Special WP2/WP5 Meeting

Tue 26th Jan. 2021, 9:00 – 12:00

Chair: Rogelio Tomás, Elias Métral

Speakers: Galina Skripka, Benjamin Bradu, Ezio Todesco, Danilo Quartullo, Benoît Salvant

Participants (zoom): Hannes Bartosik, Andrea Bersani, Nicolo Biancacci, Roderik Bruce, Xavier Buffat, Serge Claudet, Pedro Costa Pinto, Marco D'Andrea, Riccardo De Maria, Ilias Efthymiopoulos, Pasquale Fabbricatore, Stefania Farinon, Vanessa Gahier, Francesca Galluccio, Francesco Giordano, Massimo Giovannozzi, Gianni Iadarola, Mauro Migliorati, Daniele Mirarchi, Nicolas Mounet, Andrea Musso, Joao Oliveira, Yannis Papaphilippou, Konstantinos Paraschou, Marcin Patecki, Stefano Redaelli, Marta Sabate Gilarte, Guido Sterbini, Natalia Triantafyllou, Frederik Van der Veken, Christine Vollinger, Carlo Zannini, Peter Zijm

AGENDA

MEETING ACTIONS

Benoît	Clarify the need (or not) for copper coating in the Y-chamber
Frederik, Massimo	Perform DA simulation studies, exploring cases with systematic b3 at ±3, ±6 units, and with systematic b5 at ±3, ±6 units, in D2. Assess if one MCBRD with 15 units of a3 is a concern.
Benoît, Danilo, Rogelio	Clarify the bunch length to be used in power loss computations, in the case of a q-Gaussian bunch profile.
Danilo, RF team	Check if the crystal goniometer might cause issues for the longitudinal stability.
Benoît, Inigo	Check with STI if the heat loads generated in the crystal goniometer could deteriorate its performance.

GENERAL INFORMATION (ROGELIO TOMÁS)

Rogelio reviewed the minutes of the 186th WP2 meeting, on December 8th. Various subjects had been covered: the TAXN aperture, then the impact of the MCBXF on orbit control, both presented by **Riccardo**, an update on the magnets field quality by **Ezio**, and the impact of the field quality of the MCBXF on the dynamic aperture (DA) by **Frederik**. Talks about the MCBXF (by **Riccardo** and **Frederik**) were done in preparation for the TCC. The conclusion of those was that only the MCBXF situation is critical, but one should wait for field quality updates and possible improvements; the Full Remote Alignment System (FRAS) should be used only if needed. One action (on **Ezio**) was to write a document about the naming conventions - current issues are going to be fixed in optics v1.6. Another action (on **Ezio** and **Frederik**) was on the construction of a 2D model for field imperfections - the topic may be presented in a forthcoming WP2 meeting.

Nicolas mentioned one comment by **Ilias** on the TAXN alignment, which is a massive object to align - Ilias proposed in particular to cross-check this aspect. The corresponding paragraph in the minutes was changed accordingly.

Rogelio then mentioned the TCC which occurred last week. WP2 supported the proposal to deploy a new class of power converter for the arc dipoles (class 0.5), as it is much cheaper (40k). There is still time to decide. This topic will be reviewed within the steering meeting of the PC work package. Regarding the TAXN review, the need (or not) for copper coating in the Y-chamber should be assessed. **Benoît** commented that what was presented in the WP2 still holds. **Rogelio** mentioned it would be good to clarify this point (Action: **Benoît**).

The schedule of the meeting followed as foreseen, with two presentations on heat loads by **Galina** and **Benjamin** (the latter being introduced by one slide from **Peter**), followed by additional remarks presented by **Gianni**, then a report on new field quality measurements by **Ezio**, and finally two talks by **Danilo**, then **Benoît** on crystal collimator impedance.

1 Beam induced heat load on triplet beam screens (Galina Skripka)

Studies of e-cloud in HL-LHC triplets were reported previously in CERN-ACC-NOTE-2018-0009. In this report a case of non-uniform secondary emission yield (SEY) along the triplet was studied: all the triplet length is coated with aC (SEY=1.1) except for the drifts outside cold masses for which SEY=1.3 (conditioned copper). It was found that the total heat load is dominated by the contribution of the e-cloud in the uncoated drifts.

In a new coating scenario only the DRF bellows inside the triplet and the two drifts at the extremities of the triplet are left uncoated. The total uncoated length in this case is shorter by around 5 m. In this case the total heat load for IR5 was found to be around 180 W smaller (it decreased from 532 W to 353 W).

- **Serge** commented that this study shows that one can gain with a small additional coated surface, which is good news. These results allow the cryo team to have a clearer view on the heat load that needs to be compensated, thus closing the topic.

2 Beam induced heat load measured during Run2 in LSS (Peter Zijm, Benjamin Bradu, Gianni Iadarola)

As an introduction, **Peter** reviewed the updated cryogenics layout foreseen around point 1 and 2 in HL-LHC, highlighting the remaining questions on the need for aC coating in the stand-alone magnets (SAM) in IR1/5 (Q6 & Q5) and in IR2/8 (Q6, Q5 & D2/Q4).

- **Serge** mentioned that the issue with stand-alone magnets (in particular in IR2 & 8) was left open since Run 2 and one definitely needs to iterate with WP2.
- **Rogelio** asked if IR2 is the mirror of IR8 and IR5 of IR1. **Serge** answered in the affirmative.

Then, **Benjamin** reviewed the heat load measurements performed during Run 2, and the cryo priorities regarding aC coating of the beam screens. Given the strong dependency of the HL-LHC heat loads on SEY (a factor 25 between SEY=1.1 and SEY=1.3 for HL intensity, see CERN-ACC-2016-0112), an attempt was made to try to estimate the SEY from the heat loads measured during Run 2, indicating that all SAMs should have a SEY lower than 1.2, and many of them even below 1.15. He therefore suggested to perform heat loads simulations at these intermediate SEY values.

Regarding aC coating priorities in the long straight sections (LSS), there are two main limitations: one has to stay below the limit of 120 W per SAM, and below the refrigeration capabilities of each sector. In the current baseline, all inner triplet magnets (including D1) are to be coated in all IPs, which is justified by the fact they contribute the most to the heat loads, as shown by Run 2 measurements. New magnets (Q4 & D2) in IR1 and 5 are also coated in the baseline. Since the sector 23 and 78 are the weakest in terms of cooling capabilities, all LSS magnets right of IP2 and left of IP8 (Q4/D2, Q5 and Q6) should be coated (Q5L8 being already coated), as well as Q6L3 and Q6R7. In other sectors, the SAMs beam screens should be simulated with a SEY of 1.15 or 1.2, and coated if their HL-LHC heat load is higher than 120 W.

Finally, heat loads at high intensity and SEY should be re-assessed during Run 3, in order to validate the coating needs during LS3.

- **Gianni** indicated that the simplifying hypotheses mentioned in slide 8 are actually quite realistic: impedance & synchrotron radiation are contributing much less than electron cloud, and the quadrupole is dominating. **Rogelio** wondered how the heat loads from Q4 and D2 were disentangled. **Benjamin** answered he assumed it goes with the length of each magnet. **Gianni** commented that it would be more realistic to attribute the heat load to the quadrupole alone. **Benjamin** said that D2 & Q4 heat loads are of same order of magnitude anyway, and one cannot do better than this assumption. **Rogelio** asked if there are simulations for the combined D2 and Q4. **Gianni** answered they are in the report. But in any case the assumptions here can give an idea.

Gianni then showed a few more slides to complement the discussion on the need to coat (or not) the SAMs (slides are attached to the "round table" part of the meeting, in the indico page). He mentioned the difficulties to realize the coating in-situ, for the beam screens of the SAMs operating at 4.5 K (in particular Q4, Q5 and Q6) because of the presence of cryosorbers. The situation is different for IR2/8 and IR1/5. In IR1/5 it is necessary to coat the SAMs for stability considerations (the beta functions are very large there, and beam degradation was already observed in Run 2); moreover, in-situ coating might not be needed since beam screens could be extracted in IR1 & 5 during LS3. On the other hand, **Gianni** suggested that the coating of the SAMs in IR2 & 8 could be avoided: the risk taken on the heat load is of the order of 0.5 kW, which is in line with the risk taken for the arcs, and relatively small compared to the total available cryoplant capacity (10 kW).

- **Serge** agreed that if there is an impact on performance due to IR1 & 5, coating there should indeed be a priority, even if the impact on heat loads is relatively small (1 %). **Gianni** said they are not sure one can preserve the SEY over the full HL-LHC lifetime, and a value of 1.3 seems reasonable, given the fact they have sometimes seen SEY as high as 1.5 in the arcs. This has to be checked with the surface group. **Rogelio** mentioned that this could also be checked during Run 3. **Benjamin** indicated that the heat load curves are spreading for larger SEY, hence it should be easy to identify the SEY. **Gianni** commented that one should not use their simulations beyond what they were designed for. There are error bars, and one needs to be solid in what we assume for 15 years of run. SEY=1.3 is safe but not extreme. **Rogelio** asked if the beam screens of the SAMs in IR2/8 could be damaged during LS2. **Gianni** answered it is unlikely, as these beam screens are very protected during long shutdowns. The arcs are observed to be more delicate than the straight sections.
- **Gianni** said that one could perform measurements in the first year of Run 3, but there is not much one can do on the modeling side. **Serge** mentioned that if we could wait 2 years before coating

point 2 & 8, we could check heat loads in Run 3 and depending on it, decide if one should coat. **Rogelio** concluded that we should wait until one has new observations with beam.

3 Update on field quality of MCBRD and D2 (Ezio Todesco)

This presentation provides an update on the field quality of the D2 magnet and its corrector (MCBRD) in the triplet region in HL-LHC, following-up on talks during the 183th WP2 meeting and the 186th WP2 meeting.

For the D2 magnet, the short model was measured at cold, and results are rather good (b3=12.1 units, b5=9.5 units). With the nominal shimming, the expected high order multipoles of the prototype at 12 kA would then be approximately 0, 1.5 and 0.5 units for b3, b5 and b7, respectively. However, the coil being 0.4 mm too large, shimming has to be modified either on the pole or on the midplane, which may end up with b3 at -4 units while b5 would be between 5 and 7 units. Due to the small (and to some extent, unpredictable) sensitivity of b5 on shimming, it might be impossible to correct b5 without pushing further down b3. One option could be to tune again the copper wedges in the series magnets, but 1) it has to be decided very soon (February/March) despite the incomplete data, and 2) it is risky, in particular one could get a very different effect as the one expected. Another option is to correct b5 with the corrector package (CP) - having in mind that 1 unit of b5 in the triplets is equivalent to 6 units in D2. in the end, further beam dynamics simulations (exploring cases with systematic b3 at ±3, ±6 units, and with systematic b5 at ±3, ±6 units) are needed to have clearer line for the relevance of b5 - studies by **Massimo** and **Frederik** are ongoing. One should also note that the problem disappears if the coil size goes back to nominal.

A short retrospective is also presented on the series production of the LHC dipoles. Two interventions were performed along the production: one on the wedges, after 35 dipoles were produced, and one on the shimming, after 154 dipoles, with mixed results. The retrospective highlights the difficulty of controlling the magnet multipoles; this is even more true with the very small series involved in HL-LHC (e.g. 6 magnets for D2).

Regarding the D2 corrector, the source of the 10 units of b3/a3 has been identified at the edge of the yoke. An attempt was made on the blue aperture to change the material of the yoke keys to stainless steel, which greatly improves the field quality. In principle, with this change one could stay within the 10 units acceptance for b3 and a3 for any powering. The MCBRD P3 will therefore be assembled with stainless steel keys. The impact at 1.9 K will be checked on the coming magnets. New instructions will most probably be given to the Chinese collaboration. MCBRD P1 and P2 will not be disassembled to change the keys, since this operation is not free of cost and risk. One or both could be used in the prototype cold mass of D2, and therefore will have 15 units of a3 in one beam or in both beams.

- **Rogelio** asked for an explanation on slide 7, to understand better what a “reduction on the pole or on the midplane”, means. **Ezio** explained that a reduction on the pole means that all the coil is shifted down by e.g. 0.1 mm, and a reduction on the midplane means that the coil is pushed up by e.g. 0.1 mm. These are very thin and precise shimming.

- **Rogelio** asked if the large detrimental effect observed on b3, when trying to reduce b5 with shimming (see slide 9), is there only with shimming on the midplane, and not with shimming on the pole. **Ezio** answered in the negative. There are anyway too many constraints with too few handles.

- Regarding the compensation with CP, **Massimo** said that indeed there is no systematic b5 multipole for the triplets, but there is one for D1. Hence the CP should compensate for the b5 in D1, as well.

- **Rogelio** asked about the status of the DA simulations with b3 and b5 out of acceptance. **Frederik** and **Massimo** answered that they are ongoing; results are not yet known. **Rogelio** wondered if the results could be passed to **Ezio** in one or two weeks. **Frederik** answered in the affirmative (Action: **Frederik, Massimo**). **Rogelio** considered the possibility of changing the acceptance criteria. He wondered if it is already known that the magnets will not pass the criteria. **Ezio** answered that since this is a prototype, its field quality does not need to respect the acceptance criteria. Hence one should not change the criteria. The idea is more to give a warning when we are out of acceptance. **Rogelio** concluded that one should meet again when the simulations of **Frederik** will be done. **Ezio** gave the example of the first quadrupole, which had a b6 multipole at the edge of the acceptance, and yet they were able to correct it. He argued that acceptance criteria do not steer field quality, but interactions between WP2 & WP3 do. **Rogelio** replied that acceptance criteria are not yet fully defined anyway. **Ezio** answered they could be slightly changed, indeed.

- **Rogelio** mentioned that there are two D2 (two apertures) and only one CP to correct. **Ezio** answered that the two apertures have the same b5 . **Rogelio** said that for b3 it was not so clear. **Massimo** mentioned that using the b3 corrector to correct the systematic part of b3 in D2 was successful. If it is different in the two apertures, one can correct only the common part. In the case of b5, the situation is not clear. Both **Massimo** and **Frederik** mentioned that the correction of b5 is not in the simulation. **Massimo** said that it is not in the baseline; now only test simulations implement corrections of the field quality of D2. One should be careful not to exhaust the strength of the CP.

- About the MCBRD, **Rogelio** wondered about the possible later use of prototypes in the machine. **Ezio** answered that all prototypes are possible spares, hence they are working to have all prototypes installable in the tunnel if needed, to avoid wasting magnets. He mentioned still that it would not be the first choice, as they could be slightly out of specifications, which **Rogelio** confirmed. **Rogelio** asked **Massimo** if it would be worth making a simulation with one MCBRD prototype. **Massimo** said that the field quality in the D2 corrector is not a serious issue, and he is not sure it is worth making a simulation for that. It is clearly not the main driver of DA reduction, unlike the MCBXF. Moreover, the ongoing reduction of b3 goes in the right direction. **Ezio** mentioned that a3 is more a problem than b3. **Massimo** replied that it depends on the plane. In any case, the cleaner the situation, the better. **Ezio** answered that it would not cost too much, they are going in this direction anyway. But indeed, it is not critical. He also mentioned that they do not plan to re-assemble the P1 and P2 prototypes. **Rogelio** asked if there is only one MCBRD with 5 extra units in a3. **Ezio** answered in the affirmative. **Massimo** said he will discuss with **Frederik**. **Rogelio** said they could touch base on the subject when there will be a discussion on the bigger issue on the D2, when **Frederik** presents the simulation results. **Ezio** said he will continue to keep WP2 posted (Action: **Massimo** and **Frederik** to assess if one MCBRD with 15 units is a concern).

4 LONGITUDINAL IMPEDANCE MEASUREMENTS AND SIMULATIONS FOR THE CRYSTAL COLLIMATOR IN HL-LHC (DANILO QUARTULLO)

This presentation reviews the longitudinal impedance aspects of the crystal goniometer (i.e. the crystal primary collimator) in different configurations relevant for HL-LHC. The goniometers (four of them - one per plane and per beam) are already present in the LHC machine and will be used as early as 2022 for ion beams. The V2 model of the goniometer was imported from CAD and simulated with CST, removing external parts that do not give impedance contributions.

A comprehensive study was performed to analyse the impedance in several configurations, with the replacement chamber in parking, and for different crystal positions (from 2 mm to 54 mm from the beam orbit). Note that when the replacement chamber is in (standard configuration for proton beams) the goniometer is transparent to the beam - only small air gaps remain in the RF contacts, but their impedance is negligible. Studies were performed to characterize in simulation the materials of the device components in terms of complex permittivity, in particular for the crystal; it was shown that the effect of these permittivities on the impedance modes is significant only for the mode at 700 MHz.

Dedicated RF wire and probe measurements were performed on one LHC goniometer, exhibiting several discrepancies in mode parameters with respect to CST simulations, but overall the results showed that no important high-Q mode measured is missing from the simulations.

The longitudinal impedances obtained from simulations were used to compute the power loss in various scenarios, including past LHC Run 2 fills, as well as HL-LHC one with different filling schemes (including 8b4e), various longitudinal bunch profile (Gaussian or q-Gaussian), and for either ions or protons (with the goniometer at 54 mm in the latter case, i.e. without the replacement chamber). The impedance is shifted within ±20 MHz to simulate the uncertainty on the mode frequencies and the possible effect of a mode coinciding exactly with a line in the beam spectrum. From this procedure the average power loss, as well as the maximum value on the full range, are extracted. For proton beams, with Gaussian distributions an average power loss up to 77 W can be experienced with the HL-LHC 2760-bunch scheme (53 W for the 8b4e one), while it is up to 1.1 W for ion beams (with a goniometer at 2 mm distance from the beam). The maximum power loss over the full ±20 MHz range is typically 8 times higher than the average. With a q-Gaussian bunch profile, all the heat loads get 50% higher. Finally, the average power loss values obtained from the modes as obtained from measurement, are also 50% higher as when using the simulated impedance, but the maximum over the ±20 MHz range is 65% smaller than simulated.

- **Rogelio** asked why the replacement chamber was not in for studies with proton beams, and wondered if it is a kind of worst-case scenario. **Danilo** answered to the latter question that it is, but indeed during proton operation the replacement chamber should be in. They wanted to check if it is needed, for future versions of the goniometer, by testing the impact of simply retracting the crystal. **Rogelio** asked whether the final baseline is already decided regarding this aspect. **Danilo** answered he was not sure. **Benoît** said that from his point of view the replacement chamber should be parked in for protons. **Daniele** confirmed that indeed it should be in for protons, as the baseline, but always out for ions (when injecting the beam). There will be future studies on a goniometer without a replacement chamber, but this is not for now. **Rogelio** concluded that at this stage we should not mention it could be out.

- **Rogelio** wondered what the 4 σ bunch length means for a q-Gaussian profile - there is an apparent discrepancy between a 4 σ bunch length of 1.2 ns and an RMS value of 7.5 cm (it rather corresponds to 9 cm RMS). **Danilo** said that the bunch length of 1.2 ns is for a Gaussian profile, but he needs to check. **Francesco** said that the spectrum of a q-Gaussian leads to higher power loss as the spectrum is higher. The first zero of the spectrum is the same, but the q-Gaussian leads to higher power loss. **Rogelio** replied that the results are then too optimistic: for Gaussian profiles we should take 1 ns for HL-LHC. **Benoît** mentioned that this is from the specifications, as presented in WP2. The binomial distribution is the q-Gaussian one. He added that the plan was definitely not to be optimistic. **Rogelio** concluded that one should discuss and clarify this (Action: **Benoît**, **Danilo**, **Rogelio**).

- **Benoît** mentioned that there will be machine development (MD) studies with low-intensities proton beams, with the replacement chamber open, in Run 3. This will be checked with machine protection. **Daniele** confirmed, and added this was already done with ions. There is a validation to be done. Nominal proton beams were never tried. **Rogelio** said this has to be discussed in the proper committee, for the LHC.

- **Roderik** asked what the conclusion is regarding the power loss. For him 12 W (maximum power loss for ions, with a q-Gaussian profile) seems very small. He wondered about beam stability. **Danilo** answered they did not study beam dynamics. Regarding the power loss, there is an ongoing investigation to check if it is a concern. **Rogelio** wondered as well if 12W could be a lot. **Benoît** said this has to be checked with the provider of the equipment - the answer should come from STI. Even 1 W can be very detrimental - it all depends on the equipment.

5 TRANSVERSE IMPEDANCE OF THE CRYSTAL COLLIMATOR (BENOÎT SALVANT)

This presentation addresses the transverse impedance aspects of the crystal goniometer, following-up on the previous talk. This topic received a high priority at the end of 2020, because of the need of a fast approval of the V2 and V3 goniometer for Run 3, as it became the baseline for HL-LHC ion runs consequently to the recent issues on the 11 T dipoles.

The V2 goniometer model had to be simplified as it could not be digested by CST, and benchmarks with **Danilo**'s longitudinal results (see previous talk) were then performed, showing a good agreement for most modes when the crystal is at 54 mm, and a similar order of magnitude (for both frequencies and shunt impedances of the modes) for smaller distances. Regarding the broad-band part (evaluated in terms of Im(Z)/n), each goniometer represents around 1mΩ/m when the crystals are fully inserted, which accounts for 1% of the total longitudinal impedance of the machine. This contribution is divided by 5 when the crystal is retracted to 54 mm.

In transverse, the main modes observed were already present in longitudinal, and their shunt impedance is very small. The broadband impedance is around 15 kΩ/m at 2 mm, and ten times less in parking position; it is always negligible compared to the total imaginary impedance of the machine (<0.1%).

The V3 model of the goniometer is also being investigated, and preliminary results are similar to those of the V2 model, both in longitudinal and transverse. The power loss obtained is also similar to that obtained by **Danilo** (see previous talk), e.g. 1.1 W (resp. 1.9 W) for the average (resp. maximum) power loss, when shifting the impedance over the range ±20 MHz, for ions in HL-LHC with the crystal at 4 mm.

The main remaining question concerns the electromagnetic properties of the crystal in the frequency range of interest, for which measurements are needed.

- **Rogelio** asked with respect to which reference the transverse impedance is small (see slide 17). **Benoît** answered that it is small with respect to the LHC impedance with proton beams. **Rogelio** asked whether there is a large difference in the impedance with ions. **Benoît** and **Nicolas** answered that it would be negligible with respect to both reference impedances. **Stefano** and **Roderik** added that the IR7 settings are very similar for protons and ions, only the TCTs may differ, but this represents anyway a small contribution. **Nicolas** later confirmed that the impedance for ions in HL is quite similar to that for protons.

- **Roderik** commented that if we think there is an issue regarding either heating or beam stability, we should discuss it. He also noticed that the maximum power loss for ions shown in this presentation (2 W) is different from the one obtained in the previous talk (12 W). **Benoît** indicated that the modes for the V3 model are smaller, so the situation is better. **Danilo** also mentioned that the holder is different in the V3 goniometer, which is very important for the mode at 700 MHz. **Benoît** confirmed. **Roderik** wondered if it is not also due to the difference in crystal position (4 mm vs 2 mm). **Benoît** answered that the mode is what matters, and it is not modified a lot by the position, probably because the holder has a "C" shape around the crystal. In V2, it resonates more (because of the metal - in yellow in slide 18).

- **Stefano** commented that this is a high precision device. He asked whether it was checked with **Inigo Lamas Garcia** if a local heating could bend the device. **Benoît** answered it could indeed be an issue, and that it is why the impedance team alone cannot answer if 10 W or 1 W would be problematic. **Stefano** concluded that we should then take this up to the hardware team - **Inigo** is aware. **Roderik** added that there will be both V2 and V3 goniometers in the machine, hence one needs to check both the 12 W and the 2 W heat loads. **Stefano** said that **Inigo** knew about the V2 value - it is in the publication from **Danilo** which triggered a lot of discussions. Since they have asked to improve the device impedance, **Stefano** also wondered if the improvement follows the changes done. **Benoît** answered that they did not request any changes, because if one changes something the heat load might go in another place where it could have more detrimental effects. Hence the improvement was obtained more by chance. The mechanical improvement was the only motivation. For the impedance team, it is very difficult to know in advance if any change to that complex structure will improve the situation, before simulating it. **Stefano** said it would be nice if something can be improved. **Benoît** answered that we are exposing something fragile to the beam, hence it is not clear if a small heat deposited somewhere could have large consequences. He added that the old goniometer was ok with beam. **Stefano** agreed with this statement - there was a test with 600 bunches at top energy in 2018 (albeit with a rather moderate intensity), which is quite reassuring. Hence there should not be any huge issue in Run 3 - the hope is that fragile pieces will not suffer too much.

- **Rogelio** summarized the situation by saying that: 1) for protons there is no issue since there is the replacement chamber; 2) for ions there is no issue with respect to transverse stability, but one needs to check the longitudinal stability; 3) the impact of the heat load has to be checked with hardware people. **Benoît** agreed. **Stefano** commented that there will be an intensity ramp-up, hence one will monitor fill after fill. **Elias** asked if the RF team is checking the longitudinal stability. **Benoît** answered that **Christine** is aware, and **Danilo** can pass the information there (Action: **Danilo**, **RF team**). **Roderik** concluded that we only need to check with RF for the longitudinal stability, and with STI for the heat load (Action: **Benoît**, **Inigo**).

6 ROUND TABLE (ROGELIO TOMÁS)

The next WP2 meeting will take place on Feb. 9th, by zoom. The preliminary agenda is the following:

- New results of SPS CC noise emittance blowup analysis (Natalia Triantafyllou)
- Update of the effect of the crab cavities on stability in the 8b4e case (Nicolas Mounet) - to be confirmed
- MCBXF field quality 2D model (Ezio Todesco)
- DA for D2 Specification (Frederik Van Der Veken)

Reported by N. Mounet & G. Skripka