DISCUSSION: MAGNETIC REQUIREMENTS FOR COMMISSIONING

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WHAT IS THE IMPACT OF HYSTERESIS ON ORBIT CORRECTION AND FEEDBACK? - *R. STEINHAGEN*

S. Myers: When we first measured the beta-beat in LEP it was 200 %! Can we have simulations up to these values?

R. Steinhagen: The calculation scripts work only up to 100 % in the injection case and up to 70 % at collision, beyond they become unstable.

R. Assmann: You focused on one type of corrector and mentioned that the others are most like this type including the warm correctors. Will you extend the study to include all of them or do you think this is not necessary since they all have similar features?

R. Steinhagen: The main group of corrector magnets have similar beta-functions and can be treated similarly. However, though having similar beta-functions, the MCBXH nested correctors in the triplets are more difficult to control. We did not consider the warm corrector magnets, which are 8 per beam. Their effect should be small. The magnets on the list affect mainly the injection stability of the first beam. As soon as the feedback starts, the hysteresis is automatically minimized and should not be an issue anymore.

J.-P. Koutchouk: When you refer to the estimate of $50\ \mu\mathrm{m}$ of uncertainty in the closed orbit when you pre-cycle the correctors, does this include the effect of different powering history of the correctors?

R. Steinhagen: I assume, if we do the pre-cycling after the beam abort, which I think is important to be implemented, we should be within the $50\ \mu\mathrm{m}$. But it is important to note that this is only the effect due to the correctors. Concerning injections stability: as long as the total effects are below 0.5 mm, it should not pose a problem for orbit steering.

O. Brüning: I would have expected there must be two contributions: One is the remanent field once you go through a cycle and the other is the decay of the field.

R. Steinhagen: The decay of the corrector circuits is relatively small compared to the main magnets.

S. Fartoukh: You showed a hysteresis curve with a hysteresis of about 8 units so you should have at least about 1 unit decay.

R. Steinhagen: We did several measurement cycles and averaged over 10 measurements and we did not see a significant change between ramping up or down.

E. Todesco: Is this magnet cycling able to wipe out the previous history of powering?

W. Venturini: Of course, if you erase the history, you also erase the useful history, so what was learned during feedback.

R. Wolf: Under worst conditions, the maximum error you can get in setting the correctors up again is 560 nrad as with the many corrections you did, you would not know on which branch of the loop you were actually on when you saved the settings.

(Note: The pre-cycle was designed to go (for all correctors) through positive saturation only. Hence, the measured 560 nrad is the maximum expected systematic shift and has the same sign for all MCB CODs. As a consequence, this contribution changes the beam energy rather than the orbit. The part that is important for the injection stability is the random spread around the 560 nrad which is much smaller.)

S. Fartoukh: So you are saying that the hysteresis branch on which every corrector is lying cannot be the same for each corrector?

R. Steinhagen: Each of the correctors has been on a different current before doing this cycling.

F. Bordry: Why are you going through the loop of 0 A – 55 A – 0 A and not doing a degaussing cycle of -55 A – 55 A – -55 A as you don't know in beforehand to which polarity you will go?

E. Todesco: The aim is not a degaussing but a precycling to put the magnets in a reproducible state.

S. Fartoukh: This means for some magnets you will change the hysteresis branch. So in this case, these 560 nrad can be the random error for some correctors and in respect to the closed orbit this reduces the expectation by a factor of 10. So it is better to go up to +55 A or -55 A and not back to 0 A but directly to the actual value which can be positive or negative.

TRANSFER FUNCTION OF QUADRUPOLES AND EXPECTED BETA BEATING - *S. SANFILIPPO*

Y. Papaphilippou: You said that in the simulations you are taking into account the slot allocation as it was given by MEB as far as it is done. The problem is that the MEB is not approving the magnets in a sequence and this means that the sorting having holes in some sectors is not efficient at all. So I would see that this is a worst case estimate.

S. Sanfilippo: This is why when I did this analytical estimate, I did not take the 13 units, but 13 units reduced by 30 %.

Y. Papaphilippou: The target we had is a random of the b2 of 10 units. If you have an uncertainty of 10 units, the sorting is completely penalized. I then would not do any sorting at all.

S. Sanfilippo: For the moment we can guarantee that with any system that we use, we have the value of the gradient +/- 10 units rms but our ambition is of course to reduce it and to take into account all the contributions like the warm/cold correlations or the impact of magnet history.

S. Fartoukh: So these 10 units rms stand for the main quadrupoles and the stand alone quadrupoles? For the main quadrupoles, I would expect that you could have a large systematic calibration error up to 20 units, but the random error around this systematic error should be much less.

S. Sanfilippo: It is 5 units of random error for the main magnets. For the stand-alone magnets we use different measurement systems.

R. Assmann: What do you assume for the warm quadrupoles in your simulations?

S. Sanfilippo: We assume an uncertainty of the measurements of 20 units. And it also depends on the hysteresis of the magnets. An investigation of the dependence on the history of these magnets has been done.

R. Assmann: Resetting of the power supplies to the right value, what does it mean?

S. Sanfilippo: In the simulation, when we have a change of magnet for example in an arc, we assume that the average of the arc quadrupole is subtracted. There is only the random part of the uncertainty of the power supply. We measure the average of all the magnets and we correct this average.

R. Assmann: So in the machine, you will do this in beforehand or we will have to do this with beam?

J.-P. Koutchouk: You do it before based on warm measurements. And then there is in addition the warm/cold uncertainty which is included.

R. Wolf: Do you expect a significant change in the beta-beating during the injection decay?

S. Sanfilippo: Yes, we do not measure the decay for every kind of magnet. For the MQY or MQM which are working at relatively low currents, we will have a decay of more than 2 or 3 units in b2. Now, what counts is the uncertainty. For the MQY we did some tests and we can say, we can predict this to 2 units but this still needs to be confirmed.

O. Brüning: During the last Chamonix meeting my worry was the transfer function of the stand-alone quadrupoles as they have very different cycles rather than all the same nominal cycle. You said in your presentation that initially this could be known by 60 units but because you do a statistical analysis, you bring this down to 10 units, right? If this is true, this is a very remarkable result because this puts you at the accuracy of the measurements.

S. Sanfilippo: There are two main contributions: There is the contribution from the measurement system of 10 units. But there is another contribution coming from the magnetic history which we have to add quadratically. And the uncertainty on this for the moment is not well known as we do only one measurement. It is only a valid assumption provided that we do some special tests, provided that we do some modelling. I stressed this in the conclusions.

O. Brüning: But will this be done?

S. Sanfilippo: This is planned: 25 tests on MQM, MQY, and MQT in bloc4.

N. Catalan Lasheras: How many magnets do you need for this?

S. Sanfilippo: At least 3 or 5 magnets for each type, but as was done for MQY we have to run the complete cycle for one magnet.

S. Myers: Coming back to my first question. I was surprised about the beta-beating of only 5 % coming from the stand-alone magnets. Why did we have such an enormous beta-beating of 200 % at LEP coming from 8 superconducting quadrupoles which were well measured before they were installed?

J.-P. Koutchouk: This was at collision, the calculations here are at injection. In the LHC all standard quadrupoles will have a stronger effect at collision. And at collision this work remains to be done. It is not a big surprise that they don't have such a big impact in this situation.

S. Myers: So is it understood why it was so bad at LEP?

J.-P. Koutchouk: Yes, I think at LEP it was related to a cold mass of at least one of the superconducting quadrupoles in the low-beta section that moved inside the cryostat and this is equivalent to a focusing error. The motion was by many millimeters and it was consistant with the observations. This created most of the beta-beating.

S. Myers: And this corresponded to 2 % gradient error.

S. Fartoukh: The main contributor in your table is the MQX. So would it be possible to re-measure the MQX? There is sometimes a difference of the measurements of 20-30 units stemming from different calibrations of two different stretched wire measurement systems. And with such an uncertainty at collision, we will have quite some beta-beat.

S. Sanfilippo: This is not foreseen for the moment.

L. Bottura: At warm conditions it is not useful, so we would need an extra cold test.

J.-P. Koutchouk: Just to support: It is the main source of beta-beating, so it has to be cross-calibrated with the other quadrupoles in one way or another.

HYSTERESIS IN MAGNET CORRECTORS VERSUS TUNE AND CHROMATIC CORRECTION - *W. VENTURINI*

J.-P. Koutchouk: A bias for the MQT circuits would probably simplify operations, if it could be possible. Is there an issue with that?

S. Fartoukh: In pricinple this is not an issue provided that in the sectors where the tune shift quadrupoles MQT (from Q14 to Q21) have a non-zero injection setting, we rematch the corresponding LHC IR's by imposing that at Q21 we fall back on the optical functions of the regular arc (with the MQT's off, this condition is normally imposed at Q13). In practice this means that the beta-function will be perturbed from Q13 to Q21 w.r.t. the present optics inducing a loss of about 0.1 sigma in mechanical aperture but the induced beta-beating bump will be close at Q21. This loss will be manageable if we put (and have enough) additional golden quadrupoles and dipoles in the zones Q13-Q21 on both sides of the sector under consideration.

FIELD MODEL DELIVERABLES FOR SECTOR TEST AND COMMISSIONING: WHEN AND WHAT? - *M. LAMONT*

O. Brüning: Regarding the deliverables for the transfer functions and multipoles, what of these can we measure with beam in the end? Sextupoles if one doesn't correct anything?

M. Lamont: You will get the sextupole within a unit.

O. Brüning: In the sector test in a single pass?

M. Lamont: b1 obviously.

O. Brüning: And the other effects can be measured by a feed-down? - Just being curious.

M. Lamont: We will be limited by the resolution of the BPMs in a single pass.

R. Schmidt: Some years ago, Luca had some predictions on what the chromaticity will be. To what kind of level could one control it by these models and what would have to be taken by chromaticity measurements?

L. Bottura: The figures haven't changed since the discussion.

E. Todesco: What is the process of validation of your model? Are you planning to cycle over the parameters of your model to improve it and fit it better to the machine? - In the sector test and later?

M. Lamont: The transfer function is the main parameter and we will be able to measure the momentum with respect to the incoming beam. We could look at the decay of the magnets by injecting multiple times perhaps.

S. Fartoukh: I think even b3 we would be able to measure in the sector test. We would inject with a Δp of 1. or 2.10^{-3} or, conversely change the field of the MB by the same quantity, and you would have a sizable chromatic phase shift if you measure it at the end of the sector.

O. Brüning: What accuracy would we get?

No answer

E. Todesco: If you have magnets that were not measured, do you assume that on average they behave as the measured ones, independently of their cable manufacturer, or do you also use this information ?

N. Sammut: No, the cable manufacturer is not taken into account for magnets which are not cold tested.

SORTING THE MAGNETS IN THE MACHINE: WHAT DID/WILL WE GAIN? - *L. BOTTURA*

R. Assmann: What is the expected change in geometrical behaviour from the transport of the magnets from the surface to the tunnel? Are you sure it makes sense to optimize on a 100 μm level on the geometry before lowering the magnet ?

L. Bottura: We are not optimizing on a 100 μm level. We are dealing with cases that are 0.5 mm or 1 mm out of specification. With respects to the limits, the magnets are looked at on a one-by-one basis. Shifts are then set on a 100 μm level, deviations below are usually ignored. If there are changes due to transport, I assume there have been tests done, I am not an expert in that. There has been a budget allocated to all steps in the production. And with the final budget we are left with is what was shown in this presentation. We are trying to stay within that, so that we have enough space for the rest.

J.-B. Jeanneret: Yes, and stability was checked. Just before going down to the tunnels, we remeasure the extremities of the magnets and we see that the difference to the initial values is compatible with the procedure except for a few cases where we have fully understood the issue.

O. Brüning: It is quite remarkable the results that all of you have achieved with sorting. You are saying that you are right between the different parties having the magnet installation schedule and asking for field quality. Who is making in the end the decision? Who is the body?

L. Bottura: I am currently in the process of dealing with it.

P. Lebrun: By mandate the MEB has executive power.

Unknown: Are you taking dynamic properties into account in sorting?

L. Bottura: The only thing we are trying is to keep the inner cable of the magnets in a sector the same. But what we have seen is that the spread in the b3 snapback does not depend on the inner cable where it is a uniform distribution throughout the production. So in that respect there is no allocation for dynamic behaviours.

EXPECTED QUENCH LEVELS OF THE MACHINE WITHOUT BEAM: STARTING AT 7 TEV. - *P. PUGNAT*

F. Bordry: Imagine during hardware commissioning you have one magnet that will quench at 8.2 T another magnet should quench at 8.3 T but it quenches at the same time. Does this help or not?

P. Pugnat: No because a quench is always a destabilization process. A ramp without quench is a stabilization process during which the magnet coil is shaking-down. When a quench occurs, thermal gradients and then thermomechanical forces develop; the hottest turns of the coil can reach up to 400 K, but typically 300 K, whereas the others can stay around 80 K.

R. Schmidt: I remember at the string, the time that an adjacent magnet quenches is several 10 seconds, so the field in the adjacent magnet would have gone down from 8 T to e.g. 5 T and so maybe it is less critical.

S. Fartoukh: In case of two neighbouring “weak” magnets, could the training of the two be a non-convergent process? Imagine a magnet with a bad training memory close to one with a bad performance?

P. Pugnat: Of course detraining could lead to a non-convergent process that could also be triggered by beam loss effects. This is the reason why there are magnets of class R in MTF, i.e. accepted with Reserve and most frequently because they showed detraining effects during cold tests and they should be put in a quiet zone from the beam loss point of view to reduce its probability to quench.

L. Rossi: Training and detraining is real but it goes quadratically or cubically according to how far you are from the critical surface. The detraining curve that you showed was for a magnet that reached 9.5 T. Down you go, detraining is less and less an issue. This typically means within few days or a week of hardware commissioning they appear more often. So going in parallel this is one of the few cases where we gain as all quenches we see, cost two weeks to the machine commissioning and start of operation.

P. Pugnat: For the estimations of the number of quenches, I considered for the probability of a detraining only cases with a quench around nominal field.

CHASING PARASITIC MAGNETIC FIELDS IN THE LHC - *A. DEVRED*

L. Rossi: Don't we have any idea about the possible effect of the bus-bar connections as we have no cure?

J.-P. Koutchouk: It is not easy to make an estimate like that, as these busbars are not in parallel to the beam. The big difficulty is the lyra where the two conductors are spaced by a distance which is not large as compared to the distance to the beam but this occurs on short section of the interconnect and it is not a straight line.

Jean-Bernard Jeanneret: Of course only the longitudinal part of the current contributes.

Unknown: Assuming nevertheless the PbSb block would quench, with what time-constant would this happen?

A. Devred: It would quench in the order of milliseconds.

Unknown: This is a relatively long time for us.

F. Bordry: You look at the interconnect but wouldn't it be more interesting to look at the DFBs?

A. Devred: As I said, we wanted to start with something that is not too complicated. It took one month to retrieve the data from Euclid in order to introduce it in Roxie models.

F. Bordry: Isn't it possible to make a first estimation of the order of magnitude. Otherwise it will be too late.

J.-P. Koutchouk: The overhead due to this transformation is expressed in weeks while the problem has been with us now for years. The aim was to have a tool which is generic and which can now be used for other problems.

S. Russenschuck: The problem with the interconnects is an order of magnitude less than with the shielding as the field is steady. We have also made estimates for the DFBs already quite a while ago for the old version and this was a no issue. However it would make sense now to re-evaluate this for the new version.

R. Steinhagen: Do we have to expect that we get a kick due to PbSb shielding?

A. Devred: No, this issue should be solved.

R. Wolf: The bus bars are super-conducting so they should have a residual field. Would this give an additional problem?

A. Devred: We can also put it into Roxie.

G. de Rijk: Did you have a look at the experimental areas? There, the configuration is very complicated.

A. Devred: Not yet, but this is on the list of studies to be done.