SUMMARY OF SESSION 3: PERFORMANCE AND LIMITATIONS

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LIST OF PRESENTATIONS

The session on LHC Performance was divided in two subsessions. The first one focused on the analysis of luminosity data, beam losses and aperture. The agenda was:

- **Emittance growth until stable beams** (M. Kuhn)
- **Luminosity, emittance evolution and OP scans** (M. Hostettler)
- **Luminosity modeling for the LHC** (F. Antoniou)
- **How to survive a UFO attack** (B. Auchmann)
- **LHC aperture and ULO restrictions: are they a possible limitation in 2016?** (D. Mirarchi)

The second day dedicated to beam instabilities, electron cloud and beam-beam effects. The agenda was:

- **Instabilities and beam induced RF heating in 2015** (L.R. Carver)
- **Electron cloud effects** (G. Iadarola)
- **Beam-Beam head-on and long range** (T. Pieloni)

EMITTANCE GROWTH UNTIL STABLE BEAMS (M. KUHN)

O. Brüning and **S. Redaelli** comment on the 10% error bar of the emittance since discrepancies are also in the 10% level. **W. Kozanecki** asks whether the BSRT uncertainty includes all sources like optics, wire speed, etc. The answer by **M. Kuhn** and **F. Roncarolo** is positive.

W. Hofle asks whether the unexplained vertical emittance growth at injection could come from coupling. **M. Kuhn** answers that coupling has been considered in simulations and it could not make such a large contribution.

LUMINOSITY, EMITTANCE EVOLUTION AND OP SCANS (M. HOSTETTLER)

B. Salvachua asks **M. Hostettler** to estimate the optimum fill length for 2016. **M. Lamont** intervenes to draw the attention over the numbers presented by M. Solfarolli in the previous session. **O. Brüning** clarifies that at least we should know what to aim for.

G. Arduini says that it would be very interesting to see the bunch-by-bunch variations along the year.

M. Lamont requested a confirmation by W. Kozanecki on M. Hostettler's statement that the luminosity imbalance was due to the detector luminosity calibrations. **W. Kozanecki** confirmed positively and added that the mystery now is on the luminosity variation along the fill.

LUMINOSITY MODELING FOR THE LHC (F. ANTONIOU)

M. Lamont recalls that 2012 was an excellent year where luminosity per bunch was much larger than in 2015.

R. Bruce asks whether elastic scattering is considered in the calculations. **F. Antoniou** replies that this effect is negligible.

J. Jowett mentioned that he have an IBS computer code that treats non-Gaussian bunches. **F. Antoniou** replies that A. Vivoli's code also considers arbitrary distributions.

E. Shaposhnikova highlights the improved agreement in the bunch length evolution when considering the transverse emittance blow-up in the model. She also asks what would be the effect on the transverse emittance without the longitudinal cooling. **F. Antoniou** would need to check with the model.

HOW TO SURVIVE A UFO ATTACK (B. AUCHMANN)

M. Lamont, **O. Brüning** and **W. Hofle** question the 15 quenches per year assumed by B. Auchmann and how it might scale in the future. He replies that so far this is only a preliminary assumption as to what can be considered acceptable for operation. This is a trade-off between number of dumps (at a loss rate of 3 hours per dump) and number of quenches (at a rate of 8-12 hours per quench).

G. Arduini enquirers on the possible understanding of the UFO distribution within the arc cell using the model. **B. Auchmann** answers that the model misses the most critical ingredient, the particle release mechanism.

B. Goddard asks about the role of the contamination level of the vacuum system. **B. Auchmann** answers that this needs to be checked.

R. Schmidt offers collaboration, possibly with a designated contact person.

LHC APERTURE AND ULO RESTRICTIONS: ARE THEY A POSSIBLE LIMITATION IN 2016? (D. MIRARCHI)

M. Lamont highlights that performing aperture scans at the ULO produces problems. **D. Mirarchi** and **B. Salvachua** clarifies that this is not necessarily true. Actually in some scans the ULO location moved down increasing the available aperture. **S. Redaelli** adds that the measurement is done with too low intensity. Higher beam intensity might make the measurements more robust.

B. Goddard asks about the possibility of performing skew loss maps. **S. Redaelli** answers that this is indeed interesting but to achieve the required excitation is difficult.

G. Arduini asks about the possibility of the ULO being an RF finger. The answer is that this is not discarded and could be simulated. Nevertheless it might not come from the 2015 warm-up and and cool-down exercises as there is no contraction below 100 K.

INSTABILITIES AND BEAM INDUCED RF HEATING IN 2015 (L.R. CARVER)

R. Tomás asks J. Wenninger if the 1000 turns acquisition at injection will be available in 2016 as it was during Run 1 allowing for the monitoring of coupling. **J. Wenninger** replies positively. **E. Metral** also asks if the measurement could be foreseen during the full cycle. **J. Wenninger** replies that he has some concerns on the reliability of this measurement during the cycle when the transverse damper is on. **E. Metral** comments on the tunes drift observed during injection. They are planning to add this into the simulation as a function of the coupling. **J. Wenninger** comments that this year was special in the sense that CMS was switching ON/OFF his magnet and this affects the coupling. **R. Tomás** adds that next year with the DOROS BPMs we might be able to control better the coupling with nominals.

R. Schimdt asks if instabilities are better or worse than last Run. **L.R. Carver** replies that it is difficult to quantify. **R. Schimdt** comments that for HL-LHC we will need more margin for collimators. **S. Redaelli** asks if they could observe a difference depending on the collimator settings. **L.R. Carver** replies that between 8 σ and 6.5 σ they do see a difference.

G. Arduini asks if we could go below 1 ns with the MKI. This means that we could apply longitudinal blow-up and reduce the heating. **M. Barnes** replies that below 1 ns we might run into problems because the heat is not evenly distributed and it could be a limitation if we run without longitudinal blow-up.

ELECTRON CLOUD EFFECTS (G. IADAROLA)

O. Brüning comments about the use of doublets for scrubbing, he asks if they have considered to use shorter trains and fill the machine, i.e. trains of 12 bunches. **G. Iadarola** replies that with the scheme of 25 ns and the change of chromaticity and octupoles the machine was stable therefore there was no need to go for shorter trains and doublets. He comments that we were always at the limit of heat load.

M. Lamont comments on the measurement of the heat load per sector and asks if the measurement of the BLM integrated signals could be used to make objective conclusions. **S. Redaelli** comments that the fact that there is very low signal seems an indication. **G. Arduini** comments that the different conditioning could be due to the outgassing from the surface but the question is why there are different sectors. **E. Bravin** suggests that it could be related to the closing of the vacuum.

J. Wenninger also comments that we should try also to lower chromaticiy and octupoles once we are colliding seems it seems there is room. **W. Kozanecki** asks what would happen with the non-colliding bunches and expresses interest in the expected luminosity gain from removing them. T. Pieloni replies that in case of reducing chromaticity and octupoles we might consider to lower the intensity of the non-colliding bunches or blow them up a bit to make them stable or simply removing them. **G.Papotti** and **J. Wenninger** comment that we should keep them as long as they do not cause luminosity loss.

G. Papotti asks if it is understood why the instability during the IP1 OP scans does not appear for IP5. T. Pieloni replies that it is not understood.

BEAM-BEAM HEAD-ON AND LONG RANGE (T. PIELONI)

J. Wenninger comments that next year we should measure the crossing angle as it seems that we have run in 2015 with slightly larger crossing angle this year.

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