MINUTES OF THE 116th MEETING OF THE ENERGY CALIBRATION WORKING GROUP

held at CERN on January 21, 1997

This meeting was intended to discuss the production of the 93-94-95 energy files for the spring conferences.

The dipole model: rise terms (parasitic train currents and temperature effects)

Universally accepted facts:

- The parasitic currents from the railway system leaking into LEP cause the magnet to increase the field. Macroscopic patterns: due to the way the current enters the ring the dipoles in octants 1,7,8 show higher and anticorrelated a jumps wrt the other octants. This behaviour was seen in 95 with only two NMRs installed in LEP and confirmed by the 16 presently installed in 1996.
- The ISR test bench and the work of B.Dehning, E.Bravin and G.Mugnai have shown that the variation of the dipole field wrt to the magnet temperature is non trivial and different from the simple one parameter linear dependence assumed until now. Lengthy studies within the group have shown that with LEP in physics condition the *relevant* temperature dependance can be reduced to a two parameter linear expression depending on the differential change of the temperature and the max temperature reached by the magnet since it was last cycled.
- The drifts caused from parasitic currents and temperature effects do not factorize: e.g. if the field of a dipole at temperature T drifts by ΔB due to a jump induced by a current spike will react to a temperature change only when that is large enough to cause a field change in excess of ΔB

This last point is delicate: any model to be used for 94 and 93 has to be independent from direct measurements of parasitic currents and LEP dipole field in the tunnel as no such device were available at the time.

Two approaches in this respect are possible:

1. (standard) assume factorization of the effects and estimate an error about this hypothesis using the several fills where the modelling can be directly compared with resonant depolarization taken at different times within the fill
2. **M. Koratzinos** showed a new approach based on the assumption that the on average the parasitic currents are the same every day. He then uses the detailed understanding and modellling(E.Bravin) of temperature and the parasitic currents effects from the ISR test bench and applies it to physics fills a using only the time of day and magnet core temperature to predict the field drifts. First attempts to compare this predictions to the NMR4 and NMR8 readings of selected fills in 95 show very promising results: while the absolute magnitude of the changes is not well reproduced the shape is in surprising good agreement(see attached copies).

It was agreed that for what concerns the spring conferences we should stick to 1. and possibly aim to use 2. as an additional indipendent way to estimate the errors involved in the assumptions.

Peter Renton and Guy Wilkinson (+K.Yip) showed work done to try to pin down the values of the parameters of the temperature model by fitting at the same time the rise due to temperature effects (parameters $C_T$and C_T^{Max}) and to parasitic currents (function of time of day and time into the fill) for various data sets.
These parameters are non trivial especially for extracting the drift due to temperature for the NMR4 and 8 in 1995. The average ring coefficients can be combined effectively into a single one for 1993 and 1994 as the temperature changes were always positive .

1. The 1996 temperature experiment (realized at the end of the data taking by causing a large T positive and negative excursion). Fits have been performed to the data of the 16 NMRs to get the get the average ring coefficients and the so called "48" coefficients, ie. the coefficient of the

pseudo device built by adding the measurements of NMR4 and NMR8 which were the only one available in 1995. This particular experiment probes mostly the value of C_T as there is a large negative change of temperature. The first part of the experiment (temperature rising) shows a non standard behaviour as there is hardly any rise for several hours and is not presently understood. The value of C_T obtained is unquestionably negative -5.1 10^{-5} (ring) -6.6 10^{5} ("48") to be compared with -3.3 10^{-5} obtained for the magnet in the test bench at the ISR.

It should be kept in mind though that in this experiment the temperature difference between the cooling water and the core was such to demand a correction according to Enrico's model (which is normally negligible for physics runs) which amount, for example, to 50% for C_T

2. Peter fitted the model with the two parameters to the 1995 NMR48 data (using only the fills with bending modulation) and obtained C_T=-2 10^{-5} and C_T^{Max}=10 $10^{-5.}$ which are compatible with what Enrico obtains in the ISR test bench. The comparison with fills with multiple resonant depolarization show that the two params model does as well as the one param used for Warsaw (with only 5 fills one cannot really expect mircales).

Given that the two parameters model does not seem to be flawed and based on the experience from the ISR test beanch the ***group agreed*** *that the temperature model to be used from now on will be the two parameters one.*

Guy Wilkinson came up with a proposal for the values to be used: the parameters to be defined are 4: $(C_T ,C_T^{Max})_{ring}$ and $(C_T ,C_T^{Max})_{48}$. An interesting combination is the sum $(C_T +C_T^{Max})_{ring}$ as this is equivalent to the single parameter of the old linear model in case where the temperature monotnically increases like for the 93 campaign : it can be compared with the many determination of this parameter perfomed in the past on very old data and on 95 and 96 data. The estimation is extremely consistent: it always comes close to 10x10^{-5}. Guy's proposal is to use (9±2.5) x10^{-5} .

The same parameter for the "48" pseudo device has a value consistent with the ring average when determined from the 1996 data and experiments, while any determinations of it performed trying to use the NMR data collected in physics in 1995 (M.Koratzinos, Kin Yip, A. Drees et al.) indicate a value four times smaller.

Guy suggests that it could be an artifact of the limited temperature excursion (close to the sensitivity and precision level of the T-probes) and will investigate further if that could be the case.

The choice of the central value this parameter has to be made by the end of this week if we want to have files ready in time.

For the value of C_T the best determination comes from the 96 temperature experiment and the ISR test bench. The sign is undoubtedly negative and the values are compatible. Guy suggests to take C_T=(-5±2.5) both for the ring and the "48" device. The degree of correlation between the two parameters has to be studied a bit more in detail.

Work to do with respect to what had already been done for Warsawto get the final energy files:

- Correction due to "correctors" field difference between physics and calibration: data extraction from database to be done for the three years. Existing code to be checked and upgraded to take into account dispersion value at corrector. Need Joerg's input. TC to find someone to associate with MK.
- Check TIDE parameter: issue raised at the last meeting once it was realized that some of the tide experiments fills need to be revised given the temperature/train rises. Joerg Wenninger contacted after the meeting assures that the tide parametrization is very healthy and has been improved/cross checked using the BOM measurements (which should not be affected by trains etc.) thoroughly last year.
- Dispersion corrections: John Yamartino's set of corrections (file with IP correction every 15 minutes available) should be used as it is consistent with the error analysis done on this part of the model. M.Koratzinos to read John's constants in the std program.
- Implement the two parameter temperature model in std MK set of programs, refit the rise terms. Compare in details the 95 files with the ones which can be produced alternatively by Guy who has got hold of Kin programs.
- Produce files for 93 and 94. A pending issue is also the cross check on the 94 datasets: while for 93 overall consistency has been checked by Mike and Peter/Kin the only program which can handle 94 to date is Mike's one.

- On the RF side M.Hildreth does not see problems in being ready within a short time with 93 through 95 corrections.
- Evaluate errors: most effort needed on the dipole model as a lot of work had already been done for Warsaw on other parts of the model.

Next meeting this Friday, Jan 24th at 14:00 in pavillon to define the detailed schedule for the two weeks to come.

Scriba:
Tiziano Camporesi

MEMO:

Effects of energy errors on the luminosity determination for a given experiment.

T. Camporesi/PPE DELPHI

The luminosity to be used in the normalization of the experimental cross section is determined from the Bhabha counting rate comparison with the expected visible Bhabha cross section.

The visible cross section determined from the MC simulation at a given (fixed) CMS energy has to be corrected for various experimental effects which can vary from fill to fill, time to time etc.

$$\sigma_{cor}^{vis} = \sigma^{vis} \cdot \left[1+\delta_{\gamma Z}(s)\right] \cdot f_{IP} \cdot \left(\frac{s_0}{s}\right)$$

where the second term is the correction due to the electroweak interference term contribution which depends on the square of the center of mass energy s, f_{IP} includes all correction related to movements, tilts of the beams and the ratio (s_0/s) accounts for the variation of the crosss section due to the difference of the CM energy with the respect to the one used to generate the MC events (s_0).

The error induced on the luminosity determination by the error from the beam energy determination is then

$$\frac{\partial L}{L} = \left[\frac{\frac{\partial \delta_{\gamma Z}}{\partial E} \cdot E^2 - (1+\delta_{\gamma Z}) \cdot 2E}{E^2}\right]_{E=E_{beam}} \delta E \approx -\left(\left|\frac{\partial \delta_{\gamma Z}}{\partial E}\right| + \frac{2}{E}\right)_{E=E_{beam}} \delta E$$

where in the last passage we have neglected δ_{gZ} wrt 1. Around the Z resonance the slope of the electroweak correction for the DELPHI luminometer (angular acceptance 2.1 to 7.3 degrees) is (see figure below)

$$\frac{\partial \delta_{\gamma Z}}{\partial E} \approx -7 \times 10^{-3}/GeV$$

Substituting this above one gets $\frac{\delta L}{L} \approx 2.6 \frac{\delta E}{E}$

The largest single experimental relative systematic contribution to the luminosity determination in DELPHI is 0.4 10^{-3}. If we want to keep the error from the energy determination at a negligible level (half ot the largest single contribution i.e. 0.2 10^{-3}) level we need to have energy errors below 3.5 MeV. This is even more important at peak energy which has the largest weight in the determination of σ_0 : infact to the best of my knowledge this is the strongest demand on the peak energy error. The error on the energy is correlated between the various IP (mean energy totally correlated while the RF contribution is the known one) and it might imply the need to add these additional terms in the final correlation matrix to be used for the electroweak group fits.

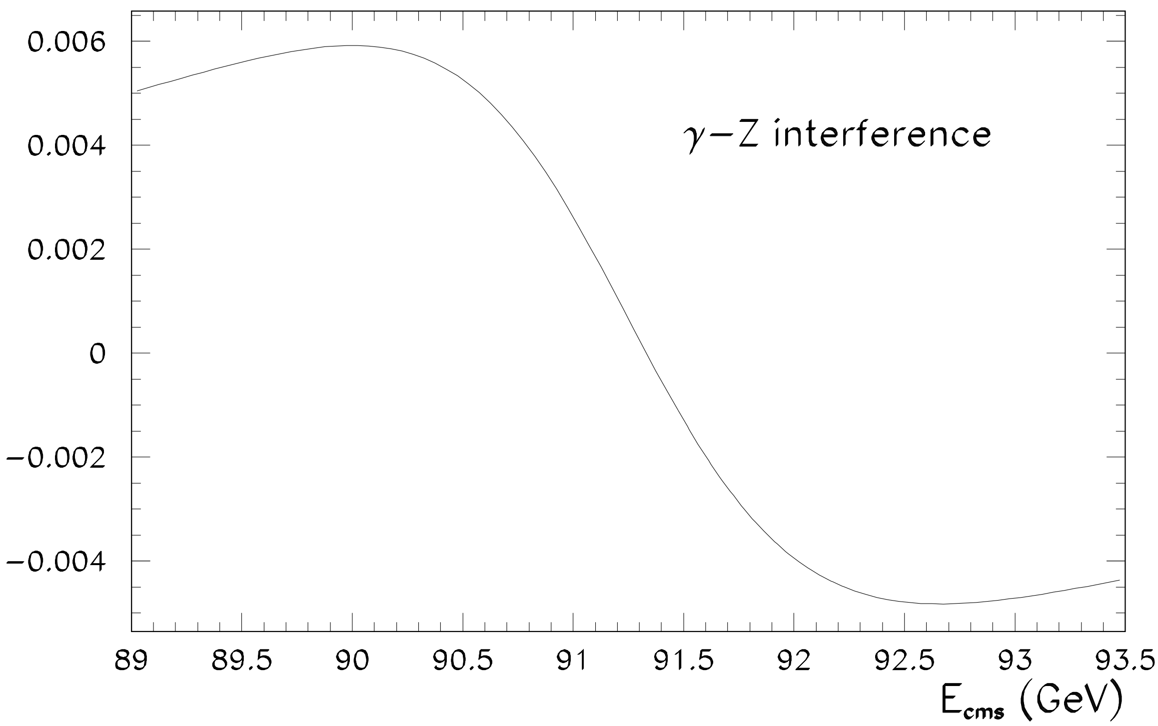

Figure 1: Contribution of the interference term, as a fraction of the QED term, vs CM energy