WP2
Meeting #170
Tue 10 March 2020, 9:00 – 12:00

Chair:	Rogelio Tomás
Speakers:	Benoît Salvant, Nicolas Mounet, Carlo Zannini, Xavier Buffat
Participants (room):	Nicolo Biancacci, Oskar Bjorkqvist, Roderik Bruce, Riccardo De Maria, Paolo Fessia, Francesco Giordano, Gianni Iadarola, Elias Métral, Galina Skripka, Frederik Van der Veken
Participants (vidyo):	Gianluigi Arduini, Skoufaris Kyriacos, Michele Modena, Yannis Papaphilippou, Guido Sterbini.

AGENDA

MEETING ACTIONS

Paolo, Massimo	Provide input on the offsets to be expected for the deformable finger bellows
Mike Barnes	Update the intensity limits for the expected operational cycle.

Carlo, Nicolas	Check Q_x-Q_y along an HL-LHC bunch train.
Nicolas	Investigate the reason(s) for the difference between the new impedance model and the previous one by Sergey Antipov.
Nicolas	Verify whether the replacement of the TCP.C6L7.B1 collimator with a new collimator with BPM buttons can explain the observed difference in impedance between the two rounds of measurements.
Nicolas, Xavier	Update the HL-LHC octupole and teleindex predictions with the new impedance model.

GENERAL INFORMATION (ROGELIO TOMÁS)

Minutes of the previous meeting are not ready yet; they will be circulated soon and discussed during the next meeting.

1 UPDATE ON IMPEDANCE CONSIDERATIONS FOR HL-LHC EQUIPMENTS (BENOÎT SALVANT)

This is an update regarding the impedance of equipment installed or to be installed during LS2 and LS3, reviewing the current knowledge accumulated by the impedance team, as well as the remaining questions.

The upcoming collimator changes during LS3 were detailed during the WP5 internal review (March 4th, https://indico.cern.ch/event/892297/), in particular the IR7 upgrade, the changes of TCT and TCL in cell 4 of IR1 & 5, and the relocation of collimators with ferrite. Significant work has been performed by the impedance team on resistivity measurement methods; the resistivity of Mo coating on many batches was measured and complies with the specification. Work is ongoing towards the acceptance of all new HL collimators going in the machine during LS2 (4 TCLDs and 3 out of 4 TCPPMs are already validated).

One new MKI-COOL has been measured for impedance and is planned to be installed in June 2020. Seven more, plus spares, will come later by and during LS3 to replace all current MKIs that may otherwise limit HL-LHC intensity due to impedance-related heat load (although the temperature increase resulting from it is not known yet). Installation has also been approved for the new TDIS and the ECR for the LHCb VELO is being drafted.

Regarding the equipment to be installed during LS3, there are no updates related to crab cavities or BPMs, but a new table for RFD modes is expected soon. On the other hand, the Y-chambers in IR1 & 5 and the VAX/TAXS/Q1 unshielded bellows have been approved by the impedance working group, while the new deformable RF fingers (DRF) are under investigation. In particular, the DRF within the crab cavities exhibit a large angle and hence a significant broad-band impedance (but no particular mode below 2 GHz); the same goes for the VAX bellows because of the large transverse offset to accommodate. The IR2 & 8 Y-chambers are under investigation but exhibit a small impedance. The model is now more accurate for each of these elements. One important remark is that the shielding becomes almost useless when there is a large angle to accommodate the offset.

The electron lens design is still ongoing, while the impedance of crystal collimators is the subject of investigations as there are discrepancies between measurements and simulations. It is a major question mark whether one can design a crystal collimator without a replacement chamber, yet transparent for protons. Other remaining questions regard the impact of radiation on resistivity for collimators, the impact of jaw alignment on measurements, and the forward physics plans for HL-LHC.

- **Nicolas** asked if we can keep the resistivity of pure molybdenum in the impedance model of collimators. **Benoît** showed the batch measurements that are very close to the pure molybdenum resistivity and therefore answered in the positive.
- **Roderik** commented that for some slots, the CFC secondaries will be replaced by a MoGr collimator (if the TCSM upgrade slot is already occupied, e.g. by crystals), while in other slots the CFC collimator will stay (but usually be operated at parking) and the MoGr collimator will then be installed in the adjacent TCSM slot.
- **Rogelio** asked if there is an estimate of the new intensity limit with the MKI-COOL. **Benoît** answered in the positive; he also mentioned that the limit is per equipment, hence as long as there are standard MKIs in the machine the limit remains the same. After the meeting, **Benoît** indicated that the cooling will be able to cope with the estimated power loss with a reasonable margin, hence there would be no intensity limit from the MKI-COOL for HL. This was confirmed by **Mike Barnes**, who also indicated that TE-ABT is currently investigating the mechanical properties of the ferrite damping cylinder and possible impact if it were to crack because of mechanical stress due to the temperature gradient (in the MKI-COOL only).
- **Gianluigi** mentioned that the new MKI-COOL is very expensive, and it would be therefore worth knowing the temperature in the current MKI before installation, in particular in the ferrite. **Benoît** said this is a very difficult information to obtain from thermal simulations (it would take the equivalent of a PhD thesis) and there are huge error bars. **Gianluigi** then wondered if this can be measured on the bench. **Oskar** answered there is no test bench and no spare, so the idea is rather to measure in the machine during Run III. **Gianluigi** then argued that one cannot know the temperature of the ferrite when the MKI is in the machine, so it might be better to simulate the temperature evolution on the bench. **Benoît** mentioned that at least with beam we would realize when the limit is reached, but **Gianluigi** said we might not reach it. **Rogelio** finally proposed that when a second MKI-COOL will be built during Run III, one could test it. **Gianluigi** and **Rogelio** said this question should be followed-up at the Technical Committee.
- **Gianni** asked what a glassy sheet is (in the TDIS). **Benoît** answered this is a (conductive) carbon sheet, perpendicular to the orbit, in contact with the edge of one jaw block (e.g. graphite), on one side, and the RF fingers on the other side, below the jaw surface facing the beam. Its purpose is to avoid that dust (from scraping of the RF fingers to the block) falls onto the beam.
- **Paolo** mentioned that the VAX is being tested now, to see what is good for the bellows.

- **Riccardo** wondered why one would need to accommodate a 10mm transverse offset with the DRF. The alignment working group still did not give its recommendation, and to his knowledge there is not such a need between crab cavities (although discussions are ongoing). Also, between the triplets, no more than a few mm offset is foreseen. **Benoît** actually said that the devices are all assumed perfectly aligned in the calculations, and the angles given are the nominal ones in operation (after the meeting, **Benoît** checked with Rama Calaga and Teddy Capelli that the maximum angle for crab cavity DRF is actually 25.5°, and 20° for the others, instead of 18° and 12°, respectively). **Riccardo** and **Paolo** asked how much is the offset. **Benoît** could not tell, but said that for e.g. the triplets the longitudinal offset is what matters most, for the angle of the RF fingers convolution. For the crab cavities, it might be different because of the smaller radius. **Gianluigi** also wondered if the offset for the VAX is not extreme. **Rogelio** said the alignment working group should tell us everything about the possible offsets, in the end (Action: **Paolo, Massimo**).
- **Paolo** offered **Benoît** his help to clarify the layout around the crab cavities.
- **Gianluigi** wondered if keeping in old collimators would be an issue regarding geometric impedance. **Benoît** answered that according to first simulations, resonant modes are expected to be low. **Nicolas** added that the dominant part is the taper, which scales as $1/(\text{half-gap})^2$. **Gianluigi** and **Elias** then asked about the impact of possible non-conformities (although they would be there already now). **Benoît** wondered if there could still be issues with RF-fingers if they do not move for a while. **Gianluigi** then said that maybe a recommendation is needed there, e.g. not to move them at all, or move them with a given periodicity. **Roderik** mentioned there is not much experience on that subject; the collimation team moves them once per year for those they do not use, but there are no statistics about this.
- **Gianluigi** asked if the intensity limits for the MKI are based on constant operation at a given intensity. **Benoît** thought so, the simulations are with a constant, but inhomogeneous thermal load. **Oskar** confirmed. **Gianluigi** said this is overly pessimistic. **Benoît** confirmed, but said that putting time dependence will add a lot more work. **Oskar** mentioned the bunch length is also affecting the heat load (which here is assumed constant). **Gianluigi** said that if required, a longitudinal blow-up could be put in place, so it is important to know how far we are from the real limit - one should not be too conservative. **Benoît** said we should not eat the margin, as there could be non-conformities as in Run I, for instance. **Rogelio** then said that actually, the power loss curve seems slightly optimistic as there is about half an hour more to be considered for injection and ramp (Action: **Mike Barnes** to update the intensity limits for the expected operational cycle).

2 Update of the HL-LHC impedance model, with considerations about MKI and triplet beam screens (Nicolas Mounet)

The update of the HL-LHC impedance model is described, and conclusions are given on the impact of a few elements recently added: the longitudinal weld in the triplet beam screens, the MKI, and the deformable RF-fingers, bellows and Y-chamber in the triplet (and crab cavity) region.

The HL-LHC impedance model has been updated to take into account more elements installed during LS2 and LS3, and to be more accurate on a few elements that were already present in the model. The list of impedance-creating equipment is given, as well as those not yet considered. The collimator almost full upgrade (keeping in CFC two IR7 secondaries), and the various changes in tertiaries and debris absorbers, are the main modifications with respect to the LHC. Also, the geometric impedance of the tapers of the collimators are now better taken into account. Among the missing elements, crystal collimators and electron lens are possibly significant but still require dedicated studies. The model is now available on the impedance webpage (https://impedance.web.cern.ch), with interactive plots and all parameters provided. The impedance is somewhat higher than the previous model by Sergey Antipov. This will be investigated in detail in the future (Action: **Nicolas**).

The impact of the longitudinal weld along the new octogonal beam screens in the triplets (up to the D2 magnet) is estimated, and a study is performed on the possible optimization of the impedance via different orientations of the beam screen, trying to put the weld on the top/bottom or left/right facets of the screen, depending on the respective β functions in each plane. Most of the beam screens (except the ones in the D2 magnet) contain both beams, hence an optimization is not possible for round optics, as optimizing one beam would be detrimental for the other one. For flat optics, an optimization is possible by putting all weld top/bottom on IR1 and left/right in IR5, but the gain in terms of stability is negligible, hence such an optimization is not worth the effort (and moreover subject to changes of optics and crossing plane).

The MKI is found not to have any impact on stability as well, due to the higher order mode nature of its impedance. On the other hand, deformable RF-fingers, unshielded bellows in VAX/TAXS/Q1 and Y-chambers have an impact on impedance, estimated to be around 4% more on the octupole threshold (computed with round optics and β*=15cm).

- **Roderik** commented that the copper layer on top of the copper-diamond tertiaries, is actually a thick cladding and not a coating. **Nicolas** said that indeed, the coating term is not appropriate but in the model the thickness is that of a cladding (0.2 mm).
- **Rogelio** asked how pessimistic is the impact of the DRF, given the possibly pessimistic offsets taken into account in the angles (see previous talk). **Nicolas** answered that for the crab cavities DRF, actually the angles could be even higher (up to 20°), but on the other hand the optics chosen for the comparison is also pessimistic, as 15cm of β* should be reached only during collisions when stability matters much less. With 40cm β*, the impact is lower.

3 LASLETT TUNE SHIFT FOR HL-LHC (CARLO ZANNINI)

The idea here is to estimate the tune shift (or excursion) between the first and last bunch of the full HL-LHC train. Computations by Sergey Antipov, using a formula by Luc Vos, indicate a tune excursion of around 1e-3. This is confirmed by PyHEADTAIL simulations taking into account dipolar and quadrupolar parts of the resistive-wall impedance (from collimators, beam screens, and beam pipe) of the HL-LHC 2019 model, using a realistic bunch train. The effect is even much stronger in the SPS, with an excursion close to 0.03 along the full train (from measurements and simulations by Michael Schenk), and it will be even much larger with LIU parameters.

- **Rogelio** expressed concerns about the HL-LHC tune excursion given, as a tight control of the tune is typically assumed (1e-3). **Elias** said that at least it is not a factor 1000 higher, as initially published in a paper by F. Zimmermann et al (this was identified as a mistake, after publication, by Sergey Antipov and Sergey Arsenyev). The next step is to try to understand better the effect of quadrupolar impedance.
- **Rogelio** commented that Q_x and Q_y do not have exactly the same pattern along the train, hence he asked about Q_x-Q_y for each bunch, as one needs to control it tightly to keep a tune split of 5e-3, and also for reasons related to coupling (Action: **Carlo, Nicolas**).
- **Gianluigi** asked if the tune excursion at injection is a factor 2 higher. **Carlo** answered in the positive. **Rogelio** then wondered if the trend is also the same. **Carlo** answered that yes. **Gianluigi** commented that the pattern looks like the one from electron cloud, so that maybe it will be difficult to optimize the working point at injection. **Gianni** then said that at injection the sensitivity is not the same as at flat top: it is closer to 1e-2 or 2e-2 and related to spread, electron cloud, and chromaticity.
- **Yannis** asked if the effect is linear with bunch charge. **Elias** said it's the bunch intensity and the filling pattern that matter here, but that one needs to check and review all these computations in detail. **Rogelio** added that we get from the injectors around 3% bunch intensity spread, but **Elias** indicated that this should not matter too much.
- **Rogelio** was surprised about the very large tune excursion in the SPS, which means the optics is very different from bunch to bunch. He wondered then how the emittance can be preserved, and how much is the effect at flat top. **Nicolas** said it will be smaller as it scales like 1/gamma, which smoothed down the concern of **Rogelio** about the injection in the LHC. **Gianluigi** mentioned that indeed the injection in the SPS will be a challenge, as tune corrections will be needed for each batch injected.

4 IMPEDANCE MODEL OF THE LHC: SUMMARY OF THE PRESENT UNDERSTANDING OF THE MEASUREMENTS (XAVIER BUFFAT)

This presentation reviews and summarizes the measurements directly related to the impedance of the LHC (essentially instability rise times and tune shifts) that were performed during Run I and II.

Coupled-bunch instability rise times were measured in 2011 on 48-bunch trains (50ns spaced), without transverse damper (ADT) and with a chromaticity close to zero, at 450 GeV and 3.5 TeV. These measurements, which essentially probe the real part of the beam screens impedance close to 8kHz, show a rather small discrepancy (less than a factor 2) at injection but a larger one at 3.5TeV, which might be due either to measurement inaccuracies, or to an unknown contribution to impedance from either collimators or copper magneto-resistance. The relatively surprising stability of the beam at zero octupole current at 3.5TeV (with ADT on) also seems to indicate unknown contributions from lattice non-linearities or high order chromaticities. If performed again, these measurements could benefit from a better knowledge of optics and the more powerful tools available (e.g. the long buffer of the ADTObsBox).

Single bunch instability rise times were also measured at injection, without ADT and for negative chromaticities, i.e. in conditions where the impedance of collimators should dominate. Results are compatible with an impedance between 1 and 2 times the model, but measurement quality is rather poor and has a large potential for improvement. Single bunch instabilities were also observed in Run II operational conditions (6.5 TeV, Q' close to 15, where the collimators also dominate the real part of the impedance), with a rise time typically slower than what is expected without Landau damping, although the statistics on 2017 end-of-squeeze instabilities show an average compatible with DELPHI calculations (without the need to add a factor of 2 on top of the model). Moreover, the unstable mode type typically follows expectations. In general, since the instability rise time combines contributions from the impedance and from Landau damping, the uncertainty on the impedance that could be deduced from its measurement suffers from the uncertainty on Landau damping. Up to now no procedure was found to circumvent this issue.

Run I single bunch tune shifts measurements (Q'~0) showed total tune shifts (dominated by the imaginary part of the collimator impedance) up to 3 times higher than the model at injection, and twice higher at 3.5 TeV. Over Run II the measurements and the model were both refined and the discrepancy was reduced to less than a factor 1.5. Beam transfer function (BTF) measurements in 2018 showed a similar (but slightly higher) discrepancy. The tune shifts of secondary collimators became increasingly closer to the model between Run I and Run II, thanks to better measurement techniques (visible on the increased reproducibility) and possibly impedance model refinements. For the primaries both Run I and Run II tune shifts proved to be in good agreement with the model. A particularly remarkable agreement was reached in 2017 for the prototype low impedance collimator. On the other hand, strong discrepancies appeared in 2018 for some TCP and TCS, the reason being still unknown - explanations range from an effect of irradiation, non-conformities, or measurement issues such as misalignments.

Overall, coupled-bunch instability rise times seem to show a good modeling of the beam screen, but single-bunch rise times at injection do not give better than a factor 2 discrepancy with the model, while at top energy they are overshadowed by the poor knowledge of Landau damping. Tune shifts of the full machine currently show no more than a factor 1.5 discrepancy with the model, while for individual collimators, the discrepancy ranges from perfect agreement to a factor of 3.

- **Rogelio** asked about the chromaticity and β^* in the coupled-bunch instability measurement at 3.5 TeV. (After the meeting, **Nicolas** checked that the chromaticity was around 2 for B1 and 1 for B2). **Rogelio** said that probably Q'' was very small, as well as non-linearities from the lattice (β^* at that time was rather large even when squeezed). **Elias** wondered how it comes that we were stable without octupoles. **Xavier** said indeed it's surprising, even with the damper.
- **Rogelio** asked if the main limitation for single-bunch rise time measurements is that we cannot switch off the octupoles fast enough. **Xavier** answered in the positive. **Elias** said that still we can measure rise times with octupoles on. **Xavier** agreed but said there are uncertainties on Landau

damping (from emittance / distributions / detuning from lattice non-linearities / high order chromaticities).

- **Rogelio** commented that the tail of the end-of-squeeze 2017 measurement statistics is rather large (slide 18). **Xavier** answered the histogram could be made better, with e.g. a kernel density estimation (now there are large error bars on the tails).
- On slide 19, **Elias** mentioned that there were uncertainties on the chromaticity value for the 2010 measurement, but Michael Schenk could reproduce this again later with less uncertainties.
- **Gianni** suggested using the feed-down from high order multipoles to decrease faster Landau damping, as the orbit can be changed fast. For instance, the feed-down from decapoles could be used. **Xavier** was very enthusiastic about the idea. **Elias** also mentioned the idea of **Nicolas** to use head-on separation to destabilize the beam relatively fast.
- **Gianluigi** asked if the dotted line in slides 22 to 24 (about single-bunch real tune shifts at injection) represents the simulation. **Xavier** answered in the positive.
- About the new 2018 measurement discrepancy on collimators, **Rogelio** asked if it is easy to check the collimator misalignment, since we have all the 2018 data from the button BPMs (as was discussed in the 161st WP2 meeting). According to **Roderik** and **Elias**, this has to be checked. **Roderik** also mentioned that there are not buttons for every collimator. **Elias** said that Sergey made simulations which showed a huge impact of the offset. **Roderik** said that the TCP.C6L7.B1 collimator was changed in the 2016-17 EYETS, when a new collimator with BPM buttons was installed. The hardware used in the measurements was thus not the same (Action: **Nicolas** to see whether this could explain the observed difference between the two rounds of measurements). **Gianluigi** then asked if these collimators are the ones to be changed to Mo-Graphite ones. **Roderik** answered in the positive (horizontal and vertical TCPs to be exchanged, while skew TCPs will stay in CFC and not be upgraded). **Benoît** also asked if the old TCPs will stay in parking, to which **Roderik** answered in the negative, since the old horizontal and vertical TCPs in CFC will not be at all in the machine anymore.
- **Gianluigi** asked if the discrepancy between 2017 and 2018 measurements could be explained by a misalignment compatible with the tolerances. **Xavier** answered in the negative, showing the last slide (in the backup): the estimation of Sergey requires a few σ which is too large. **Roderik** said that for the 3 stripe-prototype collimator we have the BPM. In 2017 there could have been a misalignment but much less in 2018. But there could always be something special happening for this particular MD. **Rogelio** insisted that this is easy to check (see Action from 161st WP2 meeting: **Alessio Mereghetti**). He wondered if the only possibility is the irradiation. **Elias** said that up to now he always mentioned only irradiation, but there are also other possibilities, as the important misalignment impact which will need to be checked carefully in MDs. **Nicolas** said there could be a threshold effect with irradiation, with the accumulation of dose. But **Rogelio** and **Xavier** wondered why it is then in all stripes, it seems quite surprising. **Nicolas** agreed, saying also that results from the Fluka team show a narrow transverse localization of the dose. **Gianluigi** concluded that we should improve the precision of the impedance measurement of the irradiated samples we have.

5 AN UPDATE ON HL-LHC OCTUPOLE REQUIREMENT (XAVIER BUFFAT)

This update starts with a reminder of the methodology used to compute the octupole current required to stabilize the beam: the octupole current is chosen such that all along the cycle (in particular during the collapse) the stability factor should remain below 0.5 (to keep a stability diagram twice larger than needed from the model), choosing a pessimistic emittance of 1.7μm. The other constraint is to keep a dynamic aperture of 6σ for a set of tunes separated by 0.006 (keeping a margin for coupling) with an emittance of 2.5μm (also pessimistic, in the other direction).

During the asynchronous collapse, it was assumed up to now that the most unstable plane is always the plane of separation. Unfortunately it is actually not the case, hence some changes in HL-LHC predictions of stability limits. In parallel, an upgrade was done on the MAD'n'PySSD code combination to speed-up the execution (by a factor 200) and consequently be less error prone. Revisiting the full IP1 collapse by checking now systematically the most unstable plane, it is seen that in the nominal HL-LHC configuration with the negative polarity the vertical plane is the most critical due to a change of sign of the direct detuning term at a separation where the indirect term is small, which reduces significantly the tune spread in the vertical plane.

Overall, with the negative polarity a synchronous collapse would work without crab angle and with β^* ~ 1m at the start of collisions, while an asynchronous one with a negative crab angle (compensating partly the crossing angle) and β^* ~1.5m. Both would require the maximum octupole current with a teleindex of 2.4 for stabilization with the full upgrade, 1 σ-retracted collimator option, which is the only option compatible with dynamic aperture requirements. The required teleindex increases to 2.5 (and for DA the crab cavities have to be OFF at the start of the fill) when taking into account lattice errors and coupling (or 2.75 with the full upgrade without retraction instead - this has to be compared to the past wrong prediction of 2.0).

For positive octupole polarity, only the asynchronous collapse offers many possibilities for β^* and crab angle at the start of collisions; still, without crab angle, β^* is rather limited. In the end, the requirements on octupoles strength and teleindex are much smaller than for negative polarity, as maximum octupole current and teleindex of 1 are sufficient even with the LS2 collimator upgrade. Yet with a crab angle the dynamic aperture is not acceptable in this case. On the other hand, it might be acceptable without crab cavities and should be checked. The most critical configuration is single-beam, so the numbers should match Sergey Antipov's results, yet they are 20% lower due to a pessimistic approximation made by Sergey on the stability diagram with positive polarity. With lattice errors and coupling, a teleindex of 1.6 is now necessary with the LS2 upgrade and the crab cavities will possibly have to be OFF during the collapse of the separation bump to maintain the DA at the start of collision. (This requirement might be relaxed with the full collimator upgrade, but should be confirmed with DA simulations).

The impact of crossing angle was also investigated, showing a similar effect as the crab angle.

- **Elias** commented that one could also check with equal emittances to be more consistent (slide 7). **Gianluigi** argued that the BCMS option is the one assuming an emittance of 1.7μm, and if this is the option chosen it means there is an uncontrolled blow-up. Therefore, choosing 2.5μm for the DA evaluation makes sense.

- **Gianluigi** asked about the meaning of the crab angle sign (slide 20). **Xavier** answered that a negative crab angle compensates the crossing angle (while positive adds to the crossing angle).
- **Gianluigi** asked if "full upgrade" means that all secondaries (in IR7) are coated. **Xavier** answered in the positive, and said this is the old model from Sergey. **Nicolas** commented that this should get worse with the newest model, and it might even compensate for the 20% error of Sergey on the positive polarity (Action: **Nicolas, Xavier**). **Roderik** asked if the σ retraction is also on the TCPs. **Xavier** answered that according to Sergey, the TCP retraction does not have much impact. After the meeting, **Xavier** confirmed that the TCPs were indeed retracted by 1 σ.
- **Yannis** commented that the new requirements on levelling from cryogenics will require a revision of the configuration (e.g. crossing angle, crabbing angle, β^*).
- **Yannis** confirmed that there is no available DA computation with positive octupole polarity and no crab cavities - this still has to be done (slide 25).
- **Riccardo** said one should check the option of reversing the polarity during collisions. **Rogelio** said the main point is to check if the positive polarity is a concern during physics. **Gianluigi** commented that the main issue with positive polarity is the dynamic aperture. Then, if we reduce octupoles, there will be issues with the non-colliding bunches. **Rogelio** wondered if we could reduce the current in the octupole by e.g. a factor of 2, and make some compromise on the non-colliding bunches.
- **Yannis** commented that all present scenarios are based on the negative polarity, where the challenge is to reach β^*=15cm (end of levelling). The best was Ioct=-300A. We can now test again what is best octupole current.
- **Riccardo** commented that for the negative polarity the challenges are rather on the optics, and it is not clear if it is possible. **Rogelio** argued that issues can be solved e.g. by playing on the phase advance as for the study of Fabien Plassard on the No MS10 option (see 168th WP2 meeting).
- **Gianluigi** said that we have to put all the numbers together. In the past there was a small preference for negative polarity, now this is not clear anymore. **Rogelio** and **Xavier** confirmed. **Xavier** also mentioned that the choice of polarity for Run III may be driven also by HL-LHC, and could be tested there, but **Rogelio** is not sure about it.
- **Yannis** said that for HL we are very sensitive at β^*=15cm. But by that moment the intensity has dropped to half, so we could cross zero octupoles (from positive to negative) in this area (testing this first in Run III). **Xavier** answered that the issue is in the non-colliding bunches, and their intensity barely decays during physics. One idea could be to use the triplet non-linear correctors when crossing the zero, a study showed that it can be done for β^*<0.3m.
- **Gianluigi** asked if there is any preference for negative or positive polarity when there is a separation. **Xavier** answered that once we choose a teleindex and polarity, it should work for any separation.
- **Gianluigi** commented that one cannot reach β^*=15cm with too high octupoles.

6 ROUND TABLE (ROGELIO TOMÁS)

The date of the next meeting has not been decided yet, the agenda will be circulated in due time.

Reported by N. Mounet