Special
Joint WP2/WP5 Meeting
Tue 15th Sept. 2020, 9:00 – 12:00

Chair:	Gianluigi Arduini, Stefano Redaelli, Rogelio Tomás
Speakers:	Daniele Mirarchi, Ezio Todesco, Sofia Kostoglou, Ewen Hamish Maclean
Participants (vidyo):	Marton Ady, Gabriella Azzopardi, Roderik Bruce, Xavier Buffat, Jean Cenede, Riccardo De Maria, Ilias Efthymiopoulos, Alex Fomin, Alexandre Frassier, Davide Gamba, Hector Garcia Morales, Massimo Giovannozzi, Cedric Hernalsteens, Gianni Iadarola, Roberto Kersevan, Michele Martino, Alessandro Masi, Elias Métral, Nicolas Mounet, Yannis Papaphilippou, Konstantinos Paraschou, Tobias Persson, Alexander Pikin, Adriana Rossi, Benoît Salvant, Gerhard Schneider, Richard Scrivens, Guido Sterbini, Gérard Tranquille, Oskari Kristian Tuormaa, Frederik Van der Veken, Daniel Wollmann, Carlo Zannini

AGENDA

MEETING ACTIONS

Daniele	Perform halo depletion simulations with off-momentum particles. The effect on the core should be also studied.
Daniele	Estimate the effect of the damper on the emittance growth due to the residual kick.
Daniele	Study the effect of higher order components on cleaning performance and core blow-up

Ezio	Update the variation of the multipoles with energy in D1/D2/MQXF, after the field quality measurement on the prototypes.
Vincent Chareyre, Julien Emonds-Alt (EN/EL)	Check and understand why the switching frequency of the UPS inverter is not as specified (6.74 kHz).
Sofia	Collect a list of the systems connected to UPS in the LHC.

GENERAL INFORMATION (GIANLUIGI ARDUINI - STEFANO REDAELLI)

The minutes of the previous WP2 meeting were not circulated yet. They will be commented on at the next meeting. Stefano also mentioned the usual collimation meeting on Friday, September 18th (link).

Today's meeting is a joint WP2/WP5 meeting, devoted in part to the hollow electron lens. Other WP2 subjects will then follow.

1 HALO REMOVAL AND EFFECTS ON BEAM CORE OF ELECTRON LENS IN HL-LHC (DANIELE MIRARCHI)

The presentation reports on the ongoing studies regarding the hollow electron-lens in HL-LHC, used as an additional means to deplete the beam halo. Based on LHC measurements, 5% of the total number of particles might be present in the tails of the beam transverse distribution (above 3.5 sigmas), which could lead to damage to primary collimators, or magnet quenches, in the case of a fast failure scenario.

The e-lens can be operated with various pulse patterns, in particular a continuous pattern (DC), a random ON-OFF pattern (RND on-off), a random amplitude pattern where the electron beam current I is modulated (RND-I), and a resonant-turn pattern where the e-lens is on for N_{ON} turns, and off for N_{OFF} turns, periodically repeated (N_{ON} - N_{OFF}). SixTrack simulations are performed (without beam-beam, with optics v1.3, beta* = 15 cm, Q'=15, octupole current at -300 A, inclusion of horizontal and vertical primary collimators at 6.7 sigmas) to evaluate the halo depletion percentage of an ideal e-lens of inner radius 5 sigmas, on one side, and the effect of a residual dipolar kick on the core, on the other side.

The RND on-off pattern is the most efficient in terms of halo depletion (more than 85% depletion after 100 seconds with 50% ON and I=5A), with significant margins both for the electron current and for the ON probability. On the other hand, the RND-I pattern, which depletes the halo as much as the RND on-off mode with 50% ON and I=3A, does not justify the hardware changes necessary to make it possible. The DC pattern turns out to perform very poorly for halo depletion, while the "N_{ON}=14 - N_{OFF}=9" pattern seems quite promising and outperforms both the DC and other resonant modes, with 35% depletion after 100 seconds.

In terms of impact of the e-lens on the beam core, a residual dipolar kick of 5 (or even 10) nrad does not result in any emittance blow-up for both the DC option and the "N_{ON}=14 - N_{OFF}=9" one, but leads to

unacceptable blow-up for the RND on-off pulse pattern (several orders of magnitude higher than the tolerance of 0.05 microns per hour) - even 0.1 nrad would be border-line with this pattern.

Going towards a more realistic set-up, it is found that a 1% noise on the pulse-to-pulse electron beam current and the residual kick, has negligible effects on the halo depletion and the emittance blow-up, respectively. Along the same line, the effect of linear coupling on the blow-up is analysed, and coupling appears to be a useful knob to distribute the possible emittance growth between planes and hence equalize the luminosities at both IPs.

In the end, the conceptual operational strategy proposed is to use the RND on-off pulse before the adjust phase, for a few seconds when needed, looking for the best compromise in terms of e-beam current, ON probability and magnetic design. During collisions, one could use instead the "N_{ON}=14 - N_{OFF}=9" pattern, with a constant depletion.

Next steps include studies in more realistic and diverse situations, in particular with respect to optics and the position in the cycle, also adding beam-beam effects, the skew TCP, and (optionally) considering an e-lens of inner radius 4.7 sigmas. On top of that, a pulse with a frequency content tuned on the halo spectrum should be tested, and a better understanding should be achieved regarding the efficiency of the resonant-turn patterns.

- Regarding the distribution tails and the fact that 5% of the beam are in the tails (slide 4), **Yannis** asked during which Run (1 or 2) this number was measured, and if it was at injection or flat top. **Daniele** answered that it was measured during both runs at top energy (This was initially presented by G. Valentino at the Review on the for a hollow e-lens for the HL-LHC, and similar measurements were recently published by A. Gorzawsky et al, PRAB 23, 044802). The best fit of the distribution is obtained with a double Gaussian. **Yannis** commented that 5% sounds very large, and said this should be consolidated during Run 3, because it drives many considerations. **Stefano** agreed, mentioning also that extrapolating on this number is very complex; measurements were done in various fills of Run 2, and the actual percentage of beam in the tails also depends on the extension of the range scanned with collimators, i.e. the tighter we scan, the better the measurement. Hence there are quite some uncertainties, but even half of this percentage would still be a large amount that calls for a mitigation strategy. **Yannis** said there is indeed no question about the need here, but argued that we should all be consistent with the different kinds of observations (lifetime, etc. - see the talks of **Hector** for instance), and present only consolidated numbers. **Rogelio** also wondered if for the definition of tails (above 3.5 sigmas), the collimation sigma is used, or the beam sigma. **Daniele** answered that an emittance of 3.5 microns was taken (as in the paper). **Rogelio** then wondered about the possible inconsistency in the definition of sigma, e.g. with respect to the 2 sigmas on which the e-lens is acting. **Daniele** answered that the information in slide 4 is just an overview of the past measurements, giving our best educated guess of the tail population. In all the remainder of the talk, sigma will be consistently computed with the 2.5 microns emittance of HL.

- **Gianluigi** asked if the percentage of halo eliminated is computed on the Gaussian distribution or the measured one. **Daniele** answered it is computed on the one measured, i.e. the double Gaussian.
- **Gianni** asked how the halo depletion simulations are performed with respect to the longitudinal plane. **Daniele** answered that in the simulations all the particles are on momentum. He performed some checks with off-momentum distributions and the results were giving similar results, but no systematic study has been done yet. **Gianni** then commented that all losses are optimistically estimated (i.e. there are less losses than in reality). **Stefano** argued that the issue is that the machine is very linear, hence we want to go to pessimistic conditions for the halo removal, and on-momentum is pessimistic. **Daniele** added that he did a check in six dimensions, and the changes are not drastic. The main goal here is to guide the choice of parameters for the final simulation in operational conditions. **Yannis** still argued that with off-momentum particles, and cross modulation, the situation is more complicated. A very large chroma (e.g. Q'=15) may give a different behaviour in the plot of slide 16. **Daniele** agreed that this has to be checked (Action: **Daniele** – including the impact on the core).
- **Stefano** commented slide 19 saying that the present magnetic design is not yet final. It is already a good sign that some scenarios show an emittance blow-up under control, but this is not final yet from the Russian collaborators: they have been asked to improve significantly the residual fields in the core (see for example the presentations to this WP5 meeting). **Gianluigi** wondered why only the vertical plane is shown on slide 20. **Daniele** indicated that both planes are shown in the plot (solid and dashed lines for the horizontal and vertical planes, respectively) but the blow-up is visible only in the vertical plane because the kick is only vertical and there is no coupling between planes. **Stefano** added that a residual kick in the horizontal plane was not considered at this stage because it is much smaller with the present "S-shape" design of the HELs.
- **Ilias** asked how the emittance growth is computed, in particular for a non Gaussian beam. **Daniele** answered that for the computation of the effect of the residual kick, there is no e-lens and no halo depletion, so the distribution stays Gaussian - one only applies a vertical dipolar kick with a dynamic modulation, and the emittance is obtained from the sigma of the distribution. **Ilias** asked if the kick changes radially. **Daniele** answered in the negative. **Davide** asked if the kick is located at the e-lens. **Daniele** said it is just a dipolar kick. **Ilias** wondered about the correlation of the core blow-up with the noise. **Stefano** commented that we have access to the full simulated magnetic field map within the lenses and these studies started with an approximation that takes only the dipolar component. He proposed to continue the discussion offline.
- **Yannis** wondered if the simulation results on slide 21 are still only on-momentum. **Daniele** answered in the affirmative. **Yannis** argued that one needs the full 6D simulation to be able to give a specification.
- Regarding the impact of the residual kick, **Xavier** indicated that the effect of the ADT damper should be included, as it is quite efficient in suppressing the emittance growth. The damper works on multiple turns, so the phase advance is not so relevant here (decoherence occurs in thousands of turns). There is a formula that works well, hence it is easy to estimate (Action: **Daniele**). **Daniele** agreed, and indicated that in the simulation they did not do it yet because it is not straightforward.

Xavier and **Sofia** agreed that including the damper in SixTrack would be very difficult indeed. After the meeting, **Xavier** provided the formula to **Daniele**. **Stefano** argued that simulations without ADT are suitable for establishing tolerances on the field quality in the core, although clearly any benefit from the ADT in reducing core blowup is welcome.

- On slide 23, **Rogelio** asked the minimum number of sigmas they want to deplete from the primaries. **Daniele** said that 2 sigmas depletion are needed when Crab Cavities are on according to previous studies that probably should be reviewed, while before going to collision the main worry comes from orbit jitter, so at least around 1 sigma is needed; generally speaking one would have to make a study if we want to reduce the 2 sigmas depletion before collision. **Rogelio** underlined that this would change everything. He wondered if the inner radius of the e-lens would change the residual kick. **Daniele** answered in the negative (see also in the backup slides of the talk). The two bends at the extremities are the main drivers of the residual kick. **Rogelio** mentioned that the bends do not pulse. **Daniele** replied that indeed the residual kick is due to the uncompensated field because the e-beam at the exit is not exactly the same as at the entrance. **Stefano** then said that if, during operation, there is evidence that we need a larger radius, this can be done very easily - a range of radii is possible. There is a target depletion rate but one can speculate how much this can be relaxed. The present specification for magnetic system and cathode size provides a depletion range of 3.5 real sigmas (with the 2.5 micron emittance) from the nominal TCP gap at 6.7 sigmas, following the initial specification of 3 sigmas (with the old 3.5 micron emittance notation). The 2 sigmas depletion target is very reasonable, and doing a study with a much smaller number of sigmas does not seem appropriate at this stage of the design. Realistic scenario could be studied later. **Rogelio** agreed. **Stefano** also mentioned the 10Hz as an interesting topic, but one would need to know the maximum oscillation amplitude they would get (dump in Run 2 were triggered with about 0.5 sigma excursion at TCPs).
- **Ilias** mentioned possible variations between bunches, including the non-colliding ones. **Daniele** answered that indeed it will have to be considered at a later stage. There are also some trains without depletion, and there is margin for tuning. **Stefano** added that the advantage of the e-lens is that it depletes an area in amplitude space before the primaries, in a well-defined, physical, amplitude. The depletion range from the primary collimators – say 4.7 sigma to 6.7 sigma for the scenario with 2 real sigma depletion – will be "empty" for all the bunches that the HEL acts on, independently of their initial core emittance. **Yannis** mentioned that in collision, bunches have different orbits due to the dynamic effects of beam-beam, hence it would be interesting to see how the efficiency of the e-lens will vary. **Stefano** agreed. **Xavier** gave tolerances on the maximum orbit excursion, which are a small fraction of a sigma, for which **Stefano** expects no specific issues. **Daniele** said that if one gets off-centered, the residual kick becomes different, hence there will be a different blow-up.
- **Gianluigi**, following the comment of **Xavier** on the effect of the damper on the emittance growth, commented that maybe the residual kick is not an issue with the damper. He asked then if there are other components than the dipole one. He also wondered what happens with momentum spread in the case of the special pattern N_{ON}=14 - N_{OFF}=9. On the first point, **Stefano** said the first order needed to be fixed first, as a worst-case scenario. Simulations with the complete multipoles

are available thanks to the implementation in SixTrack by **Alessio Mereghetti** (also discussed at the collimation meeting already mentioned above: link). The effect of the damper is indeed a good point. **Xavier** mentioned that the non-linear part cannot be fixed by the damper. Regarding the second point, **Daniele** mentioned first that the previous studies were focused on patterns with one turn ON and *n* turns OFF, while with the proposed scheme a much better performance can be reached. He agreed that the study should be repeated in more realistic situations, but insisted on the strong advantage of this pattern, which is that the core is not affected (**Action: Daniele** to study the effect of higher order components on cleaning performance and core blow-up).

- **Xavier** asked if there is a study of local coupling at the e-lens. **Tobias** said that here it was done with a global coupling.
- **Daniel** mentioned that a crab cavity failure or orbit jitter are not the only fast-failure concerns for machine protection, because there will be fast interlocks. On the other hand, cases when there is no interlock might be more worrisome, e.g. instabilities and un-compensated beam-beam forces in case of asynchronous bumps of the two beams. He hence wondered what would change with the e-lens, with a depleted distribution. **Gianluigi** mentioned that we do not rely on tails for the stabilization. **Daniel** then argued that machine protection relies on the halo, and on BLMs, for the detection of instabilities. **Daniele** replied that there will be witness bunches with un-touched halos. **Xavier** said that instabilities could affect only some bunches that are not witness bunches. **Stefano** mentioned that an equilibrium has to be found as a compromise between halo monitoring (for failure detection) and mitigation of total tail stored energy.

2 6.5/7/7.5 TeV IMPACT ON D1, D2 AND MQXF (EZIO TODESCO)

The goal of the talk is to show the variation with energy of the allowed multipoles of the main HL-LHC magnets, when spanning an energy range from 6.5 TeV to 7.5 TeV. Saturation is indeed not negligible at high fields; it is compensated in the case of the transfer function, but for higher multipoles the dependency on energy can only be limited to typically a few units.

For the MQXF, the variation of the b6 multipole is +/-0.4 units in the full energy range, well inside the acceptance window. This appears not to be an issue, but the situation has to be revised when data from the prototypes, as well as beam screen measurements, will come.

On the other hand, for both D1 and D2 the dependency on b3 with the energy is significantly larger; an energy has to be selected for the optimization, which now is 7 TeV. Going to 6.5 TeV would instead consume half of the acceptance window of b3 for D1, and one third for D2. Some manufacturing flexibility can still be achieved, provided they are anticipated appropriately.

The proposal is therefore to re-evaluate the situation after the field quality measurements of the D1 and D2 prototypes, and after the retraining of the LHC.

- **Gianluigi** asked if all the main magnets are currently optimized for 7 TeV. **Ezio** answered in the affirmative, except for the MQXF which were optimized for 6.75 TeV (in slide 3 the value at 7 TeV

has been put to zero in order to show the variation). He also added that the beam screen is not included.

- **Gianluigi** commented that the proposal seems very reasonable, and that it is the right approach to look at a range of energies and then to optimize all magnets at the same energy. We need to know first what is the most likely energy - now he would favour 7 TeV indeed. **Gianlugi** also asked if there will be enough time to make possible changes, if needed. **Ezio** answered this is not a negligible issue but not a major one either. From 6.5 to 7 TeV things change a lot, so it is not easy. On the other hand, we have the possibility of realizing some fine tuning to center the ideal range, in 2021. He also insisted on the impact of the beam screen, which is of the same order of magnitude. **Gianluigi** wondered if the point should be brought to the HL TC, since a strategy decision has to be made regarding the energy. **Ezio** agreed and said we could go to the HL TC after another update in around 3 months (Action: **Ezio**).

3 MEASUREMENTS OF THE LHC (SPARE) UPS VOLTAGE SPECTRA (SOFIA KOSTOGLOU)

This presentation reviews the measurements of the voltage spectra of the UPS (Uninterruptible Power Supplies), that were performed in order to understand if the UPS is a candidate for the high frequency cluster of 50 Hz lines observed in the LHC transverse beam spectrum.

The UPS spectra measured clearly exhibit 50 Hz lines around the harmonics of the switching frequency (SF) of the inverter (4 kHz). These 50 Hz lines are synchronized to the mains when the UPS works in normal mode; therefore the UPS is a good candidate to explain the high frequency cluster at 8 kHz in the LHC beam spectrum. Note that the switching frequency after LS1 should have been set to 6.74 kHz, while the measurements here show instead it is at 4 kHz, which was also confirmed by the manufacturer.

Tests with beam would be required to confirm the impact of the UPS on the 50 Hz lines. To do so, the proposal is to switch the three UPSs connected to the ADT from normal mode to static bypass (with low intensity beam at injection) and try to observe a reproducible effect on the beam spectrum. Currently, a new firmware is needed to perform this operation remotely. In the meanwhile, tests will first be done in SM18 to perform warm measurements of the transfer function of an LHC dipole up to 10 kHz, possibly followed by similar measurements at cold.

After the meeting, a discussion took place with **Vincent Chareyre**, where it was understood that the new firmware is not necessary as it is actually easier to perform the switch to the static bypass mode manually (instead of remotely); moreover, it is possible to inhibit the synchronization of the switching frequency, enabling another very promising test with beam.

- **Gianluigi** asked if the static bypass mode means we are bypassing the normal mode (the latter being the UPS mode used in operation). **Sofia** answered in the affirmative.
- **Rogelio** wondered how the measurement on an LHC dipole can be done in SM18. **Sofia** said they will inject noise. **Rogelio** argued that some properties will be different in warm conditions. **Yannis**

confirmed but said that this way one could have measurements both with and without beam screen, if they are possible. It would also validate the measurement procedure, and one can get an interesting model for the transfer function (at warm for the time being), even if it is not exactly the same as in the LHC.

- **Riccardo** proposed to inject a controlled source of noise on the cold orbit corrector, rather than on the main dipoles, as it would be slightly less difficult and dangerous. **Sofia** agreed and added that anyway they will first try the test in SM18 and check the results, before trying in the LHC.
- **Gianluigi** asked if **Vincent Chareyre** (and his team at EN/EL) understands why the UPS has a different switching frequency than specified. **Sofia** answered in the negative. **Gianluigi** asked if the company acknowledges this change. **Sofia** answered that it is not clear what happened. **Gianluigi** commented that it is important to understand, as we want to avoid a 8 kHz switching frequency. It would also be important to know that, in case a future consolidation is needed. After the meeting, a discussion with **Vincent Chareyre** and **Julien Emonds-Alt** took place. There is an on-going investigation to address the origin of this discrepancy (Action: **Vincent Chareyre**, **Julien Emonds-Alt**).
- **Rogelio** commented that the LMC should know which systems are powered by the UPS. **Sofia** answered that the ADT instrumentation is. **Gianluigi** asked if the main power converters are also concerned. **Michele** answered in the affirmative, more specifically the controlled part of the high-accuracy (class 1) magnets. **Rogelio** then wondered if many magnets are affected. **Michele** answered that the dipoles, quadrupoles and inner triplets are. **Gianluigi** concluded that one should make a list of the systems on the UPS, so that we can understand which part of this noise goes to the beam, and the path followed by the noise (Action: **Sofia** to collect a list of the systems connected to UPS in the LHC). After the meeting, **Vincent Chareyre** provided a list of all groups with systems connected to the UPS.

4 CROSSING ANGLE IN THE FIRST YEARS OF HL-LHC (EWEN HAMISH MACLEAN)

This presentation highlights the detrimental impact of a large crossing-angle on the optics commissioning of HL-LHC. Indeed, uncorrected nonlinear errors present in the machine could jeopardize the success of linear optics commissioning, and perturb tune measurements with the BBQ. Since 2017 the linear optics corrections are performed also with the operational crossing scheme; a large crossing angle could potentially imply a large detuning through feed-down effects. These will scale strongly with both beta* and the crossing angle. Hence, for the first years of operation where the commissioning will already be challenging, the question is whether it is possible to reduce the crossing-angle.

- **Gianluigi** commented that the question is a valid one. At the start of operation, the intensity will be 1.7e11 p+/bunch, with a larger beta* than baseline, and a smaller luminosity. Hence it would make sense to start with a smaller crossing angle. **Gianluigi** asked the audience if any issue could be expected. **Xavier** answered it depends on the luminosity target. If one reduces the crossing angle and beta* is smaller, then there will be higher long-range effects, which would be bad for

stability with the negative octupole polarity. **Rogelio** replied that beta* will not be reduced. **Gianluigi** asked **Xavier** if his comment was about the beginning of the leveling, to which **Xavier** answered in the affirmative. **Rogelio** said that instead of beta*=40 cm, one could start at beta*=50 cm or 60 cm. **Gianluigi** said it might be good to look at the parameters at the start of collisions. **Xavier** commented that anyway stability is not a show stopper - one can use the positive polarity at the beginning, before collisions. **Rogelio** asked about the options for Run 3. **Xavier** answered that the issue is the same there. **Rogelio** said he will give the numbers.

- **Yannis** commented that one can also check the dynamic aperture, but probably there is no show stopper there as well.

After the meeting, **Rogelio** sent the parameters needed to get a starting luminosity of 2.5 10^{34} $cm^{-2}s^{-1}$ without crab cavities and with a bunch population of 1.7e11 p+: a crossing angle of 190 murad & beta*=58 cm (to be compared to respectively 250 murad and 45 cm in the baseline). **Yannis** checked that both scenarios are comfortable for DA (including beam-beam effects) with negative octupoles (but the exact octupole current needed for stability has to be checked) and high chromaticity. **Xavier** also checked that stability can be achieved without teleindex, with both octupole polarities. **Rogelio** concluded that the scenario with a crossing angle of 190 murad is ok - scenarios with further reduction could be checked as well.

5 ROUND TABLE (GIANLUIGI ARDUINI)

The next WP2 meeting will take place on Sept. 29th, at 9 AM. The preliminary agenda is:

- Impact of collimator geometric impedance on tuneshift measurements - the case of TCP.C6L7.B1 (Nicolas Mounet),
- Beam-beam DA simulations with the new operational scenario (Sofia Kostoglou),
- Optics functions for the damper: requirements, and compliance of optics 1.5 with them (Wolfgang Höfle) (title to be confirmed),
- Optics functions for beam instrumentation: requirements, and compliance of optics 1.5 with them (Thibaut Lefèvre).

Reported by N. Mounet