Minutes of the 170th Energy Calibration Meeting held on 11 December 1998

1 Beam offsets and dispersion in 1998

Alfons Weber presented the analysis of opposite sign dispersion effects in 1998. Alfons will be leaving CERN next April, so a volunteer is needed to take over this analysis next year. If someone wants a self-contained project to do with the beam energy, this is a fine opportunity.

The pattern of predicted vertical dispersion from IP to IP varies during the 1998 running because the bunch train bumps were switched off for high energy running from fill 4887.

The beam-beam deflection scans have again been effective in minimising the electron-positron bunch offsets. The average luminosity weighted centre-of-mass energy shift at each IP and the luminosity weighted uncertainty are calculated and found to be (in MeV):

IP	mean	error
2	0.4	1.7
4	0.3	1.1
6	0.2	1.3
8	0.0	1.0

It is anticipated that as last year, no shift will be applied for this effect, and an uncertainty of 2 MeV, assumed to be correlated between all the IP's, will be assigned.

2 Orbit data for 1998

Joerg Wenninger showed an analysis of about 2000 recorded beam orbits in 1998, which give the average horizontal beam position in the arcs (xarc). This is used to monitor geometric changes, or equivalently central frequency changes.

The analysis is complicated by periods of data with an entire BPM station not working, which corresponds to half an arc. The data are therefore divided into three samples according to the number of bad pickups. The effect of the bad pickups can be assessed from the subset of orbits where they are working, by comparing the fits including or excluding the bad pickups. Typical systematic errors from missing BPM's are 10μm. The rms spread of xarc values for the data with most pickups working is 4.3μm, and even the data with fewer working pickups have a spread of less than 5μm.

The variation of xarc with deliberate RF frequency shifts has been measured. The variation is nice and linear, but the slope is 1.7% lower than expected. This effect was also seen with the 90/60 optics. So since it seems to be independent of optics it is thought to be more likely to be a global scale effect of the BPM's, rather than a MAD feature.

The xarc data are also very consistent with the tide model.

The momentum compaction factor, α was measured with the 60/60 optics and agreed to well within the measurement error with the MAD prediction.

There is a good correlation between xarc measured for electrons and positrons. However, there is a shift between the band for high energy data and for 45 GeV data. This may be because the missing pickups lead to a biassed sampling of the sawtooth, which would give a larger effect at high energy.

3 Central frequency measurements in 1998

Joerg described a new method for making central frequency measurements that he developed this year. The method is faster and more robust.

The old method involves setting the chromaticity, Q'_H, and cycling the RF frequency. This is repeated for different values of Q'_H, and extrapolating to find the value of Q'_H where the beam does not change position with frequency, i.e. is on the centre of the sextupoles. This takes about 20 minutes.

In the new method, a value of Q'_H is chosen. A small step of $\Delta Q'_H$ is then added and removed several times. (This is rather like applying the bend-modulation idea to the sextupoles.) The change in tune corresponding to the step $\Delta Q'_H$ is measured. This is then repeated for different offsets of the RF frequency (with the same Q'_H and $\Delta Q'_H$ step). Extrapolating to zero change of tune gives the central RF frequency.

The data quality of each measurement looks very good. In addition, at high energy, the effect of changing the horizontal correctors was checked, and found to have no influence on the central frequency as predicted by MAD.

However, in another MD at 45 GeV, some instability was seen. The old and new methods gave the same results, but jumps of a few Hz were seen, sometimes related to orbit changes. At the end of the year at 92 GeV, the consistency found when modulating the vertical instead of the horizontal chromaticity was about 2 Hz.

Comparing the central frequency and xarc measurements, the electron frequency tracks xarc to within about 2 Hz, but the positron rf frequency shows eratic behaviour, and discrepencies of up to 20 Hz. This is totally inconsistent with the good correlation between electron and positron xarc values, and is not understood.

Finally, Joerg explained how a "mistake" in measuring the central RF frequency would cancel to first order in the way that we use it. The polarisation data are corrected to the supposed central frequency, and the NMR's are calibrated. Then the shift from supposed central to actual RF frequency is taken into account in calculating the energy of physics running. The second order possible error would be of the order of 4×10^{-7}.

The central frequency evolution is used to combine different calibrations and to account for changes in central frequency w.r.t. calibration fills. Since the change in energy is given by $\Delta f/\alpha$, a "mistake" would also enter into the comparison of polarisation fills with different optics (102/90 and 60/60, with different values of α). Here, a "mistake" of 2 Hz at 45 GeV would lead to an energy difference of 1.4 MeV.

4 1997 paper

The final draft of the 1997 paper has been circulated. Minor typing errors have been found. The paper will be submitted to the European Physical Journal.

5 Discussion of the use of the flux loop

Alain Blondel again raised the question of whether optimal use is being made of the flux loop in the present analysis. This hinges on the possible of a flux loop measurement of the energy, either from an absolute calibration using the full cycle (including the unpopular wrong-polarity part of the cycle), or from constraining the flux loop using the polarisation data and using it to give a prediction of physics energy.

Following the meeting, Joerg Wenninger circulated this message, summarising the knowledge of the flux loop that he has accumulated:

i) We all agree that NMRs and Epol agree well (order 1-2 MeV) between 40 and 60 GeV.

ii) The same is true for NMRs and FL.

iii) There is an $\approx$ 20 MeV diff. between NMRs and FL at 90 GeV. In addition this difference changes with time. We do not know who is right or wrong.

   The claim is now that if one could obtain a very accurate absolute energy from the FL (I think you need something significantly better than 20 MeV i.e a few 10^{-5}!) and show that it agrees perfectly with polarization, one might actually trust the FL and discard the NMRs at high energy. And point iii) is gone !! The error goes down.

   I don't believe in this argumentation but let us see ...

   I went again through my 6 logbooks on energy calibration since 1992 and searched for FL-related plots and numbers.

iv) The FL gives you a field B_{FL} in Gauss, which is obtained from the integrated flux F_{FL} and the FL area S_{FL} measured before 1988 (comparison with a long flip coil) :

$$B_{FL} = F_{FL}/S_{FL}$$

   The quoted accuracy on S_{FL} is between 1.0 and 1.7 10^{-4} (Some "source" dependence)

   NOTE THAT THIS IS ALREADY A LARGE ERROR ON THE ABSOLUTE FL SCALE !

   I have some problems to go back 10 years in time and prove that at that time people largely over-estimated the error !

   Anyway let us ignore this point and go on ...

v) We now have FL fields. How do you go to a FL energy E_{FL} ? You need to know a few factors :

$$E_{FL} = B_{FL}[T] * L_{dipole}[m] * 0.0484104$$

L_{dipole} is the effective length of all dipoles. The numerical factor takes into account the fraction of the bending from the dipoles (0.983685 officially) and few trivial factors.

L_{dipole} must take into account some end effects and the field in the small gaps between dipoles.

Some of those numbers have been accurately measured on ONE single "typical" magnet.

I showed the dependence of L_{dipole} on energy at least twice at the ECAL meetings. It is not the most problematic correction.

vi) We are not finished : the concrete magnets have aged and you must estimate the influence of the concrete ageing on :

S_{FL} : estimated -4×10^{-4}
L_{dipole} : estimated -1×10^{-4}

Again two corrections of 5×10^{-4}. Not small on our scale !

I have problems to assign errors to those numbers ...which were determined back in 90-91. Or to estimate if they changed !

Again we ignore all warnings and continue ...

vii) FL data from 1994 : here we can compare polarization with the FL. And I actually found a plot in my logbook (p. 67, logbook 3) where FL (at that time performed by the magnet group) and Epol are compared. I have not included the PS file which I not longer have but tell you the main features :

1) The FL shows a clear downward trend of about 15 MeV over the run (about 220 days).
2) At the start of the run the FL is about 12 MeV HIGHER than the polarization data.
3) It approaches and finally merges with Epol towards the end of the run.

Note that at that time we had no NMRs in the tunnel, so no train... correction is possible.

But I find it hard to explain the drift with trains.

FROM THIS DATA I WOULD SAY THAT THE QUOTED ACCURACY OF 20 MeV FOR THE FL IS NOT TOO WRONG...

viii) I did not find any plot for 1993 although I remember comparing it with Epol...

ix) The 1996 data : In 1996 I was still full of hope and applied all the reciepes and ageing corrections ... described above.

When I compared FL and Epol (after correction for hor. correctors and estimated energy rise in the pol fills, central frequencies ...) I got :

$$E_{pol} = A + (1 + B)E_{FL}$$

with $A = -23$ MeV, $B = 0.58$ MeV/GeV $= 5.8 \times 10^{-4}$

A SCALE ERROR OF APPARENTLY $\approx 6 \times 10^{-4}$!

NOW SOME OBSERVATIONS :

1) At which energy do you get (by accident) $Epol = E_{FL} = E_{magic}$?
   $E_{magic} = A + (1 + B)E_{magic}$
   $E_{magic} = -A/B \approx 39$ GeV quite close to 45 GeV !
   In fact at 45 GeV the diff. is about 4 MeV ! This is quite consistent with (some of) the 1994 data
2) What is the error when you go from 20 to 45 GeV ?
   It is easy to work out: 14 MeV ! Again consistent within the ≈ 20 MeV accuracy quoted back in 1991...

Other observations from 1996 :

x) When you slightly change the analysis, i.e forget/ignore hor corrector corrections or energy rises, there is little change in the overall picture. The errors quoted above for 45 and 20 GeV move around by few MeV.

My conclusions in 1997 :

a) I do not believe that you can get an accurate absolute calibration of the FL. There are too many factors with hard-to-estimate systematic errors. Renormalising it accurately on Epol is not easy because the calibrations are not made at the same time and require energy rise, temperature corrections that are not easy to get with high accuracy...
b) The direct Xcalibration of the NMRs and the FL is much easier since the measurements are made at the same time, same temperature ... and are quite easy to compare. No correction is involved. Of course the field may rise in the time where you collect the NMR data. But you can use the spread of the NMR data to get a feeling for that.

6 Next meeting

The date of the next meeting will be confirmed in January 1999.

Pippa Wells, 16 December 1998