's-the attached photo may provide an explanation!) A LEP scheduling meeting was held at Prévessin every Monday and Friday at 14.00 at which we reported on the Aleph status and our performance over the last week. These meetings were very rich experiences for us and we had great times there, feeling that we were playing an important part in the operation team-especially in the champagne celebrations! (In fact these occurred quite regularly as LEP was so successful.)

It was a fantastic time for us. We could watch, day by day, the excellent work of the Aleph experimental team and also that of the LEP operations team. In Aleph, continuous effort was made to improve the efficiency of data taking and also to improve the quality of the data. We, simultaneously, watched the LEP team continue to strive to increase the luminosity, the total centre-of-mass energy and the LEP efficiency. However, we were not very popular at the LEP scheduling meetings, since Aleph had (nearly always) the highest operational efficiency. On the other hand, we did occasionally bring joy to the LEP contacts from the other experiments, when we reported a problem in Aleph and a corresponding drop in our operating efficiency. *'What has happened to your experiment?'*, the LEP co-ordinator would ask, usually with a smile!

LEP start-ups usually went very smoothly every year but in the spring of 1996 the LEP operations team could not succeed in getting circulating beams in the machine. The part of the ring that was causing the problem was located and a decision was taken to open up the beam vacuum pipe in that region. To everyone's surprise, two beer bottles were found inside the beampipe. A very explicit account of these investigations was presented at the next LEP scheduling meeting. After detailed questioning, the most important question was asked: 'What was the name of the beer?'... It was Heineken!

(Editor's note-JL: This was most fortuitous, as in their recent advertising, Heineken had used the statement: '... the beer that reaches parts that other beers do not!' I am sure CERN could have negotiated a lucrative advertising contract with the brewery!)

(Editor's note-RS: See the story below entitled 'The Running of the Lep Machine (recollections of friends)' for more about the beer-bottle incident from the machined physicists' point of view, and several other intriguing incidents.) The position of the moon in its orbit, the water level in Lac Léman, and even the TGV schedule all had an influence on LEP precision measurements but they had no impact on the LEP efficiency even an earthquake in Turkey (recorded by L3) did not affect LEP. On the other hand, power cuts, often triggered by thunderstorms, could cause very long (up to 24 hours) interruptions to data taking – affecting both the LEP machine and the experiments.

The collaboration between the accelerator teams and experiments was very good. Silicon detectors, close to the beam, in the LEP experiments could have been severely damaged by heavy beam loses. To avoid this, when the experiments detected the first signs of beam instabilities, they could enforce a beam dump in LEP. Such beam dumps, instigated by the experiments, were well accepted by the LEP team, even if they were accompanied by misleading statements like: *'The beam was lost in our experiment!'*—It is not possible to lose the beam at one place. Every one of us (Aleph physicists and LEP engineers) knew that we were participating in something exceptional. During the last year of operation, when we were in the LEP control room each morning, we were not the only ones to be asking the questions—the LEP operators were also asking us: '*Any new Higgs events*?'... When LEP was finally shut down, not only were we devastated but also the LEP operations team.



Maria and Bolek 'connecting' the Aleph Control Room at Echenevex with the LEP Control room at Prévessin.

Offline Stories



'True, I have put on a lot of weight! But I have a beauty tag!'

DALI (Display of ALeph Interactions) Hans Drevermann

The event display program, which was later called DALI, started with the idea of generalizing nonlinear 2D transformations to 3D, followed by 3D rotations. 3D was regarded as the optimum in graphics in the early 1980s. These 'non-linear 3D' transformations improved track recognition but turned out to be less intuitive! When these projections were presented to Aleph, there was no interest for the 3D projections, but only for the 2D projections.

Therefore, DALI became an event display program based on 2D projections. Many projections were investigated. The useful projections came out to be:

- the 'front' view x/y,
- the 'side' view,
- the rho/z and
- the angular views: phi/rho, phi/z and phi/ theta.

A modification of phi/theta led to the threedimensional 'V-plot', which was developed to associate TPC data to calorimeter data. It was appreciated by specialists—'*How can you work without it?*—But was not needed by many physicists—'*What use is that?*'

The command language is very simple, just a series of two-letter commands. This was very convenient for the author but not ideal for the user. Commands like *GT:EV:DN:GT:XY:DS:2* never became very popular! Therefore, a 'clickable' help file was added, so that one could use the program without needing the 'staccato' commands.

Before real data-taking many physicists made the comment, that *the simulated events did not reflect reality because they did not contain the expected high rate of noise*. As a way out, the V-plot was regarded as a reliable method to recognize tracks even in a very noisy environment. To the 'big' disappointment of the author of the V-plot, the real events turned out to be as clean as the simulated ones, so that the Vplot remained limited to use by experts.

The concept of 2D projections led to the use of several windows with different projections side by side on the screen. To aid human pattern recognition and to ease track association between different projections, tracks (and the hits belonging to them) were given a track-dependent colour. This, to the surprise of many physicists, did not depend on a physical quantity but was just chosen to be different for tracks close by in space. To ease work and understanding, new methods of 'zooming' with 'rubber bands' were introduced.

Energy deposits in the calorimeter were shown as spherical histograms which, if substantially magnified, showed the event structure clearly and gave the pictures a more appealing aspect.

With the arrival of colour printers, DALI entered the world of art. By avoiding the 'wire-frame' technique (giving many lines in the picture) but instead by just colouring the areas of the subdetectors, the pictures became the well-known view of Aleph events. This led to a much easier intuitive understanding of pictures, as expressed by many observers: '*It looks nice*'. However, due to the limited performance of colour printers, only clean colours such as red, green, blue... could be used. This was not liked much in the beginning but became so familiar that, after some time, no modification could be envisaged.

With the investigation of decays close to the interaction point, where only reconstructed tracks can be shown, 3D techniques were successfully applied... but again not widely used!

In a further step, momentum vectors were displayed instead of tracks. For the reconstruction of jets, the missing mass vector, etc., the ALPHA package was linked to DALI and could be used interactively. The DALI event displays became very popular in journals and as book covers. The largest picture, $2 \text{ m} \times 2 \text{ m}$ in glass, was shown at the world exhibition in Seville and was part of the CERN travelling exhibition. Visitors did not always know what these pictures meant—they just liked them! One science book showed a 2-jet event with the caption. 'Z⁵² event'! Aleph DALI events also appeared on CERN Swatches, T-shirts and on the front of Aleph Christmas cards.

The methods and ideas of DALI have now been transferred to a similar event display, 'ATLANTIS', for the LHC experiment, ATLAS, where further methods of interaction and operation are being developed, which, from time to time, are brought back into DALI.



Hans with his artwork.

JULIA Jürgen Knobloch

The design and implementation of the Aleph reconstruction program JULIA (the acronym stands for 'Job to Unveil LEP Interactions in Aleph'—as pure coincidence it is also the name of the Reconstruction Co-ordinator's daughter) started in 1985. The reconstruction and simulation programs were designed following the SASD methodology. After the first levels of the design,

the programs were decomposed into subtasks and implemented by teams in the various Aleph institutes spread over Europe. The discussion of the designs and the integration of the components was done during 'software weeks' held several times per year, mostly at CERN.



Planning of the JULIA project using MacProject (I got my first Macintosh in November 1984). At the time of the planning, the start of LEP was still planned for 1988. The foreseen dates were met with slight adjustments for the change in accelerator schedule.



Dataflow Diagram: schematic example of the high-level design of the reconstruction algorithms of JULIA. Each of the 'bubbles' is further decomposed into lower level diagrams. The bubbles can be identified as work packages developed independently by teams in the Aleph institutes.

ALEPH COMPUTING

Jürgen Knobloch

Timeline

November 1982

First Plenary ALEPH Software meeting

- GEANT for simulation
- ASSET
- Life-cycle

February 1983

Envisage to need 12 CERN units of computing power

July 1984 First SA-SD training

March 1985

First reconstruction workshop

October 1985

First version of reconstruction framework ready

First definition of the ALEPH DST

February 1987

Reconstruction at the detector level ready Simulation program GALEPH completed, generating simulated raw data for all detectors

February 1988

FALCON proposal:

- Farms of DEC workstations at the pit and for analysis
- Big IBM disk at the computing centre
- Powerful network

October 1988

Basic reconstruction program ready (**JULIA** version 2.19)

November 1988 first release of ALPHA

June 1989

First release of **SCANBOOK**

August 1989 Z⁰s from pilot run reconstructed October 1988, the Aleph event reconstruction program was ready. But before that...

The Aleph Computing Effort started to move in 1982 from individual scattered efforts to an organized system. It became clear that the people who could contribute to the software development were located in many different institutes. Fortunately we were helped by the emergence of key technologies that enabled us to work in this distributed way:

- Networks started to become usable for the exchange of files and electronic mail.
- Software development had turned into an engineering discipline with the advent of software engineering. We chose the SASD (Structured Analysis—Structured Design) methodology and, as a subset of the ADAMO system, the Entity-Relationship Model. This scheme allowed the decomposing of the software into small components with clearly defined interfaces.

The following choices have proven essential for the success of the software:

- Train all developers in the field of software engineering by a professional teacher.
- Use BOS as memory manager exploiting the 'event directory' feature allowing rapid access to individual events.

- Have all data structures defined in ADAMO-DDL (Data Description Language) for automatic generation of access software and documentation. The information about data types and precision from the DDL was also used for data compression and data storage.
- Have well-defined coding rules verified by an automatic tool.
- Production of the DSTs in quasi-real-time at the Aleph pit.

The Aleph computing system consists of many software and infrastructure components.

Software: GALEPH (Simulation), JULIA (Reconstruction), ALPHA (Analysis framework), SCANBOOK (bookkeeping of data and simulations), ALPROD (automatic POT, DST and MINI production and distribution system for simulated and real data), ALEPHLIB (general libraries), DALI (event display), DATABASE (detector description and run dependent constants), DDL (data description language for ALEPH data structures).

Infrastructure: FALCON (reconstruction farm at the pit), ALWS (VMS analysis cluster), ALOHA (Digital-Unix analysis cluster), FSU (Florida State University reprocessing system), HOMELABS (Aleph institute's computing system receiving DSTs for analysis and participating in Monte Carlo simulations).

SOME EARLY COMPUTING STATISTICS

Dieter Schlatter/Jürgen Knobloch

Program	Function	Lines of code
ALEPHLIB	General library	111 000
GALEPH	Simulation program	43 000
TPCSIM	TPC simulation	10 000
JULIA	Reconstruction program	113 000
BOS	Memory manager	14 000
DALI	Event display	140 000
LOOK	Data browser	3 000
ALPHA	Analysis program	42 000
SCANBOOK	Data bookkeeping	30 000
BANKDOC	Data description	22 000
ALPROD	Production control	8 000
FALCON	Online reconstruction control	16 000
Total		552 000

Processing time (CERN Units) per hadronic Z decay			
JULIA	35 s		
GALEPH	290 s		
ALPHA	1–10 s		

Recorded Data Volume (in Gbytes)



ALEPH Computing Equipment at CERN				
Year	Brand	Processors	CPU (CERN Units)	
1984–1990	ALWS VAX Stations	110	60 (1989) – 336 (1994)	
-1994	IBM+Siemens VM	2+2	12+13	
1988-1990	CRAY	4	32	
1994	ALOHA Digital Unix	15	324	
1989	FALCON DEC VMS	12	6 (1989) – 27 (1994)	
1994–1998	SHIFT 9 SGI	8	136	
1996	SHIFT50 DEC Alpha	4	320	

MORE ON ALEPH COMPUTING

Marco Cattaneo

The Aleph computing infrastructure, which is in place in 2001, bears little resemblance to what was available in 1989. In what follows we review the major changes that took place at CERN. Similar changes took place in the home laboratories, which contributed significant resources to Aleph computing, in particular for Monte Carlo production and real data reprocessing.

One of the rules for Aleph software was that only ANSI standard features of Fortran 77 should be used, with no machine-specific extensions. VAX/ VMS was the initial platform of choice throughout the collaboration (the ALWS cluster 'Falcon III' grew to more than 100 nodes in the early 1990s), but all software also had to function on IBM/ VM. This portability of the software served us well throughout the lifetime of Aleph, making it easy to migrate to new platforms and thus to take advantage of new capacity soon after it came online, often well ahead of other experiments. This was first seen with the Cray, which was heavily used by Aleph physicists as an alternative to CERNVM as soon as the first data arrived, and on which Aleph was by far the heaviest user until its retirement in 1993.

By then, the SHIFT architecture had taken over. In 1991 it was difficult to see how SHIFT could replace the Cray, because it relied on the latter for I/O, but in 1992 we decided to jump into the unknown world of Silicon Graphics with a model 3405 machine called shift3, replaced a year later by shift9 (4 CPUs), which was considered a supercomputer at the time. Shift9 was dedicated to batch analysis and disk storage. Monte Carlo production at CERN was run on a cluster of HP workstations called CSF.

Also in 1992, a young technical student, Joël Closier, installed a private cluster of DECStations, running the Ultrix operating system. This was the beginning of the Aloha cluster, which came into its own a couple of years later with the advent of AlphaStations running Digital Unix. By 1995, Aloha workstations had superseded ALWS as the desktop of choice of Aleph physicists; the cluster grew to a total of 63 workstations in 1997. In the CERN computer centre, the SAGA cluster of 8 AlphaStations was installed at the end of 1993, and was used to investigate how event level parallelism might improve the throughput of analysis jobs. This capacity was boosted in 1996 with the acquisition of an AlphaServer 8200. Experience with this machine was so positive (compared with the recurrent problems of shift9), that in 1998 it was decided to upgrade it to a 10 processor AlphaServer 8400, which replaced shift9 as both a CPU and disk server, and whose capacity was supposed to fulfil the needs of the experiment until the end of analysis in 2003.

During 1996, the Copenhagen group (in particular Anders Waananen and Bjorn Nilsson) ported the CERNLIB and the Aleph software to the Linux operating system, and began to push for

its adoption by Aleph. Shortly before his untimely death in September 1997, Ronald Hagelberg was encouraging the purchase of PCs as a desktop replacement for X-terminals. Thanks to the work of these people, and despite the fact that Linux was not officially supported in the collaboration until the Siena collaboration meeting in September 1999, Aleph was fully prepared to exploit immediately the new Linux batch capacity which first appeared in the CERN computer centre in November 1999 and which grew so rapidly that by 2001 it had made all other RISC Unix capacity insignificant. In particular, it became clear that it was no longer economical to pay for the maintenance of shift50, since one year of maintenance charges was sufficient to purchase PCs of equivalent capacity!

Of course, there have always been weak links in our systems. The early ALWS was plagued by network interruptions-the large number of nodes on the same shared Ethernet segment meant that problems in one office would cause the whole cluster to hang. (See 'Summer Student Sabotage' article by Giacomo Squazzoni.) The large multiprocessor machines (shift9 and shift50) would inexplicably hang-their crash dumps were often difficult to interpret by the vendors, who tended to blame the problems on the non-proprietary disks mounted on the machine, or on non-proprietary software-the most common solution offered was to apply the latest operating system patches. The distributed computing environment of the later years brought its own problems, in particular with the shared AFS file system.

Thanks to the adaptability of our software to this rapidly (and unpredictably) changing environment, we were always able to take full advantage of new processor technologies. The shift50 CPUs were more than 100 times faster than the original ALWS Vaxes, and 8 times faster than the AlphaStation replacements of these Vaxes. Each of the most recent Linux PCs has approximately the same processing power as a shift50 CPU, at one-tenth of the price. During the decade of Aleph data taking, the unit cost of processing power fell by a factor of approximately 300, allowing us to increase our capacity in step with the luminosity increase of the LEP machine and the increasing complexity of our software. It is amusing to note that, throughout the life of the experiment, a complete reprocessing of the latest year's data has always taken between two and four weeks of the available CPU power at the Florida supercomputer centre (for LEP1 data) and of the Falcon and Online clusters (LEP2 data)!

Disk capacity also had to keep pace with the increasing volume of Aleph data. In the beginning, it was felt important to keep all-important data permanently available on disk, and major efforts were made by the system managers to keep the data within the available disk space. The purchase of the 'Falcon 2' disk capacity for CERNVM (20 GB!) was discussed at length throughout 1989, at which time ALWS had 13 GB of disks distributed over 56 nodes. In early 2001, a 20 GB disk was considered rather small for a desktop PC; the cost of disk storage had dropped to 20 CHF/ GB, and the capacity available for staging data had grown to 4 TB. The volume of Aleph data has grown apace. The 1991 miniDST consisted of 4.5 GB of real data and 21 GB of simulated data. The 2000 miniDST is 55 GB of real data and 450 GB of simulated data. Fortunately, the capacity of tapes has also increased: the 2000 raw data would have required more than 3000 of the IBM 3480 (200 MB capacity) cartridges available in the early years.

The decade of Aleph data-taking also coincided with the invention and universal adoption of the World Wide Web. The Web was introduced to Aleph in October 1991 when it was explained at an offline meeting that 'World Wide Web is a help facility which can give help relating to many different environments: VMS HELP, IBM FIND and UNIX NEWS'. A WWW client was made available on ALWS a few months later. In November 1992, Jürgen Knobloch's Alephwww server and Olivier Callot's ALNEWS server were listed in the WWW project pages:

(http://www.w3.org/History/19921103hypertext/ hypertext/DataSources/WWW/Servers.html). They were among six 'experimental' servers, which were not considered stable enough to be in the list of the 26 reliable WWW servers known worldwide. The Web was already well accepted in Aleph in 1993, but the 'killer' applications came in 1995: these were the transfer to Alephwww of the ALPUB publication tools, a web interface to the ALWHO directory, and the LIGHT system for code documentation. These systems are still in use in 2001 but have been rendered obsolete by more modern publicly available tools, which can for example be seen on the web sites of newer experiments. Finally, the Aleph software, like most other software worldwide, weathered the infamous 'Y2K' bug without a glitch. Unlike may other organizations however, Aleph only spent minimal effort (and no money) in trying to look for a largely nonexistent bug before the event...

LUMINOSITY Peter Hansen

In the beginning of 1988 it was not yet widely realized in Aleph that the luminosity measurement was the dominant limitation in a measurement of the number of neutrino species, and neither was it Aleph folklore that this measurement of great interest could be done with the very first drop of data from LEP (however, see also Alain Blondel's contribution). For myself, it was a talk by Gary Feldman in the proceedings from a MARKII workshop in June 1987 that brought the revelation. This was fascinating. A precise measurement of the number of neutrino species would be a cornerstone of particle physics! And the luminosity was the star of the show! To be fair, there was already an estimate on the number of species from astrophysics, but this required all the big bang assumptions and was anyhow much less precise than what could be achieved at LEP.

Once this had sunk in, a lot of attention was focused on the accuracy of the luminosity measurement, which was performed by counting electron–positron elastic scattering in special lowangle detectors (of which LCAL, SATR and BCAL are described elsewhere in these recollections).

In his talk, Gary Feldman had estimated the luminosity accuracy to 3%. However, Aleph ended up doing a factor of 30 better! Irrespective of the specific detectors used, there were three general reasons for this surprising performance. The first reason was the calorimetric measurement of the scattering angle, which integrates inclusively over small angle photon emission, both from the primary QED process and from bremsstrahlung in detector material. Then there was the use of a tight acceptance cut on the electron side and a loose one on the positron side, alternating the two sides at each bunch crossing. This removed, to first order, sensitivity to the beam-crossing position (as good ideas often do, this turned out to be an old idea). The third reason was to use the energy balance between two calorimetric cells to define the acceptance. This way, the all-important inner angular cut-off became relatively independent of the electromagnetic shower simulation.

As told elsewhere, LCAL was used during the first three years of LEP running to measure the luminosity, while BCAL took care of the online monitoring. SATR was used for systematic checks. After the very first data taking, the Copenhagen group was asked if they would consider building a better LCAL. They regretfully declined, but John Rander stepped in with his experienced Saclay team and proposed SICAL, a novel calorimeter made of tungsten layers interspersed with sensitive layers with thousands of precisely positioned silicon pads. It fitted closely around a new and smaller beam pipe, and thus extended the acceptance down to 24 mrad. Not only did this greatly increase counting statistics, but it also got rid of the small dependence on Z parameters from the small, but finite, gamma-Z interference contribution to the events in the LCAL acceptance. SICAL was constructed in record time and installed in 1992. To this day, that design is regarded as one of the ultimate in electromagnetic calorimetry, as witnessed by the proposed detector for TESLA.

At the time of SICAL installation, the LCAL luminosity measurement had reached a precision of 0.43%, given by mechanical precision, but also by shower simulation, statistics and Z resonance

parameters. SICAL could get rid of all the latter uncertainties (and anyway Brigitte Bloch of Saclay took no chances on the shower simulation, pushing the CERN computers to their limit with full EGS simulations of millions of events). The last great leap in accuracy was achieved by precise measurements of the SICAL support plates as a function of temperature, which resulted in a knowledge of the inner acceptance boundary with the astonishing accuracy of 9 microns. From here on, the precision on the luminosity was one per mille (with its central value only two per mille from the old LCAL value, to the great relief of everyone).

The theoretical uncertainty on the Bhabha crosssection contributes significantly to the total error. The theorists were hard pressed to keep the calculation precision below the nose-diving experimental uncertainty. This pressure was applied with relentless energy and enthusiasm by Bolek Pietrzyk, who delighted in adding ever more non-leading logs to our simulation. In the end, the theoretical error was a mere 0.6 per mille. Thus, the luminosity measurements contributed mightily to the Aleph measurement of the Z line-shape, the most precise among all such measurements. In the LEP2 era, the bunch structure of the beam was changed, making it difficult for SICAL to measure the absolute luminosity. It stayed in place as a monitor, while LCAL again took over the task of measuring the absolute luminosity. But at that time the pressure was off-the statistical uncertainty in the number of interesting physics events far exceeded the uncertainty in the luminosity in most cases. During that era, the luminosity quality control would probably have been somewhat demoted on the priority list, had not Brigitte Bloch each year insisted on the big treatment (full simulation of the exact geometry, all the systematic checks etc). Not that any problem was ever found-the detectors were rock stable and the final numbers always came out practically equal to the online numbers. But afterwards it is nice to be absolutely sure that this was the case.

ALPHA (The Aleph offline analysis program) Jacques Boucrot

During the preparation of the Aleph offline software, before data taking, it was agreed that a powerful but easy-to-use analysis program should be written to be used as a common tool for physics analysis by the whole collaboration. In June 1987, during the Aleph Week in Copenhagen, the decision was formally taken to build this program and I (J.B. from LAL Orsay) was nominated as responsible for it.

The program was progressively designed and the code written in the two following years. About ten people participated in the first versions, the main contributor by far being Hartwig Albrecht, on leave from DESY, where he was the responsible of the ARGUS analysis program. A lot of thought was put into the design of the program, which was eventually modular enough to allow many further modifications and improvements without any basic redesign.

The program was ready and tested well before July 1989 for the beginning of the data taking. Then from 1989 to 1993 Ed Blucher took the whole responsibility of ALPHA, made a lot of improvements and implemented important physics algorithms such as muon identification, energy flow and b-tagging.

The name ALPHA was chosen as an acronym for Aleph PHysics Analysis. The basic ideas of the program were to

 provide an easy-to-use user's interface, written in such a way that all the complicated Aleph data structure was completely hidden from the user,

- offer exactly the same environment for real and simulated data, and also for all categories of Aleph data sets—RAW data, POTs, DSTs, MINIs and even NANOs (used for LEP1 data only),
- provide access to all important variables c.m.s. energy, track parameters and errors, vertices, identification variables, energy flow objects,...—through simple and, when possible, mnemonic Fortran statements,
- provide a full library of basic physics routines (Lorentz transformations, vertex packages, thrust calculation, jet finding algorithms,...),
- provide a full library of 'standard' Aleph physics algorithms—dE/dx for charged tracks, energy flow, identification of particles (photons, leptons, K^0 , π^0), beam spot, b-tagging, etc...—all these algorithms being progressively incorporated into ALPHA after discussions and approval by the collaboration, and
- provide a complete documentation on all these variables and algorithms, including detailed examples of coding, in a User's guide with a paper version (170 pages) and, from 1997 onwards, a Web version.

The MINI-DSTs used for the vast majority of Aleph analyses are built using ALPHA and its algorithms.

ALPHA has been very widely used for almost all Aleph analyses, being therefore tested and crosschecked by hundreds of users in very different situations. It has been constantly improved and updated, in particular when LEP2 data began to be analysed. Its final version contains about 40,000 lines of Fortran code, written by a total of about 35 authors and contributors.

From the user's point of view, the philosophy of ALPHA has proven to be extremely easy and efficient: a newcomer in Aleph could perform serious analyses after less than one week. Other experiments, even outside CERN (e.g. ZEUS at DESY, CDF at FERMILAB) have borrowed some of the basic ALPHA features to provide easier user access to their data. The BaBar analysis program, BETA, although a C++ program, has been developed using the basic ideas of ALPHA by a team led by a former Aleph physicist. From 1998 onwards, an object-oriented version of ALPHA, written in C++, has been developed under the name ALPHA++. Initially considered as an exercise to learn new programming technologies, it is now used by several people for real Aleph analyses.

ALEPH BOOKKEEPING AND SCANBOOK

Jacques Boucrot

As soon as the simulation program, GALEPH, and the reconstruction program JULIA were beginning to produce their first outputs in 1987/1988, it was decided to design a specific bookkeeping system for all official Aleph data sets. The project was started in autumn 1988; the design and coding was done by a small team of 4 people and the system was ready for use when the first data were taken in August 1989.

The Aleph bookkeeping system consists of a database and a user's interface.

The database, updated daily by an automatic program, contains the description of all relevant data sets in Aleph: Real data from 1989 to 2000, inclusive, and also simulated data. It contains also the description of all runs ever taken in Aleph, with their 'Run Quality' rating (as defined by the Aleph Run Quality group) and also the run energy and luminosity. The Monte Carlo data sets have their own particular description, including a keyword that can be used to define, unambiguously, a given production. This allows very flexible selections of data sets according to the required level of Run Quality, or of Monte Carlo data sets according to the event generator properties.

The communication between the database and the external world is done through specific files—there may be as many as 1000 per day—which are kept for some weeks in case of problems. This has proven to be useful several times in the last 12 years and enabled corrupted databases to be rebuilt.

The user's interface, called SCANBOOK (for SCANning of the BOOKkeeping database), allows the user to select data sets according to the above criteria. SCANBOOK produces data cards to be used directly in the analysis program ALPHA, to read the selected data sets.

This user's tool has been modified many times since its first version in 1989: adaptation to new conditions (e.g. LEP2 runs or new Monte Carlo generators) switches to new software environments (7 successive operating systems). The first version was a simple interactive interface, which didn't allow many user mis-typings. It was completely rewritten twice to adapt to more modern programming technologies: first for X-Windows 'push-button' interface in 1996 and then the whole system was converted in 1999 into an Oracle database with a Java Web interface.

The whole system has proven to be very powerful and useful; it deals now with the description of more than 100 000 official data sets (half of which are Monte Carlos) and 15 000 runs taken during 12 years of Aleph running. The last version, with Oracle and Java, ensures easy long-term maintenance.

ALEPH Weeks Outside CERN



An ALEPH expert explains the Higgs evidence to a layman (Editor's note-CG: after Vladimir Rencin...)

ABOUT THE ALEPH WEEKS... Ron Settles

Here are just a few words of explanation about the Aleph Weeks. As you can read in 'Structures and Procedures' above, we had

- The Aleph Week: four per year, one of them at a collaborating institute outside CERN.
- The Plenary meeting: two half-day meetings of general interest during Aleph weeks.
- The Steering Committee: usually four per year during the Aleph week.

So there was plenty of activity going on during these weeks, and the weeks outside CERN were something special which is the reason for this section of our scrapbook about the Aleph 'Experience'. Nevertheless a few comments about the general Aleph Weeks are appropriate here since this information doesn't appear elsewhere in this book.

There was one co-ordinator for these Aleph Weeks who later was appointed by the Steering Committee. These persons were Friedrich Dydak (1982-85), Adolf Minten (1985-88), Alain Blondel (1988-94) and me (1994-03). Organizing these weeks (by me at least) was always a bit chaotic. Whereas my predecessors, Friedrich, Adolf and Alain, were very good at doing this job (it was a 'hard act to follow' for me), the weeks seemed to be a bit more chaotic during my time (maybe this was due to a different viewpoint). All of the physicsgroup and detector co-ordinators had of course to set up their meetings themselves. The Aleph Week co-ordinator's job was to put these meetings altogether so that there was not too much overlap, because in principle we wanted that anybody in Aleph could go to any and all meetings if he/she chose (this didn't really work out in practice). Also the Aleph Week co-ordinator had to organize the speakers and topics for the Plenary meetings which were on the last two days of an Aleph Week (this was reduced to one day towards the end). The Plenary entailed reports on hot topics about the LEP operation, physics analyses and detector news, and traditionally a guest theorist was invited to give a talk.

So we had physics working groups on Electroweak, Searches, Tau, Heavy Flavour and QCD/ $\gamma\gamma$ during LEP1 days. As LEP2 turned on around the end of 1995, WW and BEW (beyond electroweak) were added as new groups, the Searches group was split into Higgs and SUSY, QCD and $\gamma\gamma$ were separated. Also there were Software and Hardware groups which had to keep the analysis and detector going. More details about the groups can be learned via the home page alephwww.cern.ch. What made this organization more chaotic during my time was the transition from LEP1 to LEP2, so there were more meetings to co-ordinate since the LEP1 analyses were still going on and the LEP2 colleagues started going full blast very quickly.

We always had a party during an Aleph Week, at Echenevex when in CERN and a Collaboration Dinner when outside CERN, and everybody always ended up dancing at them.

As said in the beginning, the outside weeks were something special, so the rest of this section now turns to them...
PISA '83 Roberto Tenchini

The first Aleph Week outside CERN took place in Pisa, during the spring of 1983. It was a very rainy March, as it can be, occasionally, in Tuscany. The legend says that after weeks of rain the members of the Aleph Pisa group were facing gloom and despair when, suddenly, on the Monday of the Aleph Week, the first day of sun came and the weather stayed sunny for the entire week. There were rumours that the unexpected and very welcome sun was there because of the legendary 'luckiness' of the Pisa group leader. Another proof of this aura of 'luckiness' was the fact that a distinguished Aleph Pisa member, Francesco Fidecaro, survived without injuries a terrible accident on his way from CERN to Pisa, between Morgex and Aosta at a 90 degree double curve near a bridge which has been called since then the 'chicane Fidecaro'. Francesco tried the chicane several times before and after the accident (sometimes rather unsuccessfully we have to say...) until the new motor road came into use making the chicane obsolete.

The very lively meetings and discussions were held at the Scuola Normale Superiore, whose historic building is in one of the most famous squares of Pisa, Piazza dei Cavalieri. The building is called Palazzo dei Cavalieri di Santo Stefano (Knights of St. Stephen who fought against the Moors during the crusades). The building itself was restructured by the Italian architect Giorgio Vasari in the sixteenth century. The curvilinear façade is decorated with graffiti and features an impressive staircase. The whole square was indeed designed by Vasari. The Scuola Normale was founded by Napoleon as a subsidiary of the Ecole Normale of Paris, becoming independent soon afterwards. The social dinner was legendary as well, taking place at Villa Mansi (Segromigno in Monte, Lucca). Among the numerous villas surrounding Lucca, Villa Mansi is one of the most typical. The Mansi belonged to a very well-known family in Europe involved in the silk trade even before the sixteenth century. The original building, built in the second half of the century, was mostly transformed in the seventeenth century by the architect Oddi from Urbino. The lawn and gardens, with old statues and ponds are very beautiful. During the most enjoyable dinner there was plenty of discussion about designing and constructing Aleph. Jacques Lefrançois was showing around with pride the first prototype of the ECAL fuses, which are well known by today's Aleph Shift crews for making possible the 'fusiblage'. Walter Blum gave a real milestone of granite as a gift to Jack Steinberger. The milestone can be seen at CERN (ask Jack!).

This was a very successful start to a very successful series of Aleph Weeks and the most senior Aleph members remember it with great pleasure.



MARSEILLE '84

Jean-Jacques Aubert

This was the starting years for the CPPM Laboratory on the Luminy Campus in Marseille.

I remember very little about the scientific content of this Aleph Week or of the detailed organization.

One anecdote, I do recall, concerns Wolfgang von Rüden who brought flowers to the secretary at the end of the meeting. Another clear memory is the bouillabaisse we had in a Cassis restaurant by the old harbour washed down with Vin de Cassis.

This scientific manifestation in a young laboratory was seen very positively by the local environment and it contributed to the long-term development of the CPPM laboratory.

Thanks to Aleph

LONDON (IC) '85 Peter Dornan

In 1983 Lorenzo had the splendid suggestion that each year we should hold a collaboration meeting outside CERN. For the newcomers to Aleph, following the demise of ELECTRA, this gave an opportunity to demonstrate our new allegiance and entertain the collaboration. In 1984 Marseille offered their hospitality and in 1985 it was Imperial College's turn.

As everyone who has organized a large collaboration meeting knows, it is not the quality of the talks, the seating or audiovisuals of the lecture halls that everyone remembers. It is the dinner. So what could London offer after the Italian and French cuisine of Pisa and Marseille. We were on test, scepticism abounded, British cuisine, to put it mildly, did not have the best reputation—yet it had to be typically British!

Fortunately we had a secret weapon, our Italian data aide and meeting organizer, Piera Brambilla. She was given the task of finding a characteristic London eating house, for about 100 people with memorable food and surroundings at a reasonable price-we like to give our staff challenges! Piera thus had the arduous task of finding and sampling numerous London eating/drinking establishments and in this, of course, I felt obliged to keep her company. So the search began and we discovered an old-established wine merchant who operated a number of atmospheric wine bars/restaurants in the financial area of London. Life became very hard as Piera and I had to take off many lunchtimes to sample the fare at these Davy's Wine Bars, before we found the ideal one-Mother Bunch's.



133

The location of Mother Bunch's was inauspicious, the entrance being through a small, poorly marked door in the arch of an old railway bridge near Blackfriars—but inside the ambience was excellent. They could just take 100, they would produce simple, high-quality British food, shellfish, poached and smoked salmon, very rare beef (the days before BSE!), pork, ham, simple and exotic salads, and finishing with traditional desserts and fine-they really do exist-English cheeses. The food was universally appreciated. However, Davy's is also an old-established wine merchant and so the quality and quantity of the wine, mainly French one must admit, was also fine. Eventually 90 people came. On entry sherry was served and then the wine flowed, white from the Loire, red from Bordeaux. After more than a bottle of wine per person had been consumed, (the bill is above), I was asked if they should bring the port—one could hardly have said no.

All reports suggest it was a most enjoyable evening, although recollections of detail are dim. The following day the meeting continued—memories of that are even dimmer, except I still recall Enrique giving the first talk from the newly joined Barcelona group. It would not be too long before we would be going there.

This started a trend. In the following years both BaBar and ZEUS would hold their dinners at a Davy's wine bar, regrettably not at Mother Bunch's as it closed a few years ago.



Peter Dornan.

MUNICH '86 Ron Settles

This Aleph Week at Munich was carefully chosen such that the Oktoberfest started the week that just followed, which put a squeeze on everybody to reserve their hotels early enough. (In fact this conjunction was more of an accident, but the story sounds better this way, and some of our famous colleagues used this coincidence to stay on another few days after the Aleph Week was over.)

I was really nervous since this was the first time I had organized an undertaking involving the magnitude of the Aleph Collaboration. We decided that the institute's building of MPI-Munich was large enough to accommodate the whole meeting including parallel sessions, and in the end in spite of some squeezes it worked out nicely due to the flexibility and understanding of all of our friends. Friends... in fact we were in the middle of building our detector and getting the analysis ready in 1986, we had been going for over four years by then, and everybody knew almost everybody and really represented a large body of friends. A glance at the agenda for the plenary meeting will give you an idea of where we were:

2 September 1986

ALEPH Plenary Meeting Munich, 18-19 September 1986

Agenda

Thursday, 18 September

9.00 a.m.	J. Harvey G. Kellner A. Putzer P. Palazzi	Status of on-line program ALEPH programming The ALEPH database ADAMO
2.30 p.m	H. Videau D. Schlatter J.J. Aubert L. Foà G. Kellner P. Dornan	Graphics Track finding Cluster finding and e/π separation μ/π separation Schedule and Summary ALEPH Data Analysis and all that

Eriday, 19 September

9.00 a.m.

Mini-vertex detector The ALEPH Data Analysis Facility

Steering Committee

G. Tonelli

E. Fernandez

During the week we were able to show to the collaboration how the production of the TPC sectors was going in our MPI workshops: things were progressing smoothly by then (we had overcome some substantial initial difficulties described in the TPC story above), but we were late. Nevertheless our colleagues from MPI technical division were proud to show their work to everybody and especially to meet our famous spokesman. The VDET was still in the process of getting off the ground; Guido Tonelli gave a thorough progress report which was nice in spite of the fact that the time schedule was way too optimistic (see VDET story above).

As usual the days of that week were packed with work. This was compensated for by the evenings with pleasant times in various Bierstuben. I remember the collaboration dinner on Thursday evening in the (famous?) village of Aying (which produces a very good beer) where there was plenty of singing (Dave Levinthal was the loudest) and entertainment in addition to the Bavarian-style menu, and Jack gave his usual speech to motivate the troops. We had a bus take everybody from and to Munich city, and the bus driver was so impressed to have a famous guy as passenger that he even personally drove the bus all the way to Jack's hotel (which helped Jack out a lot because he was just recovering from a hip operation at the time).



We were still constructing the TPC sectors at MPI in 1986 (cartoon by Mike Binder).

COPENHAGEN '87

Peter Hansen

Located today on the eastern border of Denmark, this town was originally chosen as capital because of its central location at the sound, Oeresund, separating Sjaelland, the largest island, from what is now southern Sweden. The place was ideal, because of the enormous numbers of herring—for which there was an insatiable market in the south—that each year crowded up through the sound, and also because the sound was narrow enough, so that the king could extort fees—under the threat of cannon fire—from the merchant ships travelling to and from the Baltic Sea.

Unfortunately (for the Danes) those sources of income dried up in the 17th century, but the town somehow managed to continue being a major northern metropolis, for example fostering an impressive sequence of physicists: Brahe, Roemer, Oersted and Bohr. The first one of this list enjoyed the most generous funding of a physics project ever granted, absorbing 2% of the government budget for his observatory on his little island in the sound. The last one, Bohr, did not do so badly either.

In 1920 Bohr got the funds to build a large physics institute in Copenhagen. He was not really an admirer of clarity (thinking this was to give a false impression of the world), and his building, in the construction of which he took a very active part, mirrors his taste for complexity. There are everywhere extra levels, halfway between two storeys, opening up new passageways to complicated rooms and experimental halls, especially in the basement area. It was in this labyrinth of a basement that the Aleph collaboration meeting was to take place. Somehow, however, everybody found the lecture hall and later (to my knowledge) the way out.

The various subdetectors of the experiment were at that time getting close to completion. One notable exception was LCAL, which the hosts of the meeting were supposed to build. It was especially the airtight aluminium container which caused problems (it turned out in the end that it would have been cheaper to buy a big, massive block of aluminium and drill away the space for the detector, than to weld it together as was done). But Pierre Lazeyras didn't worry in his report to the collaboration.

'Four little boxes like that', he said while giving a kick to one of the failed prototypes, 'that is no problem!' He later made sure that he was right by providing a lot of help with this part of the work.

Another topic which got much attention (if I remember correctly) was the offline computer environment, focusing on the new and exciting concept of workstation clusters. As far as the future physics was concerned, the Wisconsin group had calculated the ultimate limit we could set on the Higgs mass after 500 pb⁻¹ of LEP2 data. It was 90 GeV, so we have surpassed even Sau Lan's expectations here.

Poul Henrik Damgaard, the guest theorist, explained why lattice gauge calculations favoured a Higgs mass of 300 GeV. Let us hope he was not right. The highlight of the meeting was the collaboration dinner. It took place in a little inn next to a lighthouse which, high upon a cliff, warned the ships passing by the northern coast of Sjaelland. From the terrace of the inn, which was built into the cliff, we could watch the beautiful sunset over the water, and see as far as Kullen, the remarkable granite peninsula that cuts like the stern of a great ship out from the Swedish coast. Also the nice selection of herring, marinated, pickled and whatever, created much enthusiasm. Everybody was there including the young apprentices who had helped carry the coffee for the coffee breaks at the meeting. Initially they had not been invited, but when this came to the ears of Lorenzo Foà, he gallantly exclaimed: 'They will come, and Italy will pay'-to the joy of the two young girls.

BARCELONA '88

Enrique Fernández

This was the first big meeting, ever, organized by the Barcelona group. We had no previous experience in hosting such an event, and made many organizational mistakes, but in the end most people were happy with the meeting, or at least we got that impression.

The first session was at the (rather new) Veterinary school, and we were the first to use the sound system. That was a rocky start!

The most frequently asked question was: When do people in Barcelona sleep? We still have no answer; it seems that there are quite a few people who never sleep at all (but none from our group, as far as I know...).

The dinner was a big hit, consisting of many, many small dishes of small fish. Sau Lan Wu paid us a compliment: for her it was like a Chinese banquet!

All in all our memories of the meeting are very warm, perhaps because we all were more than 10 years younger. If given the chance, we would host an Aleph Week again!

ATHENS '89 Anna Vayaki

Way back when we were young, and before shifts and shift leaders became a focal point of Aleph, back in May of 1989, there was an Aleph Week in Athens. Well, actually in a suburb of Athens, called Vouliagmeni, as those of you who participated must recall.

At first there was trepidation, all the puritans in Aleph thought it was really indulging the flesh to come and spend a week by the sea, trying to work, when the first data run had not yet happened. The result proved them wrong, as work was carried out, in a funny schedule to be sure, with a long midday break so people could go swimming on the lovely beaches, and evening sessions to get the work done. The climax of the meeting was at the Aleph dinner, in a taverna in Vari, when we all whooshed over and sat on the floor in front of the single TV, to watch Jack Steinberger being interviewed for the evening news.

(Editor's note-JL: Jack, of course, had recently been made a Physics Nobel Laureate (along with Leon Lederman and Melvin Schwartz).)

(Editor's note-RS: These words give an impression of calmness, but actually the situation was rather tense: the detector was just starting, the DAQ was not working yet, and several people stayed in CERN getting ready for the pilot run. There were heated words exchanged (too racy to repeat here) between Jack Steinberger and Alain Blondel on the best way to analyse the first data.)

FRASCATI '90 Giorgio Capon

In 1990 the 'abroad' Aleph Week took place at Frascati from 12 to 15 September.

Frascati is situated in a nice region of hills (Castelli Romani) where various princely residences were built from the Renaissance onwards for nobles and cardinals. Paolo Laurelli, then leader of the Frascati group, chose to organize (with the help of Silvia Giromini and Cristina D'Amato) the meeting and host the participants in one of these villas—Villa Tuscolana—one of the most important villas of the 'Ager Tusculanum', now restored and adapted as a hotel and congress centre.

This villa (the highest of the old villas on the hill above the town of Frascati) was originally built in 1578 on the ruins of the ancient villa of Marco Tullio Cicerone and restructured by L. Vanvitelli in 1740. Among its several owners are famous people such as Cardinal Pietro Aldobrandini, Cardinal Francesco Sforza, Prince Luciano Bonaparte, Lady Maria Anna of Savoy and Queen Maria Cristina of Sardinia.

The weather was very nice during the meeting so that the participants during the coffee and lunch breaks could circulate in the villa's gardens and enjoy beautiful views down towards the city of Rome as well as continue their physics discussions *alfresco*.

The most notable event was surely the open air social dinner in the clear warm Roman night where the rich banquet was followed by a very lively exhibition of three young artists (the Acquaragia band, specializing in traditional music and instruments) who played guitar and accordion and sang folk songs to the great delight of our colleagues.

With their encouragement, physicists and companions launched themselves into dances while Antonella Antonelli and Fabrizio Murtas played traditional Roman folk songs and Ken Smith in a Scottish kilt gave a memorable performance on the bagpipes.

Other entertainments were also scheduled: a 'Pullmans tour' under the guidance of F. Murtas and A. Antonelli led participants across the volcanic lakes of the region of Nemi and Castelgandolfo where lunch was had in a typical restaurant.

PARIS '91 Personal memories *Claus Grupen*

(Editor's note-CG: Henri Videau, the main organizer of the Paris meeting, wasn't available to write this story so I am substituting, with some personal recollections.)

It was not 'April in Paris' but a cold November week in Paris when we held our annual external Aleph meeting in 1991. During the meeting various problems, encountered during the recent data-taking period, were discussed. Among these Werner Witzeling told us a 'Short Story'—i.e. about *electrical shorts!* There had been two such shorts in the outer field cage of the TPC during the 1991 data taking which led to some runs being classified 'MAYBE'.

However there were also some improvements on the horizon:

- The new SiCAL luminosity calorimeter was being prepared and was being *put through its paces* in a test beam. In order to install this new detector, however, we would have to kick out the nine-layer SATR drift chamber which sat in front of LCAL.
- The tedious task, endured by the shift crew during their pit tours, of checking the rows of HCAL gas bubblers was going to be a thing of the past. A new HCAL bubbler monitoring system was presented. This was an optical system, linked to a P.C., which counted the bubbles passing through each bubbler and warned the shift crew when there was a problem.

Our guest speaker from LEP, Lyndon Evans, informed us about the Current and Future Status of the LEP machine. The question was raised whether the eight-bunch operation mode, foreseen for 1992, might really give a factor of two improvement in luminosity. Lyn was cautious in his response, arguing that it should do so but also that nothing is ever certain in electron machines. He pointed out that, for operating in this mode, the beam adjustments required might take ages!



Typical Parisian evening entertainment.

When planning a few days in Paris, it is equally important to decide where to go after the physics duties are fulfilled. The Champs-Elysées and Trocadéro areas are full of tourists and some Aleph members were certainly spotted in these places. Aleph physicists obviously can resist almost anything except temptation and there were lots of temptations in these famous spots. The picture, by Henri de Toulouse-Lautrec, clearly shows that these temptations have not changed over the years and gives you an idea of what some of us did. At the traditional Aleph dinner we could enjoy the outstanding French cuisine, including an excellent selection of French wines. The Germans have high quality wines and even the British produce wines in Kent but these are nothing compared to the really good stuff we enjoyed in Paris.

GLASGOW '92 Jim Lynch

Proceedings opened with a reception in the Bute Hall of the University, a magnificent hall used for Graduation Ceremonies and other important events. A Vice-Principal of the University, who delivered a long monologue about the history of the University, welcomed participants. (Many of the collaboration seem to have adjourned before the end to one of the recommended pubs on a list thoughtfully provided by local expert Stan Thompson!)

The following morning Bert Turnbull welcomed the collaboration to Glasgow at the first working session which started with reports from the LEP2 Study groups on W Couplings, W Mass, and SUSY and other Non-Higgs searches. Next were two papers on the strong case for upgrading VDET and the practical details involved in such an upgrade.

Saturday morning started with the Steering Committee report by Adolf Minten which included the announcement that Lorenzo Foà would be the next Aleph spokesman. Ron Settles presented the run co-ordinator's report for the preceding two months during which the millionth Z event was recorded. Steve Myers presented a detailed report on the status of LEP and plans for LEP2. Eric Lançon reported on the status of reprocessing and Monte Carlo production and official proceedings closed with Alain Blondel thanking the Glasgow hosts for a very enjoyable meeting. The 'social programme' certainly contributed to participants' enjoyment. On the free afternoon a choice was offered between a cruise on Loch Lomond and a visit to the GlenGoyne whisky distillery. A bus took the cruisers to Balloch at the southern end of Loch Lomond from where the boat left and sailed in sunny weather up the loch to Luss-a village on the banks of Loch Lomond, which is featured in a Scottish TV soap entitled 'Take the High Road'—before returning to Balloch and getting the bus back to Glasgow. The whisky aficionados travelled by bus to the GlenGoyne Whisky Distillery some 20 miles outside Glasgow. There, the wonders of producing this 'Amber Nectar' were explained and demonstrated, with ample opportunity to sample the finished product at the end of the visit. On the way back to Glasgow the bus stopped off to allow participants to visit Gartness Falls where they were able to see the salmon leaping up the waterfall.

In the evening the Plenary Dinner was held in the University Staff Dining Rooms. (We could not afford the cost of hiring the Bute Hall!) A traditional Scottish menu was accompanied by 'Cellier des Dauphins' wine, which caused some amusement among the participants! An after dinner address was given by Seamus McNeil (Principal of the Glasgow School of Piping and former Senior Lecturer in the Department of Physics and Astronomy) who gave an illustrated talk on the 'Music of the Bagpipe—The Pibroch'. Afterwards, Elizabeth Martin (bravely) gave us a rendition of a Scottish folk song accompanied by Seamus on the bagpipes! On the final evening the collaboration took over the Riverside Club in the centre of Glasgow for a 'Ceilidh' (a Scottish Social Gathering which, these days, is usually an evening of Scottish Country Dancing). Instructions on how to perform the dances were provided by the Band Leader and the number of female dancers was enhanced by wives and daughters of Glasgow members as well as nurses from nearby hospitals. The entire gathering really 'let their hair down' and threw themselves into the spirit of the event!

To quote Mary Laurie, the owner of the Riverside: 'We have never had an evening quite like this at the Riverside. Normally we cater for young people, from Glasgow and the surrounding areas, who drink wine and beer. Tonight everyone here seems to be drinking Malt Whisky—our stocks at the bar have already run out and the bar staff have had to get more from our stores. They brought up a crate of 100% proof Macallan Malt Whisky which they have started serving—if you need a fleet of ambulances at the end of the night, let me know'.

The Scottish country dancing was described by the meeting secretary as 'memorable'!

It was fortunate that the important discussion and decisions regarding the upgrade of VDET had taken place the previous day since, at the final session on the Saturday morning, many people struggled in late with sore heads and bloodshot eyes!



Jack Steinberger and Jim Lynch.



INNSBRUCK '93

Dietmar Kuhn

The 1993 outside-of-CERN Aleph Week took place in Innsbruck from 22 to 25 September with 164 collaboration members participating. The parallel sessions were scheduled from Wednesday to Friday noon, followed by the Plenary sessions (and a steering group meeting) until Saturday noon. On Wednesday evening a welcome reception was given by the local authorities.

Considering the meeting place, the Innsbruck group was tempted to have the meeting in the newly built science campus outside town with its modern rooms and excellent infrastructure, where the physics institute is located in the 'Victor Franz Hess Building'. This building is named after V.F. Hess, who in 1936 was awarded the Nobel prize for the discovery of cosmic rays (sharing the prize with Anderson), and was professor at the institute by that time. However, the typical Innsbruck atmosphere is better experienced in the heart of the city with its narrow streets and picturesque arcades, and so the not so comfortable main building of the University near to the old town finally was chosen as meeting place.

The focus of interest during the meeting is best characterized by the title of the talk of the invited guest speaker, G. Altarelli: 'From LEP1 to LEP2', but of course especially in Innsbruck, the evergreen topic of 'status of hadronic fragmentation', presented by Gerald Rudolph, could not be left out. After the meeting on Friday late afternoon, a historic train was waiting for the participants right in the heart of Innsbruck, at the 'Anna-Säule' not far from the famous golden roof, and took them up to the little village of Lans, where the conference dinner took place in an old inn named 'Isserhof'. Right at the entrance the participants were welcomed by Günther Dissertori playing brilliantly on an accordion-a man of many talents! Wine, food and mood were very good—volunteer singers presented national songs, and quite a few people danced. Participants in the Saturday morning session were warned to arrive well equipped, i.e. with hiking shoes and pullovers, since after the session-and after a hearty peasants buffet-an excursion by cable-car to the Hafelekar mountain above Innsbruck was planned, where Victor Franz Hess in 1931 had set up a little laboratory in a former alpine hut for his cosmic ray studies. An ionization chamber and some other equipment used by him are still there in their original places, some muon and neutron counters were added later and are still running.

But... the weather forecast issued a warning that the warm foehn wind blowing at that time would break down on that very afternoon, resulting in a dramatic drop of temperatures and even snowfall in the mountains—however, the moment of this breakdown usually is uncertain to a couple of hours. For this reason the organizers did not dare to risk an excursion to the Hafelekar and offered the alternative programme of visiting the castle of Ambras near Innsbruck. Strolling through the park of the castle after the visit, the participants could admire the top of the surrounding mountains still in their full beauty—no rain, no snow! A handful of daring adventurers had taken the risk and had hiked (under expert guidance) to the Hafelekar and returned safely before the bad weather started. Unfortunately they did not sign the guest's book dating back to Hess and decorated with signatures of people like Piccard, Siegbahn, Leprince-Ringuet and many more. For the accompanying persons, there was a rich programme every day, planned and executed by students of the Innsbruck University, starting from a walk through Innsbruck and including excursions to the medieval town of Hall, to a farm museum, to the alpine zoo and ending with a shopping tour on Saturday.

HEIDELBERG '94 Eike-Erik Kluge

The 1994 outside Aleph Collaboration meeting took place from 11 to 15 October in Heidelberg, home to Germany's oldest university; not only that, but Heidelberg is also world famous for its old town, being situated on the banks of the Neckar River at the place where it leaves the Odenwald hills and enters the broad Rhine Valley.

In order to allow our guests to profit from the beauty of old Heidelberg and its tourist attractions, our meeting was held away from the new campus at the edge of Heidelberg where also the 'Institute for Hochenergiephysik' is located. It took place in what is now the University's 'School of Interpreting', but previously was the Department of Chemistry, built by Bunsen, Kirchhoff et al. In fact, in the very place where spectral analysis was invented i.e. our methods, albeit on another scale.

As foreseen, the participants in the collaboration meeting not only attended numerous plenary and parallel sessions, but used the occasion to convene also at more lively places nearby, cafés, restaurants, bistros, and continued their scientific discussions there, I presume. On Thursday evening, the University invited us to a reception in the so-called 'Old University' building, accompanied by a talk on its history by the Vice-Regent in the 'Aula', an auditorium built in the last century in the style prevailing at that time and mostly used for ceremonial purposes only.

On Friday evening, finally, the traditional collaboration 'dinner' took place in an old barn transformed for purposes of this kind; the attraction was a whole roasted ox accompanied by a variety of food and drinks, although the wine was not as dry as many of us are accustomed to.

However, we were lucky enough as there was a piano in the barn and thus live entertainment was offered by the usual subset of Aleph people talented also in this respect.

Altogether, it was, I guess, a successful and memorable meeting in the unique Aleph style.

BARI '95 Giorgio Maggi/Mauro de Palma

In 1995 the outside meeting was held from 2 to 6 October in Martina Franca, on the Southern Murgia Plateau, where the district of Bari borders on those of Taranto and Brindisi. The trip from the Bari-Palese airport to the heart of the Itria Valley was among hills covered with gardens and vineyards, olive trees and woodlands. It was possible to admire the Trulli, inimitable examples of spontaneous architecture, whose building technique has been passed on from countrymen to countrymen for centuries, built with local stone and with cone ceilings covered by 'chiancarelle' or flag-limestone.

Over 130 physicists attended the meeting, and most of them were for the first time in the region. Martina Franca old town with its tidy streets lined with majestic buildings and white houses, blind alleys and small arches impressed everybody. Baroque balconies decorated with wrought iron and elegant doorways reminded one of the noble origins of Martina Franca.

The Park Hotel, San Michele, hosted most of the participants. In 1300 the Prince of Taranto, Philip of Anjou, granted privileges and tax exemptions (franca). Martina Franca became a Dukedom in 1506 under the Caracciolo family who built the Palazzo Ducale in 1669. Nowadays the Ducal Palace houses the town hall and still preserves magnificent internal chambers such as the Sala dell'Arcada with paintings by Domenico Carella. Palazzo Ducale hosted our plenary meetings.

The meeting highlight was surely the food... A few thousand lira were enough to have a pleasant and decent meal. Many colleagues tried the typical dishes like *carne al fornello*. In addition fantastic coffee breaks were offered in the mornings and in the afternoons. The social dinner was held in a farm called Masseria San Lorenzo and a typical menu with 'a few' specialities was prepared. A memorable dinner...

The Friday afternoon trip was a walk in Bari old town. The tour was organized to show the historical monuments of the town, but what actually aroused curiosity was the escort of police. However, as we tried to explain at the time it had to be considered as a guard of honour.

As very few people know, the Bari patron San Nicola (in Bari: Santa Nicola) is the same as the Santa Claus character celebrated in all northern countries. This implies that in Bari there are Santa Claus relics.

(Editor's note-ISH: Quite a lot of physics was also certainly done in between eating, drinking and sightseeing?)



Guess whooo...?

CLERMONT '96

Bernard Michel

More than one hundred Aleph members took part in the Aleph Week held in Clermont-Ferrand, from the first to the fifth of October 1996.

It was at a turning point in the Collaboration's life: the scientific programme reflected this transitional period from the LEP1 to the LEP2 era. On one hand, we had a lot of discussions about the intensive analysis work on Z decays, using the complete data set of LEP1. On the other hand, 1996 saw the first data taking and results at the WW threshold and above and was the real start of LEP2: Invited talks from Michael Shifman on 'Thoughts on new physics at LEP', and from Steve Myers on LEP2 machine prospects, gave rise to a clear interest and even some excitement.

Obviously, just as in previous Aleph Weeks, we had the chance to make known some of distinctive features of the Auvergne Region: a traditional country dance party, sightseeing tours of volcanic



Puy-de-Dôme (1465 m).

sites (Puy-de-Dôme, Monts Dore) and romanesque churches (Saint-Nectaire, Orcival). The high point of these social events was a delicious typical dinner (foie gras, chars from volcanic lakes, Salers beef tenderloin), ended with a frenzied 'Macarena' executed by the whole Collaboration!

I take the opportunity to say how our group appreciated to be a part of the Aleph family and was very happy and proud to welcome the Collaboration to Clermont-Ferrand.



Romanesque church in Orcival.

OXFORD '97 John Thompson

In 1997 the annual meeting outside CERN was held at St. John's College, Oxford in the UK organized by the RAL group. St John's was founded in 1555 and being one of the richest Colleges in Oxford was able to provide excellent modern facilities as well as an old traditional atmosphere in the halls and gardens. Almost all members stayed in student accommodation on-site and the meetings were held in various lecture rooms and a large auditorium within the College. This arrangement, although somewhat Spartan, proved to be very popular and over 160 of us attended the meeting, which uncharacteristically was blessed with fine weather!

The meeting was largely devoted to discussions on the analysis of the new data at 183 GeV being collected with good luminosity following the infamous SPS fire. As one of the invited speakers, Steve Myers gave a very upbeat account of the LEP performance despite the vacuum problems caused by overheating. His confident predictions for the future running have turned out to be correct so far! Our other visitors were theoreticians-Herbi Dreiner (RAL) gave an overview of R-parity violating SUSY while Frank Close tried to persuade us that we could separate out 'glueball-like' states from quarkonia by double tagging yy events. This was also the meeting when first data from the new BCAL++ was presented; we heard an excellent account of progress in the new 'tracking' software from Dave Casper and decided to reprocess all our LEP1 data and Monte Carlo.

As usual the social events played an important role. These began with evening drinks in the ancient garden quadrangle at St John's on Tuesday (surprisingly warm enough to be outside!) to be followed by a buffet supper evening at the Divinity 'schools' on Wednesday. Located 15 minutes walk from St John's, this is claimed to be the most beautiful medieval building in Oxford with a stone fan-vaulted ceiling built in 1427-83 and a Convocation House attached where the English parliament sat during the Civil War. The Divinity schools were the original University faculty buildings of Theology and are in the old Bodleian library complex. After a short introduction, the librarian invited some of us to visit the Duke Humphreys library upstairs where medieval books are still chained to the shelves! Amazingly, given the primitive facilities, a warm buffet meal was provided with wines carefully selected by Roger (and approved by the authorities for consumption on the premises) followed by 'country' dancing to music provided by a local live band. In such hallowed surroundings, it was quite an exceptional but typical Aleph irreverent occasion.

The traditional Friday afternoon trips consisted of either a walk between pubs on the ancient Ridgeway trail close to RAL (where we had held the obligatory Plenary in the morning) or a visit to the Avebury stone circle and Silbury Hill 40 miles away. The latter is about 4700 years old and the oldest human-made mound in Europe. Although supposed to be out-of-bounds, a few of our more determined colleagues succeeded in conquering its peak (all 57 m!). One of them (PJ) famously managed to dislocate his shoulder on the way down and ended up in Swindon General Hospital for the night missing the Collaboration dinner back at the College! Rumours of his plight rapidly circulated which dampened the enjoyment of the dinner but in the end he caught his plane back safely to Geneva and everyone, not least the organizers, breathed a great sigh of relief.

Despite the traumas, it was a great week!



Silbury hill (in the distance) and Patrick Janot (before the dislocation!).

MAINZ '98 Sascha Schmeling

The 1998 ex-CERN Aleph Week was held in Mainz, 22–26 September. More than 100 Aleph physicists accommodated well with the students and staff of the Department of Economics of the University of Mainz, who provided their halls for the meeting.

The physics agenda started on Tuesday morning and in the evening the collaboration met at the City Hall for a reception by the Deputy Mayor, Mr. Krawietz, who showed his interest in science in his address, which was followed by a speech by the Mainz group leader and the spokesman. After a few glasses of regional wine Aleph physicists began exploring the old town with its restaurants and wine pubs. Most of them got the taste of Rhine wine and the specialities of the region, like 'Spundekäs", 'Zwiebelkuchen' or 'Handkäs' mit Musik' before the groups divided to get to their hotels spread over the city.

On Wednesday the last unknown spots of the old town, such as the 'Dom' and the well-known St. Stephan's Church with its famous windows by Marc Chagall were discovered on a guided tour which ended with the discovery of more wine pubs. In contrast to other Aleph Weeks, the excursion and the collaboration dinner were held on Thursday. After lunch, we started by bus to Bingen upon Rhine where we boarded the Aleph ship, the 'Pegasus', for a journey down the Rhine through the Middle Rhine Valley, along the Lorelei—which was nervously awaited by all photographers—towards St. Goar. Uphill from the town sits 'Rheinfels Castle' where we had a guided tour and afterwards had dinner in the castle's restaurant 'Schloßhotel and Villa Rheinfels'.

After two more days with plenty of meetings, the collaboration managed to finish this Aleph Week almost on time at Saturday noon. Many collaborators used the week-end to discover a bit more of Mainz and the Rhine Valley before leaving.

(Editor's note-JL: The Mainz meeting was a sad occasion for the Glasgow group. Shortly before the meeting we learned of the sad and untimely death of our friend and colleague, Colin Raine. Members of the Glasgow group had to leave the meeting early to return to Glasgow for his funeral.)

SIENA '99 Roberto Tenchini

In 1999 it was once again the turn of Italy to organize the Aleph Week. The Florence and Pisa groups accepted to organize the meeting and Siena, one of the most impressive towns of Tuscany, seemed an ideal place. The organizers were enthusiastic to have the Aleph meeting in such a beautiful historical place but soon it became evident that organizing a meeting in a mediaeval town, made for horses and not for cars, was not easy at all. Participants had to be taken to Siena from Pisa and Florence airports, and the train station itself is not in the heart of Siena. Still we wanted to have the meeting (and the hotels) in the old town and not in some maybe modern and efficient, but not so charming, congress-hotel outside town. Fortunately we had wonderful help from our colleagues of Siena University.

The meeting was held in the Aula Magna of the University (only 30 metres from Piazza del Campo) and in the rooms of the Physics Department, located in the same building. On Monday we had the reception in one of the most beautiful buildings of the world (no joking!) the Palazzo Comunale, with the Torre del Mangia, in the Piazza del Campo. We had our cocktails on the balcony looking out to the Siena hills in the evening light and we also had the opportunity to see the historical museum, opened in the evening especially for Aleph. The following day the meeting started in the Aula Magna. Our friends of the University of Siena are very professional in organizing congresses and even having been told our meetings are rather informal in their style, they said they were going to provide people to carry around the microphone at question time. So Aleph members were rather surprised (pleasantly surprised, especially some chaps...) to see young ladies, professionally dressed (... and very pretty indeed!) bringing the microphone to people asking questions. I have to say questions were very frequent at the meetings, especially by young men... Another interesting feature was the lunch, organized buffet style in the court. Food and wine were appreciated by everybody but there were some complaints from the catering service because:

- a) We in Aleph are very well educated and we queued at the buffet, while you are not expected to queue at an Italian buffet ('just go to what you like and take it, they said!')
- b) We in Aleph are not very well educated and we sat down on the ground during the buffet and in an historical Italian building you are not supposed to do that!

The social dinner was in a Villa on the hills close to Siena. We had *Risotto con Tartufo* and many other things. The *'cuoco'* had some trouble because he was not told in advance that outwith Italy vegetarians exist, so he had to discover it during that evening. After dinner we danced a lot, with live music. The lady singer's dress was strongly criticized by Aleph ladies (women are never politically correct, what's wrong with a Barbie-doll-like pink dress?). Of course one of our men was singing too: Franco, who else? In any case it was all great fun. One last thought. Everybody noticed, and laughed a lot about, an inscription in the Aula Magna, on the very high ceiling. The writing was erased just after the second world war, but with time reappeared and was the name of a famous Italian dictator. Everybody coming in noticed the name, pointed at it and laughed. This is the good side of history. Dictators, rulers, people who want to be looked at with great respect (and fear!) and wish to write their names in history, after fifty years are often just a source of laughter for future generations...



Plaque on the ceiling of the Aula Magna.

AIX-EN-PROVENCE 'OO Paschal Coyle

The external Aleph Week in 2000 took place from 18 to 23 September and was held in Aix-en-Provence, France. It was organized by the Centre de Physique des Particules de Marseille (CPPM). The 'Université de la Méditerranée', to which CPPM belongs, is shared between Marseilles and Aix-en-Provence, Aix being located about 30 km north of Marseilles.

The meeting was held in 'Le Petit Palais' an old church, which has been recently converted into a conference centre. Le Petit Palais is conveniently located just at the top the 'Cours Mirabeau' the main thoroughfare of Aix. The 120 people attending the Aleph Week stayed at various hotels close to the centre of Aix. For lunch, people fended for themselves amongst the large selection of restaurants and cafes which form the heart of Aix.

The week started on the Monday evening with a guided walking tour of the old town of Aix, followed by a welcome reception in the formal French gardens of the Pavillon Vendôme. The Pavillon is a 17th century Bastide, now a museum, built by M. Vendôme to house his mistress! The weather was very nice and people were able to enjoy live music and sample some of the local wines and nibbles under the protective shade of the plane trees. A welcome speech was given by M. Legrand, 'adjoint délégué aux universités de la mairie d'Aixen-Provence'. Unfortunately at 10 p.m. sharp, the lights went out unexpectedly and festivities had to be relocated back towards the bars at the centre of town! The next two days were devoted to physics meetings. The main excitement was of course the Higgs. During the summer, Aleph had started to observe an excess of events in the four jets channel consistent with a Higgs of mass around 115 GeV/ c^2 . As a consequence of these observations, on 5 September, six days before LEP was due to shut down for good (and two weeks before the start of the Aix Aleph Week), the LEPC had decided to extend the LEP run by a couple of months. The hunt was therefore on to collect more 'golden' Higgs candidates and to hope that the other experiments, although less sensitive than Aleph to such a high mass, would also start to see candidates. There was really a sense of excitement and the smell of a big discovery in the air! Even the possibility of running LEP the next year was being seriously discussed. Consequently, at the HTF meeting, a decision was made to rapidly publish a 'fast' paper on the Higgs results immediately after the end of data taking on 2 November. This was rather unusual as it would be the first paper published by Aleph using data for which a final reprocessing had not been applied.

The Thursday consisted of a WW plenary meeting in the morning followed by an excursion in the afternoon. A choice of two excursions was proposed, the first a boat trip from Cassis to view 'Les Calanques', the dramatic fjord-like coastline near Marseilles, followed by a walk back to Cassis along the coastal path. The second trip was to St Rémy-en-Provence and Les Baux with a visit thrown in to 'La Cathédrale des Images'. The first trip was by far the most popular, unfortunately the wind was quite strong, so it was not possible to get off the boat in the Calanques as originally planned, thus instead two separate contingents were formed; a boat party and a walking party. After such strenuous activities, a well-earned rest was provided by a sampling of the famous Cassis white wines at a local wine-cellar. Once clothes had been dried out and blisters treated, people then returned to Cassis for evening dinner.

Friday morning was a Plenary session, the highlight being the guest machine expert talk by Paul Collier and the lowlight (due to the topic not the speaker!) being the talk by Wolfgang Tejessy on the 'installation de base (INB)' procedure: Those brave souls requiring access to the pit during the dismantling, were legally bound to attend such a briefing and had to sign an attendance sheet after Wolfgang's presentation was finished.

The steering committee meeting took place on the Friday afternoon and was uncharacteristically eventful. Owing to a demand from the CERN management for Dieter Schlatter to take over as director of the PPE division, it had been unexpectedly necessary to find a new spokesman a year before the nominal end of Dieter's mandate. A search committee, chaired by Klaus Tittel, had thus been set up to find a replacement. Fortunately, a consensus was quickly reached by the committee, and Roberto Tenchini was asked and accepted to take over from Dieter from April 2001 onwards. The other 'hot' topic of the steering meeting was a discussion on the role/function of Aleph notes, triggered by a recently released internal note on the Higgs effect!

The Friday evening, was the collaboration dinner. It was held at 'Les Roches Blanches' a hotel overlooking the bay of Cassis and 'Le Cap Canaille' (the highest cliff of mainland Europe). Aperitifs by the pool were accompanied by a beautiful sunset over the Mediterranean Sea. The meal wasn't bad either. The highlight perhaps, was the cake; it was in the shape of a longitudinal cross-section of the Aleph detector, although in need of some realignment from the alignment team! After a quick vote, the collaboration decided that the cake should in fact be eaten, in spite of the reprieve in the detector's existence. Dieter, the closest we had to god's representative on earth, proceeded with the first slice and the subsequent symbolic sharing of the cake at what was potentially Aleph's last supper. Other notable events included 'the drawing of the Aleph symbol' competition, the first prize being the honour of refereeing the Higgs paper and a naked midnight swim of a couple of persons who shall remain nameless (negatives available on demand!).

After such an evening, the next morning's proceedings started a little later than normal! There were two theory talks; the first by Thomas Schucker, 'Higgs mass from non commutative geometry', which predicted the Higgs mass to be around 186 GeV/ c^2 and therefore not in agreement with the Aleph effect! His talk in fact started with a picture of an out of focus naked lady, rather appropriate given the amount of alcohol consumed the night before. The second talk from Pierre Binetruy, 'What we might learn from LEP', took the other point of view and discussed the physics consequences of a Higgs with mass around 115 GeV/ c^2 .

So all in all, the Aix-en-Provence Aleph Week turned out to be a lot more exciting than the organizers had imagined a few months before, when planning what was then thought to be the last-ever external Aleph Week. In the end, things went fairly smoothly, despite the petrol shortage which had threatened to interfere just the week before, and despite the 'cyclone' which had ravaged Marseilles during the week. It was perhaps appropriate that this Aleph Week took place in a church, as most discussions in and out of the meetings seemed to revolve around the so-called 'god' particle. Frustratingly, we will have to wait for two years to see whether we were worshipping the true messiah or just a false prophet.

LEUKERBAD 'O1 Fabiola Gianotti

'Water is the blood of the planet,' stated Leonardo Da Vinci some time ago. Armed with this statement by this famous Italian (Italian, not French...) scientist and artist, those brave Aleph collaborators who survived the diaspora to the Tevatron and LHC experiments (not to mention astroparticles...) gathered together in Leukerbad (Switzerland) on 1 to 5 October 2001, for the last Aleph Week outside CERN.

Leukerbad is a lovely village in the Swiss Alps of Valais, with spectacular mountains, pretty wooden houses, wonderful trees and meadows, flowers everywhere, and beautiful colours. In addition, Leukerbad offers a unique peculiarity compared to other resorts of this kind: it is the largest thermal centre in Switzerland. About 3.9 million litres of thermal water flow out of its sources every day, say the travel agency brochures.

The only unpleasant feature of the place is, with all due respect to our Swiss friends, its name! You can choose between the German version (Leukerbad) and the French version (Loèche-les Bains). Both are equally difficult to pronounce and... ugly.

The German version is selected here as the default.

There are three reasons why a human being chooses to spend a week in Leukerbad:

- s/he likes the mountains,
- s/he likes thermal baths,
- s/he likes both.

The only exceptions are human beings who are also members of Aleph, since they selected that place to... work! And indeed, in spite of the attracting power of the spas and swimming pools, the meetings were very well attended, thereby testifying to the unfailing seriousness and conscientiousness of our collaboration... even after ten years of hard, dedicated work.

Several presentations and lively discussions took place during the meetings. In general, the spirit was serious but relaxed. The lovely environment, the warm weather and the pleasant atmosphere resulted in friendly and peaceful discussions. The only exception was at the W session, which was quite animated... as usual! W people take it too seriously!

The excursions consisted of a walk up to one of the numerous mountain peaks surrounding the village. The idea was to walk all together, singing, chatting and laughing like on a school trip... but a few brave and fit collaborators climbed the mountain in only half an hour. The result is shown in the photo opposite.

The social dinner was gorgeous. In particular, the writer appreciated (and still remembers) the superb display of desserts and sweeties. She tried ALL of them! By the way, since then she is still on a diet... (Editor's note-JL: My vivid recollection of the Leukerbad meeting is lying in the wonderfully warm outside pool of my hotel, with many other Aleph colleagues, at just after 07.00 in the morning and watching the rising sun paint the surrounding mountains a beautiful shade of pink.)



One of the brave and fit collaborators after climbing up the mountain in half an hour (instead of the canonical two hours).

Some Physics Happenings



Is there anything beyond the Standard Model?

THE FIRST Z EVENT

Dieter Schlatter

Pilot run in August CERN bulletin MONDAY 21 AUGUST

Z⁰ marks on the spot

Late on the night of Sunday 13 August, just one month after first beam circulated and a mere 16 minutes after the start of the pilot run, LEP's first Z^0 was recorded. By midnight a total of three had been observed, and on Monday there followed 13 more—a remarkable total of 15 between the four detectors ALEPH, DELPHI, OPAL and L3 in the first 24 hours of operation.

ALEPH

Recorded its first Z^0 event with TPC readout on Monday after long hours of waiting! In hindsight the first Z^0 event we triggered was a calorimeter only event on Sunday (TPC high voltage was not on)

First Physics run, September to 31 October

150 nb⁻¹ were recorded by ALEPH corresponding to 1290 hadronic Z^0 decays. The maximum luminosity delivered was $1.5 \times 10^{29} \text{ s}^{-1} \text{ cm}^{-2}$.

First Physics Papers

Nine papers were submitted to journals during this year, the first one being *Determination of the number of light neutrino species.*

in Physics Letters B 231 (1989) 519

THE FIRST Z EVENT



ALEPH.

FIRST RESULT

At a special seminar at CERN the four experiments reported first results from the August to September run of LEP. The Aleph spokesman Jack Steinberger presented the results for the collaboration. The highlight was the measurement of the number of light neutrino families:

 $N_v = 3.27 \pm 0.30.$

The dominant error was the luminosity uncertainty which we had determined to be 2%. This was possible due to the detailed pad structure of the LCAL and a clever analysis reducing the dependence on the relative beam position.

Delphi and Opal reported a 5% systematic error and L3 none.

In 1999 using all LEP data, the combined result of all four experiments is $N_v = 2.99 \pm 0.01$ with a luminosity error of 0.1%!

The first Z^0 mass measurement was $91.17 \pm 0.07 \text{ GeV}/c^2$.

The SLC experiment at SLAC had announced similar results a few days before, but with about a factor two larger errors.



Resonance of the Z from measurements of the hadronic cross-section.

1989 CHRISTMAS CARD



After many years of preparation, this autumn saw the first successful operation of the LEP machine and the Aleph detector.

FIRST ALEPH EXPERIMENT

Aleph is one of the four particle detectors installed in the LEP collider at CERN for which the 27 km circumference tunnel extends below the Pays de Gex. The first experiment which we carried out using Aleph was a measurement of the number of different kinds of light neutrino. The result of this measurement, which is now well known, was awaited with great interest by the physics community in 1989, before the start up of LEP.

Three kinds of neutrino are known, associated respectively with the electron, muon and tau leptons and it is possible that others might be too heavy to be detected by the means presently available to us. That the total number of light neutrino species is three was, however, only a supposition and *the idea of counting the lepton families* is particularly attractive.

The problem, before LEP, was how to count the neutrinos since they are not produced in strong or electromagnetic interactions and interact only through the weak force. The Z, the intermediate boson of the weak interaction, the equivalent of the photon in electromagnetism, gives us the first experimental possibility to do so. The Z is produced abundantly at LEP, where it appears as an enormous resonance in electron-positron interactions at a total energy of 91 GeV. It disintegrates into a fermion-antifermion pair with a similar probability for all types of fermions of mass less than 45 GeV/ c^2 . The decay $Z \rightarrow v\overline{v}$ is invisible since the neutrinos interact so weakly. Such decays can, however, be detected indirectly: the more frequent the neutrino decays the less frequent will

be the decay of the Z in the detectable channels to leptons and quarks of the three families. The quark decays are particularly abundant and easy to detect for, although they are not seen directly, they produce jets of other particles, the hadrons, in spectacular events *(see Hans Drevermann's story on 'Dali' above).*

The presence of a fourth type of neutrino would be revealed by a reduction in the number of events of the type $Z \rightarrow$ hadrons, as can be seen in predictions shown in the figure at the end of this story. The elementary particles which constitute matter, the fermions of spin $\frac{1}{2}$, are apparently organized in families. The matter, which surrounds us, is composed of u and d quarks and electrons (e). The u and d quarks combine to form protons (uud) and neutrons (udd), which in turn make up atomic nuclei. With the electron is associated the electron neutrino, v_e which is produced for example in radioactive β decay.

The second family contains the 'strange' and 'charmed' quarks and the μ lepton and its neutrino ν_{μ} . This second family was completed experimentally in 1974 by the discovery of charm. Its members have properties very similar to their equivalents in the first family, except that these particles are unstable and of higher mass. The discovery of charm, completing the second family, was a very important step, resolving a whole series of experimental problems and demonstrating the sound foundation of the Standard Model, the superb theoretical structure which now accounts for the physics of the known particles.

The third family made a somewhat unexpected appearance in 1975 with the discovery of the τ lepton, the comrade of the μ and the electron. The τ decays in such a way that no one can doubt the existence of its neutrino v_{τ} . The b quark, b for beauty or bottom, partner of the d and the s, was discovered in 1977.

The top, t, (big brother of the b and counterpart of the u and the c) turns out to be very heavy with a mass of about $175 \text{ GeV}/c^2$ and was discovered at Fermi Laboratory in 1994 (the mass having been predicted indirectly by the LEP electroweak fits). This third family is very useful in explaining elementary particles' lack of respect for invariance under time reversal. However, although we have a very complete and predictive theory, nobody could yet explain why there are three families, nor exclude the possibility that there could be a fourth, or more... One notes that in each family there is a neutrino. Although the masses of the leptons and the quarks increase from one family to the next, that of the three known neutrinos has been measured to be very small. Hence the expectation that, if there is a hidden fourth family, its neutrino would probably be massless or at least very light. This would affect the size and shape of the observed Z resonance as shown in the figure. This idea had already been proposed in 1976 by John Ellis, who was frightened by the idea that a thousand new neutrinos would render the Z unobservable... The idea was taken up again in 1987 by Gary Feldman in a quantitative fashion.

In fact it is sufficient to measure the counting rate $e^- + e^+ \rightarrow hadrons$ at the peak of the resonance. When I presented this idea to the Aleph collaboration in 1987 it was received with a certain scepticism since the precise measurement of an absolute counting rate is always a delicate business. To start up a great system like Aleph: 3000 tonnes, 550 000 electronic channels, 360 collaborators at 32 laboratories and universities, seemed already a very difficult task, but to attempt to measure an absolute counting rate in less than a week of running seemed pure folly. However, the collaboration set itself to achieve this goal, which demanded very careful preparation. Did we know how to measure the collision rate $e^- + e^+ \rightarrow e^- + e^+$ which gives the normalization? (The Z does not contribute to this reaction at small scattering angles.) Could we be sure not to lose events in the data transmission?

At six o'clock in the evening on Sunday 2 October 1989, one week after start up, the small group charged with extracting this result achieved their goal. After a detailed revue of the data quality, which was excellent, we added up the total of events collected over eight days. Ed Blucher and John Harton independently counted the hadrons; Fred Bird and Peter Hansen the e^-e^+ . Two columns of figures, two additions, one division... the cross-section was 30 nanobarns (1 nanobarn = 10^{-33} cm²). 'My friends it seems to me that that makes three neutrinos' confirmed Lluis Garrido, the expert on theoretical calculations. 'You should now write the paper' Monica Pepe said to me. It was a unique moment for those who were there.

We had certainly no idea what the other collaborations were doing. Nothing filtered out during a week and a half, the time when the first period of data taking terminated. On Friday 13 October the four collaborations presented their results in the big CERN amphitheatre:

Aleph:	N = 3.27 + / -0.30
Delphi:	N = 2.40 + -0.64
Opal:	N = $3.12 + -0.42$
L3:	N = 3.42 +/- 0.48

giving a mean of 3.17 ± -0.20 . Since then the experiments have continued, accumulating hundreds of thousands of Z decays, establishing with a value for N = 2.99 ± -0.05 , that there are indeed three kinds of light neutrino and thus probably only three families of fermions.

(The above is a modified version of a paper first presented at a plenary session of the Académie des Sciences, Arts et Belles Lettres de Lyon on 17 December 1991, on the occasion of the presentation of the Fondation Thibaud prize.)



The resonance of the Z from measurement of the cross-section of hadronic events. The theoretical prediction for three or four families of neutrinos is compared with the measurements. a) In 1988 before data taking. b) In 1989 after three weeks of data taking and 3000 Zs. c) After two years and 200 000 Zs.
1989

FIRST EXPERIMENT

(The other side...)

Francesco Fidecaro/Fabrizio Palla/Monica Pepe-Altarelli

Two teams were very actively involved in the counting of the hadrons, using two very different techniques *(see preceding story, written by Alain).* Here are some personal recollections on the 'calorimetric analysis' and on those exciting days of 1989.

The precision on the N_v measurement was dominated on one hand by the hadronic event statistics and on the other hand by the luminosity measurement integrating the Bhabha cross-section over the acceptance region. This precision was such that, after a few weeks, one LEP experiment alone could already provide a significant measurement for the number of light neutrino species!

Assuming that LEP behaved as expected, the remaining problem was the Aleph detector performance. One unknown was the novel tracking technique based on the large TPC: nobody could predict whether it would perform from the start as expected or how nasty the bremsstrahlung photons from the machine would be. Jack, despite being the TPC main promoter, felt more comfortable with a selection of hadronic Z decays based on the energy deposited in the hadronic and electromagnetic calorimeters.

So, while a group led by Alain was pursuing the idea of counting hadrons using the TPC, we (Martine Bosman for the trigger, Sylvie Dugeay, Francesco Fidecaro, Marie-Noëlle Minard, Fabrizio Palla and Monica Pepe for the hadronic event selection) enthusiastically and candidly joined Jack in this enterprise, not realizing the fierce competition that would soon follow... So, the authors of this story had to put up with a rivalry that they had neither created nor anticipated, which, however, turned out to be a rather healthy psychological incentive ('Shit! We can't give up now!), later on, when things started to become difficult, with the TPC working like a dream from the very beginning, while the HCAL... ehm... ehm...

The first problem to attack was the trigger design. The idea was to use the ECAL wire information. The trigger was based on the requirement of having either at least 6 GeV in the barrel or a coincidence of the two end-caps with an energy of at least 3.5 GeV. The design was such as to guard against background while maintaining the trigger efficiency above 99% over the entire solid angle. The trigger had a relatively large acceptance also with respect to tau pair events.

From April to July 1989 the entire effort was to develop a hadronic event selection program, which did not use any tracking information. The task was complicated by the fact that the analysis program ALPHA was not yet fully developed. Therefore, a lot of data unpacking had to be performed from scratch. Moreover, the cleaning of the calorimetric information in the online processing was far from optimal. A lot of noise hits had to be suppressed inside the code, and some remained.

The selection of hadronic Z decays was easy: a minimum amount of total energy was enough to select more than 95% of the events. Two-photon interactions were the main background. Those

events, however, were such that the centre of mass of the two photons was not at rest, resulting in events boosted along the beam line, while the Z ones were symmetrically distributed. A second concern was to get rid of e⁺e⁻ events. Given that their energy was entirely deposited in the ECAL, they were easy to reject, except that when one or two electrons ended in an ECAL crack, their energy was deposited in the HCAL behind. A careful treatment was developed and a minimal amount of events survived. The problem was complicated by the fact that the HCAL end-caps were not fully functional at the beginning, to say the least. Muon pairs were recognised by their minimum-energy deposition in the ECAL and a distinct pattern in the HCAL digital readout. Background from cosmic rays was reduced by requiring the energy deposited in ECAL to be in time with the LEP bunch crossing. The most intriguing class of events were tau pairs. Those events were originally rejected by identifying isolated muons or electrons, or low multiplicity energy deposits, but a non negligible fraction remained.

The writing of the initial code was done from April to July 1989. (It was at that time that Fabrizio, a very hard working student with a modest salary, was defined by Jack as being the Aleph physicist with the highest 'scientific output over cost ratio'.) In fact the first version of the code was ready on 14 July 1989, an inspiring date celebrated with fireworks, and was named REVOLUTI.FORTRAN because IBM only supported 8-character names. The program was successfully tested on the first LEP data in the August pilot-run.

One big problem was the amount of CPU needed to run the program. In fact it required to pass through all collected triggers, unpack the BOS bank information, perform the analysis and produce histograms and statistics for all runs. In order to speed the execution we used a Cray account. Despite the tenfold increase with respect to the IBM3060 Main Frame, the data still came too fast. Two of us took night shifts to run the event selection program as soon as the PASS0 data came out. While we were spitting blood on the calorimetric data, unpacking, cleaning and recleaning noise hits, processing, reprocessing and re-reprocessing, Ed Blucher had the intuition of counting hadrons in a remarkably simple way, based on a couple of lines of code, selecting events having a charged track multiplicity greater or equal to five...

Some events, which passed the selection, had incredibly high energies, and did not match the centre-of-mass energy of the LEP machine. Those events were mainly cosmic rays and a memorable one, which had impressed us during the night of 20 September and we had left on Jack's door with a 'Buon giorno' note, is shown below.

For the first measurement, all events passing the selection were visually scanned by Francesco. He found that some of them underwent a so called 'trigger mismatch': if one sub-detector was flooded by hits, the readout processors could only deliver part of the information, the remainder being attributed to the next event, thus producing two complementary 'half-events'.

The real key of the measurement was to have the most precise determination of the luminosity, which the Copenhagen group, together with Fred Bird, did remarkably well. The main uncertainty was due to the error on the acceptance area of scattered electrons in Bhabha scattering, because of the dependence of the Bhabha cross-section on the fourth power of the scattering angle. To determine this angle one had to measure the contours of the fiducial area where electrons and positrons impinged (LCAL), relative to the LEP interaction point, which was not stable along the beam line. A trick was invented to circumvent the problem, by defining an asymmetric geometrical acceptance (loose in one calorimeter and tight in the other) and alternating the loose and tight assignments on an event-by-event basis. This allowed mitigating the steep dependence and the luminosity could be measured to better than 1%-not bad for an absolute cross-section measurement-the only limiting factor being the theory.

By Sunday 2 October 1989, both groups had a measurement of the hadronic cross-section, thus concluding an incredibly intense period that will always remain vivid in our memory and hearts. The two results were perfectly consistent, giving the collaboration confidence in the reliability of the result. Being systematically independent, the results could also be combined (and that combination put a happy end to the fights). It definitely looked like three neutrinos...





A cosmic ray shower of parallel muons in ALEPH.

FIRST PRECISE R_b MEASUREMENT

Dave Brown

After the Aleph vertex detector (VDET) was installed in 1991, it took about one year to align it and perfect the reconstruction software. As ours was the first double-sided vertex detector, we had to invent many techniques and tools that later became standard for this kind of detector. Everyone understood that the VDET would make an enormous difference in lifetime measurements, but its use in electroweak physics was uncertain. The idea of using displaced vertices as a tag for B mesons in Z decays was discussed in the CERN yellow book, but it was predicted to be only a marginal improvement over the standard leptonbased tags. Our experience and success with the VDET alignment however suggested to us that a much more powerful tag was possible.

The lifetime tag used in the first precise R_b measurement resulted from putting together several new ideas. First, we realized that we did not need to explicitly reconstruct displaced vertices from B decays in order to detect them. By combining the significance of individual track 's' separation from the primary vertex in a probabilistically correct way, we were able to extract the full discriminating power of the B-lifetime directly. To reconstruct the primary vertex, we exploited the fact that Bmesons from Z decays appeared in jets, which gave an accurate estimate of the B-meson direction. By vertexing tracks in the plane perpendicular to this jet, we were able to reconstruct an accurate primary vertex position unbiased by the decay length of the B-mesons. The resulting tag was an order of magnitude more powerful than existing tags, capable of reaching purities in the upper 90% with efficiencies on the order of 50%.

The traditional method of measuring R_b up to this time was to count the number of tagged $Z \rightarrow b\overline{b}$ events, and divide by a tag efficiency determined from Monte Carlo. The high purity and efficiency of the lifetime tag made this inefficient, as the systematic errors from the Monte Carlo would far exceed the statistical errors. Instead, we pioneered the *double-tag* technique, where each of the two B-mesons produced in the Z decay was tagged separately. This method extracts both R_b and the tag efficiency from the data itself, with only a secondorder dependence on Monte Carlo modelling.

Treating the residual errors from the doubletag analysis turned out to be a challenging task, requiring nearly a year to understand and complete. One difficulty was Z decays into charm mesons, where the lifetime of the charm could mimic B mesons, distorting both the R_b and the tag efficiency. Another problem came from correlations between the tagging of the two Bmesons due to secondary physics or detector effects. Here we invented new ways of modelling correlations using data, greatly reducing the systematic error from this source. Finally, we had to worry about backgrounds from gluons splitting into two B-mesons, which could fake the $Z \rightarrow b\overline{b}$ signal. Fortunately, theoretical estimates showed this background to be negligible.

The first presentation of the first precise R_b measurement was made at Moriond in 1993, where Aleph presented the value 0.2183 ± 0.0022 (stat) ± 0.0035 (syst). The total error on this single measurement was less than half that of the previous world average, and the value differed from the Standard Model prediction by roughly 2 σ . This result generated a lot of interest from theorists both because of the Standard Model discrepancy, but also because it was perceived as

being a very reliable result. It set the standard for future R_b measurements from Aleph and other detectors, and firmly established double-sided silicon detectors as an essential part of general-purpose HEP detectors. Making this measurement was one of the most fun experiences I have had in HEP, bringing together brilliant people, novel hardware, innovative software, and great physics.

RESOLVING 'THE R_b CRISIS' (The five-tags) Fabrizio Palla

Aleph's first R_b measurement, using a lifetime tag inspired other experiments at LEP to follow suit. The first Aleph published result on R_b , mainly the work of Dave Brown, gave (Ref. 1):

 $R_{b} = 0.2192 + -0.0022(stat) + -0.0026(syst)$

This was quickly followed by two independent analyses, one by the Clermont-Ferrand group, the other by Elizabeth Martin, which gave the combined result (Ref. 2):

 $R_b = 0.228 + -0.005(stat) + -0.005(syst)$

In consequence, by 1996 the world average measurement of R_b had rather good precision. Its value caused great excitement, since it lay more than three standard deviations above the Standard Model prediction. This discrepancy was christened *'The* R_b *Crisis'* and provoked a wealth of theoretical papers, which sought to explain it in terms of new physics.

In this context, it was vital that Aleph should improve on its first lifetime tag R_b measurement. To launch this effort, Dave Brown chaired a meeting shortly before he left Aleph, at which he passed on his experience from the first measurement and stimulated discussion on how we might improve it. Initially, the prospects looked bleak. Whilst there was no doubt that the new data would improve the statistical precision, the 'old' measurement was largely dominated by systematic errors, which appeared largely irreducible. Nonetheless, to address this challenge, a group formed, consisting of Duccio Abbaneo, Andrew Bazarko, Lorenzo Moneta, Fabrizzio Palla, Anna Stacey, Jack Steinberger and Ian Tomalin.

In order to assess a real discrepancy with respect to theory, scrutiny of the measurements was necessary. In particular, to prove the existence of new physics, the measurement had to reach an accuracy of half a per cent and also the statistical error had to be reduced.

The method used in fact was referred to in the past as 'double tag', which is based on the fact that b-hadrons are produced in pairs. From the measurement of the number of singly and doubly b-tagged hemispheres, one can directly measure the b-tag efficiency and R_b provided the hemisphere tag correlation and the lighter quarks' efficiencies are correctly predicted by simulation. Of course uncertainties on these assumptions led to systematic errors.

Besides, with double the statistics, coming from the new data collected since the first measurement, three main improvements were achieved:

First signs of a breakthrough came when we developed a higher purity (99%) b-tag, with the aim of driving down systematic errors associated with the charm and light quark backgrounds. It was based on a combination of the original lifetime tag and a new mass tag. The mass tag was worked on by calculating the invariant mass of all those tracks in a jet, which were incompatible with the primary vertex. The jet (and its associated event hemisphere) were accepted if this mass exceeded the mass of a typical weakly decaying charmed hadron. (A similar tag was developed independently by SLD, where the superior detector resolution made it even more effective.)

A second major breakthrough occurred as we studied the reasons why the b-tagging efficiencies in the two event-hemispheres were correlated. Two important sources of correlation were identified, which had previously been overlooked—and hence may have contributed to '*The* R_b Crisis'. Both arose as a result of the two hemispheres sharing a common reconstructed primary vertex. Since we were unable to measure the size of these sources of correlation in the data, we chose to eliminate them by reconstructing the primary vertex separately in each event hemisphere.

Finally, the last improvement came from the idea to measure, directly in the data, the efficiency to tag charm events with the lifetime-mass tag. In fact one hemisphere was tagged with a b-tag and the other with a suitable charm tag. A pure and efficient charm tag was very difficult to implement and, in fact, we also had to suppress the light quark background by means of a dedicated uds-tag which was then combined with the other two to measure the uds-tag efficiency. In the end, for detecting b events, three tags were employed: a lifetime-mass tag, an event shape tag and a lepton tag. The charm tag was based on a lifetime and event shape analysis, which also gave an efficient and pure udstag. Some of these tools were developed by several other people in Aleph.

These five tags were constructed to be mutually independent, so that, by measuring 20 independent quantities, we could determine R_b and 13 out of the 15 tag efficiencies and still have 6 checks for the analysis. A side effect of the so-called *'five-tags tag'* came from a reduced impact of the uncertainties on the Monte Carlo estimated hemisphere tag correlations.

The two final results were:

 $\label{eq:Rb} \begin{array}{ll} R_b = 0.2167 \mbox{ +/- } 0.0011(\mbox{stat}) \mbox{ +/- } 0.0013(\mbox{syst}) \\ \mbox{from the double tag (Ref. 3)} \\ \mbox{and} \end{array}$

 $R_b = 0.2159 + - 0.0009(stat) + - 0.0011(syst)$ from the five-tag method (Ref. 4)

both in agreement with the Standard Model.

The final Aleph R_b result was presented by Ian Tomalin at the 1996 ICHEP Conference in Warsaw. The new result was one of the main highlights of the conference, marking, as it did, not only a significant gain in precision over earlier results but also, more importantly, the end of *'The* R_b Crisis'.

References:

- 1. Aleph Coll., Phys. Lett. B 313 (1993) 535.
- 2. Aleph Coll., Phys. Lett. B 313 (1993) 549.
- 3. Aleph Coll., Phys. Lett. B 401 (1997) 150.
- 4. Aleph Coll., Phys. Lett. B 401 (1997) 163.

THE UPPER LIMIT ON THE ν_{τ} MASS Fabio Cerutti

It was in 1993 when the first idea to measure the mass of the tau neutrino by looking at the endpoint of the mass-energy spectra of hadronic tau decays started to circulate in Aleph. At that time a large effort was put into the measurement of the tau hadronic branching ratios. The key tools for this measurement were the π^0 and the charged track reconstruction in the very crowded environment of the tau jet at LEP (typical angular aperture ≈ 2 degrees) and the particle identification ($\pi/e/$ µ separation). The tools, which were developed in Aleph, allowed for a very pure and efficient selection of the five-prong tau decays. The fiveprong tau decays are particularly well suited to measure the tau neutrino mass since their invariant mass tends to be close to the kinematic end-point of the spectrum.

At that time best limits on the tau neutrino mass came from the $\Upsilon(4s)$ experiments (ARGUS and CLEO) and were based on the study of the five-prong mass spectrum end-point. The 95% CL upper limit on the tau neutrino mass was of 31 MeV/ c^2 from ARGUS obtained on a sample of 25 five-prong tau decays.

The Aleph experiment had *a priori* a much lower number of produced taus with respect to these lower energy experiments. In 1993 Aleph had collected a luminosity of $\approx 65 \text{ pb}^{-1}$ corresponding to about 76 k Z $\rightarrow \tau\tau$ decays. The idea was to overcome the lower statistics thanks to these three advantages:

- The non-tau rejection (mainly qq) was much easier at LEP than at the $\Upsilon(4s)$.
- The rejection of the most 'dangerous background' (the tau background which could mimic a massless neutrino) was reduced to a negligible level thanks to the very good granularity of the Aleph electromagnetic calorimeter and of the Aleph tracking detector.
- The use of a new method based on a simultaneous fit of the invariant mass and of the total energy of the hadronic tau decay products (referred to as the '2D-method') was expected to substantially improve the sensitivity to the tau neutrino mass.

The first channel investigated by Aleph was the decay $\tau \rightarrow 5\pi^{\pm}\nu$. Aleph was the LEP experiment with the best performance for the selection of this decay final state. On this data sample we selected 23(3) $5\pi^{\pm}(\pi^{0})$ tau decays with very low background from lower multiplicity tau decays. Based on these few events by using the '2D-method' a 95% CL upper limit of 23.8 MeV/ c^{2} on the tau neutrino mass was obtained. This limit was presented in September 1994 at the TAU workshop in Montreux and published in 1995.

The reaction from the CLEO members was quiet but after long scientific discussions and more detailed information exchanges they were convinced that our result was robust. As a consequence of this interaction they also adopted our '2D-method' in their analysis.

In the following years we realized that the threeprong channel was as promising as the five-prong one. In fact the presence of the a_1 resonance at 1.2 GeV/ c^2 which depressed the event population at large mass was compensated by the much larger branching ratio. In addition to that the use of the '2D-method' gave a non negligible sensitivity to three-prong tau decays with very high total energy and moderately high invariant mass. The combination of the three- and five-prong channels gave a 95% CL upper limit on the tau neutrino mass of 18.2 MeV/ c^2 which is up to now the best limit on this fundamental quantity. This result was published by Aleph in 1997.

Of the other LEP experiments, Opal has measured an upper limit on the tau neutrino mass of 27.6 MeV/ c^2 and Delphi of 25.0 MeV/ c^2 . The CLEO experiment produced new results based on much larger statistics (about 266 five-prong tau decays and about 207 $3\pi^+2\pi^0$) but they didn't improve the Aleph limit. The main reason was that their likelihood had a broad maximum (less than 2σ significant) for a neutrino mass of around 20 MeV/ c^2 which made their actual limit worse than the expected one. So in spite of the smaller statistics our limit is still unbeaten. The B-factories should have a much better sensitivity to the tau neutrino mass. This very competitive Aleph result can be seen as the concomitance of three positive elements: the introduction of the new '2D-method', the characteristics of the Aleph detector (very well suited to measure hadronic tau decays), and the intensive work accomplished in the tau branching ratio working group. To give an idea of the difference in performance with respect to the other experiments we can quote that the Aleph selection efficiency on a very high purity $5\pi^{\pm}$ sample was 27% to be compared with the 9% of Opal and the 3% of CLEO. On this sample the invariant mass resolution obtained by Aleph was about 10 MeV/ c^2 to be compared with the 20 MeV/ c^2 obtained by Opal.

SEARCHES AT LEP1

Mike Green

LEP opened a huge new volume of the multidimensional space in which searches for new particles can be made and Aleph physicists were not slow off the launch pad. Of the 18 physics papers with a publication date of 1989 or 1990, 12 had the word 'search' in their title and 6 the word 'Higgs'.

Indeed there seemed every opportunity for the publication rate to get out of hand. Twice-yearly updates of some analyses were occurring as data arrived at an ever-increasing rate, when Jacques Lefrançois proposed a solution (as he often did). During the unforgettable Frascati meeting in September 1990 he suggested that Aleph should publish a review paper in Physics Reports, bringing together all our results from searches using data taken to the end of the year. Moreover it should provide details of analysis procedures, many of them common to several searches.

The rationale seemed impeccable but could we get twenty or more physicists to collaborate on such an undertaking? Probably it was our respect for Jacques' judgement that persuaded us to give it a go. I was appointed editor-in-chief and soon established that Physics Reports would indeed be interested to receive such a paper and would pay me \$1,000 on publication! The project was expected to take about four months and deadlines were set. It needed a name and Jean-François Grivaz proposed The Grand Unified Paper, known ever since as The GUP. I now had a second question—what could I do with the money?

There's a long and a short answer to the first question. The short one is that it was a close call. As we might have anticipated some sections were soon ready but others were interminably slow in arriving. Our 'common analysis procedures' turned out not to be so common after all and several had to be modified in detail. To mix metaphors, the early birds chafed at the bit and began to demand that their contributions be published separately. Meanwhile I had obtained a six-month sabbatical at CERN for the first half of 1991, originally intended to be devoted to analysis but ultimately spent encouraging laggards and sub-editing to obtain a common style. Almost every figure was produced using a different graphics system and it was a nightmare getting them all into PostScript format. I suspect that even now some contributors don't know that I redrew some of their figures.

Finally it was complete. On a sunny morning in August 1991, the day I had set for driving back to England, I delivered a final version to the PPE secretariat to be made into a preprint (I can still remember the number—CERN PPE/91-159) and committed a copy to Physics Reports. At 11.30 I left CERN with much relief. The relief was a mistake, of course. After many weeks without response from the editor I enquired what was going on and discovered it had never arrived (or, more likely, got lost in the office during summer vacations). Another copy was duly sent in October and was finally published in August 1992 nearly two years after conception. The cheque duly arrived and the only sensible way to spend it seemed to be on a party! My diary shows that this was on 5 May 1993, consistent with the speed with which the paper had progressed. The whole of Aleph was naturally invited but even then there was wine left for the next party. Never again, I said to myself (and others). It therefore wasn't until 1995 that it was proposed that I edit another review paper, this one on searches for evidence of compositeness from LEP1. This time it took three years from conception to publication. Why the QCD group also embarked on a similar venture I never have understood.

THE QCD MEGAPAPER

Glen Cowan

By 1993 or so the Aleph QCD group had produced quite a few very nice measurements, including α_s , inclusive rates of various identified hadrons, and so forth. In addition there were ongoing analyses on scaling violations, properties of quark and gluon jets and there were several studies on QCD coherence and subject structure that I had worked on but not finished.

At some point around late summer or autumn of 1993, it was suggested, I'm pretty sure by Dieter Schlatter, that the QCD group should write a review paper. The idea would be, however, not just to summarize existing Aleph measurements but to include a lot of these results that had been lying around unpublished. Ramon Miquel, Michael Schmelling and I were supposed to organize the paper. We realized at the time that this would be a large project, and it was almost immediately named the 'QCD megapaper'. I'm certain, however, that we didn't realize it would take more than four years before it finally emerged as 'Studies of Quantum Chromodynamics with the Aleph Detector', Physics Reports 294 (1998) 1. (Editor's note-RS: It's interesting to see what Mike Green thought about this undertaking in his story above that precedes this one.)

In several instances we had to decide whether a certain result should be published separately or be made a section of the burgeoning megapaper. Of course the authors of individual analyses preferred a separate publication, and there were heated discussions at times as to whether the megapaper was simply the waste bin for failed analyses. The core authors of course felt that the megapaper was

indeed a high-quality product, but it is doubtless true that an analysis could easily get overlooked if it only appeared in the middle of a huge review.

After a year or two it became clear that the megapaper would benefit from more people's help, and several additional editors came into the project, including Heinz-Georg Sander, and Robert Turnbull. Ron Settles of course played an important role as head of the QCD group and eventually there was also a small army of internal referees that included Alain Blondel, Andy Halley, Marcello Maggi and Gerald Rudolph. In the end it was Ramon who got us all organized.

None of us was terribly quick at getting our contributions done on time, although Michael was probably better than most and I was certainly worse. Of the 74 emails I have archived with the word megapaper in the subject line, a large fraction are from Ramon exhorting me to meet this or that deadline. (In proof that some things never change, Ramon still has to cajole me into meeting deadlines for the Particle Data Group.) Michael Schmelling left CERN to go to Heidelberg in late 1995, before the final push was made in 1996 to get the thing finally out the door.

We had of course many meetings where we'd try to go through the whole paper, but we always became exhausted before getting to the end. I don't think we ever discussed the latter third of the paper as critically as the rest. The big final meeting took place over dinner in Clermont-Ferrand during the Aleph Week in October 1996, and the editorial board met in several sessions in mid November. Steve Wasserbaech read the entire thing over for typographic (and physics) content, and I have many pages of comments from him of the form 'No hyphen in 'with spin 1/2". On 13 December 1996, the megapaper became CERN-PPE/96-186 and was submitted to Physics Reports.

Getting the paper accepted went smoothly enough; there was not a single change requested by the referees. Having it printed by Elsevier, however, was another matter. Despite the enormous job that everyone, especially Michael and Ramon, had put in typesetting the paper in Latex, it turned out that the printers wanted to type the whole thing in again. It was claimed this would be fast and efficient, but in fact the paper didn't actually appear until 1998. We were paid 1000 guilders by Elsevier, which I would not care to convert into an hourly wage. The money was contributed to one of Aleph's barbecues at Echenevex. In the end we were pretty happy with what came out and I still refer to the megapaper quite often. It was too bad that one of our nicest QCD results, the α_s and colour factor analysis by Günther Dissertori and Michael Schmelling, came along too late to be included, but that's a detail. If I had it to do over again, I think it might have been better to wait to the end of the LEP1 era and to summarize the whole programme at that time. If we had tried that, however, it's not clear that anything would have been produced, since by that time many people were either concentrating on LEP2 or going off onto other projects. And it certainly was nice to get quite a few preliminary results finished up and published, as Dieter had in mind from the beginning.

BEAUTIFUL OSCILLATIONS Roger Forty

It is easy to forget how little was known about B physics before LEP. The average b-hadron lifetime was one of the few parameters thought to be well known at that time, close to 1 ps, but it turned out to be wrong: the true value is now known to be about 50% higher, thanks to the precise measurements at LEP. Another parameter that had recently been measured for the first time was the mixing of B⁰ mesons. ARGUS had determined in 1987 that the probability for a B⁰ to decay as its antiparticle was about 17%, much higher than had



Figure 1: Lepton-D* charge asymmetry versus decay length.

previously been expected: the top mass comes into the calculation, and at that time was thought to be much lighter than is now known to be the case. This phenomenon is known as mixing, since an initially pure sample of B^0 mesons ends up as a mixture of B^0 and B^0 . It is due to the quantum-mechanical oscillation of the B^0 state between its particle and antiparticle, familiar from the K⁰ system. However, the time dependence of this sinusoidal oscillation had never been seen for the B^0 .

A group in Aleph, led by Hans-Günther Moser, set out to search for it. The technique used was to partially reconstruct the B⁰ from its decay to D* mesons, which decay in turn to a clearly recognizable combination of a kaon and two pions. Using the recently installed silicon vertex detector, the decay point of those D* mesons could be precisely measured, and their charge indicates whether they originated from a B⁰ or B^0 . The production state of the B^0 was determined by searching for a lepton from the semileptonic decay of the accompanying b hadron produced in the initial decay of the Z. Sure enough, when the plot of the lepton-D* charge asymmetry versus decay distance was examined, the oscillations were clearly visible (Figure 1). This analysis allowed the first direct measurement to be made of the B^0 oscillation frequency, which can be directly related to the mixing probability (when the lifetime is known): the value was in triumphant agreement with the earlier results from mixing measurements,

and was published in 1993. A host of copycat measurements were spawned amongst the other LEP experiments, and the B^0 oscillation frequency is now measured with an astonishing precision of 3% (from a total of 26 separate analyses), far more precisely than theory can predict.

Meanwhile, I had been working with colleagues on an alternative analysis, using dileptons. Here a lepton comes from the b hadron decay on each side of the event, a technique used for earlier mixing measurements. The new feature was to measure the proper time of the decay, using a topological vertexing technique. Such a dilepton sample includes not only B⁰, but also B_s mesons, which are also expected to oscillate, but with a much higher frequency than the B⁰. This complication turned out to be the greatest asset for the analysis: we realized that a simultaneous fit could be made for both oscillation frequencies, under the assumption that the higher frequency was that of the B_s. Clear evidence was seen again for the B⁰ oscillation, but no higher frequency component could be resolved, which allowed the first direct limit to be set on the B_s oscillation frequency, at 1.8 ps⁻¹. Little did we realize that this was the first shot in an arms race that has continued to this day, with successively higher limits being set, the latest having a sensitivity 10 times higher than that original result. Aleph can be proud of having led the way, always providing the best limit in the world.

The first step taken to improve the pioneering analyses was to upgrade the production-state tagging, by looking not only at leptons, but also

jet charge. This was pursued by Witold Kozanecki and collaborators for the B^0 frequency measurement, and was applied by the Wisconsin group to the dilepton-style analysis to improve the B_s limit. This led to a dramatic improvement in the limit, to 6 ps⁻¹, by the time of the Summer Conference at Glasgow in 1994. I was reviewing oscillations at the conference, and the excitement of the last-minute finalization of the new result stays with me still. The

big problem at that time was the interpretation of the likelihood from the fit to the data, in terms of a limit. There were heated arguments over whether the likelihood should be referred to 'infinity' (which corresponds to comparing with a fit with no oscillation), or 'the minimum' (i.e. the best fit value, as is done in most traditional measurements). Fundamentalists, myself included, insisted that 1.92 units of log-likelihood relative to the minimum should correspond to a 95% CL limit, but this turned out to be not quite right: the complex nature of the likelihood from an oscillation fit, with the possibility of multiple minima, leads to some corrections relative to the familiar Gaussian behaviour. This lack of a clear analytic prescription meant that extremely labour-intensive toy Monte Carlo techniques had to be used to set the limit contour, and I remember the Wisconsin group whipping their powerful computing facilities to within an inch of their life, to get one more point on that contour (Figure 2).

Eventually the issue was resolved by the development of a radical new approach to limit setting, the Amplitude method, which is now generally accepted by all B_s oscillation practitioners around the world. It was inspired by the Fourier analysis approach that Hans-Günther had been promoting for some time, but was refined in the crucible of application to the Aleph analyses. Its beauty, apart from freeing our computers for more productive tasks, is in allowing the simple combination of many separate analyses.



Figure 2: Log-likelihood as a function of the B_s oscillation frequency.

This has now become an industry, and at the end of LEP we are left with a situation eerily reminiscent of the Higgs saga: The current limit for B_s oscillations is lower than that expected, due to the observation of a signal-like behaviour for a frequency around 18 ps⁻¹, which is just in the most likely region for the frequency in the Standard Model (Figure 3). However, the significance of the effect is insufficient to claim that oscillations have yet been observed. Aleph (as for the Higgs) sees the strongest indication, and people are still working hard, on data that was taken more than five years ago, to squeeze out the last drop of significance.



Figure 3: Amplitude method of fitting the B_s oscillation frequency.

B_{s} AND Λ_{b} DISCOVERY

My first attempt at a search for a new particle made of the b quark was a complete failure.

I was a CLEO graduate student in 1987 when, after the discovery of BB mixing by Argus, my thesis adviser, Hassan Jawahery, proposed a special running of the Cornell electron storage ring at the $\Upsilon(5s)$ resonance in search of the B_s meson, a particle which would demonstrate furious matter-antimatter oscillation. The machine ran beautifully and I was excited to be the graduate student entrusted with the search. To cut a long and confusing story short, we saw tantalizing and characteristic signatures of $B_s \rightarrow D_s \rightarrow \phi$ but not even one signature mode was statistically compelling enough (>3 σ) to publish. Tired and disappointed, I wrote my thesis on another topic and accepted Sau Lan Wu's offer to join the Aleph experiment at the newly commissioned LEP e+ecollider. Even though the search for the B_s failed, it addicted me to searches for new physics. As the highest energy machine preparing a run near the Z resonance, LEP promised exciting opportunities for discovering new particles and interactions. After examining detector designs of the four LEP experiments, it was clear to me that the large TPC of Aleph and its granular electromagnetic calorimeter were best suited for my intuition for particle searches. Aleph, in my mind, was going to be a giant electronic bubble chamber with a 3D view. Although I had many offers, I chose to work with Sau Lan because of her can-do spirit, her vast resources, and her promise to let me choose my own physics directions.

I arrived, as a postdoc, in Geneva after Bastille Day in 1989, just in time for the first running of LEP at the Z resonance. My immediate intention was to search for new quarks and leptons of the fourth generation. Thanks to Jürgen Knobloch's leadership as software co-ordinator, learning Aleph analysis software was easy and I could get to my searches right away. Like many of my LEP colleagues, my days and nights belonged to particle searches, the adrenaline rush was too overpowering. I don't think I slept much between September 1989 and April 1990. Nevertheless, we all failed to find any sign of new physics. The number of light neutrinos was precisely three and all our reports on limits on new particles were duly archived in Physics Letters B. The $\sin^2 \theta_w$ measurements would continue till the end of LEP running at the Z resonance, but for me it was time to acknowledge failure once again and make plans for new research directions.

I knew something about b quark physics and the $Z \rightarrow b\overline{b}$ cross-section was high. So my thoughts turned again to the B_s (and Λ_b) hadron(s) which still remained to be discovered. This was not 'new' physics but the thrill of the chase was still there. At LEP these particles would never be produced in large enough quantities to be an easy signal so it was still a 'needle in a haystack' affair requiring careful search strategies. Particle searches are a personal thing. Although many Wisconsin students wanted to be a part of these searches, remembering my CLEO experience and disappointment, I worked alone since I did not know if I would fail again.

In 1990 a wonderful addition was made to the Aleph detector although it would not be obvious for another two years what a beautiful new optic the VDET, a 3D silicon wafer based detector, would provide in Z decay studies. Based on its potential on paper, I got involved in 'care and feeding' of this device built by my MPI-Munich and Pisa colleagues. I realized that if this detector worked efficiently, it could revolutionize LEP's potential for b physics and allow it to challenge CLEO's monopoly on this subject. With John Jacobsen, an undergrad from Wisconsin, we began testing VDET's capability by using a lifetime cut to enhance S/N in charm meson yields. The differences in S/N between 'without VDET' and 'with VDET' charm meson mass plots were a beautiful surprise and an Aleph highlight in summer 1990. When the full VDET was commissioned for the 1991 run, it was clear that a new direction in LEP physics was around the corner and Aleph would be in the driving seat to exploit it for physics.

EARLY EVIDENCE FOR 6 BARYONS

LEP kept roughly doubling its integrated luminosity every year but because hadrons made of the b quark decay in millions of different ways, data accumulated between 1989 and 1990 was not enough to find signatures of new hadrons made of b quarks in a conventional way. New ideas were needed. As it turns out the white wine of Frascati made this possible in what became a eureka moment for the Λ_b discovery!

In 1990, an Aleph Week was organized in the magnificent Villa Tuscolana by our Frascati colleagues. I was to report there on the discovery potential for the B_s meson with next year's data. After multiplying all the associated branching ratios and efficiencies, the numbers were all disappointingly small! I was getting desperate and set a lower threshold for the B_s evidence by searching for an inclusive $\phi \ell^+$ signature from $B_s^0 \rightarrow D_s^{(-)-} \ell^+ \nu; D_s^{(-)-} \rightarrow \phi X$ decays. The numbers were still low and the strategy was worsened by the fact that one would have to rely on MC

generators to estimate the number of accidental $\phi \ell^+$ correlations from the fragmentation process in $Z \rightarrow bb$ hadronization. Since no reputable physicist wants to rely on Monte Carlo to show credible evidence of a new particle, I was disappointed. In anticipation of yet another failure I decided to go down and 'celebrate' with some wine at one of the open air bars in the beautiful Piazza Roma with all my calculations in hand. The wine was cheap and I drank plenty of it. Perhaps because of it, an idea came. The idea illustrated in Figure 1 below was to apply the inclusive search strategy to the $\Lambda_{\rm b}$ system where the asymmetric decay of $\Lambda \rightarrow p\pi^{-}$ would provide a 'control sample' from data of Λ produced during b quark fragmentation. Ed Blucher dropped by and we sipped some more wine and discussed this strategy and possible complications. In the end we both concluded that the idea would work statistically IF the Λ production during b quark fragmentation was small.

Upon return to CERN, it took no more than a week for me to put this idea to test. Joleen Pater, a Wisconsin student looking for a PhD thesis topic independently checked my results shown in Figure 2(a). The asymmetry was clear but now my nightmares began. What if Nature was not as simple as I had imagined? What if there were other sources of correlations. It took months for me to finally convince myself (this made Jack Steinberger very frustrated since he wanted me to publish immediately) that the difference in the $\Lambda \ell^-$ and $\Lambda \ell^+$ yields were in fact due to semileptonic



Figure 1a):
$$B_s^0 \to D_s^{(-)} - \ell^+ \nu \to \phi \ell^+ + X$$
.



Figure 1b): $\overline{\Lambda}_{b}^{(l)} \to \Lambda_{c}^{(1)} - \ell^{+} v \to \overline{\Lambda}_{c}^{(l)} \ell^{+} + X.$

decays of the $\Lambda_{\rm b}$ baryon. This surprisingly quick observation of b baryon signature was warmly greeted at the LP91-EPS meeting in Geneva where all questions after P. Roudeau's plenary talk were on this observation. We published this first evidence for b baryons from our 1990 data in Phys. Lett. (B) **278** (1992). The Opal Collaboration confirmed our observation and interpretation fairly quickly. As you might know, since ISR days, there have been many claims for the signature of $\Lambda_{\rm b}$. None of them have stood the test of time. So a quick



Figure 2(a): First evidence for Λ_b at LEP.



Figure 2(b): First measurement of Λ_h lifetime.

confirmation in our own 1991 data and also from Opal made us all very happy!

We quickly moved on to b baryon lifetime measurement for which this sample was ideal. With added data from 1991 running, Fred Weber and I analysed the lepton impact parameter for the first measurement of the Λ_b baryon lifetime. The first measured lifetime published in Phys. Lett. (B) **297** (1992) was low and it was clear that with the added precision from the VDET and rapidly increasing data samples we would be able to perform fairly precise checks of theoretical predictions of the equality of b-hadron lifetimes. Since other LEP experiments could also do this simple measurement, the significance of the lifetime measurement would be enhanced.

At the end of LEP1, the LEP average of b baryon lifetime was 1.21 ± 0.05 ps significantly smaller than B meson lifetimes. This discrepancy, being confirmed now by Tevatron experiments using exclusive Λ_b samples, has puzzled theorists and remains an unsolved issue in Heavy Flavour physics.

Semileptonic decays and lifetimes of the ${\rm B}_{\rm s}$ and ${\rm \Lambda}_{\rm b}$ hadrons

The VDET was a success since its installation in 1991, and the precise three dimensional optics it provided of the charged particle trajectories allowed Aleph to show that the average b hadron lifetime was about 1.55 ps, substantially higher than the 1.3 ps world average from PEP and PETRA wire-chamber-based measurements. Working independently, both Roger Forty (B_c) and I decided to put VDET's full power to work in searches for exclusive semileptonic decays of the B_s and Λ_b hadrons. The beautiful bubble-chamber-like views that the tracking system provided allowed Aleph to be the first in the measurement of the semileptonic decay rate and lifetime of these hadrons. Hans Drevermann made many beautiful additions to DALI to show the clear primary-secondary-tertiary



Figure 3: The decay $\overline{B}_{s}^{(t)} \rightarrow D_{s}^{+} c^{-} \overline{v}$ captured by the Aleph tracking system.

vertex structure. One of the first event displays he made using our reconstructed B_s sample is shown in Figure 3. It's an object of beauty in more ways than one and heralded the important role such detectors would play in b quark physics.

Wisconsin students Fred Weber, Owen Hayes, YongSheng Gao and Min Zheng working with me, Paul Colas et al. from Saclay (B_s), Lorenzo Moneta et al. from Pisa, Mossadek Talby et al. from Marseilles (Λ_b) quickly measured the lifetimes from such samples. Representative results are shown in Figure 4 and reaffirm the low b baryon lifetime measurements from the sample collected during LEP running.

Measurement of the ${\rm B_s}$ and ${\rm \Lambda_b}$ hadron mass

By 1992 Aleph had collected about one million hadronic Z decays. Although this was not a large data sample, it was enough for earnest attempts at B_s mass measurement to begin. There was some urgency since CDF was in middle of Run 1 data taking, and in principle, had a much larger recorded B_s sample to work with. Given the small exclusive decay rates my plan was to cast as wide a net as possible, keep the signatures extremely clean and then sum over all modes to fit for the B_s mass. Doug

Ferguson from Wisconsin helped me a lot in datamining and MC production. Unknown to me at that time, Gary Taylor (UCSC), Pascal Coyle (Marseilles) and Bob Jacobsen (CERN) were also firing up independent searches. Some in Aleph had predicted that the search for exclusive B_s signature would be 'easy' but I knew from past experience (and low production rates) that one would have to collect 'drops in the bucket' and also get a bit lucky with the reconstructed B_s event topology. With the enthusiastic help of Gerhard Lutz (author of YTOPOL, a program for kinematic mass and vertex fitting) and Hans-Günther Moser of MPI-Munich, by December 1992, I had the most precise analysis tools that one could gather.



Figure 4: Measurement of (a) B_s lifetime with $D_s^-\ell^+$ and (b) Λ_b lifetime with $\Lambda_c^+\ell^-$ samples.

Now it was time for the right events to show up in data! The target was the Moriond meeting in 1993 and I recall working furiously through the Christmas break testing and validating dozens of B_s decay channels.

By January 1993, I had reconstructed ten events with varying probabilities of being signal. After checking all background estimates (1.5 events) carefully I recall phoning Jacques Lefrançois in Orsay. Over the years I had found Jacques to be a valuable and objective judge of analyses so I wanted to run my observation by him first before making any public announcement in Aleph. He



Figure 5: The $B_s \rightarrow \psi' \phi$ event with $\psi' \rightarrow \mu^+ \mu^-, \phi \rightarrow K^+ K^-$.



Figure 6: The decay $\overline{B}_{s}^{(t)} \to D_{s}^{+} \pi^{-}$; $D_{s}^{+} \to \phi \pi^{+}$, $\phi \to K^{+} K^{-}$ which demonstrates B_{s} oscillation.

made many requests for checks and improvement but the most precious advice from him was to perform an event-by-event mass reconstruction and maximize the precision on the mass measurement. Hans Drevermann (who had included YTOPOL vertex information in DALI) and I spent hours scanning and studying the events. I reported my finding (including run and event numbers) at a 'Thursday' meeting in February 1993 as did Gary Taylor. Gary's search was not as exhaustive but more importantly, due to a impact parameter requirement in his analysis, he has missed some of the events that I found. Pascal Coyle had independently found the charmonium associated

> events I had; this was very good news. My analysis was approved for Moriond'93 where Bob Jacobsen and Alain Bonnisant reported. I gave a PPE seminar on the B_s mass and lifetime measurement on 22 March 1993. A day before my talk, Prof. Amaldi from Delphi came to my office with news that Delphi too had seen a couple of interesting events and requested that I show them, which I did. I should point out here that many of these events were ultimately missing in the subsequent Delphi publication (CERN-PPE/94-22) on the B_s mass measurement.

> The observation of the B_s was clearly an important event and many Aleph physicists with various types of expertise got involved in examining these events for imperfections and inconsistencies. Many helped me but working alone I had a difficult time answering everyone's critique immediately and sometimes this was quite frustrating. This was my first real taste of the pains associated with a discovery, even a small one! In the end, there were two spectacular and unambiguous events that the entire collaboration loved: The 'lucky' $B_s \rightarrow \psi' \phi$ event with

 $\psi' \rightarrow \mu^{+}\mu^{-}, \phi \rightarrow K^{+}K^{-}$ shown in Figure 5 and the $\overline{B}^{0} \rightarrow D^{+}_{+}\pi^{-}$ event, that Gary had missed, shown in Figure 6. The clarity within these event displays rivalled and perhaps exceeded the best bubble chamber pictures. The $B_{s} \rightarrow \psi' \phi$ event, due to all tracks going through a single silicon wafer, gave the most precise B_{s} mass while the $\overline{B}^{0} \rightarrow D^{+}_{+}\pi^{-}$ event with a same side positively charged Kaon (labelled K⁺_f in Figure 6) and a very energetic electron in the away b-jet (seen at six o'clock) gave unambiguous proof of B_{s} mixing since there are two \overline{B} mesons in the Z decay. These were not your 'typical' events!

Since we had these two spectacular events, we changed the publication strategy from reporting a fit to B_s mass histogram to focusing on these two events. This was a totally opportunistic but correct strategy. In June 1993 we sent our paper entitled 'First measurement of B_s meson mass to Physics Letters (Volume 311) where we reported the mass.

I should comment here on our competitor's results at that time. Opal in Phys. Lett. (B) 295 (1992) had reported a 'search' for B_s meson. They had found one event with a mass of 5.36 GeV and a huge mass error of 70 MeV. They gave no background probability for this event so it was hard to tell what they had seen! CDF had no result at Moriond '93 but rushed a publication in Phys. Rev. Lett. (71), 11 reporting a B_s mass based on 14 events. Their measured mass of 5383.3±4.5±5.0 MeV was about 2σ away from the Aleph measurement and at conferences there were innuendos from some CDF people that Aleph mass was wrong because it is based on two events (as opposed to CDF's 14)! Well, as it turns out a reanalysis of the CDF data published in Phys. Rev. (D) 53, 7 (1996) reported a mass of 5369.9±2.7±1.2 in excellent agreement with the Aleph value. The PDG does not use the CDF'93 and OPAL'92 results in their mass average. Later results (1994) from Opal and Delphi were consistent with the Aleph measurement but had much larger errors.

So you can see that the credit for the discovery of the B_s meson belongs to Aleph.

The precision in the Λ_b mass measurement was poorer since we did not see any $\Lambda_b \rightarrow J/\psi\Lambda$ type event. YongSheng Gao and I from Wisconsin and Paulo Spagnolo and G. Musolino from Pisa toiled away at this measurement till the end of LEP1 running. Based on four clean events (background probability $<4 \times 10^{-4}$) we reported a Λ_b mass of 5614±21±4 MeV in Phys. Lett. (B) **380** (1996) which is about 2 σ away from the Delphi measurement published in Phys. Lett. (B) **374** (1996) but in excellent agreement with the subsequent CDF measurement published in Phys. Rev. (D) **55**, 3 (1997).

In conclusion, Aleph led in the discovery and measurement of the properties of the B_s and the Λ_b hadrons. The period 1990–95 when these measurements were made was extremely intense for us. Now I look upon those days with pleasant memories!

Speaking of memories, this story is written more than ten years after the events happened and I no longer have access to Aleph notes to refresh my memory. While I have tried to be careful, I apologize deeply if I have missed the name or a valuable contribution of a participant in this extended adventure.

I would like to thank Sau Lan and the members of the Wisconsin group between 1989 and 1995 for their enthusiastic support and unwavering trust in my adventures. Interactions with Jacques Lefrançois, Lorenzo Foà, Gigi Rolandi, Dieter Schlatter, Gerhard Lutz, Hans-Günther Moser, Jürgen Knobloch, Brigitte Bloch and Hans Drevermann make excellent memories of my Aleph days.

AMAZING TALE OF THE TAU

The study of τ physics with Aleph has been one of my most enjoyable and productive research projects, so reflecting on this busy period brings with it a feeling of satisfaction, pleasure and, also, pride. Although this topic was not so high in the list of the initial physics priorities, a combination of three ingredients turned this enterprise into one of the most successful in the whole Aleph physics output. First, and most importantly, the Aleph detector was beautifully designed to study τ production and decay. Then, LEP operating at the Z resonance was the best available τ factory and τ lepton pairs could be identified with a large efficiency and very small background. Finally, and not the least, many talented colleagues, postdocs and students joined in this effort over a period of ten years, bringing original physics ideas and powerful analysis methods. This happy combination

produced a unique environment to attack in an unprecedented way many aspects of τ physics ranging from electroweak physics to QCD.

I personally got involved in τ physics with the CELLO detector we had built for PETRA in Hamburg. In the 1980s, several inconsistencies affected the results on τ decays. There was the so-called 'one-prong problem', an apparent deficit in the rate of well-identified decays compared to the inclusive one-prong fraction. So therefore several prominent physicists advocated the idea that the τ lepton might be non standard. Some progress was achieved with CELLO, pointing to more standard properties, but it was limited by statistics.

The Aleph τ group was initially convened by Christoph Geweniger, Gigi Rolandi and myself. Many groups participated actively from the beginning: Barcelona, CERN, Ecole Polytechnique, Frascati, Heidelberg, Lancaster, Orsay, Pisa, and Wisconsin. In later years, work continued essentially at Ecole Polytechnique and Orsay with Henri Videau as convener.



 $Z \rightarrow \tau \tau$ decay in Aleph, followed by the decays $\tau \rightarrow e+2v$ and $\tau \rightarrow 3$ charged hadrons+v.

It is worth dwelling on why the Aleph detector was so well suited to the job. At LEP energies, $\boldsymbol{\tau}$ pairs led to two collimated back-to-back particle jets due to the large Lorentz boost in each τ decay. This provided for a very clear signature resulting in a large selection efficiency and a small background. But the downside was a significant overlap in the calorimeters from charged particles and photons from π^0 decays, hence the necessity of a highly granular system was an obvious requirement. It is difficult in an electromagnetic calorimeter to achieve simultaneously good energy resolution and spatial particle separation. The decision made in Aleph was to deliberately choose in favour of a good granularity, mostly on arguments of particle identification in b jets. This choice turned out to be crucial for τ physics: it is the primary reason for the often-noted leading position of Aleph in this field. To set the scale: Aleph ECAL had 70 000 solid-angle cells with a three-fold longitudinal segmentation (a great help for particle identification and for finding photons in the neighbourhood of charged particles), to be compared with 7000 cells in L3 or 10 000 in Opal without segmentation in depth. Delphi had in principle good granularity, but it turned out to be harder in practice.

The exploitation of the excellent properties of the detector was made possible by a first-class software environment from data access and bookkeeping to reconstruction and simulation. In addition, several dedicated packages were developed in the τ group to take full advantage of the hardware performance. This was the case for the selection of τ -pair events: using the energy flow tool from Patrick Janot, a very efficient selector TAUSEL was designed by Laurent Duflot and Gerry Ganis, and used by Maria Girone and Gigi Rolandi to measure crosssections in a very precise way. Particle identification was crucial for the understanding of τ decays in order to separate electrons, muons and hadrons. At the beginning, most people were using cuts, but a likelihood method TAUPID was soon developed by Zhiqing Zhang and myself which proved so superior that everyone adopted the method. Later, improvements were introduced by Hyongjong Park. The next most important piece was photon and π^0 reconstruction in the situation where clusters overlap in ECAL, which was beautifully handled with the GAMPEC package, written by André Rougé, Jean-Claude Brient and Marc Verderi. The complete information on each τ decay, from charged particles to the handling of multiple π^{0} 's, was collected by PEGASUS, worked out by Ricard Alemany, Fabio Cerutti, Luca Passalacqua and myself. Other useful developments occurred for pion/kaon separation using dE/dx in the TPC and K⁰_I reconstruction in the ECAL-HCAL (Hyongjong Park and myself), and improved muon identification (Henri Videau). An invaluable tool was the DALI display from Hans Drevermann which was constantly used in the design, tuning and maintaining of our packages, as well as in the course of the various analyses. The stage was now set for physics analyses!

It is not the place here to review all the numerous τ physics results that were obtained by Aleph. Cross-section and forward–backward asymmetry were integral contributions to every update of the electroweak precision measurements (19 papers). In addition, a total sum of 29 papers were produced on different aspects of τ physics: branching fractions and spectral functions (13), charged-current couplings and τ neutrino (6), spin and polarization (5), τ lifetime (5). Let me illustrate these remarkable achievements by telling a few of their stories.

Certainly the overall description of τ decays obtained by Aleph had a profound impact. All previously reported problems (one-prong, also three-prong) have vanished and the τ lepton appears standard with leptonic couplings displaying universality with a precision of 3×10^{-3} .

We developed a global method to measure all the decay channels simultaneously, profiting from our pure and unbiased τ sample. Preliminary results were given as early as September 1990: I had organized in Orsay the first worldwide τ Workshop (still continuing in even-numbered years) and I remember the surprise expressed by our CLEO colleagues who thought they were alone in the world in this field and discovered strong competition. Zhiqing had just started his PhD and was giving the Aleph talk which was followed by an intense discussion: 'I did not know I was working

on such a hot topic!' was his first reaction. In fact, the τ Workshops turned out to be privileged meetings for the progress in τ physics and we always managed to present our newest results there. It was very satisfying to see that we were ahead for most of the topics. At the Montreux meeting in 1994, I presented our precision analysis of the leptonic branching ratios (Hyongjong's thesis) where all the relevant efficiencies were measured on the data using control samples, thereby removing systematic effects from the imperfect simulation. It was a breakthrough and soon afterwards other collaborations would follow in the same way. Since our 1996 publications on branching fractions and thanks to the hard work by Ricard Alemany, Shaomin Chen and Changzheng Yuan, Aleph measurements dominate the world averages for all channels with branching ratios above 10^{-3} . In fact, we are limited by statistics for most of our results so that we could have continued to run at the Z peak for much longer! CLEO, and now BaBar and Belle, have much larger samples and are superior



Four independent measurements with Aleph which should yield the same value if universality holds in the leptonic charged-current couplings (the values are expressed in terms of the electronic branching ratio as computed from the measured quantities assuming universality). Universality holds at the 3×10^{-3} level.

for rare modes, such as searches for lepton-flavourviolating decays or second-class currents, but they are severely limited by systematic effects for the large modes. The final paper on τ decays using the full LEP1 data is finally out in 2004 (our τ megapaper!).

One very pleasing outcome of our systematic work on τ decays was the measurement of the spectral functions, both vector and axial-vector, which are fundamental ingredients for the understanding of hadron physics. We realized early on their potential, in particular for QCD studies, benefiting from the work by theorists (Braaten, Narison, and Pich). François Le Diberder was a key person in the early work and contributed two original and important ideas. He wrote the QCD fitting program we are still using today, still complaining about its complexity and somewhat lack of transparency (Ah! The 'ignominious expression for the OPE D=4 term'...). This area of our activity grew considerably with Andreas Höcker's thesis where we really 'squeezed out all the juice' from the spectral functions (using the words of Ryszard Stroynowski from CLEO who participated in Andreas' PhD committee). It was some achievement to show that perturbative QCD was indeed working well at the τ energy scale and even down to 1 GeV at the 2% level, and to deduce a precise value for the strong coupling constant, $\boldsymbol{\alpha}_{s}\!,$ which, when evolved to the Z mass, was in perfect agreement with the result (equally precise) obtained from the Z width. Thus the strong coupling was running, as predicted in QCD, decreasing by almost a factor of 3 between the τ and the Z scales. Since the dominant uncertainty in our α_s measurement was of theoretical nature, we needed some backup from the theory community, especially as some scepticism was sometimes expressed over this nonconventional approach. Therefore, I arranged a meeting with leading QCD theorists (Altarelli, Grunberg, Nason, Neubert, Pich, Vainhstein, ...)



The inclusive vector + axial-vector spectral function shows resonant behaviour at low mass and converges toward the QCD asymptotic value. The perturbative QCD prediction agrees with the data at the 1% level when the latter is suitably averaged over the mass range: this is the property of global quark– hadron duality.

which was very productive and placed us on safe grounds. Still, at the end of the meeting, Guido Altarelli questioned: 'I like your method and the measurement is precise, but why do you want it to be the best?'!

The vector spectral functions are driven by the same physics as in e⁺e⁻ annihilation into hadrons which is a necessary ingredient to compute vacuum polarization effects. Such contributions are responsible for the running of the electromagnetic coupling and they also play an important role in the anomalous magnetic moment of the muon. It turned out that both issues were very topical and the need for increased precision was clear. As a by-product of our τ work with Aleph, I proposed to use τ data and our QCD experience from the τ analysis to compute these contributions. This led to a series of papers done mostly with Andreas which became somewhat of a reference and prompted many other analyses along the same lines. Our only disappointment came from the LEP Electroweak Working Group who failed to take early advantage of these improved calculations, which were later confirmed by BES experimental results. Increasingly precise data on e^+e^- annihilation from Novosibirsk and Frascati are now competing in accuracy with the Aleph τ data and a non-understood difference shows up between the two sets, which is not explained by the existing (small) predictions for isospin breaking. This discrepancy may reveal interesting physics.

Another success story was the measurement of the τ lifetime. At Z energies, the produced τ 's travel on average 3 mm in the LEP beam pipe before decaying, so measuring accurately their decay path is a real challenge. It was beautifully met thanks to the precise vertex detector. Here all the LEP detectors were on a close footing, but Aleph contributed with some original analysis methods. Steve Wasserbaech was a main actor in this field, inventing imaginative new variants of the impactparameter approach and gaining very useful insight into the sensitivity of the measurement to different sources of uncertainties. Detailed analyses were carried out by him, Francesco Fidecaro, Alberto Lusiani, Isidoro Ferrante and others. An unsuspected breakthrough came from Inkyu Park and Anne-Marie Lutz who developed the completely new and most powerful 3D



The discrepancy between the spectral functions from e^+e^- annihilation and τ decays is apparent when the branching ratio for the decay $\tau \rightarrow \pi \pi^0 v$ computed from e^+e^- is compared to the direct measurements. Note the accuracy of the Aleph determination. impact-parameter method. The geometry was somewhat hard to visualize, but the spirit of this new approach was best understood when Inkyu displayed and manipulated a cardboard model of opposite τ decays!

The fact that each produced τ rapidly decays through the parity-violating charged current allows one to measure its polarization. This is a unique measurement at LEP, analogous to the famous leftright asymmetry using polarized electrons at SLC. On one hand, the forward–backward τ polarization asymmetry yields the same observable as measured at SLC, i.e. a parity-violating combination of the vector and axial-vector electron couplings. On the other hand, the averaged polarization is given by the same observable, but this time for the τ couplings. Thus the τ polarization provides one with both a powerful test of $e-\tau$ universality and a measurement of the leptonic couplings which yields a precise value of $\sin^2\theta_w$. The superiority of Aleph for the τ analyses was again manifest for this measurement where one could capitalize on the tools we had developed, the acquired experience, and the very good detector properties. Many intermediate results were produced with ever increasing accuracy. The early years showed a large activity with many groups contributing (Barcelona, Ecole Polytechnique, Lancaster, Orsay, Wisconsin), either analysing different decay channels (e⁻, μ , π , ρ , a_1) or even competing on the same channels. It was a nice exploratory period where we straightened out several deficiencies in the simulation: I still remember the discussions in the **t** group with Fabian Zomer, Steve Snow, Achim Stahl, Uli Stiegler, John Harton and many other young active colleagues. The leptonic and pion channels were the easiest, at least conceptually, since only one observable retained the full polarization information, while for the multibody ρ and a_1 decays the analysis required several observables (decay angles) as André Rougé had nicely shown before. A real breakthrough occurred in summer 1992 in a quite unexpected way. At Orsay, we were looking for a more optimized way to handle the hadronic channels and François Le Diberder came up with a brilliant idea: however complex the τ decay was, there was a single variable he called κ , which contained the full polarization information. François, Laurent Duflot and myself quickly wrote a note about the new method, just in time for a τ group meeting at CERN where we expected to be cheered by our colleagues. Well, it was not exactly so, as André Rougé had got exactly the same idea and also came to the meeting with a physics note! The only difference was the name of the variable he called ξ ... It was a big excitement and we quickly decided to publish a common paper where we ended up calling the optimal variable ω ! The new method was immediately put to work on the Aleph



Left: a textbook figure of the angular dependence of the τ polarization, in perfect agreement with the expectation from e- τ universality in the weak neutral current. Right: the derived couplings from the LEP experiments, compared to the e measurement from SLD.

data and we could get the maximum sensitivity in the ρ and a_1 channels, which was a spectacular progress for the 3-body a_1 decay.

The final analysis with the full LEP1 data was done independently at Ecole Polytechnique by Jean-Claude Brient, André Rougé and Henri Videau, and at Orsay by Ricard Alemany, Irena Nikolic (her thesis) and myself. The Orsay approach made use of the information on the τ direction. The first results did not look very compatible and we had to spend many months cross-checking the two methods. It is certainly remembered by all, including our referees (Gigi Rolandi, Peter Dornan, and others at times) as a long and tedious experience. It was not easy to converge, but finally we succeeded and at the end we had gained a much better understanding of the analyses, corrected a few mistakes, and we felt very confident with the final combined result. Writing the paper was the best part, as we were very happy to bring the project to an end, especially as the results were of a high quality and the best available at LEP.

In closing, I would like to thank particularly my colleagues at Orsay who contributed so much to this effort. François Le Diberder brought many good and original ideas, while Anne-Marie Lutz was invaluable in adapting and maintaining the KORALZ generator. All the analyses were carried out in small groups with postdocs (Xiahong Chen, Ricard Alemany, Shaomin Chen, Changzheng Yuan) and students (Fabian Zomer, Zhiqing Zhang, Laurent Duflot, Hyongjong Park, Inkyu Park, Irena Nikolic, Andreas Höcker). One often hears from colleagues in other physics fields that the large particle physics collaborations are not well suited for academic work. It was certainly not the case in Aleph and it is not our least satisfaction to have provided graduate training while producing forefront hot research results, working in very small groups (often reduced to only two persons), however immersed in the stimulating and experienced environment of the Aleph collaboration.



Early times: the first τ Workshop in Orsay (1990). Left: Fabian Zomer and Achim Stahl. Middle: Uli Stiegler and Steve Snow. Right: André Rougé presenting polarization observables, with Barry Barish listening.



The second τ Workshop in Columbus: Michel Davier showing early results on the τ lifetime in the summary talk (1992).



Henri Videau (1996).



Reflecting on 15 years of work in τ physics: Françoise and Michel Davier, with Jacques Lefrançois (1996).



Zhiqing Zhang, Andreas Höcker, Michel Davier (Institut de France, 1997).

GAMMA-GAMMA

Alex Finch/Claus Grupen

Two-photon (or gamma–gamma) physics is certainly not a subject which is in the forefront of electron–positron research. At the beginning of LEP the measurement of the Z parameters were without question the hot topic. When the energy of the LEP collider was increased one realized unsurprisingly—that the event rate dropped to low levels. One could no longer see real events on the online display in the control room. This display, which was very popular in the early days, now only showed online cosmics and some other stuff, presumably beam-gas or gamma–gamma



The online event display changed at LEP2...

scattering. It was known that the cross-section for the one-boson exchange process drops as 1/s with increasing centre-of-mass energy \sqrt{s} , while the two-photon cross-section rises with s! At LEP2 the event rate would be dominated by gamma–gamma scattering.

Still, the two-photon physics group was always something of a poor relation in Aleph, as to most of the collaboration these events were an annoying background process, this was particularly reflected in the annual fight to keep the triggers for these events turned on.

However, for a small band of people the twophoton interactions were the signal and the other events were background. In the early years of Aleph the group consisted of one person, namely AF (Alex). AF was soon joined by a group of UK collaborators, particularly Mark Lehto and Paul Hodgson from Sheffield, and Alison Wright from the Rutherford Lab. Their numbers were boosted dramatically in 1997 when Siegen joined the group, with Armin Böhrer, Glen Cowan and CG (Claus) overseeing a succession of able students, mostly analysing the LEP2 data in which twophoton physics was now the dominant process. In addition to these groups Glasgow and Barcelona also contributed to the two-photon analysis. It was only natural that mainly those groups in Aleph which were concerned with the forward detectors were engaged in the analysis of photon-photon events. The two-photon subgroup was convened by AF, and later together with Armin Böhrer.

Now what is the big deal about gamma-gamma scattering? Hasn't everything been known about the photon for over a hundred years? Albert Einstein himself would not agree with this. He never understood the 'photon' particle properties of light quanta, and he famously claimed: *"All these fifty years of conscious brooding have brought me no nearer to the answer to the question, 'What are light quanta?' Nowadays every Tom, Dick and Harry thinks he knows it, but he is mistaken."* What better reason could there be to look into the structure of the photon?

In two-photon interactions the electrons are normally scattered into very small angles and remain in the beam pipe. One can generally only hope to see some of the debris of the underlying gamma–gamma scattering process. This can be a purely leptonic final state or a hadronic final state. The leptonic processes are completely described by quantum electrodynamics. Therefore the interest lies mainly in the question: What kind of ingredient is in the hadronic character of the photon? What is the quark content, how many gluons are there, what is their energy or momentum distribution? If the scattered electrons stay in the beam pipe this is called a no-tag process in the gamma–gamma community. If one of the electrons is scattered out of the beam pipe and is measured in one of the forward detectors this is named a single-tag event. In rare cases both scattered electrons are recorded, which then is a double-tag case. Mostly no-tag events are seen.

Over the years many results on the hadronic photon structure, charm production in gammagamma scattering, jet production, exclusive meson and resonance production, inclusive particle production, and glueball and new particle searches were obtained, presented at Photon conferences and finally published. For the successful analysis of the photon-photon final states many new tools had to be developed, because only part of the final state is normally seen in the detector and elaborate unfolding techniques had to be invented to account for those particles which escaped undetected.

Even though we have learned a lot more about the photon, it is still not clear what lurks in it at very small parton momenta. And here we are again in good company with Einstein who remarked in discussing quantum theory calculations with Pauli: *"The quanta really are a hopeless mess."*



Different views of a single-tag hadronic gamma-gamma event.

THE LLV STORY

Ioana Videau/Patrick Janot

(Editor's note-RS: Ioana was busy (who isn't?). This is from Patrick Janot's 'The Higgs (and all that jazz...)' below, and is repeated here for your convenience.)

"...The first one, also known as 'the $\tau\tau V$ excess' (an excess of events in the 1990 data, with a Higgs boson radiated from a tau pair, of mass equal to that of the ρ), was a long and painful experience for the collaboration. It resulted in secret drafts, short deadlines, sterile fights, ostracized analyses and over/under-estimated probabilities. Nobody was really prepared for it and it diverted us from more relevant physics studies for a while. Not less than a year and three internal referees (Ioana Videau, Lorenzo Foà and Mike Green) were needed to come out with a solution acceptable to everybody. Finally, Lorenzo presented the whole story to the world in front of a packed CERN auditorium, and even made it a success for Aleph particleidentification capabilities in a memorable *tour de force.* The $\tau\tau V$ excess was not confirmed with the data taken between 1992 and 1995..."



'A severe case of symmetry breaking!'

THE 4JETS SAGA

Patrick Janot/Peter Dornan

(Editor's note-RS: Here are other excerpts, this time from Patrick Janot's 'The Higgs (and all that jazz...)' below and from Peter Dornan's 'Reminiscences of Spokesmen' above, repeated here for your convenience.)

Patrick Janot

"... The second one, also known as 'the four-jet peak' (an excess of events in the 130-136 GeV data of 1995) arose from a search for hA pair production with no b-tagging possibilities: the new Aleph vertex detector for LEP2 had just been installed and was neither aligned nor entirely ready. This excess was immediately made public to all analysis groups in Aleph and to the other LEP collaborations. An intense collaborative effort went on, and everybody worked very efficiently in the same direction. It led to brand-new analysis methods being used for the first time (e.g. use of the matrix element squared as an event weight so as to account for the full information available, use of the rarity method to combine several variables and to test the compatibility of a sample with the expected background...), and it showed the way for many subsequent analyses. Clearly, the collaboration, led by Gigi Rolandi (who brilliantly mastered the situation), had learnt from the past and managed to handle this situation much better. We even succeeded in convincing the other three collaborations to run once again at 130 and 136 GeV. Eventually, 'we found no other explanations than a statistical fluctuation.' It was the first time ever (and probably the last) that an editorial 'we' was allowed in an Aleph paper..."

Peter Dornan

"There was also a fiery start on the physics front. The famous, or infamous, four-jet signal. At the end of the running in 1995 LEP had made its first step towards LEP2 with short runs at 130 and 136 GeV. These were used to test the emerging analyses for the Higgs boson search. Surprisingly a small excess of four-jet events appeared at 130 GeV and remarkably a similar one at 136 GeV. There was no rational explanation and the statistical significance, even together, was inadequate to make great claims. The other experiments claimed nothing but when their data was combined there was again a small effect.

What should we do? Few believed it to be more than a statistical oddity but history is littered with cases of experiments missing important results. Consequently in his last presentation to the LEPC, Gigi had suggested a new LEP run at 130–136 GeV to clear up the mystery. It was not a universally popular request, the other experiments were lukewarm, some theoretical colleagues were incredulous—after all there was no theoretical explanation and so it must be wrong. Nevertheless the wish to see this settled was endorsed at an Aleph plenary meeting and so, at my first LEPC in September 1997, I argued the case for another run. The request was for twice the original luminosity but we came to an agreement that if no excess was observed with the original luminosity when the experiments were combined, the run would be stopped. It went ahead, no excess was seen with the original luminosity, Aleph even had a small deficit, so the run stopped and the four jet saga came to an end. The theoreticians could feel exonerated."



'If the Aleph-bump at 105 GeV were real, we should have seen it!'

SEARCHES AT LEP2

Fabio Cerutti

(Editor's note-RS: Fabio was one of the conveners for searches at LEP2. We had him down for this story after the tau-neutrino-mass story he wrote above. Since he was so busy that he couldn't do this story in addition, here are a couple of my impressions. Of course these searches had many facets and a lot of people put in a lot of hard work and generated a lot of good ideas to look for new physics: Higgs and SUSY in their many variations, contact interactions, extra dimensions and so on—you name it, we (and the other LEP experiments) looked for it! Our Higgs search produced some interesting events and is covered in several stories below. And the rest of the searches also entailed a huge effort. For example almost everybody had high expectations to find some sort of evidence for supersymmetry at LEP2, since it just smelled like this beautiful idea was just around the corner. So every imaginable event topology was searched for, since SUSY has a channel somewhere which can give rise to one of them. Of course this was done in an orderly way starting from the predicted topologies, searching, setting limits on the SUSY parameters, and then iterating again when data at the next higher energy became available. In the end we found **nothing**, after an enormous intellectual exercise. But it was all not for naught, since many new ideas the theorists can come up with in future will give rise to topologies that have already been covered by these SUSY searches...)

THE HIGGS (and all that jazz...) Patrick Janot

The search for the Standard Model Higgs boson in Aleph has been an amazing story, both on the human and also on the scientific side. Indeed, because it was thought of as 'spearheading' all searches, this topic has always created serious competitive spirit among the various groups involved.

The consequences were twofold. On the one hand, many first-class algorithms came out from the Higgs searches, which resulted in substantial improvements for many other analyses. For example, the Energy-flow algorithm (developed by Vincent Bertin, Gerardo Ganis and myself) arose directly from the Hvv search at LEP1, in 1990. Similarly, the celebrated Grivaz-LeDiberder's 'N₉₅ prescription', so widely used nowadays, was developed to account for the fact that the cuts of a selection must be modified when the integrated luminosity increases. This was in contrast to an old, widespread and wrong belief and developed after a whole lot of events had been observed in the Hll channel in 1992, while no events had been selected in 1991. A new way of searching for new particles was born, more rigorous, more credible, more effective and, more importantly, revealing and making use of all the capabilities of our beautiful detector. This precursory vision allowed Aleph to keep its leadership over the other LEP collaborations until the very end, not only in Higgs boson searches, but also in many other physics topics.

On the other hand, this strong competition also generated a few excesses, in both people's behaviour and selected events... The history of Aleph is rather rich in these respects, certainly richer than statistical fluctuations would have allowed *a priori*. Whether it is related to the personalities of the belligerents remains an open question: the Higgs boson search sociology would deserve a whole chapter in this book. To summarize, Aleph emerged more mature from each of these excesses.

The first one, also known as 'the $\tau\tau V$ excess' (an excess of events in the 1990 data, with a Higgs boson radiated from a tau pair, of mass equal to that of the ρ), was a long and painful experience for the collaboration. It resulted in secret drafts, short deadlines, sterile fights, ostracized analyses and over/under-estimated probabilities. Nobody was really prepared for it and it diverted us from more relevant physics studies for a while. Not less than a year and three internal referees (Ioana Videau, Lorenzo Foà and Mike Green) were needed to come out with a solution acceptable to everybody. Finally, Lorenzo presented the whole story to the world in front of a packed CERN auditorium, and even made it a success for Aleph particleidentification capabilities in a memorable tour de force. The $\tau\tau V$ excess was not confirmed with the data taken between 1992 and 1995.
The second one, also known as 'the four-jet peak' (an excess of events in the 130-136 GeV data of 1995) arose from a search for hA pair production with no b-tagging possibilities: the new Aleph vertex detector for LEP2 had just been installed and was neither aligned nor entirely ready. This excess was immediately made public to all analysis groups in Aleph and to the other LEP collaborations. An intense collaborative effort went on, and everybody worked very efficiently in the same direction. It led to brand-new analysis methods being used for the first time (e.g. use of the matrix element squared as an event weight so as to account for the full information available, use of the rarity method to combine several variables and to test the compatibility of a sample with the expected background), and it showed the way for many subsequent analyses. Clearly, the collaboration, led by Gigi Rolandi (who brilliantly mastered the situation), had learnt from the past and managed to handle this situation much better. We even succeeded in convincing the other three collaborations to run once again at 130 and 136 GeV. Eventually, 'we found no other explanations than a statistical fluctuation.' It was the first time ever (and probably the last) that an editorial 'we' was allowed in an Aleph paper.

The last excess, also known as 'the 115 GeV/ c^2 Higgs boson', was the outcome of a five-year collaboration involving many Aleph physicists, among which was the celebrated 'Higgs Task Force'. Again, first-class algorithms had been developed and set up to make the Aleph Higgs boson search at LEP2 the most powerful of the four collaboration selections. After leading the Higgs Task Force for three years (1996–1998), I then became the LEP Physics Co-ordinator for two years (1999–2000). During these two years, the same scientific objectives and sociology strategies as those followed in the Higgs Task Force were applied to optimize the LEP running towards a possible Higgs boson discovery.

It was tough for everybody! For example, miniramps were demanding for the LEP engineers, who had to develop subtle tricks to stabilize the machine, but also for the detector physicists, who had to cope with increased backgrounds, as well as the analysis groups, who had to work with an almost continuous series of beam energies. The toughest fortnight was certainly in July 2000, when we ran exclusively above 104 GeV, with klystron trips and therefore beam losses occurring, on average, every 15 minutes. Based on this average, and on Poisson probabilities, I had even promised to the engineers that I would appear naked in the LEP control room if they managed to make two such consecutive runs last more than 45 minutes. Of course, they were very motivated by the bet and the following two fills lasted 51 minutes and 1 h 40, respectively! As expected, it never happened again afterwards, but I had to bring more champagne than ever to make them forget about my stupid bet. (Though some of them are still asking!)

To summarize, I will always remember this period as a tremendous human adventure: each single person was working in order to exploit the LEP collider, the LEP detectors and the LEP physicists' brains as much as possible, and probably more. Sadly, brains were apparently missing elsewhere... 'No consensus could be reached around the DG's proposal to close LEP' in the LEPC, the Research Board or the Committee of Council. Despite a general consensus in the scientific community in favour of continuing the adventure, the Director-General decided that LEP would be shut down forever on 17 November 2000. As a consequence, no additional data were made available to check if 'the 115 GeV/ c^2 Higgs boson' excess was due to a statistical fluctuation or was indeed the first sign of the Higgs boson discovery.

It is, however, not the end of the Higgs saga in Aleph. Indeed, there is no doubt that, if the Higgs boson discovery is confirmed by the Tevatron or the LHC at or around $115-116 \text{ GeV}/c^2$, Aleph and the LEP machine will have the sole and unambiguous paternity!

The Aleph 'experience' is not (yet) over!

1990-Now

(...like) ENERGY FLOW Patrick Janot

The words 'Energy Flow' will always remain special for me, as they remind me of the best times of my life, from both the professional and personal points-of-view. As a matter of fact, the Energy Flow algorithm, which I developed in March 1990, has been the basis of most of the Aleph analyses and successes from May 1990 onwards. It would be unfair, however, that the performance of this algorithm be associated with my sole name.

The 'Energy Flow' philosophy started with the design of the electromagnetic calorimeter (ECAL), in the early 1980s, by Henry Videau and Jacques Lefrançois. In contrast to, for example, the electromagnetic calorimeter of L3, our ECAL was purposely designed to emphasize the granularity over the energy resolution and the redundancy over the ultimate precision. The aim was an enhanced ability to identify photons and π^{0} 's in the busy environment of jets and τ decays, with the vision that a complete reconstruction of the final states would be the way to accurately determine jet directions and well-performing analysis algorithms.

These qualities were recognized early on the software side. In 1986, Jean-François Grivaz had designed the entire data structure to fully benefit from the ECAL granularity, as well as of the hadron calorimeter (HCAL) properties. On the top of this structure, the Physics Objects (photons, π^{0} 's, neutral hadrons, electrons, muons, ...) were to be stored as 'Cal-Objects' in the now well-known PCOB and PCPA structure. The work

of Alain Bonissent to fill this structure by, in particular, finding the links between the tracks, the ECAL objects and the HCAL objects, proved to be the basis of the Energy Flow reconstruction.

When I joined Aleph, in October 1987, this essential work was progressing well. The vision of Jean-François about the search for a Higgs boson in the Hvv final state, and about the need of an accurate reconstruction of missing energy, was clearer than ever. I will never forget the long discussion we had in June 1988 while walking around the lake in Seillac (a charming place close to Blois where the Laboratoire de l'Accélérateur Linéaire of Orsay holds a plenary meeting every two years): for one hour or so, he described almost exactly what would be the sequence of events in the following years, and why the development of an Energy Flow algorithm was a key issue in the search for Higgs bosons.

In parallel, another essential effort was going on with the ECAL hardware to make it as powerful as designed. The two years spent with Jean-Jacques Veillet, first at CERN Meyrin in 1988, then in the pit at Echenevex in 1989, have been extremely beneficial towards a complete understanding of the calorimeter and of its electronics. This understanding turned out to be a major asset while developing the ECAL cleaning part of the Energy Flow algorithm.

When I emerged from the pit, at the end of 1989, the first 25 000 Zs collected by Aleph had already been analysed to search for Higgs bosons. Two publications had been written by the Wisconsin group, exclusively based on the ideas presented by Jean-François Grivaz in the Aleph Week (October 1989) where the first Higgs boson limit was shown. Unfortunately, only the charged-particle tracks had been used in the event selection, since the effort led by Jean-François, in collaboration with Gerardo Ganis and Vincent Bertin, to include the calorimeter data had not yet been finalized. As a consequence, the results of Aleph were not as good as those of Opal, for example. In particular, the second publication, published in a hurry over Christmas without any discussion in the collaboration (the concept of Editorial Board came only after this event, when Jacques Lefrançois became Spokesman), led to a limit 1 GeV/ c^2 below that of Opal. The latter therefore became the result of the 1990 winter conferences and that of Aleph never made it into History.

Meanwhile, Vincent Bertin had completed his systematic analysis of the ECAL clusters, and Gerardo Ganis, that of the HCAL clusters. Both calorimeters needed some 'cleaning' before their information could be safely used in data analysis. In both cases, the design redundancy (wires and pads in the ECAL, towers and digital tubes in the HCAL) was precious to disentangle real signal from, for example, electronics noise, sparks or radioactivity shots. With these analyses, the concept of Energy Flow, as Jean-François had imagined two years before, could become a reality.

Because they contained all the bugs that one could possibly imagine, the 1989 data were just the right sample to develop the algorithm: a cleaning that would be efficient on these data would remain efficient until the end of Aleph's life. I clearly remember that I started to develop the code at the end of March 1990, when Jean-François had to leave for two weeks. The benchmark was a code developed on simulated data by Marie-Noëlle Minard, Monica Pepe and Jack Steinberger, based on 'masking' the charged-particle trajectory extrapolation in the calorimeters and counting the energy outside the masks. Our philosophy was, instead, to identify as many of the particles as possible, taking advantage of the ECAL granularity, in particular.

The charged particles were first selected as TPC tracks reconstructed with at least four hits, originating from within a cylinder of length 20 cm, radius 2 cm and centred on the nominal interaction point. (This sentence would remain a must in all Aleph publications.) In the ECAL, the fake clusters were cleaned with Vincent's algorithm and the photons were identified with EBNEUT, written by Jean Badier. Electrons were identified as EBNEUT photons linked to a charged-particle track extrapolation. Muons were identified with their characteristic penetration pattern in the HCAL, beautifully tracked by the very granular (in two dimensions) HCAL digital readout. Finally, the neutral hadrons were identified either as neutral cal-objects not identified as photons, or as a significant calorimetric energy excess with respect to the charged energy measured in this calobject, from which all identified particles (e, y, μ) and the corresponding tracks and calorimetric clusters had been previously removed.

Despite its simplicity, this algorithm turned in an energy resolution of about 9 GeV for hadronic Z decays at the first try, early April 1990, i.e., an improvement of 6 GeV with respect to the calorimetric-only energy counting, and of 3 GeV with respect to the existing masking algorithm version. This algorithm was used to select Higgs boson events in the Hvv final state, for which a measurement of the total missing energy and of the jet directions is an essential tool. A visual inspection of all candidate events selected in the 1989 data allowed all the possible defects to be tracked, understood, and accounted for in the energy-flow determination. Whether a corrupted ROC in the ECAL would give only the wire energy in a given module, or a fake spark would lead to wrongly clean an HCAL cluster, the algorithm would know it and correct for it. I remember each single candidate event with such a cleaned HCAL cluster, and the magic sentence to get it corrected almost online ('Gerri! HCAL tué!') by Gerardo

Ganis. This work further improved the resolution to about 8 GeV for hadronic Z decays, and greatly reduced the tails in the energy distribution.

The algorithm was then applied to the data taken up to the end of May 1990. After hard work and applying the bright ideas from Vincent, Gerardo and Jean-François to improve the selection, the efficiency on Higgs boson events reached about 80% for an expected background well below one event. No candidate events were observed and, with the help of the Hll selection developed by Frédéric Perrier and Patrice Perez in Saclay, a limit of 40 GeV/ c^2 was achieved. The only remaining question was 'What would be the limit presented by the competition?', i.e., the Wisconsin group in Aleph, and the other three LEP experiments.

The Aleph Week's 'Tuesday meeting' at the end of May 1990 was memorable. The pleasure of describing the Energy-Flow algorithm in front of a packed audience in the Council Chamber, the beauties of the calibration with radiative qqg events, and the successful application to the search for Higgs boson will remain for ever one of my best memories in Particle Physics. The icing on the cake came with the following talk, presented by Yi-Bin Pan (Wisconsin), in which an efficiency of 40% and a limit of 33 GeV/ c^2 , similar to that of the other three LEP experiments, was presented. The power of the Energy-Flow algorithm with single particle identification was demonstrated. Jean-François could not be present, but he received soon after, in the Kazimierz Symposium in Warsaw, a telex with the following words: 'Génial. On a 7 GeV de mieux. Mine (très) déconfite.'* This telex is still displayed, 15 years later, in both my and Jean-François' offices, to remind us of the good old times...

The successes of the Energy-Flow algorithm were not limited to the sole search for the Higgs boson. With its superior jet energy and angular resolution, it led to improvements in almost all Aleph analyses, e.g., b-tagging algorithm, heavy-flavour-decay reconstruction in the semileptonic final state, $\tau^+\tau^-$ event selection, searches for supersymmetry. In many instances, it allowed the Aleph results to surpass those of the other three experiments.

In 1991, the algorithm was further modified thanks to an improved photon identification provided by GAMPEC, written by Jean-Claude Brient and André Rougé in the Ecole Polytechnique. The philosophy of GAMPEC was to search for correlated maxima in at least two stacks of the ECAL, made possible by the excellent granularity of the electromagnetic calorimeter. A great gain in photon selection efficiency and purity followed. The substitution of GAMPEC for EBNEUT in the Energy-Flow algorithm had a dramatic effect: the energy resolution in Z hadronic decays improved from 8 to 6 GeV, and the efficiency of all Aleph analyses increased accordingly. This was yet another proof that the individual particle identification, and therefore the detector granularity, is the key issue for the energy-flow determination, rather than the energy resolution of the calorimeters. Had the hadron calorimeter granularity allowed a perfect identification of the neutral hadrons (which could only be selected with a 50% efficiency and a 50% purity with the Aleph design granularity), the energy resolution in hadronic Z decays would have dropped to 3 GeV. This observation is the basis, in particular, of the current detector design for TESLA.

^{*} Great. We are 7 GeV better. A few looked (very) downcast.

The Energy-Flow algorithm did not undergo further major changes until the end of Aleph's life. With this tool in hand, and with the powerful btagging algorithms developed for LEP1 as well, the leadership of Aleph was confirmed in most of the analysis fields over and over again, including during the second phase of LEP. For example, the Aleph Higgs boson search remained with the most efficient and the purest of the four LEP experiment selections, whether simple cuts or more sophisticated neural networks were used. If the 115 GeV/ c^2 hints are confirmed in the future, this Aleph success, among many others, will be that of a visionary detector design and of this new kind of energy-flow philosophy.

(Editor's note-RS: Those of us pushing for the best possible 'energy-flow' measurement in the linearcollider detector have gone through a hair-tearingout time with this concept. (1) Many people didn't understand what it meant—at first, then (2) different people were understanding different things when talking of 'energy-flow' and finally (3) many were not convinced that this is the best technique for measuring jets—at first.

(1) There was competition with other methods for measuring jets and there was the general sentiment for a time that compensating calorimetry alone—no tracking needed—was the best way to do the job. (2) Some would think of energy flowing into different parts of an event or of the detector caused by the underlying physics, e.g. the HERA experiments use the term that way. What WE and meanwhile most people understand is—you have a jet: what is the best way to measure it, regardless of the physics causing it?

(3) The detector has to be suitable for using this technique of course, and when this is the case, then for the vast majority of events, the charged particles are best measured by the tracking, the electromagnetic component by the Ecal and the remaining neutral-hadronic component by the Hcal. That this is better than e.g. compensating calorimetry for almost all events can be shown by a back-of-the-envelope calculation. Once you've got a suitable detector, the name of the game is to avoid double counting of the energy while at the same time losing as little information as possible due to overlapping signals arising from different particles.

In the end, what has been decided by the linearcollider community is to call it 'particle flow', since the technique involves reconstructing as many of the individual particles as possible of an event and then merging the information. However, many still use the term 'energy flow', so the confusion will continue for a while...)

HIGGS STORY (The cuts-stream perspective) Gavin Davies/Pedro Teixeira-Dias

In retrospect, one could say that the start of LEP2 was rather gentle: in 1996, despite running at what were then excitingly high energies of 161 and 172 GeV, only about 10 pb^{-1} of data were accumulated at each point, by each experiment. However, in the following years LEP went on to deliver large amounts of data at 183, 189, 192, 196, 200 GeV, and well beyond. For the Higgs hunters, such performance was a dream come true.

Every year the mass reach was significantly enhanced by virtue of the increase in the centreof-mass energy. The possibility of discovery was therefore not far from anyone's mind. In the Higgs Task Force (HTF), as well as in the wider Aleph community, it was felt that we should be fully prepared for any eventual discovery. Thus, rather than rely on only one or the other Higgs search (the dilemma was between cuts and neural networks (NN)), the collaboration elders pressed for having both.

Historically, it took some time to achieve this goal. The first Aleph Higgs paper at LEP2 (161/172 GeV) used only cut-based selections. In the following years (183 and 189 GeV data), the cuts analyses were published, as well as new NN selections for the four-jet, missing energy and tau channels. Only in 1999 was true parallelization of the analyses achieved: for the first time all results in the paper appear in duplicate: cuts-stream and NN-stream.

The HTF operated an 'open-source' policy regarding the searches: the code for the event selection in each channel was posted on the Web and accessible to all in Aleph. This meant that the searches were well scrutinized (read 'debugged'). It also encouraged a wide participation (read 'competition') towards the development of each search channel, which led to highly performing searches. The culture was one of extremely hard work (nights? weekends?... made for work!) and professionalism.

Within the HTF we strictly adhered to the policy that all analyses had to be frozen before data taking. Thus analyses were improved and optimized on Monte Carlo using the expected accelerator performance for the following year. For the 1999 and 2000 data-taking, automatic combinations of all these 'frozen' channels were provided via 'Behold' (invented by the Wisconsin group—see following story) and visible to the whole collaboration on the Web.

Through the hard and excellent work of far more people than could be named here, all of these objectives were for the first time simultaneously achieved by the start of the 1999 run.

We were ready.

THE CUTS-STREAM

In the cuts-stream, three of the four search channels were treated with a cut-based selection. (In the NN-stream, three of the four search channels were treated with a NN.) These were the four-jet, the missing energy, and the leptonic channel. The tau final states were the trickiest, as usual, given their more ambiguous signature (always hesitating between jets and leptons, with missing energy to boot) and were best treated with a NN. The cuts analyses, despite being simpler, were still competitive when compared to their neural network counterparts. For instance, in the four-jet case, the cuts selection had only 10% more background than the NN selection, for the same signal efficiency (or say 'with expected exclusion limits less than a 0.5 GeV lower').

The two streams differed in another important aspect: the discriminant variables (or rather 'the shapes', as everyone in the HTF referred to them... much to the despair of Alain Blondel, who once reminded us that this concept had been invented 'already some time ago' and went by the more widely-recognised name of probability density function, pdf). In order to improve the signal-to-background separation, in addition to the total signal and background rates, we used the distribution (the 'shape') of some variables, such as the reconstructed Higgs mass, the b-tagging output, or the NN output. The cuts-stream used only the reconstructed Higgs mass for each channel (the leptonic channel was the exception: in order to not reduce further the already low expected signal rate, no b-tagging cut was applied. The b-tagging

had instead to be used as a discriminant, in addition to the reconstructed Higgs Mass), whereas the NN-stream used less straightforward two-dimensional discriminants: e.g., the reconstructed mass and the NN output, in the case of the four-jet channel.

The two streams are compared in the table below. In essence, the cuts-stream relies mostly on cut-based selections and onedimensional discriminants, whereas the NN-stream relies mostly on NN selections and two-dimensional discriminants. The cuts-stream also provided an ideal testing ground for promising event selection variables. An example of this is the Higgs and Z decay angles which were demonstrated to be of value in the fourjet cuts-stream, and were subsequently adopted for inclusion in the four-jet NN.

THE YEAR 2000

And so to 2000 data-taking. The HTF was running like well-oiled machinery: blind analyses, optimized before data-taking, automatic channel combinations provided online via Behold, full parallelization between the cuts and NN-streams, experienced teams of people running each search channel and the channel combinations... All this, and luminosity at unheard of energies.

The stage was set. It is hard to express the expectation/excitement of that year.

The first few months of data taking were filled with the standard Monte Carlo vs data comparisons, b-tag checks and checks on the pdfs and their interpolation. Recall that in 2000, rather than operating at a few set centre-of-mass energies, LEP delivered data over a 'continuum' of energies, as it pushed the boundary of the energy–luminosity envelope. This turned the preparation, testing, and interpolation of the discriminant variable pdfs into a minor industry within the HTF.

Channel	Cuts-stream	NN-stream	
Four-jet, Hqq	cuts; $X = m_{rec}$	NN; $\overline{X} = (m_{rec}, NN_{output})$	
Missing energy, $Hv\overline{v}$	cuts; $X = m_{rec}$	NN; $\overline{X} = (m_{rec}, NN_{output})$	
Leptonic, $H\ell^+\ell^-$	cuts; $\overline{X} = (m_{rec}, b\tau$ -tag)		
Taus, $\tau^+\tau^-q\bar{q}$	NN; $X = m_{\rm rec}$		

The two analysis streams: 'cuts' and 'NN' denote the type of event selection used for the given search channel. The observables X indicate the discriminant variables used for the calculation of the confidence levels. The Hℓℓ and ττqq analyses are treated in exactly the same way in the two streams. Towards the end of June, almost as soon as the machine first reached 206.7 GeV, came our first exciting candidate, a four-jet (bbqq) event with a reconstructed Higgs mass $m_{\rm rec}$ of 114 GeV, and a neural net 'score' of 0.997 out of a possible maximum of one. This 'golden' candidate (known in the papers as 'candidate c') surely went on to be the most studied event at LEP!

In early July both analysis streams registered another interesting candidate ('e'), again at 206.7 GeV centre-of-mass energy and in the fourjet channel, with $m_{\rm rec} \approx 115$ GeV, but a lower NN score of 0.820. Thanks, in the main, to these two candidates our observed limits on the Higgs mass



Candidate 'c' 54698 4881.



Candidate 'b' 56065 3253.

at the 20th of July LEPC were 1.2 and 1.6 GeV below expectation for the NN and cuts-streams respectively. The discrepancy between the expected and observed mass limits would eventually grow to more than 3 GeV.

Right from the beginning the significance of the excess in the two streams tracked each other, despite responding to individual candidates in quite different ways. Whilst both streams are sensitive to the centre-of-mass at which a candidate is recorded, the cuts-stream uses only a 1-D discriminant ($m_{\rm rec}$) whereas the NN stream used a 2-D discriminant (such as $m_{\rm rec}$ and the NN output). Thus the two high-mass candidates

> mentioned above contributed almost equally to the 1.6 GeV discrepancy in the cuts-stream, whereas the 1.2 GeV discrepancy in the NN excess was driven almost exclusively by the golden event.

> Then, right at the end of July, too late for the LEPC came candidate 'b', with $m_{\rm rec} \approx 113$ GeV and an NN output of 0.999: it was clearly going to make itself felt. And it did! Both streams had 3 GeV discrepancy between observed and expected limit, with our incompatibility with background around 2.5 sigma.

> And so began a truly great summer! In quick succession in August came candidates 'a' and 'd'—forget limits!

> For the HTF meeting at the end of August, just prior to the September LEPC we had 3.7 sigma excess in the cuts-stream, with a similarly significant discrepancy in the NN-stream.

During those heady days the cuts-stream really proved its worth, as, being simpler in its philosophy, it made consistency checks of the excess simple. Furthermore, as it used a 1-D discriminant rather than the 2-D discriminants of the NN-stream it was immune from the necessarily complicated treatment of the correlations between the two discriminants in the case of the latter. The fact that, despite the different treatments (event selection technology, discriminant variables), the significance of the excess in both streams tracked each other was of great reassurance.

As was to be expected, there was much debate of the excess at the September Aleph Week, that year in Aix-en-Provence. It almost seemed as if nature was trying to tell us something. Most of us travelled down during the worst night of storms for a century, which caused floods, roads closed, trains stopped... Quite spookily, when we got to the hotel we found that our rooms numbers were 114 (PTD) and 105 (GJD)... corresponding to the masses in GeV of the most prominent candidates in the channels we were respectively working on! (Now what is the probability of THAT?) It was in Aix that the 'final sprint' was planned: what checks still needed to be done, by whom, etc. It was also in Aix that we decided to publish a 'rapid paper'. The plan was to submit it within two weeks of the end of the data taking in November, if no showstoppers were found in the mean time.



Pedro slaving away on the Higgsometer at Aix, and the final published version was...

By the time of the November LEPC neither stream had new high-mass candidates; but both streams were still above 3 sigma. (The data taken since the September LEPC was 60% compatible with background, and 40% compatible with signal+background.) The continued checks had convinced us the excesses were robust—this was either really it, or a cruel fluctuation in background.



Candidate 'a' 56698 7455.

The two weeks after the end of data-taking were an emotional roller-coaster for all those that felt that LEP deserved a stay of execution. One could endlessly debate the way in which each experiment approached this exciting period, and the decision to not run in 2001, and indeed the way in which it was made... but that's higgstory (uh...we mean history), as they say. Aleph published a 3 sigma excess, around a Higgs mass of 115 GeV, observed in both analysis streams in December of 2000. Twelve months later, after more detailed analysis of the data (including full reprocessing of all events, final centre-of-mass energies, additional Monte Carlo, study of beam-related backgrounds, and a complete study of the systematics) our final paper in 2001 confirmed the excess in both streams, with an estimated uncertainty of ≈0.2 sigma. If our sensitivity to a Higgs of 115 GeV reflects the hard work of the LEP accelerator and Aleph online and detector folk, then the consistency of the excess reflects the commitment and professionalism of all.

Let's hope that excess was the first sign of the Higgs...



Draft for a recommendation for an additional year of running in search of the Higgs.

HIGGS STORY (The Wisconsin perspective) Steve Armstrong/Peter McNamara/Sau Lan Wu

PERSONAL RECOLLECTIONS OF THE WISCONSIN GROUP ON HIGGS IN ALEPH

In a large collaboration such as Aleph, success is contributed by a large number of physicists from a large number of institutions. Over the course of the many years of the Higgs saga, many people were involved, some of whom we competed with and some of whom we came to collaborate with. In this reminiscence of the Higgs saga of the Aleph Experience, we are restricting ourselves largely to our own recollection of the activities of the Wisconsin group led by one of us (Sau Lan Wu) on the Higgs studies. Hereafter in the following story, we authors will be referred to simply by our initials, S.A., P.McN. and S.L.W.

In 1980, the Wisconsin group joined forces with a number of physicists led by Jack Steinberger, and later named ourselves the Aleph Collaboration (see story on Letter of Intent). We have enjoyed thoroughly our more than two decades in this Collaboration, from the initial design of the detector (we were heavily involved with the TPC design and construction) to the present. Perhaps the main reason is the very exciting discoveries in physics during this period.

The excitement began shortly after the turnon of LEP in 1989: it was determined from the line shape of Z that there are precisely three light neutrinos, within the constraints of the standard electroweak model. Wisconsin postdoc John Harton and graduate student Jim Wear carried out one of the two parallel analyses with the hadronic events using only the charged tracks in the TPC (the TPC track reconstruction was developed at that time by Wisconsin postdoc Robert Johnson) to fit for the line shape of the Z resonance and the electroweak parameters, while the other analysis made use of the calorimeters. It is not surprising, but nevertheless satisfying, that the two analyses gave the same answer.

Within the Aleph effort to measure the electroweak parameters, the Wisconsin contribution was pursued by postdocs John Conway, Saul Gonzalez, Michael Schmitt, Julian von Wimmersperg-Toeller and John Yamartino, graduate students Zheng Feng, Douglas Ferguson, Jim Grahl and Michael Walsh, and S.L.W.

A couple of years later, two new hadrons were discovered in Aleph: Λ_b and B_s^0 . This work was carried out mostly by Wisconsin postdoc Vivek Sharma and graduate student Joleen Pater (see that story 'B_s and Λ_b Discovery' by Vivek Sharma above).

Other B physics studies were vigorously pursued in parallel by Wisconsin postdocs Robert Johnson and Ian Scott, and graduate students Steve Armstrong, Leo Bellantoni, David Cinabro, Peter Elmer, Yongsheng Gao, Owen Hayes and Fred Weber. It is probably fair to say that one of the greatest excitements at LEP concerns the Higgs particle. In the Standard Model, there is one particle, the Higgs particle, which is responsible for giving mass to all particles with mass. In this sense, the Higgs particle occupies a unique position.

S.L.W. chose the search of the Higgs boson to be the primary challenge for her and some members of the Wisconsin group at LEP as soon as the Aleph Collaboration was formed. Many members of the Aleph Wisconsin group have contributed significantly to this effort.

In 1983, S.L.W. took the job of answering question 6 of the LEP Committee (LEPC):

'What strategy with respect to data acquisition and analysis would you follow to search for Higgs in Z⁰ decay? Suppose one needs $10^7 Z^0$ decays to observe 10 events of the type $Z^0 \rightarrow e^+e^-H$ with $H \rightarrow$ hadrons, $M_H = 50$ GeV.'

Since the software of Aleph at the time was in a primitive state, we had to produce all the tools necessary for this study, for example, to produce a simple version of detector simulation and event generation for Higgs production and decay. An Aleph note (No. 120 December 21, 1983) was written by the Wisconsin group (Michael Mermikides, Haimo Zobernig, Eric Wicklund, S.L.W.). We worked so hard that after the note was completed, S.L.W. said to Haimo, 'if we already feel burned out now, what happens when LEP turns on?'

In the fall of 1984, the LEPC decided that serious and detailed study of LEP physics was required and formed five topical working groups: New Particles, Toponium Physics, Precision Z Measurements, QCD and Heavy Quark Physics, and High Energy LEP2 Physics. The Wisconsin group represented the Aleph Collaboration in the New Particles working group and contributed a major portion of the work for that section. The New Particles section was subdivided into three parts: Higgs Particles, Supersymmetric Particles and Exotics. Our group was responsible for the Higgs section, and we also contributed extensively to the section on Supersymmetric Particles. The work of our group was done by S.L.W., Haimo Zobernig, and graduate student Steven Ritz. The results were included in CERN Yellow Report (CERN 86–02).

Our contributions to the groundwork for Higgs searches at LEP did not end with the LEPC working group studies as discussed above. In 1986, the European Committee on Future Accelerators (ECFA) Workshop in Aachen, Germany, on LEP2 physics was organized to address physics which could be achieved with LEP2. The work on this topic was again divided into working groups. S.L.W. was the co-ordinator of the Higgs working group, and she presented the conclusions of the workshop which helped to justify the upgrade of LEP1 to LEP2.

In our studies, we concentrated on looking for the Higgs boson through the Higgsstrahlung process, $e^+e^- \rightarrow HZ$, where the Z is virtual at LEP1 and real at LEP2. A considerable amount of preparatory work was necessary to simulate these processes realistically. Haimo Zobernig modified an HZ generator, written by Ronald Kleiss, to include all Z decay modes. Graduate students John Hilgart and Steven Ritz wrote a full event generator for the W+W⁻ and ZZ background processes. We also developed a fast, but realistic, detector simulation, and a novel method in which to store these simulated data.

The results of this study were encouraging. They were presented by S.L.W. in detail in the Aachen workshop in September 1986 and were then published: 'Search for Neutral Higgs at LEP 200' by S.L.W. in the Proceedings of the ECFA Workshop LEP 200, September 1986, Aachen, W. Germany (also CERN-EP 87/40). The conclusion of this study was that if the neutral Standard Model Higgs boson existed with a mass below 90 GeV/ c^2 , LEP2 is the cleanest place (compared to possible ep, pp, or pp machines) to look for it, and Aleph would certainly find it.

The results of our studies were also very useful in another aspect: our group developed several new and powerful techniques for jet analysis which we published separately in 'Jet Analysis in Higgs Search', J. Hilgart, M. Mermikides, S. Ritz, S.L.W., and H. Zobernig, *Zeitschrift für Physik* C**35** (1987) 347. These new jet analysis techniques, developed from ideas of S.L.W. and Haimo Zobernig, permit the reconstruction of massive states which decay into jets of hadrons and have been widely used because they help to overcome the inherent errors in jet measurements due to missing particles.

LEP1 data-taking started in August 1989. Members of our group (John Hilgart, Yibin Pan and S.L.W.) worked on the channels $Z \rightarrow Hv\overline{v}$ and $Z \rightarrow H\ell^+\ell^-$. Our work was a major contribution to the publication 'Search for the Neutral Higgs Boson from Z Decay' (CERN-EP/89-157, December 1, 1989 and *Physics Letters B* 236 (1990) 233), which gave a lower limit of Higgs mass of 15 GeV/ c^2 at 95% Confidence Level.

In a parallel effort, graduate students Leo Bellantoni, Douglas Cowen and Joleen Pater obtained results on the charged Higgs searches. The Wisconsin group also launched a significant effort on SUSY searches: Wisconsin postdocs John Conway, Saul Gonzalez and Michael Schmitt, and graduate students Joe Boudreau, Jane Nachtman and William Orejudos.

On 3 January 1990, S.L.W. was scheduled to give an invited talk 'Search for Neutral Higgs Bosons' at the plenary session of the Annual Meeting of the Division of Particles and Fields of the American Physical Society in the U.S. On 22 December 1989, the Opal Collaboration submitted a CERN preprint giving a result of the lower limit $M_{\rm H} > 19.3 \text{ GeV}/c^2$ at 95% Confidence Level. S.L.W. convinced Yibin Pan, then a Wisconsin graduate student, to stay over Christmas to analyse the newly acquired data and to make an update on the Higgs mass for her talk. Jack Steinberger as Aleph spokesperson gave her permission to report the new Higgs lower limit of 24 GeV at the APS/DPF meeting. This study resulted in the publication 'Search for the Neutral Higgs Boson from Z Decay in the Higgs Mass Range between 11 and 24 GeV' (CERN-EP/90-16, January 31, 1990 and *Physics Letters B* 241 (1990) 141).

Life went on with the Higgs search in LEP1, but background became increasingly severe as the Higgs mass became heavier. LEP2 turned on in 1995, with the initial centre-of-mass energy of 130 GeV and reached 172 GeV in 1996, 184 GeV in 1997, 189 GeV in 1998 and 202 GeV in 1999. Centre-of-mass energies ranging from 200 to 210 GeV were achieved in 2000, the final year of LEP data-taking. At LEP2, the production process for Higgs is Higgsstrahlung $e^+e^- \rightarrow HZ$ where both H and Z are on-shell and the Higgs decays predominantly to two b-jets. This process provides a cleaner environment than at LEP1. Our group pioneered the b-jet tagging using a neural network, which gave superior performance. Yibin Pan, by then a postdoc in the Wisconsin group, and two of us, S.A. and P.McN., then graduate students, were the central figures in this b-tagging effort.

Intensive efforts were made by our group to cover all channels; i.e.

- (1) Two-lepton channels: $H \rightarrow b\overline{b}$, $Z \rightarrow \ell^+ \ell^ (\ell^{\pm} = e^{\pm}, \mu^{\pm})$ (Wisconsin graduate student Tom Greening)
- (2) Missing energy channel: $H \rightarrow b\overline{b}$, $Z \rightarrow v\overline{v}$ (Wisconsin postdoc Yibin Pan, and graduate students Jennifer Kyle and Xidong Wu; a study of four-fermion processes with the LEP2 data by Wisconsin postdoc Saul Gonzalez paved the way for understanding this channel.)
- (3) Two-tau channels: $H \rightarrow b\overline{b}$, $Z \rightarrow \tau^+\tau^-$ and $Z \rightarrow q\overline{q}$, (Wisconsin postdocs Yibin Pan and Ian Scott, and graduate student Jason Nielsen)
- (4) Four-jet channel: H → bb, Z → qq (S.L.W., Wisconsin postdocs S.A., Yuanning Gao, Hongbo Hu, Shan Jin, and graduate student Jinwei Wu)

At LEP2, the major decay modes for the H and the Z are respectively $H \rightarrow b\overline{b}$ and $Z \rightarrow quarks$. Since the branching ratios are between 70% and 80%, the process (4) in the above list has the major advantage of a large branching ratio of over 50%, but also a relatively large background.

For the two-lepton channels, a cut analysis was used. For the channels $Z \rightarrow \nu \overline{\nu}$, $\tau^+ \tau^-$, $q\overline{q}$, our group first extensively developed cut analyses, in particular, Yibin Pan, Ian Scott and Xidong Wu had developed the cut analysis for $Z \rightarrow \nu \overline{\nu}$ and $Z \rightarrow \tau^+ \tau^-$, and Shan Jin, S.A. and S.L.W. had developed the cut analysis for the $Z \rightarrow q\bar{q}$ channel. Using these cut analyses, we then successfully developed and applied Neural Network techniques to enhance the performance. One characteristic of Higgs at LEP2 is that the expected number of Higgs events produced is small and therefore sophisticated and powerful methods in statistical treatment are necessary. A new approach to calculating confidence levels was conceived by the Wisconsin group (Hongbo Hu and Jason Nielsen). This analytic method uses a Fast Fourier Transform (FFT) to find the behaviour of a large group of hypothetical experiments, given the expected behaviour of one of those experiments. Since the time necessary to calculate confidence levels with this algorithm is nearly independent of the number of expected events, the calculations and combination can be completed in less than 1% of the time needed with the old method; this new method has revolutionized the confidence level calculation for Higgs searches within Aleph. The algorithm (referred to as CLFFT) was implemented in a complete library with a flexible interface allowing customization for each event selection. This library was tested by P.McN. and Jason Nielsen using event selections at $E_{\rm cm}$ = 189 GeV. Graduate student P.McN. was chosen by the Aleph Collaboration to be its representative in the LEP Higgs Working Group.

In the course of study of the analytic confidence level technique, P.McN. discovered that the combination procedure used previously by Aleph was significantly sub-optimal when background is subtracted. This contributed to the rapid adoption of the new Wisconsin confidence level calculation technique within Aleph. During 1999 the Wisconsin Fast Fourier Transform confidence level calculation package became the standard used throughout Aleph Higgs searches (including charged and invisibly decaying Higgs boson searches). It was also used by the LEP SUSY Working Group. The method was published as 'Analytic Confidence Level Calculations Using the Likelihood Ratio and Fourier Transform', Hongbo Hu and Jason Nielsen in High Energy Physics and Nuclear Physics 24 (2000) 445.

In 1999, S.L.W. had the idea that it would be great if we had a chain of analyses so that each day the data are analysed and results (limit or discovery significance) are available immediately. S.A. and Jason Nielsen then started writing the code for what became the birth of the package 'BEHOLD!' (BE Higgs Online Limit and Discovery). They developed a series of automatic tasks which ran in an online fashion during the 1999 Aleph data-taking period and again during the 2000 Aleph data-taking period. The implementation of this system BEHOLD! required a profound understanding of all aspects of Aleph Higgs analyses. These tasks immediately apply all of the final-state event selections developed for the various Standard Model Higgs search channels $(b\overline{b}\ell^+\ell^-, b\overline{b}\nu\overline{\nu}, b\overline{b}\tau^+\tau^-, \tau^+\tau^-q\overline{q}, b\overline{b}q\overline{q})$ as well as those for the neutral Higgs bosons of the MSSM. Combined confidence levels indicating the level of exclusion or discovery of the Higgs boson were calculated, and the results were accessible via the World Wide Web to the Aleph collaboration.

This BEHOLD! procedure for Higgs limit and discovery was viewed as a top priority by the Aleph leadership for 1999 and 2000. Since the discovery of the Higgs boson was the primary goal of LEP2, our BEHOLD! results were monitored closely on a daily basis by as many as 50 of our Aleph collaborators.

The success of the BEHOLD! system during 1999 and 2000 was unquestioned. It was cited as a most impressive achievement during the plenary meeting in the external Aleph Week at Siena, Italy in September 1999 by Aleph spokesperson Peter Dornan. BEHOLD! allowed an extremely rapid extraction of results, typically within 24 hours (previously such efforts involved six people working as long as three weeks). This allowed preliminary results based upon the full data sample to be presented during the November 1999 LEPC meeting. Furthermore, BEHOLD! allowed a valuable cross-check of results to be accomplished at multiple points during the year, making the Aleph results more robust. It was, however, during the analysis of the high-energy data in 2000 that BEHOLD! demonstrated its power. The automatically generated results were the first



Stephen Armstrong, Sau Lan Wu and Shan Jin in Siena, Italy during the Aleph Collaboration Meeting, September 1999.

indication of the excess present in the data, and the early warning allowed the collaboration to begin making checks of the data and the analyses to confirm the result. Later, BEHOLD! was used to produce the final results for the Aleph publication just days after data-taking was completed.

After the centre-of-mass energy of LEP reached 205 GeV in the year 2000, excess candidates began to show up in the Standard Model Higgs analysis, as first seen with BEHOLD! As expected, the excess is especially notable in the channel where both the H and the Z decay into two jets, leading to a four-jet final state.

On 14 July 2000, a 'golden' event (run 54698, event 4881) with extraordinary signal-like characteristics in the four-jet channel was observed in the Aleph data by the BEHOLD! system. In this event, the H decays into two unmistakable bjets, while the Z decays into two non-b jets. The Higgs mass was found to be $114.3 \text{ GeV}/c^2$. Using the four-jet analysis as pioneered, implemented, and maintained by the Wisconsin group, S.A., Yuanning Gao, Hongbo Hu, Shan Jin, P.McN., Jason Nielsen, Jinwei Wu and S.L.W. used this unprecedented opportunity to examine this event in great detail and investigated similar events in the data to ascertain that no systematic bias or uncertainty could explain it. (Editor's note-RS: The 'golden' event is shown here again (see below) because it is so nice; see the preceding story about the 'Cuts Stream' for all three most significant Higgs event displays.)

As data taking continued in 2000, this event was complemented with the observation by BEHOLD! of two more golden four-jet Higgs events (run 56065, event 3253 recorded on 30 July and run 56698, event 7455 recorded on 21 August) which when combined together resulted in a distribution in excess of background expectation in the reconstructed mass region from 109 to 115 GeV. These events were also identified as very signal-like by the Wisconsin four-jet neural network analysis and also by the Aleph cut analysis (see 'Higgs Story (The Cuts-Stream Perspective) by Gavin Davies/ Pedro Teixeira-Dias preceding this one).



The 'golden' event (known in the papers as 'candidate c')





The confidence levels for background-only hypothesis using the mass with other discriminating variables from BEHOLD! were:

and the corresponding significance distributions (from the special BEHOLD! Discovery Page):

On 5 September 2000, Dieter Schlatter, the Aleph Spokesperson, was scheduled to give a talk at the LEPC to announce the Aleph finding. Several days prior to his talk, he was in our Wisconsin corridor on an hourly basis, and we supplied him with numbers, figures, and details for his talk. The news had generated world-wide excitement. The Wisconsin group documented the observation in an Aleph Note 2000-079, Physic 2000-026 on 13 September 2000: 'Observation of Higgs candidates with masses around 114 GeV/ c^2 in four-jet events at $\sqrt{s} > 205$ GeV using neural network techniques' (by S.A., Kyle Cranmer, Yuanning Gao, Hongbo Hu, Shan Jin, Jennifer Kyle, P.McN., Jason Nielsen, Yibin Pan, Jinwei Wu and S.L.W.). At the suggestion of the Aleph spokesperson, the title of this Aleph note was later changed to 'Studies of Higgs candidates with masses around 114 GeV/ c^2 in four-jet events at $\sqrt{s} > 205$ GeV using neural network techniques'. At the time of the LEPC on 5 September 2000, Aleph was the only experiment to observe a significant signal for the Standard Model Higgs. The four LEP experiments unanimously called for an extension of LEP running in order to give LEP an opportunity to confirm this exciting observation.

In the days following the special session, debate centred on the question 'Why is Aleph the only experiment which has observed these events?' A comparison of the performances of the fourjet analyses of the various LEP experiments demonstrated that the Aleph four-jet analysis was significantly more sensitive than the next most sensitive analysis, which was the four-jet Higgs analysis at Delphi. In the wake of this comparison, the Delphi experiment updated their analysis using techniques based on KEYS, the work of Wisconsin graduate student Kyle Cranmer. This reanalysis improved the Delphi Higgs sensitivity by 1.4 GeV, which helped them to find a strongly Higgs-like candidate in the region of 114 GeV.

The Delphi reanalysis helped the LEPC to decide to extend LEP operations into November 2000. In the wake of the LEPC meeting and their decision to extend LEP, the search for the Higgs boson received a great deal of cautiously optimistic coverage in the press. The observations were described as 'tantalizing Higgs hints' (Physics Today), which may allow us to answer 'one of the most fundamental questions we can ask' (New York Times). While finding the Higgs boson was described as 'vitally important' (BBC News), the chances of confirming the Higgs were being weighed against the possible future financial impact on the LHC programme. The extension of LEP into November 2000, rather than December as requested by the LEP experiments, was due to the fact that such an extension would 'delay LHC civil engineering work' (minutes of LEPC meeting) and have a potentially large impact on the LHC schedule.

	Expt	E _{cm} (GeV)	Decay channel	$M_{\square}^{\rm rec}$ (GeV/ c^2)	ln(1+ <i>s/b</i>) ¹ 115 GeV/ <i>c</i> ²
1	ALEPH	206.7	4-jet	114.3	1.73
2	ALEPH	206.7	4-jet	112.9	1.21
3	ALEPH	206.5	4-jet	110.0	0.64
4	L3	206.4	E-miss	115.0	0.53
5	OPAL	206.6	4-jet	110.7	0.53
6	DELPHI	206.7	4-jet	114.3	0.49
7	ALEPH	205.0	Lept	118.1	0.47
8	ALEPH	208.1	Tau	115.4	0.41
9	ALEPH	206.5	4-jet	114.5	0.40
10	OPAL	205.4	4-jet	112.6	0.40

Some properties of the ten most significant Higgs candidates

¹ (s/b signal over background).

Weighing the evidence, the LEPC found that 'the combined evidence for a Higgs near $115 \text{ GeV}/c^2$ already to be quite significant' but noted that an extension of LEP 'could have a serious impact on the LHC' (LEPC Minutes) and was unable to decide whether LEP should be extended into 2001. The situation was called 'a fairly pleasant emergency' by CERN Director-General Luciano Maiani (LA Times), who briefly postponed the decision to permanently shut down LEP. The scientists' side of the debate was summarized by S.L.W., who argued 'It's within our reach. We should have the chance' (LA Times). The situation was compared to the US election, as in the article 'Far from Florida, a cliffhanger recount in Physics' (NY Times).

What was the situation with the experimental evidence for the Higgs from LEP? The 10 most signal-like events from the four LEP experiments are listed in the Table above [P.McN. and S.L.W., Rep. Prog. Phys. **65** (2002) 465]. This table gives a summary of the major sources of our present knowledge about the first possible experimental observation of the Higgs boson. From these and similar events, the most likely mass for the Higgs boson is $115 \text{ GeV}/c^2$.

The closure of LEP, unfortunately, made it impossible to obtain further Higgs candidates at LEP. As graduate student Jason Nielsen was quoted saying, 'Among Physicists, we believe we have them. But we don't believe we have enough of them' (LA Times). Although intensely disappointing, the decision to close LEP leaves the task of confirming the discovery of the Higgs boson to the present Tevatron experiments at Fermilab or the future LHC experiments at CERN.

THE W MASS STORY (The professor's perspective) John Thompson

The precise measurement of the W mass is recognized as one of the principle aims of the LEP2 programme. Before 1996, there were numerous LEP-wide physics workshops studying in depth the analysis methods and likely systematic uncertainties. Combining the results from all four experiments, the hope was that a precision of 25 MeV/ c^2 could be achieved so that the mass of the Higgs could be constrained to be less than 200 GeV/ c^2 from the Standard Model global fits. During this time, a 'WW working group' convened by Alain Blondel was formed with members from many institutes (>10). We participated actively in these workshops and were fully prepared when the time came for data taking.

The real story began in 1996 when the energy of LEP beams exceeded the mass of the W boson for the first time. At 161 GeV, just above the Wpair production threshold, 11 pb⁻¹ were collected and analysed in all decay channels using several methods, such was the enthusiasm to have the best possible result. The handful of purely leptonic and semileptonic events was straightforward to identify whereas the fully hadronic (4q) channel suffered from a large background. For some of us, this was our first introduction to neural networks in event selection and we were unconvinced they worked until it was shown that more conventional discriminant analyses gave similar results. Using our measured W-pair cross-section, we achieved a precision of 350 MeV/ c^2 in the mass dominated completely by the statistical error, but entirely consistent in value with expectations. Needless to say, this was comfortably the most accurate measurement of the four experiments. We were greatly encouraged, even though this was far from

the ultimate goal. Some of us wanted to spend more time taking data at 161 GeV rather than what seemed to be a futile exercise at 172 GeV, where the integrated luminosity would be too limited to make a useful measurement using the direct reconstruction of the decay products. This thwarted intention returned years later when a window of opportunity in 2000 was foreseen if the LEP klystrons failed to reach the performance demanded by the Higgs search. The idea was to take more data at 161 GeV so that we could make a more serious comparison of the mass measurement in the 4q channel from direct reconstruction with that derived from the cross-section. Evidence had been accumulating from particle flow studies that 'colour reconnection' due to non-perturbative gluon exchanges between the W decays could substantially modify the event topologies and hence the reconstructed masses, whereas the total cross-section is largely unaffected. However, again the opportunity was lost to add to our statistics at 161 GeV as the klystrons performed beautifully. In retrospect, it was the best outcome.

A relatively small sample of data was taken in 1997 at 183 GeV from which a mass measurement was published. This analysis tested our event reconstruction, kinematic fitting and reweighting procedures for the final mass extraction. However, the largest single sample at one LEP energy came from 174 pb⁻¹ collected in 1998 at 189 GeV which demanded for the first time a thorough analysis of all systematic uncertainties applicable to the direct reconstruction method. When published, we achieved a statistical precision of 61 MeV/ c^2 using all measurements combined. However, the systematic uncertainties, quoted as 47 MeV/ c^2 ,

were becoming more important owing mainly to the coherent effect of quark fragmentation and finalstate interactions specific to the 4q channel. Both of these effects were evaluated entirely with Monte Carlo models. For the latter, colour reconnection remained as a particular problem since none of the models could make a solid prediction of the scale of the effect on the mass. Guided by theorists that a reconnection 'probability' of 30% was reasonable as an upper limit, the worst-case prediction of 30 MeV/ c^2 from the so-called SK1 model was applied. This had the effect, together with Bose-Einstein correlations, also unknown at that stage, of suppressing the contribution of the 4q channel to the final result. This was unfortunate, since otherwise its contribution was pre-eminent. The other three experiments were more conservative in their interpretations of these uncertainties which led to some interesting exchanges at LEP W meetings.

By the end of LEP2 in November 2000, three times more data were accumulated for analysis than previously published, thanks to the excellent performance of the collider. This meant that the ultimate goal of 25 MeV/ c^2 was within our sights provided the stiff challenges of the dominant systematic uncertainties could be beaten. We were also facing the prospect of a diminishing band of experts and students available to tackle the long list of uncertainties, which would need to be quantified. We decided to subdivide the data into eight separate samples between 183 GeV and 207 GeV for separate (re)analysis, thus enlarging our task even more. Also, it was realized that we would need to generate $\sim 10^8$ fully simulated events even though the total number of selected W pairs turned out to be only 8717. As usual, Brigitte Bloch took this enormous task quietly in her stride. Once a common selection for all channels for both the cross-section and mass analysis had been agreed, guided by Anne Ealet, Stephane Jezequel produced the data and Monte Carlo ntuples needed for the final steps of the analysis and studies of systematics. Over the following four years, he would have to repeat the ntuple production process many times (>10) as errors were found and improvements made. I don't remember any complaints!

222

Our big shock came soon after the Leukerbad meeting in 2001. We had developed a series of special event reconstructions for the 4q channel where *either* low-momentum particles were progressively eliminated from the hadron jets as a whole or cones of decreasing radii applied to remove peripheral objects attached to the originally selected jets. Depending on the level present, all models predict that colour reconnection influences the distributions of these low-momentum particles and a progressive change in the W mass is expected for each reconstruction. To check for other possible sources of instability, it was decided to perform this series of analyses on all three semileptonic channels. It was discovered that the electron channel was particularly unstable, an effect not observed with the 189 GeV data alone. It took at least a year to fully evaluate the causes since there were few experts left with time available. For a long time we knew that the ECAL simulation had failed to explain the high multiplicity of lowenergy 'satellites' observed around electromagnetic deposits. This had not mattered in the past for almost all published analyses but now it had to be resolved. A million W-pair events were generated where the 'parametrized' electromagnetic showers in ECAL derived from old test beam data were replaced using an EGS simulation, which took account of coherent effects in the shower development. This was able to explain most of the 'satellite' production provided surprisingly large numbers of 'single stack' deposits seen in the data were removed. It was now clear that these neutral objects, confined to just one of the three stacks of ECAL, were anomalous but their origin is still not fully understood. Some of us remember 'random sparking', especially in the endcap modules, but the barrel builders always denied their existence. This may explain part of the single stack multiplicity; others may have come from halo muons parallel to the beams. In any case, their removal corrected the reconstructed jet masses. Unfortunately, the EGS simulation was unable to describe the lateral extent of the showers properly and required more work. In any case, we could not contemplate regenerating 10⁸ events using EGS. The only course was to compromise and increase the energy thresholds of the neutral objects included

in the jet reconstruction to at least 1.5 GeV so that the multiplicity spectra and 'jet boosts' in Monte Carlo then agreed well with the data. The penalty was a small degradation in the statistical precision of the mass measurement. Marie-Noelle Minard 'returned' to guide us in this work and provided a student, Renaud Brunelière, who made many invaluable contributions. In particular, his work on the spatial extent of activity around the 'isolated' electron in Bhabha events was the clue to extending the 'locking out' of such objects to 8° from the lepton direction for all semileptonic events. Previously, some of these objects had been included, mistakenly, in the original hadronic jets.

In early 2003, we were able to report these findings to a LEP W meeting and show that our masses from the semileptonic channels were stable. The other experiments duly took note and checked their ECAL reconstructions. They claimed to be safe from such problems, so our beautiful fine grained detector came to haunt us in the end. We also reported that our mass results were close to final. That was almost two years ago! Since then, most of the draft paper has been written and all systematic uncertainties evaluated satisfactorily *except* for colour reconnection. Fortunately, we found there is no evidence for Bose–Einstein correlations between the decay products of the two W's, limiting a possible mass shift to $6 \text{ MeV}/c^2$ and thus being insignificant. The data in the 4q channel show no hint of instability from the special reconstructions and the measured difference between the masses derived in the 4q and semileptonic channels is not significant. At the time of writing, the remaining task is to quantify these observations and set a limit on the mass shift in the 4q channel from colour reconnection using a technique which does not bias significantly the overall final result. The indications are that this leads to a systematic uncertainty comparable with but smaller than the statistical error in the mass from the most precise measurement using all particles in the jets. If confirmed, I can confidently predict that the combined mass result from Aleph will be the most precise of the four experiments, justifying the long journey to reach a conclusion. The credit goes to the following unmentioned group members who have contributed so much to this endeavour from beginning to end: Andrea Venturi, Ann Moutoussi, Eric Lançon, Franco Ligabue, John Thompson, Paolo Azzurri and Patrice Perez.

February 2005

THE W MASS STORY (The student's perspective)

Thomas Ziegler

After the very successful running of LEP on the Z^0 resonance from 1989 to 1995, LEP increased the centre-of-mass energy and produced the first W pairs in 1996 at the production threshold of 161 GeV.

The work of the LEP people was very impressive and the first days passed by in a much less spectacular fashion than some of the hardware people feared. The first Dali plots of the first W candidates were produced. The art of producing these plots seemed always to be confined to a very limited number of people. The syntax producing them wasn't exactly helpful and I remember sitting at my computer on the brink of insanity trying to plot one of these events, fighting with the colours and finally The W boson was the second known Standard Model gauge boson that was the target of the experiments at the LEP collider. It was created in e⁺e⁻ collisions in pairs (W⁺W⁻) and each W decayed either leptonically or hadronically which resulted in three event-decay topologies: purely leptonically in about 9% of cases, semileptonically and fully hadronically (both in about 45% of cases). For serious measurements, the statistics of the first decay channel was simply not large enough. As the fully hadronic channel delivers two jets that can be fully reconstructed it was the event topology of choice for the measurement of the W mass and width. However, very early on there were two systematic errors that threatened to jeopardize the W mass measurement. The Bose-Einstein

giving up, consulting all-time the hero Hans Drevermann. I explained what I wanted to do and he said: "That's easy, you have to use 'SX', like 'sexy'". This is possibly the only Dali command I ever remembered. In the first ALPHA meetings it was amusing to see at times that the Dali plots presented by the Higgs people disturbingly often coincided with the W candidates.



First W candidate 1996.

effect and colour reconnection. Both cause particle correlations between the particles of the jets from different W bosons, which in first order were expected to be independent, causing a possible significant shift in the W mass measurement.

When I started to look into the effect of colour reconnection (that must have been in the summer of 1996) I implemented the only available model for colour reconnection at the time in PYTHIA and started to look into possible effects in Monte Carlo simulations, mainly various QCD distributions, particle production between the jets and things like that. What we saw was pretty much... absolutely nothing. It took quite a bit of work to convince ourselves, that the Monte Carlo simulations were actually right. We tweaked some parameters a bit and finally saw a small effect using 10 000 Monte Carlo events, nothing we ever would be able to see with the available data. Surprising enough the W mass seemed to be quite sensitive to that effect and we started out at the very beginning with a systematic error of 120 MeV which was completely unacceptable.

At some point, after some good red wine, we decided that we should perhaps measure the W mass in the semileptonic channel and use the fully hadronic channel to measure the colour reconnection effect. This idea wasn't very welcome to the W mass people and was discarded shortly afterwards. The same happened to the Bose–Einstein effect. The W mass measurement seemed to be the only way to make sure of detecting it. A lot of sweat and brain went into new reconstruction and fitting procedures that were less sensitive to these hadronization effects and indeed finally reduced the systematic error from these effects on the W mass measurement to an acceptable level.

The last I heard is 'there seems to be a problem with the W mass'. No news really. It is now January 2005, Aleph vanished into history and parts of it to the many member institutes all over Europe, some people still claim the LHC is going to start in 2007 and many people work on these deadlines. As Douglas Adams put it: "I love the 'wooshing' sound they make when they pass."

Somehow it is good to hear that a small group of people are still sitting at their desks, trying to get the best out of the Aleph data.

I realize that my memory fails me on details, so if the facts are not right, I hope they are at least well invented.

ALEPH GOES COSMIC (CosmoLEP and CosmoALEPH)

Horst Wachsmuth

THE COSMOLEP IDEA

The idea was to search for Cosmic Ray (CR)-induced events larger than 'standard' Extended Air Showers (EAS). Events spreading over many kilometres would be indicative of extra-atmospheric radiation created in spectacular interactions, e.g. particles produced in cosmic beam dumps or in interactions of highest-energy cosmic nuclei with interstellar matter, or energetic cosmic dust grains which have partially disintegrated at a large distance from earth causing several time-correlated air showers.

We had observed cosmic ray events of 100 and more muons traversing Aleph. An early event back in 1989 (Figure 1) was dubbed a 'cosmic debugger'.

Many more were recorded during LEP operation, i.e. within the about two microseconds wide beam gate. These events, one of which is shown in Figure 2, are most likely due to CR-induced EAS. The question was: Would there be events extending much further in space?

These questions led to the proposal to switch the four LEP experiments and possibly other detectors around the 27 km long LEP ring in coincidence and to look for such probably very rare—events beyond EAS (By the way, 'work in the field of cosmic rays' was foreseen in CERN's convention.)



Figure 1: 'Cosmic Debugger'



Figure 2: A cosmic ray event recorded during LEP data taking.

The muon-recording subdetectors of the LEP experiments had to have LEP-beam-independent trigger possibility (for 100% duty cycle). LEP's Beam Synchronous Timing (BST) signal read out with every cosmic event could be used to search offline for CR coincidences over distances up to some 8 km.

A CosmoLEP proposal which I drafted in December 1993 interested colleagues from the LEP experiments. However, it soon turned out during first meetings back in 1994 that only Aleph could provide a LEP-beam-independent trigger: the digital wire signals from HCAL.

L3 was preparing its own CR experiment (and ran it with 100% duty cycle from 1997 to 2000), for Opal and Delphi the project was too exploratory to put extra money and manpower into it.

COSMOALEPH

So, some Aleph colleagues decided to set up a pilot experiment—called CosmoALEPH—consisting of HCAL and stand-alone scintillator stations in the Aleph cavern and in the LEP tunnel to test the method of offline synchronisation using the BST signal and thereby study 'standard' CR showers in a depth where no other CR experiment exists: 125 m overburden absorbs all shower particles other than muons (and neutrinos) and corresponds to an energy cutoff for vertical muons of about 70 GeV.

The Steering Committee was asked to allow the use of HCAL. They agreed under the condition of zero interference with Aleph data taking.

LEPC and BR permitted us to install scintillator stations in the bypass tunnel and in the alcove RE42 (925 m away from Aleph) and to use Ethernet for data transmission. LEP provided the BST signal (every 88.9 μ s). A fast clock (80 MHz) was designed (BR) and constructed at ECP division to obtain 12.5 ns time windows in between the BST pulses.

HCAL colleagues built a splitter board to give us the wire signals from double planes of the 24 HCAL modules. They were used in self-triggering mode and read out together with BST, fast clock, and Vaxtime (later also GPS time) if at least eight double planes have fired in one supermodule simultaneously with at least eight double planes in any of the three supermodules opposite in azimuth (trigger built by Heidelberg).

The scintillator arrays used old Heidelberg scintillators from the ISR/SFM experiment equipped with photomultipliers (PMs) on both ends and arranged in stacks (two scintillators on top of each other). An event was recorded together with its time stamp in stand-alone readout systems if all four PMs of at least one stack simultaneously fired.

Software for offline synchronization (detecting events with zero time difference in different stations) was written and Monte Carlo simulations were initiated (BR, MS, ASM, etc.).

Results were obtained after lengthy scintillator and HCAL efficiency analyses:

- double coincidences between stations pairs yielding the 'decoherence curve' (normalized coincidence rates versus the distances between the detector stations), see Figure 3,
- a limit on day-night differences of muon fluxes,
- dependence of muon flux on atmospheric pressure,
- one remarkable triple coincidence event extended over 1.18 km,
- ...

They led to six diploma theses (at Siegen and Mainz universities) and were presented at several conferences.



Figure 3: Normalized coincidence rates versus distances between the detector stations. The line is a preliminary Monte Carlo fit.

COSMOLEP

In May 1998 the CosmoLEP meetings resumed, now with Karsten Eggert who had joined the project in 1997. I hoped to revitalize the original CosmoLEP idea. CosmoLEP had now become an EP activity. The four LEP experiments showed their interest in a common CosmoLEP experiment. However, Opal and Delphi were still not ready to build a LEP-independent trigger. Instead, the general interest shifted towards multimuon events. A consistent data set of multimuon events recorded by Aleph during LEP operation was analysed and published.

Two dedicated cosmic runs were taken in 1998 and 1999 with TPC and HCAL readout being triggered by the CosmoLEP trigger; their muon multiplicity analysis is in progress.

A proposal to install muon chambers close to Aleph to study multimuon events in more detail was rejected.

What remains of CosmoLEP?

L3 and Aleph will check for time correlations between their events over a distance of 6 km.

Colleagues who have Contributed over The Years

A. Bechini,
C. Grupen,
K. Eggert,
J. Kempa,
S. Luitz,
M. Maggi,
A.-S. Müller,
A. Putzer,
B. Rensch,
H.-G. Sander,
S. Schmeling,
M. Schmelling,
H. Wachsmuth,
W. Wiedenmann,
Th. Ziegler.

Other Interesting Stories

EARLY ALEPH (Personal reminiscences) Peter Norton

My recollections of the first year or so of Aleph are based on the miraculous discovery of several old documents (but the gaps are significant) and on my memory (which is not as good as it used to be, but the failing is more with recent events).

Our introduction to what was to become Aleph was from Erwin Gabathuler in a bar in Turin. Hence I appeared at what was presumably the first full meeting on 30 October 1980. My recollection was that many people who spoke at the meeting had done a lot of work already and we had done none. This was not too surprising in my case since the previous year had been spent in preparations for the EMC results to be presented at the Rochester Conference in Madison.

One of my first things I had to do was cement together a larger UK involvement. Lancaster and Sheffield had been somehow included in the original invitation to join (although it is interesting, at least to me, that their institute names did not appear on official lists until 1982). I was approached by Glasgow (and subsequently by Edinburgh) with a wish to join, which I endorsed. Ted Bellamy of Westfield College, London had already been approached by Lorenzo Foà (they both worked on a North Area experiment together). Hence we had the makings of a strong UK collaboration to match our rivals. The first task we undertook was to look at muon chambers—not surprising considering our experience on EMC. With the assistance of David Frame from Glasgow, I adapted a program written by Alan Grant of CERN to simulate muon punch-through and multiple scattering. This was a new undertaking, starting from scratch, and so it is not surprising that it took until September 1981 to produce our first report. We preferred an evolutionary approach, based on BEBC and UA1, but later on I was among the first to recognize the obvious advantages of a muon system integrated with the hadron calorimeter, a development which relied on the adoption of a tube HCAL.

This of course brings me to the most memorable part of those days. We had decisions to make and we (or rather Jack Steinberger) set very clear timetables for making them. Among these decisions were, of course:

- The choice between the '**Big Sphere**' and a **Superconducting Solenoid**.

My recollection is that the motivation for the sphere was the uniformity of calorimetry and the reduction of the cost of the iron by tapering the magnet ends. The calorimetry at the time was assumed to be scintillators with wavelengthshifting fibres, which would be integrated with the iron and coil of the Big Sphere. The Solenoid Group produced a thick report by the middle of 1981 (amazingly quickly)—it was a feasible project. The Big Sphere, for all of Petrucci's efforts, was too complicated and it was abandoned in September 1981. - The decision between a TPC and an Axial-Wire Chamber.

This decision was quite difficult at the time, as I recall, simply because there was no properly working TPC in existence. Reports were produced from both proponents. There were worries for the TPC about field uniformities, complexity of electronics, space charge effects, etc. Despite these the TPC was declared 'our preferred solution' in October 1981, although I recall that the wire chamber solution appeared as a back-up in the Letter of Intent.

 The Electromagnetic Calorimeter, we all know and love, was not everyone's choice.
 At first there were scintillator solutions and leadglass solutions and, of course, liquid argon. The decision between wires and liquid argon, at a meeting in Saclay, was one I remember clearly. I am convinced we made the right choice.

 The Hadron Calorimeter also had a famous 'showdown' between scintillators and wires (Bellettini and Foà, November 1981).
 The result was the best pictorial muon detection system at LEP (although ELECTRA were proposing something very similar at that time).

 The Inner Tracking detector was a choice between the cathode strip chamber proposed by Veillet and the Imperial College Axial Wire device.

Imperial College had to convince the collaboration that they could trigger on track segments in the z-projection. In fact, conditions at LEP proved so clean that this was not of vital importance, at least early on.

 Some other possible features of the detector were also being discussed around this time and were subsequently abandoned e.g. a timeof-flight scintillation counter and a forward transition radiation detector. The most important development for us in the UK was our move out of muon chambers and into calorimeter mechanics in early 1983. We had always expressed an interest in ECAL electronics, so the shift was not painful at all.

Of course this account cannot be entirely timesequential. We got our approval and then other groups joined us (in the UK, Imperial College). Westfield College London closed its physics department and the people concerned moved to Royal Holloway. The 'Technical Report' (or was it 'Proposal'?) came out in 1983. As far as I can remember, Aleph got its name very early in 1982 (announced at the Steering Committee in March 1982). I was always happy to have the choice of the letter 'A', because that meant that, in the UK, I usually spoke before Delphi or Opal, which was a distinct advantage.

The social side of the collaboration should not be forgotten. I recall a dinner in the CERN Coop on Thanksgiving in 1982. Following a conversation that I had with Sau Lan and Lorenzo, Lorenzo announced his invitation for us to meet in Pisa the following May. Of course I do not claim any credit here; Lorenzo probably already had it in his mind. This was the first of so many enjoyable meetings outside CERN. Indeed I also tried to encourage subdetector meetings away from CERN. I recall meetings in Paris and also going to Heidelberg, Siegen and Glasgow at various stages.

I hope that these rambling memories may have been of some interest. It's been a great 20 years, the best physics of my life.

1985

THE CHOICE OF EXPERIMENTAL ZONE Pierre Lazeyras

After approval of the four experiments, immediately the question arose of where each experiment should be located. The choice for Opal and L3 was obvious for technical and financial reasons: zone 2 for L3 and zone 6 for Opal were equipped with enough electrical power and zone 2 was closest to the surface, minimizing the civil engineering cost for the especially large access shaft for L3.

Between Aleph and Delphi, the choice was not so obvious and both teams made their arguments, more or less the same, both of them preferring zone 8.

The main reason was the fact that the initial civil engineering was such that zone 8 was to be available for detector installation 10 months before zone 4, or about 18 months before LEP start-up, and 18 months was more or less the time required for installation.

Other arguments were made, concerning the distance to CERN, the depth of the pit, etc.

As usual in politics, no decision was taken because it was not really urgent and so was postponed as long as possible. But the time came when one could not avoid a decision; the zones are not identical, in particular the access pit for material in one zone is the mirror of the other. Thus some equipment to be constructed was zone dependent. The Director of Research was of the opinion that such a fundamental decision was too important for him, and only the Director-General, Herwig Shopper, was in a position to settle this question. Thus a meeting was organized, in May 1985, with the DG, the Director of Research, F. Bonaudi and representatives of Aleph and Delphi. For Aleph these were Jack Steinberger as spokesman and the Technical Co-ordinator, Pierre Lazeyras. For Delphi, if I remember correctly, U. Amaldi as Spokesman, J. Allaby, and H.J. Hilke as Technical Co-ordinators. Arguments were presented again and the Solomonic judgement was made.

The DG decided to draw lots; not a very scientific way of deciding was the comment of our spokesman. For such an important circumstance, a 2 CHF coin, provided by the DG, was used and immediately recuperated, apparently in the fear that the 2 CHF coin might be stolen. Ian Butterworth flipped the coin and Delphi won the toss, so...

The result was: Aleph is in point 4 at Echenevex.

A couple of months later, due to civil engineering problems, the general civil engineering planning was completely modified. Zone 4 was separated from the rest and assigned to Philip Holzmann, the German component of EUROLEP, with the result that zone 4 was supposed to be completed 2 months before zone 8!... And so it was.

We later used extensively the fact that zone 4 was so far away from CERN to get some extra 'help' from CERN in the form of money for some facilities...



Planting the Aleph Tree at Echenevex in 1989.

1989

THE LEP INAUGURATION (Unofficial part) Hans Taureg

In 1989 CERN inaugurated its latest accelerator, LEP, in the presence of kings, heads of state, presidents etc. from all Member States of CERN. During the course of the festivities the VIPs formed four groups to visit the four experiments installed at LEP.

Early in the morning point 4 was invaded by an assortment of French security services, police units and the like. Access was restricted, the area searched for bombs and tightly watched.

Aleph was visited by the King Carl Gustaf XVI of Sweden, the President of the French Republic F. Mitterand, the President of the Swiss Confederation J.P. Delamuraz, and the Vice President of the Italian State Council C. Martelli. The DG, C. Rubbia, and J. Lefrançois from Aleph gave the VIPs a guided tour of point 4 and Aleph. But they did not keep to the planned route so that the TV camera teams, which had installed themselves before in the UX cavern, saw mainly the backs of all the celebrities. After about an hour the visit was over and the VIPs went to the official lunch at restaurant 2, Tortella's. At point 4, however, the story continued. About half an hour after the VIPs had left, a group of 30 to 40 persons showed up on the access road to point 4. They were equipped with banners and posters. They essentially blocked the road close to the entrance gate of the site. Some of the police officers, in uniform, and persons from Aleph asked what their intentions were and what this was all about. The group wanted to block the access of the VIPs to point 4 and thus protest against the social conditions in France. When told by the police officer that they were too late and the visit had passed already an hour ago, they did not believe the police officer. The persons from Aleph were no more successful in convincing the would-be protesters. There was, however, a very convincing way out. The group from Aleph decided on the spot to give the protesters a tour of point 4. We invited them in. They could see by themselves that there was no VIP at point 4. We split the protesters into three or four groups and made a tour. We visited the control room of Aleph, the experimental cavern and the LEP machine for about an hour. There were no VIPs for them but plenty of new and amazing things. In the early afternoon the group of would be protesters left peacefully with the banners under their arms and a new image of CERN in their heads.

1984

ALEPH FULL-SCALE MODEL Karl-Heinz Steinberg

After acceptance by the LEP experiments committee (LEPC) on 18 November 1982, the first problem very quickly preoccupied the technical coordinator, Pierre Lazeyras, and his collaborators, the physicists responsible for the different detectors: *How to route the 500 000 connection channels needed between the 9 detectors and the counting rooms in cavern UX45?* These amounted to some 700 km of multiconductor and coaxial cables.

It was thus reckoned to be worth while to build a full-scale model representing one eighth of the experiment to resolve the connection problems (1.7 million connections) and the problems of compatibility between cabling and the other services (water, gas, and ventilation) within a restricted volume.

The construction work was started on 20 February 1984 in Building 157 under the responsibility of M. Ferro-Luzzi with assistance from C. Rosset, for the construction plans and the assembly, and with the CEGELEC team directed by G. Sigaud, under the supervision of K-H. Steinberg.

THE BARREL

This involved construction of the following scale model elements:

 8 HCAL barrel modules, 3 metres long, in tubes of welded steel, with a representation on the front face of 24 layers of iron plus gaps and the notches for the passage of the cables, gas and water pipes and the assembly of the intervening modules

- The surface and the internal parts of the solenoid
- 4 of the hexagonal sectors of the electromagnetic calorimeter (ECAL)
- 2 half-shells of the LCAL detector mounted on the bottom support and three electronic crates
- The detector TRACK part of face A of the TPC with its 'K, M, and W' sectors
- The circular patch panel of the TPC detector
- The laser tube
- The vacuum tube with a group of vacuum pumps
- The first layer of *caillebotis*
- The first layer of muon chambers
- The second layer of *caillebotis*
- The second layer of muon chambers
- The electronic (mixer) and high tension crates mounted on telescopic slides between the two layers of muon chambers
- A cableway on the face of the ECAL detector.

END-CAP A

This involved constructing the following scale model components:

- The rails for moving the end-cap
- Three metal structures of welded steel tubes representing three sextants of the HCAL and ECAL calorimeters of the end-cap
- Two layers of intermediate angle muon chambers
- A layer of *caillebotis*
- Two layers of vertical muon chambers against the rear face of the end-cap mounted on metal girder structures.

ROUTING AND CONGESTION TESTS FOR THE CABLES AND PIPES

When the mechanical modelling of the different detectors was complete it was necessary to start the study and test for routing, congestion and securing of the cables, water and gas pipes.

In order to find and realize the best solutions for routing and fixing the cables around the barrel for the outer HADRON and MUON detectors and in the notches for the central detectors ECAL, TPC, ITC, LCAL, TPC and MINI-VERTEX, several methods and routings were tried out with the collaboration of I. Pizer and R. Pintus. They were in constant contact with the 26 institutes (now 32) distributed through the 9 nations involved in the experiment. This also involved updating the number and type of cables, water and gas pipes. G. Sigaud's team, under the direction of K-H. Steinberg then started the realization of the following systems:

For the Barrel

- The distribution and guidance system of the flat digital and analog cables, the round multiconductor, multicoax and coax cables between the *caillebotis* and the mixer crates mounted on the telescopic slides around the barrel.
- For the high tension crates, also on the slides, the fixing of the cables was by small chains of linked cable ties.
- The distribution and fixing of the multiconductor cables for a big sector of the TPC (K, M, W) and their positioning up to the first layer of *caillebotis* passing through a notch.
- Trials for the installation on the surface of the TPC of the multiconductor and multicoax cables for the ITC and TPC from the patch panel and from the electronic crates of the LCAL detector.
- The complete cabling of notch 12 with all the cables, multiconductor flat and round, high tension, gas multi-tubes as well as the distilled water tubes foreseen for the TPC, ECAL and HADRON detectors.

For End-cap A

- The arrangement of a main set of cable racks with the various cables, water tubes and gas multitubes for the hadronic calorimeter, the muon chambers and the end-cap electromagnetic calorimeter, for the link between the end-cap and its counting room.
- The construction and mounting below a sextant of a group of four mixer crates on slides and the guidance system of the cables and water tubes to the rear.



First Aleph models and cabling.



A more sophisticated version a couple of years later.

THE 1:20 SCALE MODEL OF ALEPH AND CAVERN

Jean-Claude Dusseux

The construction of the model of the Aleph detector and cavern was undertaken in 1987 when, for the first time, it became apparent that a model was needed to show the distribution of services in the experimental zone and the access to different levels of barracks. Then, thanks to the goodwill and enthusiasm of our modellers, Christian Bontaz and Patrick Gave, we undertook the '*Maquettisation*' (a new word!) of the Aleph experiment. The model has been the 'hub of the world' for the Aleph family; a meeting place for long discussions between Peter Schilly, Karl-Heinz Steinberg, Bernard Chadaj, Jean-Claude Dusseux, Claude Ferigoule, Christian Lasseur and others, about the infrastructure and services in the experimental zone.

The model, like Ulysses, has made many wonderful voyages to different places in Europe:



Scale model of Aleph and the Cavern.
- Scotland at the University of Glasgow, twice, with Jim Lynch, as part of the CERN exhibition.
- The extreme South of Italy at Lecce, the Venice of the South, in the Château de Charlequin with Alessandro Pascolini.
- Orsay, Paris with Philippe Heusse.
- Saclay with Jean Heitzman as part of L'Exposition de Physique, Port de Versailles.
- Padua in the magnificent 'Pallazo delle Regione Padova' where the model was carried by eight people to the first floor, then slid on bed covers to protect the ancient parquet floor.
- Milan at the 'Fierra de Milano', Frascati, INFN, with Giampaolo Mannocchi.
- Munich at the Max Planck Institute with Ron Settles.

The model has always arrived safely at its destination thanks to the care of our friend Paul Fermine, CERN transport, who *'roulait pour nous'*. It was he who insisted that a 'porthole' be cut in the large container used to transport the model, to allow customs officers to inspect the contents without opening.

(Editor's note-JL: With the end of the LEP experiments the detector part of the Aleph Maquette has found a good home for the foreseeable future in the Department of Physics and Astronomy, University of Glasgow.)

ALEPH COOLING Ron Settles

We were really proud of this very successful 'technical detail' which passed by unnoticed to probably 99% of the Aleph users (it isn't even mentioned in the Aleph Handbook) but definitely contributed to the overall successful performance. The names behind the design of the cooling system are many, Pierre Lazeyras and Wolfgang Richter being two of the main persons. Of course all of the conceptual design engineers and subdetector builders were involved, but I can't start listing names since there are too many to remember them all.

The main tolerance and goal was that the whole detector should be at a temperature of 20±0.25° when in full operation. We probably didn't quite make the ±0.25°, but we came close. We decided that distilled water at 5 bar flowing through copper tubing (aluminium forbidden) should be the main cooling medium of the front-end electronics. Each subdetector had to take care of its own 'backyard', and everyone took this seriously, carefully designing and testing their cooling concept. The TPC represented a special case because of its large gas volume and because the original Pep4 TPC had to struggle with temperature gradients for its tracking corrections. That is one reason we went for a twopronged attack in which a forced-air cooling was used in addition to the water. The air was blown into the sandwich structure of the TPC sectors: so the TPC drift volume was separated from the frontend pre-amplifiers mechanically by the sandwich structure and heat-wise by air blowing into the sandwich with all pre-amps encapsulated in heat 'jackets'. The jackets were made of aluminium to reduce material, and the water-carrying copper tubes were glued into them. This forced-air cooling flowed out around the ITC and the ECAL and was also to their benefit since it is essentially impossible to capture all of the heat by water cooling alone in the relative complex geometry (if you can get 90% of it, you are doing quite well). During all of the alignment work in the TPC, temperature gradients in the drift region were never seen.

The VDET was another special case since it was supposed to be built with zero material and since water at 5 bar down in that region didn't seem like a good idea (to put it mildly). For VDET1 we decided (after much testing) that forced-air cooling should do the job without water, since the half of the electronic chips (for the z-side) were distributed along the face (the chips for the $r\phi$ -side were situated at the end-caps), so an air-blowing system (separate from the TPC one) was built for that. For VDET2 there were more channels and more electronics and all of it was concentrated at the end-caps, so we used water at 1 bar, cooling a pitch-fibre plate glued to the end-caps in addition to the air cooling. The two VDETs saw some slow, small (≈10-20 microns) long-term movements which were easy to monitor and correct for...

ALEPH BEAMPIPE STORY

Patrick Lepeule

INTRODUCTION

The 27 km vacuum chamber transporting electron /positron beams passes through the centre of each experiment. Particles from collisions must first pass through the vacuum chamber wall before reaching the experiment's detectors. This means that the experimental vacuum chamber or 'beampipe' has a strict set of requirements coming from both LEP machine and experiments. In general terms, the LEP machine needs to ensure a stable, unobstructed path for the beams whilst the experiment needs a beampipe that causes the minimum of interference with their detector.

Even if all four LEP experiments have different designs of beampipes, all share the same beam and a common, safe set of design criteria was established. In this way, both the correct operation of the machine and the mechanical integrity of the beampipe can be ensured.

ALEPH BEAMPIPE

The Aleph beampipe is fitted between the two superconducting low-ß magnets and makes an independent sector with its own optimized vacuum equipment (pumps and valves). This approximately 6 metre long beampipe, crossing end-caps and the Aleph barrel is the one of the most sensitive in LEP and has had four main upgrades during more than ten years of operation. These four upgrades were called 'generations' and their main features are described below.

SUCCESSIVE GENERATIONS OF BEAMPIPE

1st generation

This first pipe had an inner diameter of 165 mm, a thickness of 0.5 mm with thin reinforcement ribs and two conical ends. The full length was machined from aluminium alloy massive tubes.

We can see two important features:

This pipe was installed in ITC from a vertical position in the BEBC hall which was the only surface building with a 15 metre high crane. The horizontal installation in the TPC was done in hall 156 in October 1988 followed by transportation to pit 4 at the end of November.

The second interesting feature of the design was the support of the vertex (ITC) detector on the central beampipe.

2nd generation

After a successful start of LEP in July 1989 and the improvements of the LEP machine, the inner diameter of the beampipe was reduced to 106 mm. This version introduced a 760 mm central length of beryllium with 4 tubes manufactured in carbon fibre composite with an aluminium liner.

This horizontal installation was done for the first time in the Aleph pit in December 1990.

A new micro-vertex detector (VDET1), fully decoupled from the beampipe, was then introduced.

3rd generation

A new vacuum chamber was manufactured with the same 760 mm central length of beryllium. The carbon composite parts were replaced by smooth 1.5 mm thick aluminium alloy tubes. This installation was done in the spring of 1992 with the introduction of the new SICAL forward detector.

This pipe was also removed and reinstalled during the 1992/93 winter shutdown in order to allow an intervention on the ITC detector.

4th generation

The increasing LEP energy with associated background in the detector required this new generation of beampipe. The optimal requested position for heavy tungsten masks and shielding led to a complex design. A pair of 6 kg masks were symmetrically nested inside the fragile beam pipe whilst long shieldings were adjusted as close as possible outside the beampipe. The requested accuracy for mask position imposed new survey techniques and a perfect alignment of the ITC detector to the LEP machine. This new beampipe was installed with success during the 1995/96 shutdown.



Second-generation beampipe.

1983

CHAMBERS OR TUBES?

Claus Grupen

At the time of the proposal in 1983 not all subdetectors were fixed in their technical details. In discussion meetings different designs were put forward to optimize the performance and costs of the detector parts.

The Siegen group was interested in building the muon chamber system. At that time the idea of electrodeless drift chambers was 'en vogue' and physicists in Siegen built many different chamber types along these lines: rectangular, circular and tube-like chambers. They were easy to construct and prototypes of all geometries worked surprisingly well. The disadvantages of the classical multi-wire drift chambers, namely charging up of insulating parts of the chambers deteriorating its performance, was turned to an advantage. The ion deposition itself could be used for field shaping! The trick was to give the insulating elements some residual conductivity to avoid overcharging. This technique allowed the construction of drift chambers several square metres in area with only one single anode wire. There was, however, a slight disadvantage with this design: the final field configuration necessary for the electron drift was only achieved after some charging-up time in which a sufficient number of positive ions was required to shape the field. During this chargingup time the chamber was not fully efficient. To achieve this one needed some ionizing particles to produce a certain amount of ionization. This was then amplified on the anode wire in the process of avalanche formation after which it drifted to the field forming semi-insulating electrodes to create the desired field configuration. Particles to initiate this process were delivered for free by the omnipresent cosmic ray muons. The question, of course, was: 'Would the cosmic ray flux in the Aleph pit be sufficient to do the job?'

The electrodeless drift chamber design for the muon system was presented by Claus Grupen in one of the Aleph Plenary meetings. A realistic estimate led to total costs of about 2 million Swiss Francs for the entire muon system.

The Aleph collaboration was sceptical. The majority was aiming at a more conservative design along the usual arguments: 'We never did it that way!' When Lorenzo Foà presented an alternative proposing to add a double layer of streamer tubes of the type already accepted for the sampling layers in the hadron calorimeter, this idea was highly appreciated. Using the robust and well-proven technique of streamer tubes for the muon chambers, which he claimed could even be produced at less than one third of the cost of the electrodeless design, made it easy for the collaboration to support Lorenzo's idea.

Whether the muon chambers were actually made for 600 kCHF is a different story. Maybe the Siegen group should have gone one step further by improving on the electrodeless chamber idea by developing it into a 'wireless' chamber to avoid wire breakages!

THE CASE OF THE STRANGE DOWEL PINS Claus Grupen

In the early days the luminosity of Aleph was determined by the Copenhagen-built luminosity calorimeter LCAL, a device in construction very similar to the large electromagnetic calorimeter (ECAL). Since a high-precision measurement of forward Bhabha scattering was vital, it was necessary to unambiguously identify the scattered electrons and to determine their polar angle, especially at the lower boundary of the acceptance. To support this measurement a robust nine-layer tracking device, the Small Angle Tracker (SATR) built by the Siegen group, was mounted in front of LCAL. This device consisted of neatly arranged single-wire drift tubes covering the azimuth completely without any dead regions. This system had to be mounted on the front plate of the LCAL. Since the energy measurement in a calorimeter is degraded by material in front of it, the Siegen group was asked to use as little material as possible for the construction and the mounting of the tracking system. In particular it was decided to use aluminium dowel pins as the supporting structure of SATR which at the same time should be used to fix it to the front plate of LCAL. Aluminium has a relatively long radiation length and so would not harm the shower development seriously. The subdetector co-ordinator responsible for the construction and mounting of the SATR at that time was Claus Grupen.

It is well known that the responsible person rarely does the actual work. Instead Claus asked Karlheinz Stupperich to look after the construction and the installation. Stupperich did a fine job in building and testing the system, and he even followed the condition of making the dowel pins out of aluminium. As usual, the system had to be mounted to and dismounted from the face of LCAL several times before the final installation at the beam took place. In this process it turned out that the choice of aluminium for the dowel pins was—at least from the mechanical point of view a disadvantage. In the course of repeated mounting and dismounting it was found increasingly difficult to get the dowel pins into the thread and in particular to get them out again. The dowel pins were made of a soft aluminium alloy, which wore out in this process and got more and more sticky. For Karlheinz there existed a clear solution: one must use a different material, which was easier to mount and dismount. And this he did.

The required precision and alignment properties were achieved with the SATR/LCAL system and the luminosity was measured very accurately. Detailed studies and an improved understanding of the luminosity monitor enabled an unprecedented accuracy. In the process of doing this, all kinds of tests were performed to reduce the systematic error on the luminosity measurement. Among others the uniformity of the energy deposition and Bhabha rate over the acceptance of LCAL was investigated. To the great surprise of the Copenhagen group a few spots on the LCAL face were discovered which showed a somewhat reduced energy deposition and Bhabha rate. It was hard to conceive that the extremely well tested quantum electrodynamics cross-section describing the and angular dependence for Bhabha scattering should have an azimuthal asymmetry. On top of that the regions of reduced energy deposit exhibited a regular pattern which hinted at some more mundane explanation.

These places coincided with the positions of the aluminium dowel pins by which the SATR was fixed to LCAL. It was clear that the detector Monte Carlo, which knew the positions of the pins, was unable to describe their effect correctly. The easiest way to resolve this was to check the Monte Carlo for a correct implementation of the composition and position of the material in front of LCAL. This was done and everything was found to be in agreement with the drawings.

However, the drawings are one thing, but the reality is a different story. The responsible person for SATR was asked to check the material composition of the chamber construction and mounting. Karlheinz, who effectively built the system, unfortunately had already left to earn his money by accelerating chickens and throwing them at aeroplane windows to simulate the effect of impacts of sea gulls during flight conditions in a different Lorentz system. Physics education obviously has very practical aspects, and at some time the postdocs have to leave university because they very rarely get permanent positions.

The only way to make sure that the mechanical composition of SATR was okay was to dismantle the system during the next winter shutdown. The culprit was easily discovered: the suspect dowel pins were made of brass instead of aluminium. Since this was now known and confirmed it could be integrated into the Monte Carlo. The precision of the luminosity determination was not affected by this since the pins were away from the critical lower acceptance boundary of LCAL, but the luminosity group was now really convinced that they understood all properties of their subdetector. So for this part of the game Copenhagen defeated Siegen by 1:0.



SATR and the Dowel Pins.



ALEPH TPC GAS (To seed or not to seed?) *Ken Ledingham*

In 1982 I set up the group at Glasgow called Laser Ionization Studies (LIS) to develop the technique of Resonance Enhanced Multiphoton Ionization (REMPI) for the detection of environmentally hazardous molecules using Excimer pumped dye laser systems.

Earlier, in the UA1 experiment at CERN, small nitrogen lasers had been used to produce ionized tracks in the central tracker for calibration purposes. Unfortunately the ionization produced by these lasers gradually deteriorated due, it was believed, to the UA1 gas purification system removing the impurities that were responsible for the ionization.

Ian Hughes (Glasgow Particle Physics Group Leader at that time) had the vision to realize that there was an important synergy between the two groups. He could supply the lasers from Particle Physics sources and LIS could carry out research into the possibility of seeding trace molecules into multiwire drift chambers and time projection chambers for laser calibration purposes.

Our task was:

- Identify the optimum laser and laser wavelength
- Decide if it was necessary to seed the Aleph TPC gas with impurities to ensure that the laser calibration system, envisaged for the Aleph TPC, would continue to function throughout the lifetime of Aleph.

- Ensure that such impurities would not affect the efficient operation of the TPC.

The work would also help the long-term goal of the LIS Group in sensitive environmental monitoring.

Thus started arguably the most exciting time in my research career, which has continued to this day. Working in great harmony, principally with Colin Raine, Ken Smith and Jim Lynch, we built a large number of small proportional counters with vacuum bits and pieces from CERN stores. These, I recall, had difficulties in passing through the X-ray machines in Geneva Airport since we were smuggling the pieces in our hand luggage—but we were of course vacuum salesmen!

Along with a number of other groups in Europe, we seeded a number of hydrocarbon gases into the counters with the normal P10 gas and passed laser beams through the mixtures and caused control ionization via multiphoton processes. This was exhilarating stuff which gave us lots of NIM papers but which, as I remember, almost cost me my marriage because much of the initial work was done during the Christmas holiday of 1982. It did not take us long to realize that REMPI was an unbelievably sensitive analytical technique. No matter how small the quantity of the seed we added to the proportional counters, we still got large amounts of ion signal. Indeed we could get a large ion signal without adding any seed at all! This initiated an exciting chase to find out what gas we

were indeed ionizing. This was the subject of the Glasgow Ph.D. thesis written by Mike Towrie who has gone on to make an important career on the Laser Facility at RAL. With great patience he carried out a wavelength-dependent experiment using our dye laser system on the unseeded Ar/CH_4 mixture. After a great effort using a frequency doubled UV laser, we produced the amazing spectrum shown in Figure 1, which was identified to correspond to the absorption spectrum of phenol gas. Where was this coming from? Another exhaustive forensic study revealed that the culprit came from the outgassing of polythene tubing, which was used to fill the gases into the counters. We thus realized that phenol was the ideal seed, which only needed to be put into the counter at parts per million concentrations, and hence was unlikely to affect the long-term efficiency of the drift chambers. If you look at the spectrum in Figure 1, you can see that there is a large signal at all wavelengths in the UV. We had thus cracked the best laser and laser

wavelength problem. A wavelength quadrupled Nd:Yag laser operating at 266 nm was considered to be the best option. This laser was a readily available, robust and 'turn-key' instrument that did not need a triple Ph.D. to operate—just the ticket for *elementary* particle physicists! Indeed even as I write this piece, one of the original Spectron Lasers from Aleph has just found its way back into my laser laboratory and will be immediately put to good use—what an amazing closure to this tale.

Thus ended for me a remarkable collaboration which produced a solution for an important Aleph problem but which subsequently had a crucial application in the fight against terrorism namely a sensitive detector for explosive molecules. Thus, yet again, a blue-sky CERN project produced an important solution far beyond the original HEP application.



Figure 1: Two-photon REMPI spectrum in phenol gas.

B-FIELD BLUES Ron Settles

This may come as a surprise to most people, but our magnetic-field map turned out to be a mild disaster. I am allowed to say that since we built the field-mapping gadget at MPI-Munich, because Werner Wiedenmann worked hard on reducing the alignment uncertainties in the TPC to a hitherto-undreamed-of low level, roughly 30 μ m, compared with the originally-planned 100 μ m and because I was in close touch with Werner during this time.

But the disaster was not due to the apparatus or people working to make the field map. The main reason for this is contained in a sentence from the article on 'Tracking Alignment' by Alain Bonissent:

'The magnetic field measurements were made in a very short period during the first mounting of Aleph, and the experimental conditions were not ideal. After the complete assembly, such measurements could never be repeated, so that this will remain forever as an uncertainty.'

So the field-map was made in 1989, when the coil was first commissioned in CERN, the B-measuring equipment was first commissioned, and never repeated. Quite a lot of work went into producing a good map—I remember something like a fit of some parametrization to the data which produced residuals of around 7 Gauss (0.5 per mille). But there were small effects that Werner could see which looked funny. In striving to get the best possible resolution out of the TPC and alignment in general, Werner was trying out many ways to parametrize the effects using physical models, and it is clear that some of the models were affected by not-understood-B-field effects.

What is the message to future experiments? *Don't* expect things to work perfectly the first time you use them. This is really a trivial statement that everyone knows, but the politics at the time of our B-mapping saga dominated over common sense. As I recall we were on a tight time schedule for the installation because we wanted to be the first experiment to be ready and hoped we could be the first experiment to 'see' beam. Well, it turned out that all of the experiments saw the first collisions at the same time anyway (the 'pilot run'). So, the politics didn't pay off. Had we had one more iteration of about one week, to remeasure the B-field after analysis of the original data, it would have really saved us a lot of time and uncertainty in the later years...



Hall probe measuring devices being set up in the coil.

HCAL VISUALIZATION

Giuseppe Zito



HCAL occupation scatter diagram.

Many of you will remember the above image on a screen in the Aleph control room. To develop this image I started with a paper model of the end-cap section as shown on the right.

If you examine the computer image you can see that each sextant is represented as in the paper model with the unnecessary parts cut away. When data taking started, this image was intended to visualize only single events but, after some time and a lot of online shifts, our colleague Pierluigi Campana from Frascati discovered that, by superimposing many events on the same image (as in the display above), it was clear which part of the detector was working and which was not. From then on this display was kept running in the control room to continuously monitor the HCAL performance online.



Paper model of the HCAL end-cap section.

HCAL (as seen by an 'outsider') Pierre Lazeyras

The first episode of this saga, for the outsider, i.e. the Technical Co-ordinator at that time, was the choice of the gas, but this is about something else.

More or less at the same time, we, (I mean G. Petrucci, M. Ferro-Luzzi and P. Lazevras) were lacking information on the construction of the calorimeter, like dimensions of the modules, position of the fittings, etc.-many details interfering amongst other things with the construction of the iron yoke. Thus we decided to pay a visit to our colleagues at Frascati where the tube construction was concentrated. We were very well received, everyone was very friendly and we were invited to have a look at all the installations where the PVC tubes were graphite painted, the wires were stretched, etc. We discussed thickness, tolerances, all aspects of the construction interfering with the iron yoke design and other installation aspects, finishing with an excellent lunch in a nice restaurant in Frascati. One of the issues was to get execution drawings for this calorimeter and we were promised them very soon. Effectively we received shortly after a huge pile of drawings of the various machines developed and constructed for tube production, but none of the modules themselves, the only drawings in which we were really interested. Never mind, it was a big effort to produce such sets of drawings and had shown the goodwill of the team.

It was decided that the module construction would take place at CERN, as well as the testing and assembly in the iron modules, for both endcaps and barrel; first the barrel, then the endcaps. These operations were to take place in the East Hall on the Meyrin site. The area was prepared, cleaned up and provisions were made for flammable detection and alarm. The various safety aspects were discussed. The main point was the gas tightness of these modules. The modules were not in fact gas tight and we could not find a solution to make them gas tight. After long discussions with the Safety Authorities, it was agreed that we would do our best and make sure that the leak of a given module did not exceed 1% of the flow.

This had consequences on the Aleph construction. A flammable gas detection system was anyhow foreseen, but in addition we should have to permanently blow air inside Aleph in order to avoid accumulation of flammable gas and automatically replace the air by some inert gas in case of detection of a large leak. (*Editor's note-RS: This air inside Aleph was automatically provided by the TPC cooling. See the 'Aleph Cooling' story above.*)

Two CERN technicians had been measuring the leak rate of all modules, by flammable gas detection. One day, all of a sudden, a series of modules was perfectly gas tight. It was somewhat unexpected so the technician made some investigations and discovered that the flow of flammable gas had been switched off. No miracle! The East Area, or a part of it, close to a large mockup of Aleph, had been assigned to the assembly team. A bunker in the form of concrete blocks was installed where all the work on the modules themselves was done, the roof being used as a storage area for the various components. The highvoltage tests were performed inside the bunker equipped with a gas detection alarm system directly connected to the fire brigade. This system from time to time gave problems and there was a tendency to try to solve the leak problems by somewhat unprofessional means. At least once this resulted in a rather noisy explosion when a tube exploded, fortunately without causing serious damage.

A strange event took place during this period. I was once called by the lady in charge of the telephone exchange. She had discovered that some person had found a way to call outside CERN, short circuiting the normal procedure, in such a way that they could make telephone calls without the knowledge of the operators. Thus some people were able to call Italy and, with the help of a friend there, listen to the football matches on the Italian radio. Apparently one of these people was calling from one of the Aleph area telephones. I never discovered the culprit; maybe it was better so.

At some stage, after the Beijing Group had joined Aleph and taken the responsibility of producing the second layer of muon chambers, a number of Chinese colleagues came to CERN and participated in the assembly and tests of the modules, before taking full responsibility for the muon chambers. After leak testing outside the bunker, each plane of the calorimeter was introduced into the corresponding gap of the iron yoke; this operation went generally smoothly.

One of the main difficulties with the HCAL at this stage of the construction was getting the final information on cables; the number of cables varied for quite some time, with the derivative always positive, which did not simplify life for those who were trying to get the cables outside Aleph to the electronics barracks.

In the end life was not always simple with this team, working very hard, but not on the same schedule as the rest of the world. It is surprising to see how many difficulties of all kinds could come out in a very short time, but the people there were so friendly, always in such a good mood, that one could not do anything other than forgive and be good friends.

1991

(and a fast fix from the Ile d'Yeu) Olivier Callot

When Aleph started to take data smoothly, in 1991 or so, the Online group started to implement various performance measurements to see where we should put our efforts. One of the tools is a permanent display in front of the Shift Leader displaying the various efficiencies and statistics. The official name of the tasks is DISFIL for Display Fill parameters. But after a few months, a sticker was put on the screen with the name by which everybody knows it now: BIG BROTHER. Was this invented by a frustrated shift leader after an inefficient shift? In July 1996, it appeared that one needed a task to recover from a Fastbus crate trip. This recovery involves many aspects of the system, like slow control commands, FIC reboot, Fastbus initialization, and Run control commands. And this was always difficult to do completely and properly by the shift crew. A first version of the task called FBFIX was installed by the author just before leaving for vacation. A few days later, FBFIX had to work on a real case, and failed. It was then removed from the normal running conditions. The next day, the DAQ co-ordinator was astonished (and not completely happy) to receive a mail informing her that FBFIX had been 'fixed' and put back in operation from sunny Ile d'Yeu using a Minitel!

1994

SUMMER STUDENT SABOTAGE

Giacomo Sguazzoni

My first contact with the Aleph collaboration was during my Summer Student fellowship back in 1994. I was a young and inexperienced undergraduate, almost null in English (as now) but really proud to have the possibility to come to CERN and very curious to take a close look to this legendary reality.

I was supposed to participate in the test beam activity for the VDET200, and on my arrival I came in touch with Paschal Coyle, my supervisor, who offered to me a desk in his office. There was no computer there, but after a phone call, a big blackand-white VAXstation arrived... incredible Swiss efficiency! In a few hours I already had an office, a fully furnished workspace with a computer, all for me, and, last but not least, 1000 CHF in my pocket as a first part of my subsistence... but this is marginal to the story.

Just the time to organize my desk and the network link crashed... the interruption lasted for an unusual long time forcing everyone in Building 2 to hang around, doing nothing.

After two (two!) hours the network guys (one later recognized as Joel) stepped into the room with the verdict: 'The problem is here...'. The following short investigation revealed my workstation disconnected from the Ethernet coaxial cable. The BNC lock was not correctly fixed during the installation and my desktop cleanup made the rest... my English was too poor to articulate a credible defence. From then on I was the Summer Student Saboteur. Nevertheless, now I'm looking back to that episode with different eyes... I have to thank fate for giving me the opportunity to leave my first sign in Aleph. A sign that, as branded in stone, any Aleph collaborator had to face each morning before obstinately moving the limits of human knowledge. (See Aleph computer page opposite.)

... Yes, I was (partially) guilty. I hope you'll forgive me, now. Even if, as a consequence of the rebel nature of a young guy, I tried other ways to mark indelibly the Aleph story... Once I took one of the test beam area safety keys (without which you have no beam) home during the night, forcing Paschal, who luckily knew where I lived (a premonition?), to make an extra overnight drive to Thoiry. A few days after, I dropped the same key into a two-centimetre-wide space between two ten-ton concrete blocks of the test beam area, but John Carr was able to recuperate it with a long stick.

It is incredible but, as all of you can realize, Aleph survived me... *and not in a bad shape*.

AXALB2

```
Aleph Offline VMScluster
```

Username: sguazzon Password: Welcome to OpenVMS ALPHA V7.1

Last interactive login on Wednesday, 20-NOV-2002 17:04:33.21 Last non-interactive login on Tuesday, 30-JUL-2002 17:07:03.35

>>>> AXP-specific ALEPH login starting...

A note to newcomers in Aleph...

If you are using a workstation, do not touch the Ethernet (coaxial) cable connecting the computer to the network. Doing so may have catastrophic consequences on everyone using this system (offenders will be dealt with *harshly*).

Thank you.

SHUTDOWNS Peter Schilly

Since the functioning of the LEP machine in 1989, each year has had the so-called 'shutdown' period, which normally runs from November till May of the following year. Expensive winter electricity and unpleasant winter weather travelling conditions are good reasons to stop the accelerator and the experiments for the annual improvement and maintenance programme.

Aleph's shift crews are exhausted after 6 months of continuous 24 hour-per-day data taking. Nevertheless, proud of their achievement, Olivier Callot takes care of sending out invitations to the traditional 'end-of-run' party at 'La Chenaille' in Echenevex: Family members participate, even with their youngest kids. Spouses are happy to look forward to a better family time in the following months though kids might be somewhat anxious to see more of their 'severe' daddy. The crew of technicians takes a last sip of wine before the busiest part of their year begins...

The planning of the shutdown has already been prepared some months in advance and has been presented by the Technical Co-ordinator to the collaboration at the last Plenary Meeting some weeks before. During the first year of Aleph this planning was handwritten, but was rapidly overtaken by a computer program. Nowadays Bernard Chadaj writes the planning directly into the Web and every week a new version with the latest modifications can be found under the Aleph news on the Web. Officially, the shutdown starts with the last colliding beams: Shift crews, who have worked non-stop for 6 months, want to get out of the place—but every shutdown starts with the bad surprise that one still has to purge the flammable gas out of the Aleph detector. So, the gas experts, Ivan Lehraus and Wolfgang Tejessy, explain once more why one needs more than a whole week of extra shifts while the shift crew has the strong feeling that just a few days would really be sufficient. Finally, with 'safety first' in mind, the shifts continue until the gas analysis looks good and the experimental area can be declared a non-flammable zone.

Additionally, these days of gas purge are also used for all kinds of calibrations and checks of equipment, and Mokhtar Chmeissani and his Barcelona Team can already take care of the BCAL detectors at the quadrupoles in the machine tunnel near the detector. These BCALs are today already in their second generation (BCAL++), always better and more precise, better adjusted during the years and, when they are taken out now, it is not for improvement but for protection from LEP machine work on the quadrupoles during the shutdown.

The next step is to open the end-caps so that one can install the scaffolding, which allows safe access to all subdetectors. Moving the end-caps is always affected by some surprises in the hydraulic system or the handmade electronic remote system. It sometimes happened in the past that the 500 tonne end-caps had to be removed by hand without the use of the hydraulic motors! Now all high voltage on the detector is switched off, the cryogenic plant is stopped, so our superconducting magnet is slowly warming up. The cooling water plant is also stopped and, as no other means of cooling are available, the oil diffusion vacuum pumps are connected to normal domestic water. As the loss of cooling water in our electronic racks is always a problem, an auxiliary water cooling plant of about 200 kW was installed in 1998. This allows us to get some 10% of our approximately 200 racks running if urgent tests have to be performed.

Once the scaffolding is in place, the first work is to measure the position of SICAL and the LCAL detector. Brigitte Bloch and CERN's survey crew want to be sure about the positions of these detectors in case they have to be moved. Normally, these detectors have to be taken off to give access to the next detectors like TPC sectors, VDET, ITC, TPC-laser, SAMBA or the LEP beam pipe itself.

Major modifications over the years came with a new beampipe and a new VDET. The track detector was replaced by the SICAL detector. Two new Vertex detectors were installed as well as two generations of SAMBAs. A clever tungsten shield, held in place by an equally clever 'casserole' was put around the LEP beampipe between our SAMBA and the SICAL detector.

Maintenance and repair work is done all over the detector by all participating institutes. At weekly shutdown meetings at Echenevex, under the leadership of the Technical Co-ordinator, the day-to-day follow-up of the work is discussed and the planning prepared for the next two weeks.

One of the most exciting pieces of repair work was the repair of our magnet in 1993/94. A major leak had been found in the helium line inside the cryostat of our superconducting coil and, due to an enormous effort by Saclay and CERN technical staff, an amazing repair was done on this cryostat. But this is described in another contribution. (See 'The Magnet Leak' article by Pierre Lazeyras.)

In the second half of the shutdown, say from February on, all detectors which were dismantled are reinstalled back into place and are then carefully checked out. Cooling water and cryogenics come back, the magnet can cool down again. All safety and alarm systems are carefully tested and maintained and soon, about Easter, the scaffolding is taken down and the end-caps can be closed.

Now all BCAL can go back in the quadrupoles and magnet tests can be performed by Serge Waeffler. Flammable gas is poured back into the detector and the experimental area is once more declared a flammable gas zone: no fire, no sparks! Shift crews under Olivier Callot start their round-theclock work for safety as well as for checking out the detector and the data-taking software.

Two weeks later LEP closes the machine tunnel and starts the machine set-up. Another two weeks later the physics programme can start with the first colliding beams... and by then Olivier Callot has already organized a 'start-of-run' party.

Ready? Steady! GO! => Another period of data taking has started!!

THE 'MUR TYMPAN' (an interesting geological problem) Peter Schilly

In the underground cavern there is a cylindrical shaped wall with a vertical end wall on its Jura side, near the PZ elevator, the so-called 'mur tympan', which means, strictly translated, 'eardrum wall'.

On one side of this ≈30 cm thick concrete wall ≈18 m high and ≈21 m wide, the Aleph detector with all its infrastructure is installed. On the other side there is the ground into which the hall was dug. The ground consists of molasse, a rather compact sandstone which was carefully anchored by many deep tie rods and a solid steel grid welded to these rods. Owing to this construction technique a free space of ≈1 metre between the solidified molasse and the freestanding 'mur tympan' had been created when the experimental underground hall was finished. CERN's civil engineers anxiously monitored the position and possible movements of this constructional peculiarity. It would be a horrible mess if the pressure of the mountain should overcome the stability of the wall and allow the mountain to crush it and flood the pit in a landslide.

This nightmare scenario caused the surveyors to come once a year and measure carefully possible movements of the wall. Since the 'mur tympan' feature is shared by all four LEP experiments this had to be done in all four pits. It was at Delphi where the wall moved so much that it was decided to drill observation holes into all four 'mur tympans'. Once we could look through these holes which had a diameter of ≈ 50 cm, we could see that a lot of molasse had already moved against the wall. The welding of the grid was partially broken and a substantial amount of the solidified molasse had already broken down.

The inspection holes also served as ventilation for the otherwise closed space between the wall and the mountain and allowed us to get rid of the humidity behind the wall.

Additional survey marks were installed to monitor the movements of the walls with even higher sensitivity. Since nothing frightening appeared in the Aleph pit, we are not in danger when working in our cavern. In contrast, in the Delphi pit, important repair work and water-sealing of their 'mur tympan' had to be undertaken. Let us hope that, in the future 'LHC life', these fragile walls will sustain their stability.

In the mean time, I was contacted in November 1999 by a CERN civil engineer who wanted to discuss with us the construction of a new wall behind Aleph which should be completed before the start-up of LHC. Perhaps he prefers just to be on the safe side rather than being haunted by nightmares in his sleep!

Now, in summer 2000 it has been decided to further reinforce the molasse behind our 'mur tympan' as soon as Aleph has cleared the space at around summer 2001.

SAFETY INSPECTIONS

Hans Taureg

There have been many safety inspections, visits, etc. at all levels, in general without anything particular worth mentioning, with three exceptions:

FIRST GENERAL TEST OF THE EMERGENCY STOPS

A nice Saturday morning at 8 a.m.... Aleph is requested to attend in order to provide information if requested by the safety experts. All experts from the Electrical Services and TIS are present. When everything is in place, the Expert breaks the first emergency stop in the Electricity Building SE4. The power is cut off, that is OK, but...

- The Diesel providing the so-called 'ensured main' does not start.
- The fire brigade, which is supposed to be called, is not reacting.
- The Expert tries to call the fire brigade, but no telephone.

The analysis of the situation showed that the Uninterruptible Power Supply (UPS) was dead. Its batteries were dead. It was found out later that the maintenance of these batteries had been somehow forgotten—a kind of problem we will encounter a number of times during Aleph exploitation!

EVACUATION TEST IN UX45

A document was produced instructing persons present in the UX45 on what to do in case of fire alarm, then, under the pressure of TIS an evacuation test was organized in the UX45:

The test was made, without prior warning to the Aleph personnel (with the exception of the Technical Co-ordinator). Experts from TIS were located at various places, to monitor the actions taken by everyone, those present in the cavern, the shift leader, the fire brigade, etc.

- At time zero, heavy black smoke was produced. No reaction whatsoever!
- Then the person in charge of the exercise triggered the siren, a horrifying noise that nobody can stop, except the fire brigade when they arrive.
- At that time everybody in the cavern reacted as expected, with one exception:

A Chinese member of Aleph, when in the safe zone at the bottom of the pit, realized he had left his passport in an electronic barrack. He then left the safe zone, traversed the smoke filled zone to recover his passport, which was apparently more important than his life! On this occasion it was discovered by us that the instructions to the fire brigade were such that, when arriving, their first move was to block the lift, preventing any evacuation!

One always learns something in such tests!!

TEST ON WATER PUMPING AND FIRE HOSES

In principle water in the drains in UX45 is pumped permanently up to the surface with two independent pumps, one for redundancy. But in case of flood, in addition, the fire brigade has a mobile pump in case of non-functioning of the permanent ones.

The fire brigade decided to make a test of this mobile pump at all pits, beginning with Aleph, the deepest zone.

- After quite a long preparation to connect the pump to a pipe for evacuation, the pump was switched on. No water showed up at the surface! On reading the specification notice of the pump, it was realized that the outlet pressure of the pump was just too low for the 150 m or so depth of the Aleph cavern.
- At this point, since he was there, the Chief of the fire brigade decided to test the hoses. One of the hoses was unrolled, two solid members of the fire brigade holding it. Then the water valve was opened: A trickle of water came out, just good enough for watering the garden. It transpired that, for an unknown reason, which nobody ever understood, the pressure reducer was not the specified one.

Again, one always learns something in such tests!!!

VIP VISITS TO ALEPH

Jim Lynch

On Saturday morning, 1 July 1989, Aleph was honoured by the visit of the famous Russian physicist, Andrei Sakharov accompanied by his wife, Elena Bonner.



Andrei Sakharov and his wife, Elena Bonner, in the Aleph TPC barrack with Jack Steinberger and Jürgen May (see also picture at end of 'End Cap Modules (Glasgow)' article).



During the official LEP Inauguration Ceremony on 13 November 1989 the following were among the VIPs to visit Aleph:

Mr. François Mitterand President of the French Republic

> H. M. King Gustaf XVI King of Sweden

Mr. J-P. Delamuraz President of the Swiss Confederation



The Presidents of the Swiss Confederation, Jean-Paul Delamuraz, the French Republic, François Mitterand, and the Mayor of Echenevex in the Aleph pit.

Since 1991, records of official visits to Aleph have been kept and the following VIP visits are listed:

Name	Title/Position	Date of visit
Mr J Puhol	President of Catalonia, Spain	15/01/1991
Mr A Goncz	President of Republic of Hungary	05/02/1991
Dr P H Rebut	Director of JET, Abingdon, U.K.	13/05/1991
Visit by Ambassadors of I	Member Countries of the E.U. in Geneva	14/05/1991
Dr F M Pandolfi	Vice President, Commission of E.U.	16/05/1991
Mr M Marin-Brosch	Ambassador of Mexico	12/07/1991
Prof W D P Stewart	Chief Science Adviser to U.K Cabinet	18/11/1991
Dr J F Decker	Deputy Director, Office of Energy Research., U.S.	13/03/1991
Mr D Bensari	Director, National Centre Sci. & Tech., Rabat, Morocco	20/03/1992
Prof J-U Anderson	Chairman of Danish Science Research Council	24/04/1992
Dr M Michaud	Science Counsellor, U.S. Embassy, Paris	14/05/1992
Sir W K Fraser	Principal, University of Glasgow, Scotland	31/07/1992
Mr J-F de Bay	Director of General Affairs, EDF, France	19/11/1992
Mr C Détraz	Director of IN2P3, Paris, France	21/01/1993
H.E. Dr A Jelonek	Ambassador, German Repres. to U.N.	14/06/1993
H.E. Mr A Jonsson	Danish Ambassador to Switzerland	05/07/1993
Prof N Gowar	Principal, Royal Holloway College, University of London	06/08/1993
Prof U Colombo	Minister, Universities & Research, Italy	30/11/1993
H.E. Mr L Lawson	U.S. Ambassador to Switzerland	26/05/1994
Dr E Malloy	Director, Office of Science, Technology & Health, U.S.	22/06/1994
Mr M Blackman	Vice President, Yorktown Research Center, IBM, U.S.	27/06/1994
Mr I Taylor	Under Secetary for Trade & Technology, U.K.	21/11/1994
Dr M A Kreba	Director of Office of Energy Research, U.S.	30/11/1994
H.E. Mr O L Scalfaro	President of Italy	20/04/1995
Mrs N K Furey	Consul of Belize	15/06/1995
Mr Z M Hajar	Consul of Yemen	15/06/1995
Mr A Goncalves Pedro	Consulate of Portugal	15/06/1995
Mrs E Baha	British Consulate	15/06/1995
Mr V Haesen	Consulate of the Netherlands	15/06/1995
Mr T M Desta	Consul of Ethiopia	15/06/1995
Mr E B Garcis-V	Consul of Spain	15/06/1995
Mrs L A Overviek	Consul of El Salvador	15/06/1995
Mr H A A Shaheen	Consul-General of Saudi Arabia	15/06/1995
Prof H Zacher	President, Max-Planck Institute, Munich, Germany	13/07/1995
Mr R Barnett	U.K.Science & Technology Counsellor to Switzerland	24/10/1995
S.E. M D Bernard	French Ambassador to United Nations	17/11/1995
The Commission of Science & Technology of the European Council 21/11/1995		
Mr J R Nichols	H.M. Consul-General, Geneva, Switzerland	18/12/1995
H.E Mrs A Anderson	Irish Ambassador to United Nations	16/01/1996

Name	Title/Position	Date of visit	
Dr K-A Jones	Deputy Assoc. Director, Nat. Sec.& Intl Affairs,U.S.	26/01/1996	
Mr F Jensen	Minister for Research & I.T., Denmark	16/02/1996	
Mr M C Pitt	Charge d'Affaires, U.S. Embassy, Berne	11/04/1996	
Visit of Permanent Repre	30/05/1996		
H.E. Mr H Kried, Ambassador, Permanent Representative, Austria			
H.E. Mr L Williams, Am			
H.E. Mr B Ekblom, Amb			
H.E. Mr D Bernard, Am			
H.E. Mr U Rosengarten,			
H.E. Mr P Naray, Ambassador, Permanent Representative, Hungary			
H.E. Mr G Baldocci, Am			
H.E. Mr B Skogmo, Amb	bassador, Permanent Representative, Norway		
H.E. Mr L Dembinsky, A	mbassador, Permanent Representative, Poland		
H.E. Mrs M Krasnohorsk	a, Ambassador, Permanent Representative, Slovak Republic		
H.E. Mr R P Hernandez	y Torra, Ambassador, Permanent Representative, Spain		
H.E. Mr E Hofer, Ambas	sador, Division Multilatérale, Switzerland		
H.E. Mr N Williams, Am	ıbassador, Permanent Representative, United Kingdom		
Mr P Lynch	First Sec. (desig.) Sci.&Tech., British Embassy, Tokyo	04/06/1996	
Mr A Layden	Head, W. European Department, Foreign Office,U.K.	05/06/1996	
Mr G Morin	Director of Communication, CEA, France	03/09/1996	
Mr C Whaley	Counsellor, Sci&Tech, British Embassy, Washington	24/09/1996	
Prof L Berlinguer	Minister, University & Scientific Res.&Tech., Italy	19/11/1996	
Mr L Dini	Minister Foreign Affairs, Italy	21/01/1997	
Mr C Svoboda	Deputy Minister Foreign Affairs, Czech Republic	13/03/1997	
Mrs J Hilden	Minister for Research & I.T., Denmark	18/04/1997	
H.E. Mr C Hulse	British Ambassador in Berne	13/05/1997	
Visit of 1996 Nobel Prize in Physics Laureates:		07/07/1997	
Prof D M Lee, Cornell University, U.S.			
Prof D D Osheroff, Stanf	ord University, U.S.		
Prof R C Richardson, Cornell University, U.S.			
Dr H J Helms	Hon. Dir-Gen, E.U. Commission, Joint Research	28/08/1997	
Mr J Battle	Minister of State, Science, Energy & Industry, U.K	23/02/1998	
UNESCO World Wide Prize Winners.		27/04/1998	
H.E. Mr G Moose	Ambassador, U.S. Permanent Representative to U.N.	20/05/1998	
Prof S Chu	Stanford University, 1997 Physics Nobel Laureate	22/06/1998	
Prof C Smekal	Rector, Leopold Franzens University, Innsbruck, Austria	02/12/1998	
Prof P Galison	History of Science & Physics, Harvard University, U.S.	12/01/1999	
Visit by the Delegates at t	05/02/1999		
Visit by Participants of S Institute of International	05/02/1999		
Dr A Airaghi	Director-General, Finmeccanica	16/02/1999	

Name	Title/Position	Date of visit
Dr J Taylor	Director-General of Research Councils, U.K.	24/03/1999
Prof I Halliday	Chief Executive, PPARC, U.K.	24/03/1999
Mr G Costigan	Office of Science and Technology, U.K.	24/03/1999
Mr J Joy	Foreign Commission Service, U.S. Embassy, Berne	29/04/1999
Members of Social & He	alth Committee, Parliament of the Czech Republic	06/05/1999
French Teachers, Ain Region, France		16/06/1999
Mr F Leblond	Préfet de la Région Auvergne et du Puy-de-Dôme	25/06/1999
Mr J Fontaine	Président, Université Blaise Pascal, Clermont-Ferrand	25/06/1999
Prof J-C Montret	Ancien Directeur, Laboratoire Corpusculaire	25/06/1999
Comité de Direction	Air Liquide	29/06/1999
Sir G Roberts	Vice Chancellor of Sheffield University & President of Institute of Physics, U.K.	09/07/1999
Dr P Cooper	Director of Science, Institute of Physics, U.K.	09/07/1999
Interministerial Commiss	ion on Economic Planning, INFN	15/07/1999
Delegation from IBM		06/09/1999
Prof Sir R May	Chief Scientific Adviser to U.K. Government and Head of U.K. Office of Science & Technology	16/09/1999
Prof I Halliday	U.K. Delegate to CERN	16/09/1999
Mme A Zimmermann	Swiss Delegate at TREF	29/09/1999
Dr J Marburger	Director, Brookhaven National Laboratory, U.S.	27/10/1999
Dr T Kirk	Associate Director, Brookhaven National Laboratory, U.S.	27/10/1999
Prof F Onida	President, Institute for Foreign Trade, Italy	17/11/1999
Dr A N Bunner	Science Programme Director, NASA, U.S.	04/04/2000
Mr K Levison	Director, Business in Europe, Department of Trade and Industry, U.K.	30/05/2000
Mr I Crees	H.M. Consul, U.K.	30/05/2000
Mrs E Baha	H.M. Vice-Consul (Commercial), U.K.	30/05/2000
Delegation from IBM		23/08/2000
Mrs L Poulton	Science & Environment Officer, U.S. Mission, Geneva	29/08/2000
Mr E Fabian	Economics Officer, U.S. Mission, Geneva	29/08/2000
Prof D Lewis	Nycomed Amersham, Council Member of PPARC	08/09/2000
Prof H Markl	President of Max Planck Society, Munich	15/09/2000
Prof Dr T W Hansch	Max Planck Society, Munich	15/09/2000
Prof Dr S Bethe	Director, Max Planck Institute for Physics, Munich	15/09/2000
Prof Dr J Trümper	Director, MPI for Extraterrestial Physics, Munich	15/09/2000
Mr R Genet	Adjoint du Chef, Dept. d'Ingénierie, CEA	03/10/2000
Mme E Nicaise	Directeur Adjoint d'Activité à S.G.N., CEA	03/10/2000
Mr P Vivini	Adjoint au Chef du Dépt., Lasers du Puissance, CEA	03/10/2000
Mr M Jacquemet	Chef du Service, Direction des Sciences de la Matière, CEA	03/10/2000
Prof K Niwa & Administ	07/12/2000	

OPEN DAYS AT ECHENEVEX

Marco Cattaneo

On 27 November 1993, CERN organized an open day at LEP and the LEP experiments, in the context of the First European Week for Scientific Culture. This event was widely advertised in the local press and generated enormous interest among the general public. At Aleph, several thousands of visitors swamped the 40 collaboration members who had volunteered to act as guides. The lift down to the

site, where they boarded buses to the different LEP experiments. This helped smooth the flow of visitors, who were greeted at Aleph by 47 guides and 3 hostesses (the vast majority of whom were members of the collaboration). At any one time 15 guides were in the cavern, with 5 more introducing the experiment to the visitors in a specially prepared exhibition area on the surface.

pit, and the car park, were unable to absorb the flow of visitors, making this first open day somewhat chaotic and a victim of its own success.

Throughout the lifetime of LEP, the research community became increasingly aware of the need to explain to the public the goals of basic research. As part of this effort, more open days were organized in 1996, 1998, 1999 and 2000. The first of these, on 11 May 1996, was planned well in advance. This time it was decided to direct visitors to a central despatching point on the Meyrin



Open day in the cavern.

The flow of 1400 visitors was controlled by a team of 6 people from the Echenevex pit crew, who ensured that the personnel (PZ) lift was used to full capacity (10 visitors plus one guide every three minutes), that guides kept to their allocated times in the cavern (30 minutes in Aleph, 15 minutes in the LEP tunnel, on two independent itineraries ending at the machine (PM) lift), and that buses parked in the designated areas. Despite this, many visitors had long waits at peak times, in particular when their arrival coincided with that of one of several bus loads of Italian schoolchildren.

The open day on 4 April 1998 was even more ambitious. The invitation to visit CERN was extended not only to the local communities, but also to all the schools which were on the waiting list for the regular Saturday visits. The logistics at Aleph were even more military, with 51 guides backed by 6 hostesses and 16 helpers. This allowed us to receive 1555 visitors in 18 scheduled CERN 'navettes' and 13 rather less well-scheduled Italian school buses. The latter were again the cause of long queues: no amount of planning could cope with the simultaneous arrival of 120 Italian-speaking school children. At one point, the need for Italian speaking guides was so severe that two tours were given by an English-speaking guide (Alison Wright) with simultaneous Italian translation by Ariella.

For 1999 there had been grandiose plans to include CERN in the year long programme of celebrations in the Canton of Geneva, leading up to the year 2000. In the end, a more modest program was put in place on 6 March 1999. The LEP pits were reserved for pre-booked schools, local visitors were directed to the Atlas and CMS construction sites, and the 50 winners of a competition organized by the Tribune de Genève travelled on the LEP monorail between points 1 and 2 of LEP. The logistics were nevertheless impressive, and Aleph was able to welcome 561 visitors from 15 different schools. A final schools day was organized on 4 March 2000, attended by 430 visitors to Aleph.

ALEPH AND THE BARCELONA GROUP

Enrique Fernández

For the Barcelona Group this is a special history, since it is in fact our own: Aleph was our first experiment and we developed as a group with it.

How the barcelona group joined the aleph collaboration

I first learned that Spain was going to re-join the CERN laboratory sometime in 1983. I had been outside Spain for the previous ten years and, except for the group at CIEMAT in Madrid, did not know in detail what were the plans of other groups. This changed in November of that year, when I received a letter from the then Rector of the Universitat Autònoma de Barcelona (UAB), Prof. Serra Ramoneda, informing me of their intentions and of the first steps to create a group in Barcelona, and offering me a contract at UAB to assume its direction. After some exchange of letters and telephone conversations with Ramon Pascual and with Jose M. Crespo, I finally visited the university in September of 1984 and, shortly afterwards, committed myself to come to Barcelona in a few months' time.

A crucial decision for a group in experimental highenergy physics, particularly for a new group, is the choice of experiments in which to get involved. It was clear to all of us that we should join one of the LEP experiments. LEP, the Large Electron– Positron Collider was being built at CERN and the experiments were in the preparation phase. For me this was also the desired choice as I had been working in e⁺e⁻ physics since 1979 and was actually involved in the upgrade of the Mark-II experiment for the SLC. Furthermore some students from Barcelona had been given scholarships to go to the CIEMAT group in Madrid, which was participating in the Mark-J collaboration at the DESY Laboratory in Hamburg. The idea was that these students, Lluis Garrido and Manel Martínez, would return to Barcelona after some time at DESY to join the new group. What was not so clear was in which of the four experiments we should participate. We had in fact some pressure to join L3 or Delphi, since there were other groups in Spain which were part of those collaborations and there were reservations that we could be successful in a large LEP experiment by ourselves, being a very small group. The story here is long and I will describe it only very briefly.

My recommendation during the visit to Barcelona in September of 1984 was to contact the Aleph experiment and explore what we could do there. The spokesman of the Aleph collaboration, Jack Steinberger, was then invited to Barcelona at the end of November, and he clearly manifested his disposition to welcome us in Aleph. We had a presentation of our project to the CICYT (the Spanish Funding Agency) in February of 1985 and then again in Valencia, shortly after I moved to Barcelona in May of that year. At the meeting in Valencia we were asked explicitly by the Advisory Committee to the CICYT to explore further what we could do in the Aleph and Delphi collaborations. Based on our findings we should present a plan of work on both collaborations, and, on the basis of that plan, the Committee would approve our participation in one or the other project. Therefore, during roughly the next six months we were working on both Aleph and Delphi. The work was particularly intense during the summer of 1985 at CERN. In fact I remember quite well having to give two talks, one to the Delphi collaboration in Padova (about a possible detector to be used as a veto in the region between the central and forward calorimeters of Delphi) and, three days later, to the Aleph collaboration in London (about the strategy for offline computing in Aleph). After this work we were convinced that we should join Aleph, a conviction that we managed to transmit to the CICYT Committee, early in 1986.

I should briefly comment on why I was inclined towards Aleph in the first place. Basically there were three reasons:

During the previous two years I had been involved in the preparation for the experiments at the SLC accelerator at SLAC, which had the same goals as LEP. During that time I had read quite carefully the LEP proposals and was particularly attracted by the conceptual simplicity of the Aleph detector.

I also believed, contrary to the CICYT Committee, that it was not good for us, a new group, to have to rely on the help of other Spanish groups. In my opinion this would dilute our responsibilities from the start, and would have a detrimental effect on the development of the group. I also happened to listen to a seminar that Jack Steinberger gave at SLAC. Although I had heard stories about Steinberger, having worked in the US with two of his former students of Columbia University and also having worked, from 1974 to 1979, in neutrino physics, I had never seen him. I was very impressed by his talk. His language was straightforward, and the presentation of the Aleph detector and its physics objectives was very simple and clear. He also gave credits to people in a fair way. And he concluded his talk by saying that the preparation of the Aleph experiment was giving him and his co-workers a lot of 'pleasure'. Who knows, may be that word made all the difference in my choice! Shortly after the visit of Steinberger to Barcelona I had my first conversation with him, which was over the phone. For some reason he was in Pisa. I remember that I was supposed to call at noon Pisa time, or 3 a.m. Palo Alto time. I had the impression that the conversation did not go very well. Let's say that Jack, as a Spokesman, did not sound as gentle as Jack speaking to an audience in SLAC, but this, I learned, betters with time. In any case, I had indeed a lot of pleasure working with him for the next several years!

This short history only covers how the Barcelona group joined the Aleph Collaboration and ends in 1989, when Aleph started to take data. (Our contributions to the Aleph experiment, BCAL and FALCON are described elsewhere in this book)... But of course the real experiment starts with the physics. By the end of 2002, a total of 22 Doctoral theses had been completed in Barcelona about Aleph. In this respect Aleph has also been for us a wonderful experiment!

BEIJING PARTICIPATION IN ALEPH Weimin Wu

When people talk about Chinese participation in the LEP experiments most of them will mention L3. Actually, although little known by the public, the Beijing/Aleph group was formed earlier in the Institute of High-Energy Physics (IHEP) than the Beijing/L3 group, and has made a much larger scale contribution to the construction of the detector than did the Beijing/L3 group.

THE FIRST CHINESE GROUP FROM THE PEOPLE'S REPUBLIC AT CERN

In November 1979, three Chinese visiting-scholars from the Institute of High-Energy Physics, Beijing, China, came to CERN, in their 'Mao uniform', and appeared in the CDHS neutrino experiment. This was one of the first groups of physicists from China to go abroad after the ten-year 'cultural revolution'. China was very isolated from whole world during this period. Chinese physicists only touched the theory of QCD in the summer of 1979 when T.D. Lee gave an intensive lecture course in Beijing, and then selected some visitorscholars from China go to CERN and the USA, to participate in experiments of which CDHS was one. Western countries were totally strange for these Chinese visitors; it was a tremendous cultural, scientific and ideological shock for them to be at CERN. They had strict orders to 'behave' themselves and to go to the Chinese consulate in Geneva every weekend for 'political study'. Three people had to be together all the time if they wanted to go outside of CERN, etc.

However, more or less, they not only learned physics, but also learned what Western society really is like.

Later in 1981, Beijing cancelled its project to build a 50 GeV proton machine and called these visitors back to China. One of them, Weimin Wu, financially supported by CERN, was able to stay a few months longer at CERN, and he got the chance to learn about new developments including the formation of the Aleph collaboration, during late 1981, which had many members of CDHS.

CHINESE PARTICIPATION IN ALEPH

After Weimin Wu returned to China, he immediately tried to push IHEP in Beijing to participate in Aleph. The task was not easy. The only experiment in which China had participated at that time was Mark-J, led by S. Ting, who has a well known 'special character' and played a 'special role' in the business of Chinese High-Energy Physics. However, the relationship between Ting and IHEP at that moment was cool because of some complaints from one of the leaders in IHEP about the Chinese being used as 'cheap labour', provided by IHEP to Mark-J, and the atmosphere was not good for creating an L3/Beijing group at that time. The only important Chinese issue for L3 was the BGO crystal supply from Shanghai. Weimin Wu took this opportunity to write a report to IHEP and Academia Sinica, emphasizing the importance of participation at LEP with the Aleph experiment and emphasized the importance of Chinese collaboration with 'big name' physicists, such as Jack Steinberger and Sau Lan Wu. This report was supported by the executive deputy director of IHEP, Zhang Houying, and the deputy president of Academia Sinica, Qian Sanqiang, but met very strong opposition from various people, especially from some members of the IHEP/Mark-J group.

There was intensive discussion between Weimin Wu and Jack Steinberger, Sau Lan Wu and Lorenzo Foà during 1982 and 1983. It was proposed that IHEP could collaborate with an Italian group to build muon chambers for Aleph. *(See: 'The Muon System' article)*.

WWW IN CHINA

It is well known that CERN invented the World Wide Web (WWW), but it is little known that it was the Beijing (Institute for High-Energy Physics)/ Aleph group which set up the first international computer network from inside China to outside of China, for the Aleph experiment.

While muon chamber construction was going on in China, Jack Steinberger discussed with Weimin Wu the possibility of setting up a computer network between CERN and the Institute of High-Energy Physics in Beijing, for the purpose of participating in the Aleph data analysis in Beijing, and asked Paolo Palazzi from CERN, DD, to work with Weimin Wu on this issue.



Chinese delegation at CERN.

The concept of a computer network was totally new in China at that time. Very few people had a vision of the importance of such a network. Fortunately there was one, Prof. Xio Je, who was a senior professor in the Institute of High-Energy Physics, and a very influential person in the Institute. Xio Je supported this project. One of the reasons was that the BES (Beijing Electron–Positron Spectrometer) at BEPC (Beijing Electron-Positron Collider) gained much benefit from the Beijing/Aleph collaboration, particularly from the software development. The strategy was to combine the effort of BES and the Beijing/Aleph collaboration in setting up the computer networking between CERN and IHEP. In this way the Beijing/Aleph group got extra funding and manpower for this purpose in collaboration with BES. Without this strategy, it would have been financially and politically impossible to set up this task in China. The working group was established on the IHEP side, including Qian Zuxuan, Wang Shuqing, Zhang Baochang, Zhao Weiren and Wu Weimin. Qian played an important role in technical issues on the IHEP side, Xiao Jian played a special role as a bridge between BES and Beijing/Aleph, and Paolo Palazzi and CERN DD made a very great contribution to this success.

THE FIRST NETWORK

The initial network was very naive. Since the quality of commercial telephone lines was very bad in Beijing, it was necessary to use wireless communication via an antenna on the roof of the IHEP building. This communicated with the M-160 computer in the Institute of Hydroelectricity, which is next to the Beijing Telecommunication Bureau. From the Beijing Telecommunication Bureau a commercial international telephone linked us to Radio Vienna in Austria and thus to CERN via a telephone line.

The project was started in 1983. After much development and hard work, by the end of May 1984, it was possible to log in to CERN computers from IHEP in China and to exchange email. On 1 July 1984, its success was formally announced. Although it was very slow, with characters appearing almost one by one on the screen, it was indeed the first International Networking in China, and became a standard item on the 'tour' for foreign visitors to IHEP.

Over the following years major radical improvements were made to the national and international links in China and today IHEP has become a cradle of the Chinese network system with more than 1.5 million computers and more than 4 million users in the network.

1987

FIRE IN BEIJING

Pierre Lazeyras

I do not believe it is a good idea to speak about it, but the facts are as follows:

The Chinese group in Beijing had taken the responsibility of the production of the second layer of muon chambers, in collaboration with the Italian groups.

They have received the raw materials, i.e. the tubes, wires, fittings etc. from Italy, plus some testing equipment from Italy and from CERN. All was installed in a lab in their Institute and the production and tests were progressing quite well.

During a night—17(?) October 1987, nobody being present, a leak developed in the fridge of the gas system, where they were using the standard flammable gas mixture. Apparently the gas started to burn on the hot part of the fridge, and the fire propagated in the lab. In the absence of any fire detection system, but in a quite well closed lab, the fire went on until it stopped for lack of oxygen, but by this time the equipment was seriously damaged.

In the end the Chinese group was able to recover and produce more or less in time for the installation of the muon chambers.

REMEMBRANCES *Xie Yigang*

Another episode is from the summer of 1990. Some of our Beijing Aleph group were invited to be Jack's guests at his home. Not only did we cook Chinese dumpling together but also Jack cooked two special dishes by himself: Mexican rice and Spanish fried shrimp cake. On leaving there was a storm of rain and Jack insisted on driving us home in his jeep. Through the midnight storm he took us back home one by one and said: *'It's too late tonight to get enough sleep. Tomorrow you would not catch*

pick you up one by one and take you to CERN'. We thanked him for his kindness and concern on that occasion and also many others, which we will always remember.

the CERN shuttle bus in the early morning so let me

I would like to take this opportunity, on behalf of all Beijing Aleph members, to express our deep appreciation to all the Aleph friends who have helped and supported us: J. Steinberger, L. Foà, P. Lazeyras, S.L. Wu, R. Settles, D. Schlatter, C. Bradaschia, P. Laurelli, G. Maggi, P. Picchi, G. Mannocchi, P. Campana, G. Iaselli, P. Palazzi, H. Taureg, M. Schmelling...

The friendship and goodwill, which we have experienced in Aleph, have been a great encouragement to us in our international collaboration and our work in high-energy physics.

On the night of 13 August 1989, a sick groaning sound was heard in the hostel (Foyer) of CERN at St. Genis from my colleague Xu asking for help. With the aid of my colleague Zhang an urgent ambulance quickly took Xu to the 'Hôpital de la Tour' near CERN. Examination revealed that Xu suffered a serious myocardial infarction and was in a dangerous condition. Zhang passed this information to Jack Steinberger. Jack was very much concerned about this and he at once contacted Prof. L. Foà. Foà sorted out Xu's medical insurance from the INFN side, so that the necessary operation could go ahead. Xu was out of danger in three days. In the first two weeks, Jack went to the hospital to see Xu almost every day (including holidays) and reminded doctors and nurses to take special care of him. After the operation and therapy Jack Steinberger arranged for Xu to move to a sanatorium near Geneva to recuperate. Several months later after Xu had returned to Beijing, Jack attended an international conference held there. He took the opportunity to visit our institute. He met Xu and said: Just say if you need some medicine which can not be found in China and I will find it for you'.
1987-1993

FOND RECOLLECTIONS

Dave Cinabro

I have fond recollections of my Aleph experience as a graduate student. I do not have one big story but a bunch of fragments:

- Working in the TPC lab while a CERN workman was welding a bracket in place for a cable tray. The sparks set off a large fire in a waste basket which was followed by a mad scramble with half of us rushing to protect electronic prototypes and the other half rushing to fight the fire. In the smoky aftermath Wolfgang Tejessy escorted the workman out and told him never to return. That cable tray never did appear.
- The TPC wrapped in plastic and Styrofoam insulation on the back of a truck looking like a giant bag of peanuts.
- During a meeting of the TPC group in the pit, the phone rings, Jürgen May answers, listens and then says: 'No, this is not Aleph. This is Delphi.' and hangs up. We continue our meeting as if it had never happened.
- Running up and down the stairs to the pit three times to reset the TPPs during an open day, as the lift was crammed with visitors. The cosmic tracks in the TPC were really spectacular for us at that stage.

- Gigi Rolandi writing on the board: 'S. Gnit– Higgs Observation' as we left our weekly Aleph meeting and Opal came in for theirs.
- A reporter calling me about IIV. My telling him that the statistics made it impossible to say if we had observed anything or not. After this Sau Lan Wu telling our group to refer all reporters to her.
- Jim Wear scanning hadronic events and finding one with TPC laser calibration tracks in it.
- My wife still claims that once in my sleep I was searching in the bed claiming that I was trying to find muons. I spent way too much time tweaking QMUIDO, an early muon subroutine.
- Werner Witzeling giving me a hammer on my last day in Aleph as a reminder of my attempts to get a TPP prototype to fit into a rack with a hammer. I still have that hammer on my desk.



PHOTOGRAPHIC MEMORIES

Dave Casper

The following is a selection of photographs that Dave took during Aleph parties in the 1990s.



Maria, Marco & Offspring.



Patrick & Eric.



Elizabeth & Ron.



Pere, Beat & The Harveys (with Corinne behind the bar).



Elizabeth.



Alain.



Francesco, Franco et al.



Elizabeth & Jim.



Olivier (Chef de BBQ).

2000

SKETCHES AT AIX Franco Ligabue

Here are some sketches made by Franco during some 'not-so-interesting' talks during the Aix-en-Provence meeting:



Maria Girone.



Ann Moutoussi.



Pedro Teixeira-Dias.

Roger Clifft.



Jim Lynch.





Peter Hansen.

Paul Colas.

Thomas Shuker.

1995-???

THE ANNUAL ALLONDON FRISBEE CHALLENGE

Roger Forty/Jim Lynch

The game was born during one of the annual Aleph barbecues, organized by the CERN group on the banks of the river Allondon. Although a little beer used to be drunk, cooled in the river, these began as rather genteel affairs, with a quiet game of Frisbee on the river bank. Then one year, to general astonishment, Fred Bird grabbed the Frisbee and ran into the river (perhaps looking for a beer?). Others soon followed, and the rest is history. However, in its early years the game suffered from a complete lack of rules, leading to many disputed points. After one such dispute, I (RF) was sufficiently provoked to write down the set of rules below, which were more-or-less followed on subsequent occasions. However, in the heat of combat they tended to get ignored (particularly the one concerning no physical contact)...

RULES:

- 1. The game shall be played in the river, in an area between two clear landmarks (to be decided before the game starts); the area beyond each landmark is the 'end-zone' of the team playing in that direction.
- 2. The object of the game is to throw the Frisbee so that one of your team members catches it in your end-zone; the person that catches it must not enter the end-zone before the Frisbee is thrown.
- 3. The game is started (or restarted after a goal) by one team throwing the Frisbee towards its endzone from the opponent's end-zone; the opposing team then has possession.

- 4. A member of the opposing team may then throw the Frisbee to any other member of his or her team; when holding the Frisbee you may not move more than one pace in any direction.
- 5. Play continues until either a goal is scored or possession passes to the other team: this happens if a member of the team catches the Frisbee or the Frisbee touches the water (or ground).
- 6. Blocking a throw is allowed but only at a distance from the thrower equal to his or her arm length; no physical contact is permitted.
- 7. During a period of possession one pass (only) may be made to a team member standing out of the water.
- 8. Injured players are to be removed from the river before play continues; if they are not revived by Alain's massage the teams will be redistributed.
- 9. The referee (Roger)'s decision is final and binding in all circumstances; he may change any of the above rules at will.

Although the records of many of these challenges are lost in antiquity, the following photographs were recorded during the 1997 Challenge match:



Roger (the referee) dictating the rules.



It is a strategic game—physical contact is NOT permitted.



Gigi (re)starting the game.



The referee's decision is final!

1990

RUNNING THE LEP MACHINE

Here is another story, and it's not about what you might think from the title (see the next story for that). But it is true, I swear to it on a stack of bibles.

I had started running marathons about once a year around 1985 (during the construction of the TPC sectors—it was a way of unfrazzling my nerves). When LEP started to work in 1989, it occurred to me, gee, wouldn't it be kind of fun to run the full length of the tunnel, since 27 km is roughly a distance you do from time to time while training for a marathon. It seemed like a good way to kick off the training for the 1990 Munich marathon (on 6 May), run the LEP tunnel during the 1989/90 winter shutdown, say in early February when things were still relaxed since there was not yet much start-of-run pressure on the technical side of things.

First I went to Emilio Piccasso (the then LEP chief) in January to get his permission, but he replied, "Oh, don't bother me with that, I've got too many other things to worry about!" (an answer I probably should have expected—I wonder if he remembers my request—Steve Myers probably would have been more sympathetic to the idea, but he wasn't boss yet). Thus, I concluded that formal channels were so ill-defined that they wouldn't work, and maybe there was a way around the non-existent but infinite red tape.

How to do this 'project'? After thinking about the infrastructure a bit, it seemed the best way was to try a practice run in the tunnel to 'learn the ropes', so to say, and find out if the whole idea was feasible. So I decided to run the tunnel from Aleph to L3

and back on Sunday morning, 28 January 1990, just to see what would happen. The advantages of that day of the week and that time were that there would be little going on since nobody likes to work on Sunday morning. I came to Pit 4 around nine o'clock. We weren't holding our standard 'pit meeting'-see Olivier Callot's story above-it was still too early in the year, but there was some activity around some of the subdetectors and there was full access, also to the machine (the doors were open). I just wore standard street clothes (with jogging shoes of course) so as not to appear different as might have been the case in some kind of running attire. I had my CERN identification badge with me and a story cooked up about 'inspection of machine something or other' (I forget what), in case somebody might ask. I took the elevator the 150 m down to the Aleph detector, went up the stairs and through the door to the machine tunnel, and started off.

As everybody knows, the machine elements were on the inside of the ring and there were a couple of metres outside for maintenance/etc. access which gave ample space for jogging. So I ran over to L3 and back, without trying to traverse the detector region which was a bit more complicated and where there might be doors or barriers. There wasn't anyone around that morning, so the test run of about 13 km worked just great! An interesting 'feature' about that stretch of the tunnel is related below, so please read on...

Ratcheting up the 'project' to the full level, I chose to give it a try and to run the whole machine two weeks later, on Sunday, 11 February (the day before my birthday). I arrived at the pit at around nine o'clock again and set off as for the test run described above, but in the other direction, towards Opal in Pit 6. I got to Opal, maneuvered around the detector region-there was a door or two on the way but they were not locked, and continued on to Delphi (Pit 8). Of all things, between Opal and Delphi I ran into a head-wind! And there was a little bit of something gritty or sandy in the wind—I thought to myself, "This gadget (the LEP machine) will never work." It wasn't a really strong wind, but still around 20 km/h, so it was kind of spooky (were the gods against me?). The 'sandstorm' subsided after a kilometre or two, thank goodness. I continued around a couple more unlocked doors and the Delphi-detector region, and pushed on towards L3 (Pit 2). After maneuvering around L3, I was on the 'home stretch' since I had done that two weeks earlier. Passing through the famous region under the Allondon, which had dumped huge amounts of water into the tunnel when it was being dug/excavated and caused delays and lots of extra costs, was rather interesting because the tunnel walls were partially wet, the tunnel size was smaller where it had been strengthened, and there was a little stream of water running down the middle of the (remember, tilted) tunnel ("...will this gadget ever work?"). So, I got back to Aleph after two and a quarter hours and 27 km.

Finally, perhaps the weirdest thing of all was that, after cooking up a story to tell anybody who might have stopped me to ask what the hell I was doing there, you want to know how many people I saw during that 27 km? None. NOBODY! Not one single soul. I could run the whole machine, everything was opened, and no one was doing anything! So the only witness that I have is Ken Smith, who was run co-ordinator (I think) at the time and down in the pit when I emerged. I perspiringly said to him in passing "I just ran around the machine..." but forget what he answered. In the meantime he might have also forgotten about the incident, in which case I have no witnesses. So do you, dear reader, think that this a true story, or have I been pulling your leg?¹

During the start-up preparations a couple of months later, Albert Hoffmann (another of our famous machine physicists) walked around the whole tunnel on an inspection tour (it took him 5 hours) and was proud of the fact to be the first person to have accomplished that feat. When I told him my story, he was a little bit disappointed (he's probably also forgotten)...



¹ "That's for me to know and you to find out,' he said with a tiny but saucy grin..."

1989-2000

RUNNING THE LEP MACHINE

(Recollections of friends)

Steve Myers/Mike Lamont/John Poole/Helmut Burkhardt

STEVE MYERS—(MOSTLY) VERY SHORT STORIES

Ring my Z⁰ Bell

In August 1989 when we had the first 'pilot' run for physics, one of the experiments had connected the Z⁰ signal to a bell which sounded loudly and proudly on the arrival of the particle. Opal's bell was performing very well while that of Aleph was silent. (It turned out that when beams collided for the first time in the pilot run, Aleph had just the background monitoring system running; the rest of the tracking detectors along with the TPC were only turned on later when collimators were moved in. Opal had taken the risk of turning on earlier.) At some point Aleph also had the assumption that the beams were not colliding in their interaction point, as I recall, but about one hour later this theory evaporated when Aleph's bell got rung on successive rapid occasions. As the luminosity increased the bell became a bloody nuisance and was switched off. I would pay several beers to have that bell in my memorabilia!

Open Heart Surgery on the Superconducting Cavities

Cavity antennae were picking up beam-induced signals and causing severe heating. We were severely limited in intensity and bunch length in order to stay below the 8 watt limit. Lots of sophisticated beam optics, wiggler excitations etc., etc. so as not to exceed this limit. I was not taking this limitation too seriously until we found a molten cable inside the cavities leading from the antennae. We then put together a crash programme for the repair. This involved 'open heart' surgery on the cavities so as to replace the cables. We needed and found technicians with small enough hands to get at the offending cables. At this time there was no Skills and Talents Inventory.

Saviour of L3

The luminosity in L3 was less (15–20%) than the other experiments, or so they measured. We measured everything, beta values, position of the waist of the low beta, possible non perfect collisions and found nothing. In fact all our measurements indicated that the luminosity in L3 should be higher than the others. We suspected a calibration error in the luminosity detectors in the experiment. I was 'asked' to make a presentation to the full L3 collaboration to explain the missing luminosity.

The spokesman introduced me as the 'saviour of L3'. Before I started my talk I outstretched my hands in the form of a cross and reminded the audience what happened to a famous previous 'saviour'. Sam Ting was not amused!

Trains and Boats and Planes

The energy calibration results showed serious random variations on the energy signal during all parts of the day with the exception of a few hours in the middle of the night when the signal was noise-free. We discussed this at every opportunity and everybody had their pet theory. I believed it was some sort of effect coming from the planes interacting with the electrical supply cables. Some nights later I was to be seen sitting in a car park on the Jura at two o'clock in the morning to see if I could prove my theory by some visual effects. Of course it was very dark and all the planes had anyway stopped landing several hours beforehand. Experiment inconclusive! The real culprit, the TGV, was found out by accident a few weeks later during a discussion with a railway engineer: Leakage currents on the French rail track flowing through the LEP vacuum chamber with the return path by the Versoix river back to Cornavin!

Molten Lead 'Protectors'

Massimo Placidi decided to protect his polarimeter equipment by building a lead shielding around it. Sometime later after some high-energy (high synchrotron radiation) running he returned to the tunnel to find his little lead hut looking like a lead Gaudi structure.

'Bollocks to Bollock'

This plaintive cry was heard to come from the control room on several occasions when the operations crew was congratulated for the 45th time on their excellent performance.

Imploded Vacuum Chamber

I think it was in Delphi where the beryllium liner on the detector vacuum pipe 'bubbled' inwards and caused serious background problems and beam loss.

Two Green Bottles

Everybody has heard some form of this story and it has 'grown in the telling'. However, I was there! The problem is that I have told the story so often I cannot remember which of the embellishments are the truth and which were done for good storytelling. Facts. I was at a conference and the poor operations guys were struggling to get a beam around for several days. I left the conference as soon as I heard the bad news and returned to CERN to find many exhausted colleagues who had run out of ideas. It was clear, based on all the evidence which my dear colleagues had collected that there was an obstruction in the vacuum pipe. We used the pre-planned method to detect the location of the offending obstacle using the beam position system. It appeared to be around point 1. We decided to open the vacuum and look for the obstruction. We looked down the inside of the vacuum pipe with special mirrors, endoscopes etc., and could not see anything clearly. Finally in frustration (since I gave the OK to open the vacuum) I managed to get my head (which was now much smaller) between the vacuum flanges. I looked down the pipe and was confused by many reflections etc. but something appeared in the distance like a green concave lens. I thought "This looks like the bottom of a beer bottle", but of course due to the very low probability of that being true, did not utter a word to anyone in the vicinity. I then went to the opposite open end of the vacuum section and peered again into the beam pipe. This time I saw a green circular disk somewhat closer. I thought the same thought and did not utter a word. We then waited for someone to get a long pole to poke out the offending article (high technology!). While we were waiting for this to happen, I was standing beside a vacuum technician who was already impressed that we located the obstruction to such accuracy. He asked me what I thought it was and I whispered "A beer bottle". He looked at me in disbelief and out came "What type of beer?" I was stumped but nevertheless thought it was worth while to guess at a producer who uses green beer bottles. I thought Heineken or Kronenberg...? I confidently replied "Heineken". Out it came, and my 50% probability guess was correct: it was Heineken. The technician was very impressed.

We were all about to close up the vacuum and go back to the control room when I recalled my Belfast bomb motto. "If you find a bomb in a building, it doesn't necessarily mean that it is the ONLY bomb." So I asked the vacuum people to search again around the area and low and behold there was a second bomb; bottle I mean.

On inspection of the offending beer containers it was clear that the control room guys had almost pulled it off. There was a scorch burn along the Heineken label indicating that they had steered the beam between the small gap of the bottle and the upper limit of the vacuum chamber. If there had only been a single bottle they may have succeeded in making the beam circulate.

The Swiss police interviewed me the next week concerning this serious act of sabotage. They were looking for a motive and asked me if someone in CERN could have done this to make me look incompetent and possibly get my job! Without hesitation I replied "So you are looking for someone who is mentally deranged!" They didn't understand the witticism.

I have never been able to trace what happened to those bottles and I would dearly love to retrieve them for LEP memorabilia.

energy running and four months from mid-May would effectively have ruined our year. After a lot of very heated discussion (following which the LEP2 project leader was officially reprimanded by the CERN DG for over-aggressiveness), the RAG people conceded that "2.5 months would be a challenge". The turning point in the discussions came when I pointed out in front of the insurance company representative that the total hourly cost of LEP (taking into account amortization of the capital investment and the operating costs) was about 100 kCHF (2 MCHF/day), and that there were about 1000 of the world's best high-energy physicists waiting for beam. We set up a combined RAG/CERN team and supervised the clean-up during daily meetings. The work was completed in about six weeks with non-urgent repairs delayed until the normal winter shutdown.

The Sextupole Bus-bar Bi-Metal Strip

Every year at Chamonix we decided to change the LEP optics. During the construction of the machine, savings were made on the powering capabilities of the sextupole families. So each year we were obliged before testing the new optics to reconfigure the sextupole powering. During one start-up (I have forgotten which year) we had a strange problem. About 20 minutes after powering up the magnets, a chain of sextupoles would trip on 'overcurrent'. When we tried to power them

RAG to Riches

There was a major fire in BA3 (building on the right of the entrance to the then control room) on 13 May 1997. Power supplies and RF equipment were destroyed but no-one was injured. The most severe damage was caused by acid smoke and soot mainly coming from the burning of the PVC cables. In addition the clean-up was hindered by "chlore" mercury from an ignitron, and lead and cadmium had been detected. The Swiss RAG company were contacted to do the clean-up of all the electronic cards, roof, insulation etc., etc. Initially they estimated that the repair would take four months! We were about to embark on higher



Looks like another case of symmetry breaking.

again it worked and BOOM! about 15 minutes later they tripped with the same fault. The analysis of this fault was easier than most since we knew it was the sextupoles and we also knew we had recently re-configured the bus-bars. The problem was the number of 'jumpers'. So we set up teams to go to each of the connecting boxes and do a visual inspection. In this case a picture is worth a thousand words so please look at the photo. The connections were all like little 'humped-back bridges', except one which was connected the wrong way up (How was this possible or even allowed by construction to be possible?). Net result, we had a large bi-metal like strip which when heated expanded and touched its neighbour. Conclusion, tired electricians can make very obvious mistakes.

"You Dirty Rat"

Problem with some controls timing cables. Gary et al. go to investigate and find a large rat which had eaten his cables. Believing discretion to be the safer part of valour he immediately phones for the CERN firemen. Before presenting this story to the Annual Divisional meeting I searched the internet for websites on rats... found more sites than there were rats in LEP.

The Romeo and Juliet Deers

Two deers were found electrocuted simultaneously and still in a lover's embrace (poetic!). Only the female had bitten the offending electrical cable. Bet that was painful for Romeo.

Highest Possible Award to the RF People (Günther Geschonke)

"The ...RF system is now almost nearly fully operational". This was the Operations Group official statement at Chamonix after the RF guys had pushed the system well above its design values and achieved a fantastic availability. The operations people are a demanding lot when it comes to equipment groups.

Closure, Janot, Official LEP 'Wake'

The impending closure of LEP, when we were all sure we were about to discover the Higgs, was perceived like the death of a dear friend by most of the Lepers. After each of the public debates on the subject a group of us would meet in some local pub, drink a few beers, curse the disbelievers, and cry on each other's shoulders. Our dear friend and co-ordinator Patrick went a little bit further!

When our dear friend LEP was finally laid to rest we met one last time for an official wake.

MIKE LAMONT

Operating LEP

So what can go wrong when you're operating 27 km of particle accelerator, whizzing two counter-rotating beams of ultrarelativistic leptons around the ring at 11,250 times a second. Well, let's see, you've got the magnets, the power converters, the vacuum system, the control system, the cryogenics system, the cooling and ventilation system, the beam instrumentation—all of it, the control system, fibres, networks, routers, gateways, software, databases, the separators, the kickers, the beam dump, RF—woah—the RF, klystrons, HV, interlocks, synchronization, timing, feedback, did I mention the control system? And, of course, the experiments, their ability to dump the beam, L3's girder, L3. And then you've got people.

The Experiments

So we get on with our job, colliding bunches of electrons and positrons in the middle of four huge experiments. Four huge experiment collaborations. Collaborations of individuals with desires, needs, egos, usually intelligent but not always smart. There were quite a number of things we could do wrong: dump some beam into their precious, sensitive detectors; spray unwanted particles (photons, electrons, positrons etc.—collectively known as background) into their precious, sensitive detectors while they were trying to take data; and probably the worst sin of all—give the other three experiments more collisions than they were getting. You'd think we were doing it on purpose.

Each of the experiments had their own particular collective character, and their own way of dealing with an operations group that displayed signs, which in an individual would be classed as a serious personality disorder. We verged between accommodating, belligerent, maverick, dedicated, professional and, very occasionally, hopelessly amateur. All within the time span of one shift, depending on the attendant pressures.

Now the experiments had a clear figure of merit the operational efficiency—the percentage of time the experiment was taking data while we were delivering collisions. The experiments were allowed to parade their efficiency numbers (plus complaints or congratulations) at bi-weekly scheduling meetings.

Aleph were the élitists. Well run and disciplined they always (or nearly always) had the highest efficiency figures. Their appearances at scheduling meeting were nearly always a simple, smug statement of 97.8% or there abouts. This was livened in the later years by repeat appearances of one of their co-ordinators, a Polish guy (Bolek Pietrzyk), who insisted on congratulating us every time we stepped up in energy or luminosity. A strongly Polish accented "Congratulations! You have achieved the highest energy electron-positron collisions in the universe!" was always gratifying. When something went wrong in the machine, the phone would ring in real-time and Aleph would either explain to us what had happened or demand to know when we were going to re-fill.

Probably equally professional, but a lot more relaxed about it were Opal (maybe it was the strong British and German contingent). These guys understood human nature. Quite simply they bribed us. Every time we passed a luminosity target or hit a new energy record they'd turn up in the control room with Champagne, or better still, crates of German beer. Naturally we'd do anything for them, background optimization, luminosity lower than the others, we'd happily shift heaven and earth to resolve their problems. We had the impression that L3 and Delphi were a little behind the curve in the organizational stakes. Delphi, for example, ran their detector as a state machine. All well and good, but it depended on us changing the mode to dump beam at the end of a fill. Something which occasionally got skipped, leaving Delphi's sub-detectors on and them ringing us desperately for a mode change. They'd staff their control room with students, who'd ring us up and ask if we were going with the single beam we'd just taken up a test ramp into physics and things like that.

Filling and ramping were demanding periods during the operational sequence and a lot of concentration was required to avoid missing any one of the myriad essentials. The experiments did well not to ring and make too many demands at this stage. "Tell them to **** off" can, and did, cause offence.

Giving Access

I mean ****. I mean for Christ's sake. This is the largest particle accelerator in the frigging world. Why can't we get people in and out of the machine without spending a couple of hours afterwards trying to recover from the experience? Shut those bloody doors behind you.

We had a few problems here. People needed to get in of course. The experiments to their detectors, the instrumentation guys to their instruments etc. We used to collect requests on the white board in the control room and ring around when the opportunity for access arose. The first problem was getting them in. There was one access console, about 40 access doors all equipped with TV and intercom. Access was declared and war started. All over the ring, people starting buzzing the control room: I'm coming. I'm coming, please put in your card, take a key, the door has been released, noput your key in the door and turn it, OK I'll rerelease the door, try the key now, Hey! Only one person can go in on that card, no, no, you have to put your card in and take a key as well, no, I can't let you in without a card, well you're have to go back to your office and get it, I'm sorry.

Next problem: getting the buggers out. Two hours access scheduled, and after three, there's some guy buried in the bowels of IP1, uncontactable, diligently fixing whatever, with us apoplectic in the control room: "Right, that's the last time he gets told that there's an access coming up".

Recovery from access was a random walk around interlock space as we tried to get the machine ready and safe for beam again. And, of course, there were the inevitable trips to the depths in point 5 to reset a door. Personally I used to find something to do elsewhere and scoot during access if at all possible.

Ramping—The Early Years

The aim of the game can be simply stated: inject as much current into both beams, ramp it in energy to 45 GeV. Squeeze the beam size down at the collision point, collide, and then spend a few hours delivering events to the experiments. The reality was hours of furious concentration, optimization, manipulations, and in the early days frustrating disappointment.

Filling LEP was delicate, an hour-long process of parameter adjustment, tweaking, coaxing beam into the machine, but it was kind of OK, you could always reverse that last manipulation. The ramp was different. When we thought we had enough current, we used to load functions to the power converters, separators and RF and arm the front-end control equipment. At this point the ramp was ready to go. On request of the operator a timing event would be sent out and everything would smoothly, synchronously move together and the beam would be taken to the pre-programmed energy. That was the theory.

In practice, in practice, oh my God, those early years. At the start of the ramp, eddy current effects used to produce quite serious perturbations in the magnetic fields seen by the beam. The beam did not like it. On a good day it used to wobble alarmingly (we had a real-time feed of the transverse profile of both beams from the UV telescopes so you could see what the beam though of it), we'd lose a bit and the rest would struggle on up the ramp. The ramp used to last minutes. There was nothing you could do. You'd stand there, watching the lifetime buck and dip and watch the beam so carefully injected slowly or quickly drift out of the machine. The price of failure was a turn around and re-fill, success brought the opportunity to chance the squeeze, an equally hazardous manoeuvre, and then perhaps a physics fill, and a period of a relative calm.

On a bad day, most of the beam would disappear suddenly in the first seconds of the ramp, futile attempt after futile attempt.

The Robot

I won't mention any names but it was Alan Spinks who told me. Suspended from the ceiling all the way around the 27 km of the ring was the infamous monorail. Why it was infamous I don't know, in twelve years I never saw it run once. Tucked away near the access points were the buggies that hung beneath the rail, presumably used in distant times to transport workers around the ring.

We were having a problem with synchrotron radiation during the LEP2 run, and of course, we couldn't go in the tunnel while there was beam circulating. Needing to measure the radiation levels near the RF units, someone had the bright idea of fitting radiation monitors to the monorail car and moving it backwards and forwards around the region of interest by remote control while there was beam in the machine. Cool. So it was rigged up and the lads stood outside playing blind Scalectrix with a buggy strung up with radiation monitors. Things went alright until it was time to park the thing. It got a bit stuck apparently, and they moved it backwards and forwards a few times when suddenly there was an almighty crash from inside the tunnel. The thing was parked for good. Sheepishly they tiptoed away and told no-one, well nearly no-one.

The Bottle

And then you've got the subtle stuff like sabotage. I was co-ordinating the start-up after the long shutdown. Fairly standard stuff, a couple of weeks of checking everything out before you take beam. When beam finally arrives we'd steer the first turn, checking on the luminescent screens situated around the ring as we went. It was one of those simple, exciting things with progress directly measured by a splash of light on the screens.

We never usually had much problem with the first turn, and this time the positrons went sailing around almost a full turn of the ring before being lost. Hum. I remember the physics co-ordinator, Pippa Wells, looking over our shoulders and saying "Congratulations". I frowned back at her "We aren't all the way around yet". You just know, you just know, something, instinctively, isn't right. The positrons stubbornly brick-walled at point one. The next obvious step was to take electrons from the other direction and sure enough they got bumped out at the same place. There then followed five days of thrashing around, meetings, explanations, checks of the magnets, visits to the tunnels, analysis, orbit bumps, checks of the magnets. By bumping the beam around in that region we could get the beam to do two or even three turns but no more. Beam was splashed into the beam position monitors downstream of the obstacle confusing the issue even further. Someone did a fit to the signals on these monitors which suggested a magnet fault. Eventually, eventually, we gave in and opened up.

The night before the grand opening, I sacrificed myself to another check with a different machine optics; by bumping and scraping we managed three turns and got quite excited at one point before collapsing the following morning. Roger did try and get out of bed when they opened up the vacuum pipe, but I was dead. The rest, of course, was an endless play on a beer refreshing the particles that other beers cannot reach. Very amusing.

The Butt

Subject: follow-up of LEP main quadrupole fault Date: Fri, 07 Apr 2000 18:07:10 +0200 From: Wilhelm KALBREIER <Willi.Kalbreier@cern.ch> Organization: CERN To:Steve Myers <Steve.Myers@cern.ch>, Roger Bailey <Roger.Bailey@cern.ch>, Mike Lamont <Mike.Lamont@cern.ch>, Kurt Hubner <Kurt.Hubner@cern.ch>, John Poole <John.Poole@cern.ch>, Alan Spinks <Alan.Spinks@cern.ch>,

Dear colleagues

During the cold check-out we had a short to earth on a main quadrupole in LEP at location 424 in the arc. As the fault was in the upper half we could replace it by a spare half without breaking the vacuum within a few hours only.

Now we have managed with some ad hoc made tooling to separate the two yokes of the faulty part. To our great surprise we found that the origin of the short was due a damaged insulation layer in one corner of the coil; here the insulation was burnt away by a cigarette butt!! Apparently this butt seemed to have been placed on the top of the quadrupole and then fell down into this new type of ash tray where it continued burning without being noticed. In fact there has been a major activity during the last shutdown of an outside company cutting pipes in this arc.

So fortunately the fault is not due to synchrotron radiation damage of the insulation and as there will be little access we are confident that the magnets will stand the last year.

Finally we have been lucky that the fault has happened at an upper coil; the lower half of such a quadrupole cannot be replaced without breaking the vacuum, because the plate for one support foot is welded across both yokes.

cheers

Willi Kalbreier CERN/SL-MS European Organization for Nuclear Research tel +41 22 767 5278 CH–1211 Geneva 23, Switzerland email Willi.Kalbreier@cern.ch mobile +41 79 201 3049

More Memories in Brief

15 days of hell—1998:

- 5 June RF unit out of phase—4 hour lifetime in physics
- 5 June Power converter oscillating
- 5 & 7 June Leaky SPS × 2, Main power supplies
- 8–10 June Transformer, vacuum valves, power converters, access system, SPS MPS, ramp problems
- 12 June Vacuum leaks near wire scanner, and again
- 13 June Vacuum valves stuck
- 13 June RF frequency synthesizer broke
- 13 June Vacuum leak on separator flange (local heating by SR)

Golden orbit names:

- The gold-plated plastic raincoat
- Dolce far niente. Not terrific but good for Delphi
- The magic mushroom
- Raperonzolo's second baby (0.068 bbts with 4.9 mA)
- Raperonzolo's granddaugther
- Baby Raperonzolo's granddaughter for 45 GeV
- Raperonzolo's black beast
- Ronny's bête noire

Others:

- A rat ate my timing cable
- The ego-maniac physics co-ordinator
- Polarities: skew quads, insertion quads
- John Ellis, the visiting Chinese dignitaries and the Free Tibet Website

1998—a pretty bad year:

- Burnt cables early on
- Timeout for emergency repairs
- Maximum beam–beam tune-shift 0.055
- Integrated luminosity for the year 105 pb⁻¹
- Karl-Heinz impeached
- GIORGIO CAVALLARI—HERO!

JOHN POOLE

Day 1

When we were putting the first beam into the ring, the control room was full of people including the DG several directors and dozens of others. Steve was in charge and I was operating the hardwaretyping in long command strings to move equipment and operate cameras. The tension was enormous-the OK was given to put the first shot in and we saw it strike the luminescent screen on the beam stopper before the first experiment. The beam was fairly central on the screen so I moved the stopper out to allow the beam to pass through the experiment (L3? I can't remember if we were using positrons or electrons)-success again. We continued laboriously like this through Aleph and moved on to the upstream side of Opal (if it was positrons)-still OK but not quite centred. I moved the stopper out and displayed the stopper on the other side of Opal-nothing! We tried several shots and still nothing. Jean-Pierre Koutchouk took out his pocket calculator and calculated a change to a vertical corrector which should centre the beam on the upstream screen. Whilst this was introduced in the power converter by the head of the power group I checked the stopper again and realised that I was displaying the electron screen, which was on the other side of the stopper block! I moved the camera across and we took another shot and there it was and it was full speed ahead from there on!

Control System

In the early days we had Apollo computers which had the feature that you could run a process on any of them from any other one. This allowed for some amusing antics which included connecting to a colleague's machine where he was fighting with the system to get something working and running the 'melt' process. This had the effect of distorting the image on his display in such a way that it appeared to be melting.

Luminosity

For a couple of years the luminosity was much lower in L3 than elsewhere and this was ultimately understood to be due to the superconducting quadrupole cold mass moving after the support strap failed. The luminosity difference was -20%with respect to the other experiments and it is rumoured that the DG said that it was OK as long as it was only L3 that was losing out.

Software Testing

One of the young co-operants who was working for Dick Keyser was very proud of his piece of software which controlled the main dipole power converters for LEP. The 5.7 MVA converters were fed from massive circuit breakers which were the first thing to be switched before running up the converters. The young chap wanted to test his software so he built a small loop which switched these breakers in and out at a few second intervals. It worked extremely well but the guys from the power group who were busy installing control electronics in SR2 were not so impressed-each time the breaker switches there is an enormous bang in the building where they were working. The software engineer was soon introduced to some realities about hardware from our colleagues in the power group!

HELMUT BURKHARDT

I joined Aleph as CERN based member of the Siegen team in 1985 and moved on to LEP operation as EIC (engineer in charge) in July 1990. I was involved in Aleph in the preparation for LEP physics, in luminosity and background monitoring and the signal exchange between the Aleph experiment and the LEP machine. The counting rates and dark currents of several Aleph detector components were collected and made available online to the LEP control room.

This work was done in close collaboration with Joe Rothberg on the Aleph side and Georg von Holtey on the machine side. Luminosity and background information from the experiments were always very important for LEP operation.

LEP was operated from the Prévessin control room together with the SPS. There were typically three people on shift, the EIC who operated LEP, and the SPS shift leader and a control room technician looking after the SPS and dealing with access into the experimental areas and the rings. LEP operation was mainly in the hands of the small group of about eight EICs. We worked on shifts for periods of one week, starting with two or three morning shifts, followed by two to three afternoon and two to three night shifts. In a year this would add up to about one hundred shifts per EIC.

There were always rather close and direct contacts between LEP operation and run co-ordinators and LEP contact persons from the experiments. There were two LEP schedule meetings per week with reports from accelerator operation and the experiments. In addition, it was common to have frequent telephone conversations between the experiments and the LEP control room, and not too rare to have people from the experiments in the LEP/SPS control room. A television screen (Page 1) was kept up to date with the machine status, luminosity and background numbers. LEP operation implied continuous control and tuning of many critical, often slowly drifting parameters like orbits and betatron tunes. There were over 500 pickups to measure the beam orbits around the ring and a similar number of orbit correctors. The tuning was essential to keep good beam lifetimes, optimize luminosity and keep backgrounds at acceptable levels. Higher luminosity generally implied also more blow-up due to the beam-beam interaction and higher backgrounds to the experiments. The beginning of new fills were particularly critical. The pressure from the experiments on LEP operation could be very strong. People running shifts in L3 were instructed to call the LEP control room regularly in case of high backgrounds, or if their luminosity was lower than that of other experiments.

In May 1991, the pressure on LEP operation by telephone calls and people coming to the control room reached a point were the overall efficiency clearly suffered, such that the physics co-ordinator and the division leader had to intervene. A different kind of very close and interesting collaboration between the experiments and the machine has been on energy calibration. The method of resonant depolarization, originally developed in Novosibirsk, was successfully applied in LEP and allowed a measurement of the mean beam energy around the LEP ring with very high precision. Several very small, often rather surprising effects became visible. The first, and probably most popular one, was the observation of tidal effects on the LEP energy. Even if it was in principal well known that small circumference changes produce over thousand times larger energy shifts in big machines, it came to most of us as a surprise that tidal effects from the moon and the sun actually resulted in measurable energy changes in LEP. For LEP this was first explained by Gerry Fischer from SLAC and Albert Hofmann from CERN and discussed in the January 1992 Chamonix workshop. Another small effect found later was that the TGV trains caused ground currents in the Pays de Gex which partially passed through the LEP ring and modified the magnetic fields and LEP energy.

1989

'DID YOU FIND THE HIGGS?'

Steve Wasserbaech

I remember a humorous incident at the plenary 'Tuesday' meeting that was held in the LEP Auditorium on Friday 17 November 1989. We had only collected a few weeks' worth of data since the first collisions in LEP. Jean-François Grivaz was allotted 30 minutes to speak about the Higgs search. (My notes say 'Search for Higgs above $\mu^+\mu^-$ threshold'.) Jean-François gave what I thought was a fine and thorough presentation. At the end of the talk Jack Steinberger raised his hand and said, 'I was asleep for most of this. Did you find the Higgs?'

1993-2000

"...OR DID THE RUN CO-ORDINATOR FIND YOU?"

On Saturday 16 July 1994 the Tour de France came to Aleph or, to be more precise, passed by on the St Genis–Gex road. While my thoughts turned to the opportunity to see the world's finest cyclists racing by, Dieter Schlatter pointed out that Tour de France = closed roads and that I, as Run Co-ordinator needed to solve the problem of getting experts to the pit if needed. My first thought was an edict that no-one was to leave after the 9 o'clock meeting but a study of local maps identified that the Cessy to Echenevex road passes under the D984 and a call to the local police confirmed that it would remain open. Maps were drawn and all subdetector co-ordinators were given a copy well in advance but in the event LEP had problems and all was peaceful with Aleph.

POMPIERC 110						
LEP (PCR) 112 4	144	FUNCTION	NAME	63H (4)	OFFICE	HOME
ICR (General sources praces) 722	201	RUN	MikeGreen	163014	76077	10 04 SD2m(20)
NAVETTE many home 769	63 MESAT ANDA	DAG	P. MATO	160855	78696	10-04 50 101 45
Spokesman 163	30/5	ECHENEVEX	D. HUTCHEROFT	Eclourex	76077	07521031
La Chenaille (BBD) 771	103	. WET	J. ROTHEERE	16.3045	7-3003	10-04-50-42-08-76
SECURITON 1012	05 A)	. ITC .	J. NOWELL	16-0171	75810	3.
GSS / MAGNET 15	051-416 0065	TPC	7. MATO	16-0855	78696	10-04-20-41-03-57
CARD REPDER : No. RAMA 16-0	0107	ECAL	J. LYNCH	TATODECAL	78057	10-04-50206391
LEP Physics codes 16-	0049 - marine	HCAL	F. MURTAS	16 0808	76418	10-04-50209426
GLIMOS JP. FABRE 1605	32	SAMBA	J. HESS	14-3055	72796	7670224
SLIMOS - WAT LEADER	the \$ 95% an	SICAL	O. Buckmatter	SICAL	निशाअ	04.50 282851
ALEPH CHIET 2000 will be and	the surge of	LCAL	see Ecal	TATCO	1	0
SI/AM PHONE IC-SESC		BCAL	JANTER LAPER	163002	76063	10-04-50-42-01-27
SCIENT THOME TO OTON	1	long of	G. LEIBENGUTH	16-41 51	76308	
50N/160 ST 22202 /	MAGAVET	head 2	Mike Green	16,3014	76077	10 04 50206331
ZONE 14401		LEP	J. ROTABERG	- Joye	7-3003	6 01- 50- 42- 08- N
TPC Beep : GATING take go	160121	1. 2. 1		No. of Street, or other		
CRY0 PIQUET : 16-0041						
Constal fast +62.81 /		OFFLINE	MAZO CATTANED	16-03-32	74046	
A. PORKET . BOARD	9	BRBUS	Here Knotontalide	The state of the s	73003	PHONE VER THESE
	2012	TPC LASER	5 Schmellina	16-4272	72376	X0-0950412868
(05 mg ALEPH 73318) 16- 92	121	Prent GAS	D. FROMM	16.04.42	F18335	04.50. 20. 59.66
0	The second	Frai sas	5 10 1-11 345	No.	and the	Contraction of the
Reconcert and the				-		1

The whiteboard in the Control Room on Mike's last shift in 2000 we were still organized towards the end...

A number of us decided that we would walk down the road to watch the race. During what seemed an interminable wait I wondered how we would know when it would arrive but the helicopters provided a good clue followed by a brief buzz as all 160 or so cyclists passed in a formation that would have done Napoleonic soldiers proud in earlier times. I learned that the riders decide some days are not for racing but for a gentle tour of the countryside, although still at a speed that Bertram Rensch would have found hard to match on his daily visit to the pit.

Since my annual fortnight as Run Co-ordinator was normally in July or August I developed a lot of experience at co-ordinating recovery from storms. However, the most 'interesting' power cut I had occurred (possibly in August 1993) as a result of a pylon failure close to CERN since it led not just to a power cut but also to the failure of the phone system to the main CERN site. Of course, Aleph had a backup battery supply for just such an event but backups need regular maintenance if they are to work... Since this was in the days before mobile phones, it appeared that one of us would have to drive to CERN to explain that we were incommunicado and call the subdetector experts from there. Then in a flash of lateral thinking someone remembered the public phone box in Echenevex and, armed with a number of 10 FF pieces (remember those?), went off to make international phone calls to International Rescue (Editor's note-RS: I think that should read 'to the subdetector co-ordinators').

A similar (lack of) maintenance problem with a system no-one ever thought about, occurred in the latter days when a hose on the air conditioning system in barrack A1 failed one morning and filled the cable space under the floor with water. Peter Norton and I spent most of the rest of the morning drying it out—I don't now even recall all the details of how we did it except that there were no paper towels left in the toilets by the time we had finished. (Editor's note-RS: I remember that, too—barrack A1 (A one, I mean) had sunken into disorderly chaos and was filled with a batch of chaotic volunteers like me trying to mop up back to orderly chaos. As often happened, we were lucky (Aluckyeph) and got things going again without losing much data-taking time because the LEP machine had some problems also...)

2000

THE LAST DAY AT ECHENEVEX

Peter Dornan

It was 09.00 on Thursday 2 Nov. 2000, spirits were high, the champagne was ready; it had been an incredible year; we may have at last discovered the Higgs. Few could recall such excitement at CERN. The 2000 run was over, this would be the final Echenevex meeting of the year but surely there must now be another year's running when the great discovery could be made.



Beat, Olivier, Jim & Laurent.



General view of meeting.



Champagne time!



Our Run Co-ordinator, Ioana.

Soon all we discovered was that this day was to be the very last Echenevex meeting, the very last time we would check the detector. At least our ignorance allowed us to enjoy very well deserved champagne at the end of the meeting.



Last day in the Control Room.



Maria leaving Aleph for the last time.

(Editor's note1-JL: I attach the minutes of this last meeting:

Folder:	ALEPH.ECHENEVEX		
From:	AXAONL:ALEPH_SHIFT		
Subject:	9:00 meeting minutes,	Thursday	2-NOV-2000
Date:	2-NOV-2000 08:37:28		
Expires:	17-NOV-2000 09:37:28		

Minutes of the 9 o'clock Echenevex Meeting, Thursday 2-NOV-2000

Run Co-ordinator:	Ioana Videau
LEP Status:	Fill 8986 is con_access
ALEPH Status:	Shutting down, but not dead yet!

The ONLINE scoreboard for fills ended before 9 AM today:

		Energy	LEP lumi	On Tape	effi	Offline	
Fill 8972	@	206.53	195.563	191.574	97.96	190.110	
Fill 8976			153.756	149.329	97.12	146.523	Ĺ
	6	205.93	35.483	33.192	93.54	32.569	Ĺ
	6	206.71	113.573	111.522	98.19	109.426	Ĺ
	6	207.92	0.846	0.833	98.46	0.817	Ĺ
Fill 8977			54.748	53.513	97.75	60.428	
	6	205.33	29.424	28.746	97.69	32.460	Ĺ
	6	206.51	23.448	22.938	97.82	25.902	Ĺ
Fill 8978			269.657	264.897	98.23	251.090	
	6	206.54	236.328	231.992	98.16	219.900	
	6	207.92	32.325	31.914	98.73	30.251	
Fill 8979	6	206.52	96.841	95.095	98.20	96.886	
Fill 8980			220.298	216.032	98.06	214.094	
	6	206.50	213.458	209.300	98.05	207.422	
	6	207.89	5.446	5.361	98.44	5.313	
Fill 8981	6	206.52	144.063	140.907	97.81	128.670	
Fill 8982			145.834	142.775	97.90	129.994	
	6	204.97	60.179	58.852	97.79	53.583	
	6	206.07	81.548	79.895	97.97	72.743	
Fill 8984			77.487	75.679	97.67	78.320	
	6	204.93	73.424	71.701	97.65	74.203	
	0	205.76	0.849	0.832	98.04	0.861	

Year 2000 in pb-1	232.128	221.900	95.59	226.627	
< 91> = 91.28	4.497	4.318	96.02	4.261	
<200> = 199.88	0.836	0.821	98.21	0.812	ĺ
<202> = 201.84	0.688	0.670	97.37	0.666	
<203> = 202.79	1.748	1.719	98.30	1.722	ĺ
<204> = 203.8	7.263	6.847	94.28	6.962	ĺ
<205> = 205.15	72.727	69.122	95.04	70.743	ĺ
<206> = 206.23	20.268	19.487	96.15	19.945	ĺ
<207> = 206.66	113.830	109.199	95.93	111.995	
<208> = 208.13	8.367	7.909	94.53	8.110	ĺ
<209> = 208.75	0.111	0.108	96.58	0.110	ĺ
					_

Shift crew for the next hours :

2-NOV 6:30/15:00 : Andre TILQUIN, Renaud BRUNELIERE 2-NOV 14:30/23:00 : David CLARKE, Aris KYRIAKIS 2-NOV 22:30/ 7:00 : Borss TUCHMAN, Kay HUETTMANN 3-NOV 6:30/15:00 : David CLARK, Renuad BRUNELIERE 3-NOV 14:30/23:00 : Vincent LEMAITRE, Aris KYRIAKIS 3-NOV 22:30/ 7:00 : *** No shift ***

1. Run Coordinator's News (Ioana VIDEAU)

Not a very good day but a good day none the less. LEP delivered 1.293pb^-1 of Data which ALEPH collected with 98% efficiency. 9 fills made physics with about 3 lost. This is good evidence that having happy LEP operators make for smooth running.

The last fill (8984) gave 77.5nb^-1 of which ALEPH collected only 97.67%, this Was ramped to 105 GeV per beam at 8am, unfortunately it did not make it! If it Had we might have still been running.

The operation efficiency was 100%, DAQ only 99.9% as there was a TPC missing source.

The run and DAQ coordinators would like to request that their mobile phones are replaced with more modern ones in the event of a run next year.

2. LEP news (Jim LYNCH => Maria GIRONE)

It was somewhat difficult to get technical details this morning, the party however was very successful and gave some very smooth running.

There was one problem with a power converter though.

3. Subdetector Reports

VDET (Piero-Giorgio VERDINI =>) : OK

The heroic VDET coordinator was still fixing faults at 2:30 this morning! A long standing problem with one Z side that goes to 50% efficiency for a while (probably a thermal effect) happened, its OK now though.....

ITC (Julia SEDGBEER) : OK

TPC (Pere Mato) : OK

ECAL (Laurent DUFLOT) : OK

The expert requested permission to dismantle a crate with a hammer, this is considered, perhaps after tomorrow.

HCAL (Luca PASSALACQUA) : OK

HCAL decided to celebrate by giving the normal small problems.

```
SAMBA ( Gerrit PRANGE ) : OK
 _____
SICAL ( Bertrand VALLAGE ) : OK
-------
LCAL ( See ECAL ) : OK
_____
BCAL ( David PANEQUE ) : OK
_____
LEVEL 1 ( Richard CAVANAUGH ) : OK
 _____
LEVEL 2 ( David Hutchcroft ) : OK
_____
DAQ ( Pere MATO ) : OK
_____
Could the TPC be removed next year? The missing sources are ruining the Efficiency.
ECHENEVEX ( David Hutchcroft ) : OK
```

The coffee machine is not yet scheduled for dimantling.

From tomorrow there are no night shifts. All systems must be off that would Suffer in the event of a cooling/power cut from tomorrow afternoon.

For the year we took an online efficiency of 95.5%, 1.5% extra on tape, 2.3% We will not use. So overall 94.8% for the year, good (in fact the best yet) but we should do better next year!

4. Todays plans

####	###	#	£	###	####	#######	#	÷
#	#	#	#	#	#	#	#	#
#	#	#	#	#	#	#	#	#
####	###	#	#	###	####	#	ī	#
#		####	###	#	#	#	ī	#
#		#	#	#	#	#	ī	#
#		#	#	#	#	#	Ŧ	#

This evening there will be the inaugural (annual) end of ALEPH party. Please register.

NOTE: The first shutdown meeting is at 9am on Tuesday.

Submitted by David HUTCHCROFT

Editor's note2-JL:

As you can see from the minutes:

- Everyone in Aleph was convinced that there was sufficient evidence in the year 2000 data to justify continuation of LEP in 2001.
- I was LEP contact on this day and the enthusiasm for running in 2001 was also evident in the LEP control room, which was in a party mood. The control room was full of LEP personnel, people from the media and champagne! While I was there Steve Myers was being interviewed by 'Radio 5 Live' which was being broadcast live in the UK.

Unfortunately, these hopes were dashed a few weeks later when the CERN management finally decided that LEP running was finished and that the dismantling of LEP and the LEP detectors should start.)

2000-????

ALEPH-WHERE IS IT NOW?

Weekly meetings to discuss and plan the dismantling of Aleph started on 19 September 2000, some two months before the CERN management took the final and misguided decision to 'pull the plug on LEP'. The official confirmation to commence dismantling arrived on the afternoon of Tuesday 21 November 2000.

At the outset there were grandiose ideas about the long-term future of the Aleph detector. Discussions took place about keeping Aleph (or a large part of it) in the cavern at Echenevex as an exhibition for visitors to CERN during the LHC construction phase. This idea was eventually abandoned for practical reasons and the Delphi detector in the LHCb pit was chosen for this purpose.

Other plans for displaying large parts of Aleph included:

- The mounting of one of the Aleph HCAL endcaps at a site in or near CERN and among the sites considered were:
 - 1. The roundabout near St. Genis.
 - 2. The CERN car park.
 - 3. The site of the CERN kindergarten.

Unfortunately, none of these plans have come to fruition, although a final decision is still awaited from the 'Communauté de Communes'.

A Swiss Clock museum had plans to exhibit the Aleph TPC, and a museum in Southern Italy, at one stage, expressed interest in displaying both the Aleph TPC and Coil. Again these plans have been abandoned but, at the present time, it is still hoped to display the TPC in the Technoparc at St Genis. On 2 December 2004 there was an inauguration of an exhibition in the Musée International d'Horlogerie in Geneva where one TPC sector was on display which measures the real time of a collision.

Nevertheless the Aleph detector has not been scrapped and parts of it are on display or still being used in many of the institutes of the Aleph Collaboration and beyond.

Adopting the conventional order used at the morning meetings at Echenevex to describe the fate of the various parts of Aleph:

Subdetector	Location/Institute
VDET1	Bari, Frascati, Munich, Pisa
VDET2	Marseille, Glasgow
ITC	Imperial College
TPC	Technoparc St. Genis
TPC (Sectors)	MPI Munich (3), CERN (3), Barcelona (1), Glasgow (1), Marseilles (1), Orsay (1), Royal Holloway (1), Saclay (1)
TPC Lasers	Mainz, Glasgow
ECAL (Barrel Segments)	Clermont, Marseille, Orsay, Saclay
ECAL (Barrel Electronics)	Orsay
ECAL (End-cap Petals)	RAL, Glasgow (a wire plane only)
ECAL Gas System	Marseille
ECAL Gas (Xe/CO ₂)	ALICE
Xenon Purifier	ALICE
Aleph Coil: (Supplies, cold box, valves,	Saclay
electronics etc.)	
HCAL (Tubes, HT supplies etc.)	A Chinese group
Muon Chambers (plus HCAL tubes)	Beijing
LCAL	Copenhagen
SiCAL	Saclay
BCAL	Barcelona
Samba	Siegen
Level 2 Trigger	Royal Holloway

Among other important items from Aleph that have been recuperated for future use are:

Aleph BBQ	Echenevex, under LHCb (Olivier's?) supervision
Aleph maquette	Glasgow (detector part only)
Aleph drinks machine	SD Division, CERN
ECAL xenon gas	ALICE
Radioactive sources (from ITC, ECAL, HCAL)	TIS Group, CERN
Beryllium beampipe	Vacuum Group, CERN
ECAL flexible gas pipes	CMS
E2 soundproof platform	CMS
Stainless steel water manifolds	CMS
VME crates	Various Aleph Institutes
Large screen monitors	Various Aleph Institutes
Fastbus crates	CERN EP Pool

In conclusion, Aleph continues to serve a useful purpose in many places, inspiring future physicists and engineers and also contributing to future research efforts.

1980-2003

A POEM Anna Vayaki

ALEPH

It had to start with 'A' for a prime mover and great expectations. Slowly and laboriously, built in many labs, it grew almost as if it were a biological organism, needing nursing, tender loving care and long nights of vigils in underground haunted halls, where sometimes bagpipes resounded weirdly, and magnetic fields played havoc with displays. A grand masterpiece, a Stradivarius of detectors, it played the tunes in bytes and bits, morphing to lovely images, obsessed by the search for the melody of melodies that in the end tantalized us all. Caretakers and scholars, we observed the perfect manifestation of nature in microcosm, lured continually onwards by hopes and glimpses of the newest ever theories. Now the song is sung and the last chords die out, Aleph just a memory but recorded well.

1989-2004

ALEPH PUBLICATIONS

- Determination of the Number of Light Neutrino Species Phys.Lett.B231:519,1989
- Properties of Hadronic Events in e⁺e[−] Annihilation at √s = 91 GeV Phys.Lett.B234:209,1990
- Determination of the Leptonic Branching Ratios of the Z Phys.Lett.B234:399,1990
- Search for the Neutral Higgs Boson from Z⁰ Decay Phys.Lett.B236:233,1990
- Search for Supersymmetric Particles Using Acoplanar Charged Particle Pairs from Z⁰ Decays Phys.Lett.B236:86,1990
- A Search for New Quarks and Leptons from Z⁰ Decay Phys.Lett.B236:511,1990
- Search for Excited Leptons in Z⁰ Decay Phys.Lett.B236:501,1990
- 8. Search for Neutral Higgs Bosons from Supersymmetry in Z Decays Phys.Lett.B237:291,1990
- 9. A Precise Determination of the Number of Families with Light Neutrinos and of the Z Boson Partial Widths Phys.Lett.B235:399,1990
- Search for the Neutral Higgs Boson from Z⁰ Decay in the Higgs Mass Range Between 11 GeV and 24 GeV Phys.Lett.B241:141,1990

- ALEPH: A Detector for Electron-Positron Annihilations at LEP Nucl.Instrum.Meth.A294:121-178,1990, Erratum-ibid.A303:393,1991
- A Search for Pair Produced Charged Higgs Bosons in Z⁰ Decays Phys.Lett.B241:623,1990
- Search for Decays of the Z⁰ into a Photon and a Pseudoscalar Meson Phys.Lett.B241:635,1990
- 14. Heavy Flavor Production in Z Decays Phys.Lett.B244:551-565,1990
- Search for Neutralino Production in Z Decays Phys.Lett.B244:541-550,1990
- Search for a Very Light Higgs Boson in Z Decays Phys.Lett.B245:289-297,1990
- 17. Searches for the Standard Higgs Boson Phys.Lett.B246:306-314,1990
- Measurement of Electroweak Parameters from Z Decays into Fermion Pairs Z.Phys.C48:365-392,1990
- 19. Search for Excited Neutrinos in Z Decay Phys.Lett.B250:172-182,1990
- 20. Measurement of the Strong Coupling Constant as from Global Event Shape Variables of Hadronic Z Decays Phys.Lett.B255:623-633,1991
- 21. Measurement of BB Mixing at the Z Phys.Lett.B258:236-246,1991

- 22. Measurement of the B Hadron Lifetime Phys.Lett.B257:492-504,1991
- Measurement of α_s from the Structure of Particle Clusters Produced in Hadronic Z Decays Phys.Lett.B257:479-491,1991
- 24. Measurement of Charge Asymmetry in Hadronic Z Decays Phys.Lett.B259:377-388,1991
- 25. Search for a New Weakly Interacting Particle Phys.Lett.B262:139-147,1991
- Charged Particle Pair Production Associated with a Lepton Pair in Z Decays: Indication of an Excess in the τ Channel Phys.Lett.B263:112-122,1991
- 27. Measurement of the Forward-Backward Asymmetry in $Z \rightarrow b\overline{b}$ and $Z \rightarrow c\overline{c}$ Phys.Lett.B263:325-336,1991
- Measurement of Isolated Photon Production in Hadronic Z Decays Phys.Lett.B264:476-486,1991
- Measurement of the Polarization of τ Leptons Produced in Z Decays Phys.Lett.B265:430-444,1991
- Improved Measurements of Electroweak Parameters from Z Decays into Fermion Pairs Z.Phys.C53:1-20,1992
- 31. Search for the Neutral Higgs Bosons of the MSSM and Other two Doublet Models Phys.Lett.B265:475-486,1991
- 32. An Investigation into Intermittency Z.Phys.C53:21-32,1992
- Production and Decay of Charmed Mesons at the Z Resonance Phys.Lett.B266:218-230,1991
- 34. Searches for New Particles in Z Decays Using the ALEPH Detector Phys.Rept.216:253-340,1992
- 35. Measurement of the Charged Particle Multiplicity Distribution in Hadronic Z Decays Phys.Lett.B273:181-192,1991

- 36. Measurement of the Absolute Luminosity with the ALEPH Detector Z.Phys.C53:375-390,1992
- A Study of Bose-Einstein Correlations in e⁺e⁻ Annihilation at 91 GeV Z.Phys.C54:75-86,1992
- Measurement of τ Branching Ratios Z.Phys.C54:211-228,1992
- 39. Evidence for B Baryons in Z Decays Phys.Lett.B278:209-216,1992
- 40. Measurement of the τ Lepton Lifetime Phys.Lett.B279:411-421,1992
- Evidence for the Triple Gluon Vertex from Measurements of the QCD Color Factors in Z Decay into Four Jets Phys.Lett.B284:151-162,1992
- 42. Measurement of α_s in Hadronic Z Decays Using all Orders Resummed Predictions Phys.Lett.B284:163-176,1992
- 43. Search for a Very Light CP Odd Neutral Higgs Boson of the MSSM Phys.Lett.B285:309-318,1992
- Measurement of BB Mixing at the Z Using a Jet Charge Method Phys.Lett.B284:177-190,1992
- 45. Measurement of the Production Rates of η and η' in Hadronic Z Decays Phys.Lett.B292:210-220,1992
- Properties of Hadronic Decays and Test of QCD Generators Z.Phys.C55:209-234,1992
- 47. Observation of the Semileptonic Decays of B_s and Λ_b Hadrons at LEP Phys.Lett.B294:145-156,1992
- Updated Measurement of the Average B Hadron Lifetime Phys.Lett.B295:174-186,1992
- 49. A Measurement of the B Baryon Lifetime Phys.Lett.B297:449-458,1992
- Measurement of Mean Lifetime and Branching Fractions of B Hadrons Decaying to J/ψ Phys.Lett.B295:396-408,1992

- Measurement of Prompt Photon Production in Hadronic Z Decays Z.Phys.C57:17-36,1993
- 52. Search for CP Violation in $Z \rightarrow \tau \tau$ Phys.Lett.B297:459-468,1992
- 53. Measurement of the $B \rightarrow \tau \overline{\nu_{\tau}} X$ Branching Ratio Phys.Lett.B298:479-491,1993
- 54. A Precise Measurement of the τ Lepton Lifetime Phys.Lett.B297:432-448,1992
- 55. Search for Particles with Unexpected Mass and Charge in Z Decays Published in Phys.Lett.B303:198-208,1993
- Measurement of the Strong Coupling Constant Using τ Decays Phys.Lett.B307:209-220,1993
- 57. Measurement of the B
 ⁰ and B-Meson Lifetimes
 Phys.Lett.B307:194-208,1993,
 Erratum-ibid.B325:537-538,1994
- Measurement of the τ Polarization at the Z Resonance Z.Phys.C59:369-386,1993
- 59. Search for Contact Interactions in the Reaction $e^+e^- \rightarrow \ell^+\ell^-$ and $e^+e^- \rightarrow \gamma\gamma$ Z.Phys.C59:215-230,1993
- 60. Update of Electroweak Parameters from Z Decays Z.Phys.C60:71-82,1993
- 61. Search for High Mass Photon Pairs in $e^+e^- \rightarrow f\bar{f}\gamma\gamma$ (f = e, μ,τ,ν,q) at LEP Phys.Lett.B308:425,1993
- 62. An Experimental Study of γγ → Hadrons at LEP Phys.Lett.B313:509-519,1993
- 63. Search for the Standard Model Higgs Boson Phys.Lett.B313:299-311,1993
- 64. Search for a Nonminimal Higgs Boson Produced in the Reaction e⁺e⁻ → HZ* Phys.Lett.B313:312-325,1993

- 65. Measurement of the B Hadron Lifetime with the Dipole Method Phys.Lett.B314:459-470,1993
- 66. Observation of the Time Dependence of B⁽¹⁾_d-B⁽¹⁾_d Mixing Phys.Lett.B313:498-508,1993
- 67. First Measurement of the B_s Meson Mass Phys.Lett.B311:425-430,1993, Erratum-ibid.B316:631,1993
- 68. A Precise Measurement of $\Gamma(Z \rightarrow b\overline{b})/\Gamma(Z \rightarrow hadrons)$ Phys.Lett.B313:535-548,1993
- 69. Measurement of the Ratio $\Gamma_{b\overline{b}}/\Gamma_{hadron}$ Using Event Shape Variables Phys.Lett.B313:549-563,1993
- 70. A Direct Measurement of the Invisible Width of the Z from Single Photon Counting Phys.Lett.B313:520-534,1993
- 71. Correlation Measurements in $Z \rightarrow \tau^+\tau^-$ and the ν_{τ} Helicity Phys.Lett.B321:168-176,1994
- 72. Production of Charmed Mesons in Z Decays Z.Phys.C62:1-14,1994
- 73. Measurement of the B⁰ Lifetime Phys.Lett.B322:275-286,1994
- 74. An Investigation of B^0_d and B^0_s Oscillation Phys.Lett.B322:441-458,1994
- 75. Heavy Flavor Production and Decay with Prompt Leptons in the ALEPH Detector Z.Phys.C62:179-198,1994
- 76. Heavy Quark Tagging with Leptons in the ALEPH Detector Nucl.Instrum.Meth.A346:461-475,1994
- Z Production Cross-Sections and Lepton Pair Forward-Backward Asymmetries Z.Phys.C62:539-550,1994
- 78. One Prong τ Decays into Charged Kaons Phys.Lett.B332:209-218,1994
- K⁰ Production in one Prong τ Decays Phys.Lett.B332:219-227,1994

- Production of K⁰ and Λ in Hadronic Z Decays Z.Phys.C64:361-374,1994
- Measurement of A_b^{PB} in Lifetime Tagged Heavy Flavor Z Decays Phys.Lett.B335:99-108,1994
- 82. Observation of Mono-Jet Events and Tentative Interpretation Phys.Lett.B334:244-252,1994
- 83. Measurement of the $B \rightarrow \tau \overline{\nu}_{\tau} X$ Branching Ratio and an Upper Limit on $B^- \rightarrow \tau \overline{\nu}_{\tau}$ Phys.Lett.B343:444-452,1995
- Study of the Four Fermion Final States at the Z Resonance Z.Phys.C66:3-18,1995
- 85. Performance of the ALEPH Detector at LEP Nucl.Instrum.Meth.A360:481-506,1995
- 86. Search for CP Violation in the Decay $Z \rightarrow \tau^+\tau^-$ Phys.Lett.B346:371-378,1995
- A Study of D*±π Production in Semileptonic B Decay Phys.Lett.B345:103-114,1995
- Study of the Subjet Structure of Quark and Gluon Jets Phys.Lett.B346:389-398,1995
- Inclusive π[±]K[±] and p,p Differential Cross-Sections at the Z Resonance Z.Phys.C66:355-366,1995
- 90. Michel Parameters and v_τ Helicity from Decay Correlations in Z → τ⁺τ⁻ Phys.Lett.B346:379-388,1995, Erratum-ibid.B363:265,1995
- 91. An Upper Limit for the τ Neutrino Mass from $\tau \rightarrow 5\pi \ (\pi^0) \ \nu_{\tau}$ -Decays Phys.Lett.B349:585-596,1995
- 92. Search for Supersymmetric Particles with R-Parity Violation in Z Decays Phys.Lett.B349:238-252,1995
- 93. Improved τ Polarization Measurement Z.Phys.C69:183-194,1996
- 94. Test of the Flavor Independence of α_s Phys.Lett.B355:381-393,1995

- 95. Measurement of the D*±-Cross-Section in two Photon Collisions at LEP Phys.Lett.B355:595-605,1995
- 96. The Forward-Backward Asymmetry for Charm Quarks at the Z Pole Phys.Lett.B352:479-486,1995
- 97. Measurement of the B Baryon Lifetime Phys.Lett.B357:685-698,1995
- 98. Measurements of the Charged Particle Multiplicity Distribution in Restricted Rapidity Intervals Z.Phys.C69:15-26,1995
- 99. Limit on B⁰ Oscillation Using a Jet Charge Method Phys.Lett.B356:409-422,1995
- 100. First Measurement of the Quark to Photon Fragmentation Function Z.Phys.C69:365-378,1996
- 101. Measurement of D^+_{ς} Meson Production in Z Decays and the $\overline{B}^0_{\varsigma}$ Lifetime Z.Phys.C69:585-596,1996
- 102. Measurement of α_s from Scaling Violations in Fragmentation Functions in e⁺e⁻ Annihilation Phys.Lett.B357:487-499,1995, Erratum-ibid.B364:247-248,1995
- 103. Inclusive Production of Neutral Vector Mesons in Hadronic Z Decays Z.Phys.C69:379-392,1996
- 104. Measurement of the Effective b Quark Fragmentation Function at the Z Resonance Phys.Lett.B357:699-714,1995
- 105. A Measurement of the $|V_{cb}|$ from $\overline{B}^0 \rightarrow D^{*\pm} \ell \overline{\ell} \nu$ Phys.Lett.B359:236-248,1995
- 106. Production of Excited Beauty States in Z Decays Z.Phys.C69:393-404,1996
- 107. Measurement of the B⁽⁾ Lifetime and Production Rate with $D_s \ell^+$ Combinations in Z Decays Phys.Lett.B361:221-233,1995
- 108. Measurement of the τ Lepton Lifetime Z.Phys.C70:549-560,1996

- 109. τ Leptonic Branching Ratios Z.Phys.C70:561-578,1996
- 110. A Precise Measurement of the Average B Hadronic Lifetime Phys.Lett.B369:151-162,1996
- 111. τ Hadronic Branching Ratios Z.Phys.C70:579-608,1996
- 112. Measurement of Λ_b Polarization in Z Decays Phys.Lett.B365:437-447,1996
- 113. Quark and Gluon Jet Properties in Symmetric Three Jet Events Phys.Lett.B384:353-364,1996
- 114. Measurement of Λ Polarization from Z Decays Phys.Lett.B374:319-330,1996
- 115. Improved Measurement of the B⁰ and B-Meson Lifetimes
 Z.Phys.C71:31-44,1996 (Title changed in journal)
- 116. Search for Supersymmetric Particles in e⁺e⁻ Collisions at Center-of-Mass Energies of 130 GeV and 136 GeV Phys.Lett.B373:246-260,1996
- 117. Determination of sin²0^{cff}_{vv} Using Jet Charge Measurements in Hadronic Z Decays Z.Phys.C71:357-378,1996
- 118. Study of the $B_{s}^{0}-\overline{B}_{s}^{0}$ Oscillation Frequency Using $D_{s}\ell^{+}$ Combinations in Z Decays Phys.Lett.B377:205-221,1996
- 119. Measurement of the Mass of the Λ_b Baryon Phys.Lett.B380:442-452,1996
- 120. A Study of Single and Multi-Photon Production in e⁺e⁻ Collisions at Center-of-Mass Energies of 130 GeV and 136 GeV Phys.Lett.B384:333-342,1996
- 121. Four Jet Final State Production in e⁺e⁻ Collisions at Center-of-Mass Energies of 130 GeV and 136 GeV Z.Phys.C71:179-198,1996
- 122. Measurement of the B Forward-Backward Asymmetry and Mixing Using High p_T Leptons Phys.Lett.B384:414-426,1996

- 123. Mass Limit for the Standard Model Higgs Boson with the Full LEP-1 ALEPH Data Sample Phys.Lett.B384:427-438,1996
- 124. Search for CP Violation in the Decay $Z \rightarrow b\overline{b}g$ Z.Phys.C69:585-596,1996
- 125. Strange Baryon Production and Lifetime in Z Decays Phys.Lett.B384:449-460,1996
- 126. Mass Limit for the Lightest Neutralino Z.Phys.C72:549-559,1996, hep-ex/9607009
- 127. Search for Heavy Lepton Pair Production in e⁺e⁻ Collisions at Center-of-Mass Energies of 130 GeV and 136 GeV Phys.Lett.B384:439-448,1996
- 128. Studies of QCD in e⁺e⁻ → Hadrons at ECM = 130 GeV and 136 GeV Z.Phys.C73:409-420,1997
- 129. Transverse Momentum Correlations in Hadronic Z Decays Z.Phys.C73:421-432,1997
- 130. Search for Excited Leptons at 130 GeV and 140 GeV Phys.Lett.B385:445-453,1996
- 131. Four Fermion Production in e⁺e⁻ Collisions at Center-of-Mass Energies of 130 GeV and 136 GeV Phys.Lett.B388:419-430,1996
- 132. Measurement of Hadron and Lepton Pair Production from e⁺e⁻ Annihilation at Center-of-Mass Energies of 130 GeV and 136 GeV Phys.Lett.B378:373-384,1996
- 133. Search for Charginos and Neutralinos with R-Parity Violation at √s = 130 GeV and 136 GeV Phys.Lett.B384:461-470,1996
- 134. Production of Orbitally Excited Charm Mesons in Semileptonic B Decays Z.Phys.C73:601-612,1997
- 135. A Study of τ Decays Involving η and ω Mesons Z.Phys.C74:263-273,1997

308

- 136. Improved Measurement of the $B_d^0 \overline{B}_d^0$ Oscillation Frequency Z.Phys.C75:397-407,1997
- 137. Observation of Charmless Hadronic B Decays Phys.Lett.B384:471-480,1996
- 138. Charm Counting in B Decays Phys.Lett.B388:648-658,1996
- 139. Measurements of $|V_{cb}|$, Form-Factors and Branching Fractions in the Decays $\overline{B}^0 \rightarrow D^{*\pm} \ell \overline{\ell} \nu$ and $\overline{B}^0 \rightarrow D^+ \ell \overline{\ell} \nu$ Phys.Lett.B395:373-387,1997
- 140. Inclusive Production of Neutral Pions in Hadronic Z Decays Z.Phys.C74:451-461,1997
- 141. Studies of Quantum Chromodynamics with the ALEPH Detector Phys.Rep.294:1-165,1998
- 142. The Topology Dependence of Charged Particle Multiplicities in Three Jet Events Z.Phys.C76:191-199,1997
- 143. A Measurement of the QCD Color Factors and a Limit on the Light Gluino Z.Phys.C76:1-14,1997
- 144. Study of the Muon Pair Production at Center-of-Mass Energies from 20 GeV to 136 GeV with the ALEPH Detector Phys.Lett.B399:329-341,1997
- 145. Measurement of the Spectral Functions of Vector Current Hadronic τ Decays Z.Phys.C76:15-33,1997
- 146. Measurement of the τ Lepton Lifetime with the Three-Dimensional Impact Parameter Method Z.Phys.C74:387-398,1997
- 147. A Measurement of R_b Using a Lifetime Mass Tag Phys.Lett.B401:150-162,1997
- 148. A Measurement of R_b Using Mutually Exclusive Tags Phys.Lett.B401:163-175,1997
- 149. Measurement of the Branching Fraction for $D^0 \rightarrow K^-\pi^+$ Phys.Lett.B403:367-376,1997

- 150. Measurement of the W Mass in e⁺e⁻ Collisions at Production Threshold Phys.Lett.B401:347-362,1997
- 151. Search for the B_c Meson in Hadronic Z Decays Phys.Lett.B402:213-226,1997
- 152. Measurement of the Transverse Spin Correlations in the Decay $Z \rightarrow \tau^+\tau^-$ Phys.Lett.B405:191-201,1997
- 153. Search for Sleptons in e⁺e⁻ Collisions at Center-of-Mass Energies of 161 GeV and 172 GeV Phys.Lett.B407:377-388,1997, hep-ex/9706006
- 154. Search for Pair Production of Long-Lived Heavy Charged Particles in e⁺e⁻ Annihilation Phys.Lett.B405:379-388,1997, hep-ex/9706013
- 155. Three Prong τ Decays with Charged Kaons Eur.Phys.J.C1:65-79,1998
- 156. Search for the Standard Model Higgs Boson in e⁺e[−] Collisions at √s = 161 GeV, 170 GeV and 172 GeV Phys.Lett.B412:155-172,1997
- 157. Search for the Neutral Higgs Bosons of the MSSM in e⁺e[−] Collisions at √s from 130 GeV to 172 GeV Phys.Lett.B412:173-188,1997
- 158. Searches for Scalar Top and Scalar Bottom Quarks at LEP2 Phys.Lett.B413:431-446,1997, hep-ex/9708013
- 159. Updated Measurement of the τ Lifetime Phys.Lett.B414:362-372,1997, hep-ex/9710026
- 160. Measurement of the W Pair Cross-Section in e⁺e⁻ Collisions at 172 GeV Phys.Lett.B415:435-444,1997
- 161. Measurement of the B Baryon Lifetime and Branching Fractions in Z Decays Eur.Phys.J.C2:197-211,1998

- 162. Searches for Supersymmetry in Photon(s) plus Missing Energy Channels at $\sqrt{s} = 161$ GeV and 172 GeV Phys.Lett.B420:127-139,1998, hep-ex/9710009
- 163. Search for Charged Higgs Bosons in e⁺e[−] Collisions at Center-of-Mass Energies from 130 GeV to 172 GeV Phys.Lett.B418:419-429,1998
- 164. Searches for Charginos and Neutralinos in e⁺e[−] Collisions at √s = 161 GeV and 172 GeV Eur.Phys.J.C2:417-439,1998, hep-ex/9710012
- 165. An Upper Limit on the τ Neutrino Mass from Three-Prong and Five-Prong τ Decays. Eur.Phys.J.C2:395-406,1998
- 166. Search for Supersymmetry with a Dominant R-Parity Violating ℓℓe⁻ Coupling in e⁺e⁻ Collisions at Center-of-Mass Energies of 130 GeV to 182 GeV. Eur.Phys.J.C4:433-451,1998, e-Print Archive: hep-ex/9712013
- 167. A Measurement of the Semileptonic Branching Ratio BR(B_{baryon} → pℓνX and a Study of Inclusive π[±], K[±], (p,p) Production in Z Decays Eur.Phys.J.C5:205-227,1998
- 168. Four Final State Production in e⁺e⁻ Collisions at Center-of-Mass Energies Ranging from 130 GeV to 184 GeV Phys.Lett.B420:196-204,1998
- 169. Study of B⁰ Oscillations and Lifetime Using Fully Reconstructed D_s-Decays Eur.Phys.J.C4:367-385,1998
- 170. Measurement of the W Mass by Direct Reconstruction in e⁺e⁻ Collisions at 172 GeV Phys.Lett.B422:384-398,1998
- 171. Measurement of Triple Gauge Boson Couplings at 172 GeV Phys.Lett.B422:369-383,1998
- 172. K⁽⁾ Production in τ Decays Eur.Phys.J.C4:29-45,1998

- 173. Measurement of the Spectral Functions of Axial-Vector Hadronic τ Decays and Determination of $\alpha_s(M^2(\tau))$ Eur.Phys.J.C4:409-431,1998
- 174. Measurement of the Structure of Quark and Gluon Jets in Hadronic Z Decays Eur.Phys.J.C17:1-18,2000
- 175. Resonant Structure and Flavor Tagging in the Bπ[±] System Using Fully Reconstructed B Decays Phys.Lett.B425:215-226,1998
- 176. Search for Evidence of Compositeness at LEP I Eur.Phys.J.C4:571-590,1998
- 177. Observation of Doubly Charmed B Decays at LEP Eur.Phys.J.C4:387-407,1998
- 178. Determination of Λ_b^{FB} Using Jet Charge Measurements in Z Decays Phys.Lett.B426:217-230,1998
- 179. Measurement of the Fraction of Hadronic Z Decays into Charm Quark Pairs Eur.Phys.J.C4:557-570,1998
- 180. A Measurement of the Inclusive b → sγ Branching Ratio Phys.Lett.B429:169-187,1998
- 181. Single Photon and Multiphoton Production in e⁺e⁻ Collisions at a Center-of-Mass Energy of 183 GeV Phys.Lett.B429:201-214,1998
- 182. Determination of $|V_{ub}|$ from the measurement of the Inclusive Charmless Semileptonic Branching Ratio of B Hadrons Eur.Phys.J.C6:555-574,1999
- 183. Scalar Quark Searches in e^+e^- Collisions at $\sqrt{s} = 181 \text{ GeV}-184 \text{ GeV}$ Phys.Lett.B434:189-199,1998, hep-ex/9810028
- 184. Search for Sleptons in e⁺e⁻ Collisions at Center-of-Mass Energies up to 184 GeV Phys.Lett.B433:176-194,1998
- 185. The Forward-Backward Asymmetry for Charm Quarks at the Z Phys.Lett.B434:415-425,1998, hep-ex/9811015
- 186. A Measurement of the Gluon Splitting Rate into bb pairs in Hadronic Z Decays Phys.Lett.B434:437-450,1998
- 187. Search for B⁰ Oscillations Using Inclusive Lepton Events Eur.Phys.J.C7:553-569,1999, hep-ex/9811018
- 188. Study of D⁰-D⁰ Mixing and D⁰ Doubly Cabibbo Suppressed Decays Phys.Lett.B436:211-221,1998, hep-ex/9811021
- 189. Search for the Standard Model Higgs Boson at the LEP2 Collider Near √s = 183 GeV Phys.Lett.B440:403-418,1998, Phys.Lett. B447:336-351,1999, hep-ex/9811032
- 190. Search for the Neutral Higgs Boson of the MSSM in e⁺e[−] Collisions at Center-of-Mass Energies of 181 GeV to 184 GeV Phys.Lett.B440:419-434,1998
- 191. Search for Supersymmetry with a Dominant R-Parity Violating LQD Coupling in e⁺e⁻ Collisions at Center-of-Mass Energies of 130 GeV to 172 GeV Eur.Phys.J.C7:383-405,1999, hep-ex/9811033
- 192. Measurement of Triple Gauge WWγ Couplings at LEP2 Using Photonic Events Phys.Lett.B445:239-248.1998, hep-ex/9901030
- 193. Analysis of Transverse Momentum Correlations in Hadronic Z Decays Phys.Lett.B447:183-198,1999
- 194. Search for Charged Higgs Bosons in e⁺e⁻ Collisions at √s = 181 GeV–184 GeV Phys.Lett.B450:467,1999, hep-ex/9902031
- 195. Search for Invisible Higgs Boson Decays in e⁺e⁻ Collisions at Center-of-Mass Energies up to 184 GeV Phys.Lett.B450:301-312,1999

- 196. Search for Charginos and Neutralinos in e⁺e⁻ Collisions at Center-of-Mass Energies Near 183 GeV and Constraints on the MSSM Parameter Space Eur.Phys.J.C11:193-216,1999
- 197. One Prong τ Decays with Kaons Eur.Phys.J.C10:1-18,1999, hep-ex/9903014
- 198. Study of τ Decays Involving Kaons, Spectral Functions and Determination of the Strange Quark Mass Eur.Phys.J.C11:599-618,1999, hep-ex/9903015
- 199. Measurement of W Pair Production in e⁺e⁻ Collisions at 183 GeV Phys.Lett.B453:107-120,1999, hep-ex/9903053
- 200. Measurement of the W Mass in e⁺e⁻ Collisions at 183 GeV Phys.Lett.B453:121-137,1999
- 201. Study of Fermion Pair Production in e⁺e⁻ Collisions at 130 GeV to 183 GeV Eur.Phys.J.C12:183-207,2000, hep-ex/9904011
- 202. Measurement of the Hadronic Photon Structure Function at LEP-1 for Q² Values between 9.9 GeV² and 284 GeV² Phys.Lett.B458:152-166,1999
- 203. A Study of Single W Production in e^+e^- Collisions at $\sqrt{s} = 161$ GeV to 183 GeV Phys.Lett.B462:389-400,1999
- 204. Determination of the LEP Center-of-Mass Energy from Zγ Events Phys.Lett.B464:339-349,1999, hep-ex/9907043
- 205. Search for R-Parity Violating Decays of Supersymmetric Particles in e⁺e⁻ Collisions at Center-of-Mass Energies Near 183 GeV Eur.Phys.J.C13:29-46,2000
- 206. Measurement of the Z Resonance Parameters at LEP Eur.Phys.J.C14:1-50,2000
- 207. Study of Charm Production in Z Decays Eur.Phys.J.C16:597-611,2000, hep-ex/9909032

- 208. Inclusive Production of π^0 , η , η' (958), K^0_{\backslash} and Λ in Two Jet and Three Jet Events from Hadronic Z Decays Eur.Phys.J.C16:613,2000
- 209. Search for an Invisibly Decaying Higgs Boson in e⁺e⁻ Collisions at 189 GeV Phys.Lett.B466:50-60,1999
- 210. A Direct Measurement of |V_{cs}| in Hadronic W Decays Using a Charm Tag Phys.Lett.B465:349-362,1999
- 211. Measurement of the e⁺e → ZZ Production Cross-Section at Center-of-Mass Energies of 183 GeV and 189 GeV Phys.Lett.B469:287-302,1999, hep-ex/9911003
- 212. Searches for Sleptons and Squarks in e⁺e⁻ Collisions at 189 GeV Phys.Lett.B469:303-314,1999
- 213. Search for the Glueball Candidates f_0^{1500} and f_J^{1720} in $\gamma\gamma$ Collisions Phys.Lett.B472:189-199,2000, hep-ex/9911022
- 214. Bose-Einstein Correlations in W Pair Decays Phys.Lett.B478:50-64,2000
- 215. Search for Gauge Mediated SUSY Breaking Topologies at √s Similar to 189 GeV Eur.Phys.J.C16:71-85,2000
- 216. Fermi-Dirac Correlations in Λ Pairs in Hadronic Z Decays Phys.Lett.B475:395-406,2000
- 217. Search for the Neutral Higgs Bosons of the Standard Model and the MSSM in e^+e^- Collisions at $\sqrt{s} = 189$ GeV Eur.Phys.J.C17:223-240,2000
- 218. A Study of the Decay Width Difference in thee $B_{\chi}^{()}-\overline{B}_{\chi}^{()}$ System Using $\varphi\varphi$ Correlations Phys.Lett.B486:286-299,2000
- 219. Measurement of the W Mass and Width in e⁺e⁻ Collisions at 189 GeV Eur.Phys.J.C17:241-261,2000

- 220. Measurement of W Pair production in e⁺e⁻ Collisions at 189 GeV Phys.Lett.B484:205-217,2000, hep-ex/0005043
- 221. Search for gg Decays of a Higgs Boson Produced in Association with a Fermion Pair in e⁺e⁻ Collisions at LEP Phys.Lett.B487:241-252,2000, hep-ex/0008004
- 222. Search for a Scalar Top almost Degenerate with the Lightest Neutralino in e⁺e⁻ Collisions at √s up to 202 GeV Phys.Lett.B488:234-246,2000
- 223. Search for Charged Higgs Bosons in e⁺e⁻ Collisions at Energies up to √s = 189 GeV Phys.Lett.B487:253-263,2000, hep-ex/0008005
- 224. A Measurement of the b Quark Mass from Hadronic Z decays Eur.Phys.J.C18:1-13,2000, hep-ex/0008013
- 225. Measurement of the \overline{B}^0 and B Meson Lifetimes Phys.Lett.B492:275-287,2000, hep-ex/0008016
- 226. Investigation of Inclusive CP Asymmetries in B⁰ Decays Eur.Phys.J.C20:431-443,2001
- 227. Search for Single Top Production in e^+e^- Collisions at $\sqrt{s} = 189$ GeV–202 GeV Phys. Lett. B494:33-45,2000
- 228. Study of the CP Asymmetry of $B^0 \rightarrow J/\psi K^0_{s}$ Decays in ALEPH Phys.Lett.B492:259-274,2000, hep-ex/0009058
- 229. Measurements of BR($b \rightarrow \tau \overline{\nu_{\tau}}X$) and BR($b \rightarrow \tau \overline{\nu_{\tau}}D^{*\pm}X$) and Upper Limits on BR($b \rightarrow \tau \overline{\nu_{\tau}}$) and BR($b \rightarrow sv\overline{v}$) Eur.Phys.J.C19:213-227,2001, hep-ex/0010022
- 230. Searches for Neutral Higgs Bosons in e⁺e⁻ Collisions at Center-of-Mass Energies from 189 GeV up to 202 GeV Phys.Lett.B499:53-66,2001, hep-ex/0010062

- 231. Search for R-Parity Violating Decays of Supersymmetric Particles in e⁺e⁻ Collisions at Center-of-Mass Energies from 189 GeV up to 202 GeV Eur.Phys.J.C19:415-428,2001, hep-ex/0011008
- 232. Observation of an Excess in the Search for the Standard Model Higgs Boson at ALEPH Phys.Lett.B495:1-17,2000, hep-ex/0011045
- 233. Search for Supersymmetric Particles in e⁺e[−] Collisions at √s up to 202 GeV and Mass Limit for the Lightest Neutralino Phys.Lett.B499:67-84,2001, hep-ex/0011047
- 234. Measurement of Triple Gauge Boson Couplings at LEP Energies up to 189 GeV Eur.Phys.J.C21:423-441,2001, hep-ex/0104034
- 235. Measurement of the τ Polarization at LEP Eur.Phys.J.C20:401-430,2001, hep-ex/0104038
- 236. Study of the Fragmentation of b Quarks into B Mesons at the Z Peak Phys.Lett.B512:30-48,2001, hep-ex/0106051
- 237. Measurement of the Michel Parameters and the v_{τ} Helicity in τ Lepton Decays Eur.Phys.J.C22:217-230,2001
- 238. Measurement of AbB Using Inclusive B Hadron Decays Eur.Phys.J.C22:201-215,2001, hep-ex/0107033
- 239. Inclusive Semileptonic Branching Ratios of b Hadrons Produced in Z Decays Eur.Phys.J.C22:613-626,2002, hep-ex/0108007
- 240. Production of D_s^{**} Mesons in Hadronic Z Decays Phys.Lett.B526:34-49,2002, hep-ex/0112010
- 241. Search for Scalar Leptons in e⁺e⁻ Collisions at Center-of-Mass Energies up to 209 GeV Phys.Lett.B526:206-220,2002, hep-ex/0112011

- 242. Inclusive Production of the ω and η Mesons in Z Decays and the Muonic Branching Ratio of the ω Phys.Lett.B528:19-33,2002, hep-ex/0201012
- 243. Search for R-Parity Violating Production of Single Sneutrinos in e⁺e⁻ Collisions at $\sqrt{s} = 189 \text{ GeV}$ to 209 GeV Eur.Phys.J.C25:1-12,2002, hep-ex/0201013
- 244. Final Results of the Searches for Neutral Higgs Bosons in e⁺e[−] Collisions at √s up to 209 GeV Phys.Lett.B526:191-205,2002, hep-ex/0201014
- 245. Leptonic Decays of the D_s Meson Phys.Lett.B528:1-18,2002, hep-ex/0201024
- 246. Search for $\gamma\gamma \rightarrow \eta_b$ in e⁺e⁻ Collisions at LEP-2 Phys.Lett.B530:56-66,2002, hep-ex/0202011
- 247. Measurement of the Forward Backward Asymmetry in $Z \rightarrow b\overline{b}$ and $Z \rightarrow c\overline{c}$ with Leptons Eur.Phys.J.C24:177-191,2002
- 248. Improved Search for B^0_s - \overline{B}^0_s -Oscillations Eur. Phys. J., C29:143-170,2003
- 249. Search for Charginos Nearly Mass Degenerate with the Lightest Neutralino in e⁺e⁻ Collisions at Center-of-Mass Energies up to 209 GeV Phys.Lett.B533:223-236,2002, hep-ex/0203020
- 250. Search for Gauge Mediated SUSY Breaking Topologies in e⁺e⁻ Collisions at Center-of-Mass Energies up to 209 GeV Eur.Phys.J.C25:339-351,2002, hep-ex/0203024
- 251. Search for Scalar Quarks in e⁺e[−] Collisions at √s up to 209 GeV Phys.Lett.B537:5-20,2002, hep-ex/0204036
- 252. A Flavor Independent Higgs Boson Search in e⁺e[−] Collisions at √s up to 209 GeV Phys.Lett.B544:25-34,2002, hep-ex/0205055

- 253. Measurements of the Strong Coupling Constant and the QCD Color Factors Using Four Jet Observables from Hadronic Z Decays Eur.Phys.J.C27:1-17,2003
- 254. Single Photon and Multiphoton Production in e⁺e[−] Collisions at √s up to 209 GeV Eur.Phys.J.C28:1-13,2003
- 255. Search for Single Top Production in e⁺e⁻ Collisions at √s up to 209 GeV Phys.Lett.B543:173-182,2002, hep-ex/0206070
- 256. Search for Charged Higgs Bosons in e⁺e⁻ Collisions at Energies up to √s = 209 GeV Phys.Lett.B543:1-13,2002, hep-ex/0207054
- 257. Absolute Lower Limits on the Masses of Selectrons and Sneutrinos in the MSSM Phys.Lett.B544:73-88,2002, hep-ex/0207056
- 258. Search for γγ Decays of a Higgs Boson in e⁺e⁻ Collisions at √s up to 209 GeV Phys.Lett.B544:16-24,2002
- 259. Search for Anomalous Dipole Moments of the τ Lepton Submitted to Eur.Phys.J.C, hep-ex/0209066
- 260. Search for Supersymmetric Particles with Parity Violating Decays in e⁺e[−] Collisions at √s up to 209 GeV Submitted to Eur.Phys.J.C, hep-ex/0210014
- 261. Measurement of the Inclusive D*± Production in γγ Collisions at LEP Eur.Phys.J.C28: 437-449,2003, hep-ex/0301034
- 262. Study of Hadronic Final States from Double Tagged γγ Events at LEP Submitted to Eur.Phys.J.C, hep-ex/0305107
- 263. Search for stable hadronizing squarks and gluinos in e^+e^- collisions up to $\sqrt{s} = 209 \text{ GeV}$ Eur.Phys.J.C31:327-342,2003
- 264. A Measurement of the Gluon Splitting into cc Pairs in Hadronic Z Decays Phys.Lett.B561:213-224,2003

- 265. Exclusive Production of Pion and Kaon Meson Pairs in Two Photon Collisions at LEP Phys.Lett.B569:140-150,2003
- 266. Measurement of the Hadronic Photon Structure Function F2(x,Q²) in Two-Photon Collisions at LEP Eur.Phys.J.C30:145-158,2003
- 267. Absolute Mass Lower Limit for the Lightest Neutralino of the MSSM from e⁺e[−] Data at √s up to 209 GeV Phys.Lett.B583:247-263,2004
- 268. Two-Dimensional Analysis of Bose–Einstein Correlations in Hadronic Z Decays at LEP Eur.Phys.J.C36:147-159,2004
- 269. Studies of QCD at e⁺e⁻ Centre-of-Mass Energies between 91 GeV and 209 GeV Eur.J.Phys.C35:457-486,2004
- 270. Search for Pentaquark states in Z decays Phys.Lett.B599:1-16,2004
- 271 Constraints on Anomalous QCG's in e⁺e⁻ Interactions from 183 to 209 GeV Phys.Lett.B602:31-40,2004
- 272 Measurement of W-pair production in e⁺e⁻ collisions at center of mass energies from 183 to 209 GeV CERN-PH-EP-2004-012; Submitted to Eur.Phys.J.C
- 273 Two particle correlations in pp, pp, K₀⁰-K₀⁰
 pairs from hadronic Z decays
 CERN-PH-EP-2004-033;
 Submitted to Phys.Lett.B.
- 274 Single Vector Boson production in e⁺e⁻ collisions at centre of mass energies from 183 to 209 GeV
 CERN-PH-EP-2004-034;
 Submitted to Phys.Lett.B.
- 275 Bose–Einstein correlations in W-pair decays with an event–event technique CERN-PH-EP-2004-053; Submitted to Phys.Lett.B.

To be continued...

1989-2004

'ETERNAL' ALEPH AUTHOR LIST

The following list contains the names of all Aleph personnel whose names have appeared on at least one Aleph publication:

AACHEN I – Physikalisches Institut der RWTH – AACHEN, Germany A. Heister, S. Schael

ANNECY – Laboratoire de Physique des Particules, LAPP, France

R. Barate, R. Bruneliere, I. de Bonis, D. Buskulic, D. Decamp, B. Deschineaux,

D. Dufournaud, P. Ghez, C. Goy, S. Jezequel, J-P. Lees, A. Lucotte, F. Martin,

E. Merle, M.-N. Minard, J-Y. Nief, P. Odier, B. Pietrzyk, B. Trocme.

BARCELONA - Institut de Fisica d'Altes Energies, Universitat Autonoma de Barcelona, Spain

R. Alemany, F. Ariztizabal, G. Boix, S. Bravo, M.P. Casado, M. Chmeissani, P. Comas, J.M. Crespo, M. Delfino, I. Efthymiopolous, E. Fernández, M. Fernández-Bosman, V. Gaitan, Ll. Garrido, E. Grauges, A. Juste, J. Lopez, M. Martinez, P. Mato, T. Mattison, G. Merino, R. Miquel, M.Ll. Mir, P. Morawitz, S. Orteu, A. Pacheco, C. Padilla, F. Palla, D. Paneque, I.C. Park, A. Pascual, J.A. Perlas, I. Riu, H. Ruiz, F. Sanchez, F. Teubert, E. Tubau.

BARI – Dipartimento di Fisica dell'Universita, INFN Sezione di Bari, Italy

M.G. Catanesi, A. Colaleo, D. Creanza, N. de Filippis, M. de Palma, A. Farilla, G. Gelao, M. Girone, G. Iaselli, G. Maggi, M. Maggi, N. Marinelli, A. Mastrogiacomo, S. Natali, S. Nuzzo, M. Quattromini, A. Ranieri, G. Raso, F. Romano, F. Ruggieri, S. Selvaggi, L. Silvestris, P. Tempesta, A. Tricomi, G. Zito.

BEIJING - Institute of Particle Physics, Academia Sinica, Beijing, China

Y. Chai, Y. Chen, Y. Gao, H. Hu, D. Huang, X. Huang, S. Jin, J.F. Lin, J. Lou, C. Qiao, Q. Ouyang, T. Ruan, S.G. Wang, T. Wang, X.L. Wang, W.M. Wu, Y. Xie, D. Xu, R. Xu, Y.L. Xu, S. Xue, W.G. Yan, J. Zhang, L. Zhang, W. Zhao.

CERN - European Laboratory for Particle Physics, Geneva, Switzerland

D. Abbaneo, H. Albrecht, W.B. Atwood, A. Ball, A. Bazarko, R. Benetta, F. Bird, E. Blucher,
G. Bonvicini, P. Bright-Thomas, T.H. Burnett, D. Casper, D.G. Cassel, M. Cattaneo, F. Cerutti,
T. Charity, B. Clerbaux, G. Dissertori, H. Drevermann, F. Dydak, A. Engelhardt, M. Ferro-Luzzi,
R. Forty, M. Frank, C. Gay, F. Gianotti, C. Grab, R. Grabit, J. Griffith, R. Grueb, R. Hagelberg,
J.B. Hansen, J. Harvey, B. Ivesdal, R. Jacobsen, P. Janot, P. Jarron, B. Jost, G. Kellner, J. Knobloch,
A. Lacourt, P. Lazeyras, I. Lehraus, B. Lofstedt, T. Lohse, D. Lueke, D.L. Leutze, A. Marchioro,
P. Maley, P. Mato, J-M. Maugain, H. Meinhard, A. Moutoussi, J. May, S. Menary, V. Mertens,
A. Minten, A. Miotto, K. Miotto, K. Moffeit, T. Oest, P. Palazzi, R. Pintus, L. Pergernig, M. Price,

J-F. Pusztaszeri, N. Qi, F. Ranjard, G. Redlinger, P. Rensing, J. Richstein, W. Richter, L. Rolandi, A. Roth, W. von Rueden, J-C. Santiard, P. Schilly, D. Schlatter, M. Schmitt, O. Schneider, G. Stefanini, F. Sefkow, J. Steinberger, H. Taureg, W. Tejessy, F. Teubert, I. Tomalin, R. Veenhof, H. Verweij, M. Vreeswijk, H. Wachsmuth, A. Wagner, H. Wahl, S. Wheeler, W. Witzeling, J. Wotschack.

Clermont - Laboratoire de Physique Corpusculaire, Université Blaise Pascal, France

Z. Ajaltouni, F. Badaud, M. Bardadin-Otwinowska, A. Barres, A.M. Bencheikh, C. Boyer, G. Chazelle,
O. Deschamps, S. Dessagne, R. El Fellous, A. Falvard, D. Fayolle, C. Ferdi, P. Gay, C. Guicheney,
P. Henrard, J-M. Jousset, B. Michel, J.C. Montret, S. Monteil, D. Pallin, J.M. Pascolo, P. Perret,
B. Pietrzyk, F. Podlyski, J. Prat, J. Proriol, F. Prulhiere, P. Rosnet, J.M. Rossignol, F. Saadi, G. Stimpfl.

Copenhagen – Niels Bohr Institute, University of Copenhagen, Denmark

H. Bertelsen, T. Fearnley, F. Hansen, J.D. Hansen, J.R. Hansen, P.H. Hansen, A. Kraan, A. Lindahl, B. Madsen, R. Moellerud, R. Muresan, B.S. Nilsson, G. Petersen, B.A. Peterson, B. Rensch, A. Waeaenaenen.

Demokritos – Nuclear Research Centre for Physical Sciences, Athens, Greece G. Daskalakis, I. Efthymiopoulos, A. Kyriakis, C. Markou,

E. Simopoulou, I. Siotis, A. Vayaki, K. Zachariadou.

Edinburgh – Department of Physics, University of Edinburgh, Scotland

D. Candlin, A. J. Main, M.I. Parsons, E. Veitch.

Firenze – Dipartimento di Fisica dell'Universita, INFN Sezione di Firenze, Italy T. Boccali, V. Ciulli, A. Conti, E. Focardi, L. Moneta, G. Parrini, E. Scarlini, K. Zachariadou.

Florida – Department of Physics, Florida State University, USA

W. Burley, R. Cavanaugh, M. Corden, M. Delfino, C. Georgiopoulos, J.H. Goldman, T. Huehn, M. Ikeda, D.E. Jaffe, D. Levinthal, J. Lannutti, M. Mermikides, L. Sawyer, G. Stimpfl, S. Wasserbaech.

Frascati – Laboratori Nazionali dell'INFN (LNF – INFN), Italy

A. Antonelli, M. Antonelli, R. Baldini, G. Bencivenni, G. Bologna, F. Bossi,
P. Campana, G. Capon, V. Chiarella, G. de Ninno, B. d'Etorre-Piazzoli, G. Felici,
P. Laurelli, G. Mannocchi, F. Massimo Brancaccio, F. Murtas, G.P. Murtas, G. Nicoletti,
L. Passalacqua, M. Pepe-Altarelli, P. Picchi, S. Salomone, P. Zografou.

Frascati & Turin – Cosmo-Geofisica Laboratori, Italy

G. Mannocchi.

Glasgow - Department of Physics & Astronomy, University of Glasgow, Scotland

B. Altoon, O. Boyle, M. Chalmers, P. Colrain, L. Curtis, S. Dorris, A. Flavell, A.W. Halley, I. TenHave,J. Hearns, I.S. Hughes, J. Kennedy, I.G. Knowles, J.G. Lynch, W. Maitland, D. Martin, W.T. Morton,P. Negus, R. O'Neill, V. O'Shea, C. Raine, B. Raeven, P. Reeves, J.M. Scarr, K.M. Smith, M.G. Smith,P. Teixeira-Dias, A.S. Thompson, E. Thomson, F. Thomson, S. Thorn, R.M. Turnbull, J.J. Ward, J. Wells.

Haverford – Department of Physics, Haverford College, USA S. Wasserbaech.

Heidelberg – Kirchhoff-Institut für Physik, Universität Heidelberg, Germany U. Becker, B. Brandl, O. Braun, O. Buchmueller, R. Cavanaugh, S. Christ, S. Dhamotharan R. Geiges, C. Geweniger, G. Graefe, H. Flaecher, P. Hanke, G. Hansper, V. Hepp,W. Heyde, E.E. Kluge, J. Krause, G. Leibenguth, Y. Maumary, A.L. Ong, M. Panter,A. Putzer, B. Rensch, C. Rittger, G. Schmidt, M. Schmidt, K. Schmitt, J. Sommer, A. Stahl,H. Stenzel, K. Tittel, D. Topaj, E. Wannemacher, S. Werner, M. Wolf, M. Wunsch.

Imperial College – Department of Physics, University of London, England

D. Abbaneo, G.J. Barber, A.T. Belk, R. Beuselinck, D.M. Binnie, W. Cameron, M. Cattaneo,
D.J. Colling, G. Davies, P.J. Dornan, S. Dugeay, R.W. Forty, D.N. Gentry, M. Girone,
S. Goodsir, A.M. Greene, J.F. Hassard, A.P. Heinson, R.D. Hill, N. Konstantinidis, N.M. Lieske,
M. MacDermott, N. Marinelli, E.B. Martin, D.G. Miller, L. Moneta, P. Morawitz, A. Moutoussi,
J. Nash, J. Nowell, S.J. Patton, D.G. Payne, M.J. Phillips, D.R. Price, H. Przysiezniak,
S.A. Rutherford, G. San Martin, A. Sciaba, J.K. Sedgbeer, P. Spagnolo, A.M. Stacey, G. Taylor,
J.C. Thompson, E. Thomson, I.R. Tomalin, A.P. White, R. White, M.D. Williams, A.G. Wright.

Innsbruck – Institut für Experimentalphysik, Universität Innsbruck, Austria

G. Dissertori, V.M. Ghete, P. Girtler, A. Hoertnagl, E. Kneringer, D. Kuhn, L.K. Marie, G. Rudolph, R. Vogl.

Lancaster - Department of Physics, University of Lancaster, England

A.P. Betteridge, E. Bouhova-Thacker, C.K. Bowdery, T.J. Brodbeck, P.G. Buck, D.P. Clarke,P. Colrain, G. Crawford, G. Ellis, A.J. Finch, F. Foster, G. Hughes, D. Jackson,R.W.L. Jones, N.R. Keemer, M. Nuttal, A. Patel, M.R. Pearson, N.A. Robertson,B.S. Rowlingson, T. Sloan, M. Smizanska, S.W. Snow, E.P. Whelan, M.I. Williams.

Louvain-La-Neuve – Département de Physique, Université Catholique de Louvain, France

O. van der Aa, C. Delaere, G. de Hemptinne, G. Leibenguth, V. Lemaitre.

Mainz - Institut für Physik, Universität Mainz, Germany

T. Barczewski, L. Bauerdick, U. Blumenschein, A. Galla, P. van Gemmeren, I. Giehl,
A.M. Greene, C. Hoffmann, F. Holldorfer, K. Jacobs, M. Kasemann, F. Kayser, K. Kleinknecht,
M. Kroeker, A.S. Mueller, H-A. Nuernberger, D. Pollmann, G. Quast, J. Raab, B. Renk,
S. Roehn, E. Rohne, H.G. Sander, H. Schmidt, S. Schmeling, M. Schmelling, F. Steeg,
H. Wachsmuth, S.M. Walther, R. Wanke, B. Wolf, C. Zeitnitz, T. Ziegler.

Marseille - Centre de Physique des Particules, Université de la Méditerranée, France

J-P. Albanese, J-J. Aubert, R. Bazzoli, A.M. Bencheikh, C. Benchouk, V. Bernard, A. Bonissent,
G. Bujosa, D. Calvet, J. Carr, D. Courvoisier, P. Coyle, C. Curtil, C. Diaconu, J. Drinkard,
A. Ealet, F. Etienne, D. Fouchez, Y. Gally, T. Kachelhofer, E. Kajfasz, N. Konstantinidis, O. Leroy,
E. Matsinos, F. Motsch, R. Nacash, D. Nicod, S. Papalexiou, P. Payre, B. Pietrzyk, J. Raguet, L. Roos,
D. Rousseau, A. Sadouki, P. Schwemling, Z. Qian, M. Talby, M. Thulasidas, A. Tilquin, K. Trabelsi.

Milano – Dipartimento di Fisica dell'Universita, INFN Sezione di Milano, Italy M. Aleppo, M. Antonelli, S. Gilardoni, F. Ragusa.

MPI München - Max Planck Institut für Physik, Werner Heisenberg Institut, Germany

I. Abt, S. Adlung, R. Assmann, C. Bauer, H. Becker, R. Berlich, T. Boulos, W. Blum, D. Brown,

V. Buescher, P. Cattaneo, M. Comin, G. Cowan, A. David, B. Dehning, H. Dietl, F. Dydak,

M. Fernández-Bosman, M. Frank, G. Ganis, C. Gotzhein, A.W. Halley, T. Hansl-Kozanecki,

D. Hauff, G. Hauser, K. Huettmann, K. Jakobs, A. Jahn, W. Kozanecki, H. Kroha,

E. Lange, J. Lauber, G. Luetjens, G. Lutz, W. Manner, C. Mannert, M. Maier, H.-G. Moser,

Y. Pan, P. Pelikan, R. Richter, A. Rosado-Schlosser, H. Rotscheidt, S. Schael, J. Schroeder, A. Schwarz, R. Settles, H. Seywerd, R.St. Denis, H. Stenzel, U. Stiegler, U. Stierlin, G. Stimpf, M. Takashima, J. Thomas, M. Villegas, G. Waltermann, W. Wiedenmann, G. Wolf.

Orsay – Laboratoire de l'Accélérateur Linéaire, Université de Paris Sud, France R. Alemany, P. Azzurri, V. Bertin, J. Boucrot, O. Callot, S. Chen, X. Chen, Y. Choi, A. Cordier, F. Courault, M. Davier, G. de Bouard, L. Duflot, C. Fournie, G. Ganis, J.F. Grivaz, P. Heusse, A. Hoecker, A. Jacholkowska, M. Jacquet, D.E. Jaffe, P. Janot, V. Journe, M. Kado, D.W. Kim, F. LeDiberder, J. Lefrançois, D. Lloyd-Owen, A-M. Lutz, P. Marotte, G. Musolino, I. Nikolic, H.J. Park, I.C. Park, M-H. Schune, L. Serin, S. Simion, E. Tournefier, J.J. Veillet, I. Videau, J.B. deVivie de Regie, C. Yuan, Z. Zhang, D. Zerwas, F. Zomer.

Pisa – INFN Sezione di Pisa, Scuola Normale Superiore, Dipertmento di Fisica dell'Universite, Italy
S.R. Amendolia, G. Bagliesi, G. Batignani, S. Bettarini, T. Boccali, L. Bosisio, U. Bottigli, C. Bozzi,
C. Bradaschia, G. Calderini, M.A. Ciocci, R. dell'Orso, R. Fantechi, I. Ferrante, F. Fidecaro, L. Foà,
E. Focardi, F. Forti, A. Giammanco, A. Giassi, M.A. Giorgi, A. Gregorio, F. Ligabue, A. Lusiani,
E.B. Mannelli, P.S. Marrocchesi, A. Messineo, F. Palla, G. Rizzo, G. Sanguinetti, S. Scapellato, A. Sciaba,
G. Sguazzoni, J. Steinberger, R. Tenchini, G. Tonelli, G. Triggiani, C. Vannini, A. Venturi, P.G. Verdini.

Polytechnique – Laboratoire de Physique Nucléaire Hautes Energies, Ecole Polytechnique, France
J. Badier, D. Bernard, A. Blondel, G. Bonneaud, P. Bourdon, J. Bourotte, F. Braems, J.C. Brient,
A. Busata, M. Cerruti, M.A. Ciocci, J. Doublet, G. Fouque, A. Gamess, R. Guirlet, J. Harvey,
C. Lemoine, F. Machefert, P. Matricon, M. Maubras, P. Mine, R. Morano, S. Orteu, L. Passalacqua,
J-Y. Parey, P. Poilleux, A. Rosowsky, A. Rouge, C. Roy, M. Rumpf, M. Swynghedauw,
R. Tanaka, A. Valassi, M. Verderi, H. Videau, I. Videau, C. Violet, D. Zwierski.

RAL - High Energy Physics Division, Rutherford-Appleton Laboratory, England

V. Bertin, D.R. Botterill, R.W. Clifft, T.R. Edgecock, M. Edwards, S.M. Fisher, J. Harvey, S.J. Haywood, D.L. Hill, T.J. Jones, P. Maley, G. McPherson, M. Morrissey, P.R. Norton, D.P. Salmon, G.J. Tappern, J.C. Thompson, I.R. Tomalin, A. Wright.

Royal Holloway - Department of Physics, Royal Holloway & Bedford New College, England

O. Awunor, A.P. Betteridge, G.A. Blair, L.M. Bryant, J.M. Carter, F. Cerruti, J.T. Chambers, J. Coles, G. Cowan, A. Garcia-Bellido M.G. Green, B.J. Green, D.E. Hutchcroft, D.L. Johnson, L.T. Jones, A.K. McKeney, P.V. March, T. Medcalf, A. Misiejuk, P. Perrodo, M.R. Saich, J.A. Strong, P. Teixeira-Dias, R.M. Thomas, T. Wildish, J.H. Wimmersperg-Toeller.

Saclay – Dapnia/SPP, CE/Saclay, France

B. Bloch-Devaux, D. Bouediene, P. Colas, H. Duarte, S. Emery, B. Fabbro, G. Faif,
C. Klopfenstein, W. Kozanecki, E. Lancon, M.C. Lemaire, E. Locci, S. Loucatos, B. Marx,
L. Mirabito, E. Monnier, P. Perez, J.A. Perlas, F. Perrier, B. Pignard, H. Przysiezniak,
J. Rander, J-F. Renardy, A. Rosowsky, A. Roussarie, J-P. Schuller, J. Schwindling,

P. Seager, D. Si Mohand, A. Trabelsi, B. Tuchming, R. Turlay, B. Vallage.

Santa Cruz – Santa Cruz Institute for Particle Physics, University of California, USA S.N. Black, J.H. Dann, R.P. Johnson, H.J. Kim, N. Konstantinidis, A.M. Litke, C. Loomis, A.M. McNeil, G. Taylor, J. Wear.

Sheffield – Department of Physics, University of Sheffield, England J.G. Ashman, W. Babbage, A. Beddall, C.N. Booth, R. Boswell, C.A.J. Brew, C.M. Buttar, R. Carney, S. Cartwright, F. Combley, I. Dawson, M. Dinsdale, M. Dogru, F. Hatfield, P.N. Hodgson, M.S. Kelly, A. Koksal, M. Lehto, J. Martin, W. Newton, D. Parker, C. Rankin, J. Reeve, P. Reeves, L.F. Thompson.

Siegen – Fachbereich Physik, Universität Siegen, Germany

K. Affholderbach, E. Bach, E. Barberio, A. Boehrer, S. Brandt, H. Burkhardt, V. Buescher, G. Cowan,
E. Feigl, J. Foss, G. Gillessen, C. Grupen, J. Hess, G. Heitner, C. Koob, O. Krasel, G. Lutters,
H. Meinhard, J. Minguet-Rodriguez, L. Mirabito, A. Misiejuk, E. Neugebauer, A. Ngac, M. Olpp,
M. Otto, G. Prange, F. Rivera, M. Roschangar, P. Saraiva, U. Schaefer, R. Seibert, H. Seywerd, U. Sieler,
L. Smolik, F. Stephan, C. Stupperich, K. Stupperich, H. Trier, P. van Gemmeren, L. Wurmbach, V. Zeuner.

Trieste –INFN Sezione di Trieste, Dipartimento di Fisica dell'Universita, Italy

G. Apollinari, M. Apollonio, C. Borean, L. Bosisio, R. Della Marina, G. Ganis, G. Giannini, B. Gobbo, F. Liello, A. Mansutti, E. Milotti, G. Musolino, L. Pitis, F. Ragusa, L. Rolandi, U. Stiegler.

Utah Valley – Department of Physics, Utay Valley State College, USA S. Wasserbaech.

Washington – Experimental Particle Physics, University of Washington, USA H. Kim, H. He, J. Putz, J. Rothberg, S. Wasserbaech

Wisconsin - Physics Department, University of Wisconsin at Madison, USA

S.R. Armstrong, T. Barklow, L. Bellantoni, K. Berkelman, J.F. Boudreau, A. Caldwell, M. Cherney,
D. Cinabro, M. Convery, J.S. Conway, D.F. Cowen, K. Cranmer, A.J. deWeerd, P. Elmer, Z. Feng,
D. Ferguson, Y. Gao, Y.S. Gao, S. Gonzalez, J. Grahl, T. Greening, J.L. Harton, O.J. Hayes, J. Hilgart,
H. Hu, J. Izen, J.E. Jacobsen, R.C. Jared, S. Jin, R.P. Johnson, J. Kile, B.W. LeClaire, P.A. McNamara,
M. Mermikides, T.C. Meyer, D. Muller, J. Nachtman, J. Nielsen, W. Orejudos, Y.B. Pan, T. Parker,
J.R. Pater, S. Ritz, G. Rudolph, Y. Saadi, M. Schmitt, I.J. Scott, V. Sharma, N. Shao, D. Strom,
M. Takashima, J.H. von Wimmersperg-Toeller, A.M. Walsh, J. Walsh, J.A. Wear, F.V. Weber,
E. Wicklund, W. Wiedenmann, J. Wu, Sau Lan Wu, S.T. Xue, X. Wu, J.M. Yamartino, G. Zobernig.

Zurich – Institute for Particle Physics, ETH Honggerberg HPK, Switzerland

G. Dissertori.

ISBN 92-9083-233-9