MINUTES 7th EVIAN WORKSHOP - SESSION 4, "PERFORMANCE 2"

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M. HOSTETTLER: HOW WELL DO WE KNOW OUR BEAMS?

An overview was given of various key parameters of the beam, how they are measured, and how reliable the measurements are estimated to be. FBCT measurements of bunch-by-bunch intensities over the year were presented, with an estimated uncertainty of 1–2%, given by the discrepancy with the reference measurements of the total intensity by the DBCT. For the transverse emittance, results from wire scanners and the BSRT were shown. The estimated systematic error is around 10–20%. Bunch length measurements were shown from the BQM, the BSRL, and the RF 40 GS/s scope, for which uncertainties arise from making Gaussian fits to non-Gaussian bunches. The most important changes of the measured parameters over the year were given by the change from nominal to BCMS beams, resulting in a significant reduction of the transverse emittance from about 3.5 μm to about 2 μm, and a decrease in bunch length target from 1.25 ns to 1.1 ns. It was also observed that the beams were not round during a large part of the year, which is compatible with the observed luminosity difference between ATLAS and CMS. However, in the last part of the run, more round beams were observed, but the luminosity imbalance was still present. The reason is still to be understood.

Discussion:

E. Shaposhnikova had a remark on the longitudinal profile measurements on slide 13, where she mentioned that what is always measured is the FWHM. She explained that one multiplies this value with a constant in order to obtain the 4σ equivalent to a Gaussian profile. The measured value itself, however, is independent of the distribution i.e., it not being Gaussian does not render it meaningless.

W. Höfle asked referring to slide 7 and the cross calibration between BCTs, how satellite bunches were treated. **E. Bravin** explained that everything was included and that the FBCTs measures these as well.

M. Lamont asked about the beam roundness towards the end of the year, and whether it was clear how these evolved during a fill. **M. Hostettler** replied that this was difficult to assess since there were no independent measurements. He explained they had only about 1-2 scans and that, with this, the comparison becomes very hard due to the lack of reference. **J. Boyd** added that they had done the crossing angle reduction scan and that this revealed that the crossing angle was causing a significant difference, hence, the beams may not have been round. However, he added that this was done with special beams. **W. Kozanecki** agreed that this was puzzling and that he believed that there may be an issue with the scale. He remarked that there were a couple of subtleties but that the emittance estimates by ATLAS and CMS agree very well. There have been very small differences in H and V which, finally, are very hard to interpret.

F. ANTONIOU: CAN WE PREDICT LUMINOSITY?

A luminosity model was presented, including burnoff, IBS, synchrotron radiation, and elastic scattering. All parameters can evolve according to the underlying physics, or some variables can instead be taken from data. The model was used to simulate the emittance along the cycle, where an unexplained blowup was observed in the ramp. The calculated peak luminosity agrees fairly well with measurements, although some discrepancies are observed, especially towards the end of the year. Along the fills, the model overestimates the measured luminosity, unless both the emittance and bunch length are taken from the data instead of the model. An additional emittance blowup of about 0.1 μm/h is observed, on top of what is predicted. Significant losses, not explained by burnoff, are observed in the first few hours in stable beams. The impact on integrated luminosity for each effect is estimated at a few percent for most fills. Finally, losses per bunch in stable beams were presented, and higher losses were observed on bunches with full long-range encounters after the reduction of crossing angle.

Discussion:

E. Bravin asked about quoting peak luminosity, whether this meant the peak value of the average over all bunches? **F. Anoniou** replied that this values is taken bunch-by-bunch.

O. Brüning asked whether the additional emittance blowup was intensity dependent. **F. Anoniou** explained that she had looked at the high intensity fills but still needed to check the low intensity ones in order to make this correlation.

W. Kozanecki asked with respect to the impact of the LHCb polarity whether there was any correlation with the beam loss monitors and whether there was any information in the sensitivity to crossing angles. **Y. Papaphilipou** pointed out that they had made global observations which show clear correlations, however, so far no checks were made in detail to see what is going on close to LHCb. Simulations do show some long-range effects but, more importantly, differing head-on effects. The dynamic aperture can be worse from one case to another. There are ongoing efforts trying to interpret the simulation results. He remarked that optimizing the tune can solve this problem in any case. **T. Pieloni** with respect to this remarked, that in the long-range beam-beam MDs they had also observed a tune shift despite the passive compensation in IP1 and IP5. These tune shifts are comparable to what can be expected from the change of polarity and the corresponding head-on effects in IP8. She pointed out that B. Salvachua had optimized the working point according to the values found in the MD which had brought a significant improvement.

G. Trad: made a remark on the discrepancies between predictions and measurements of, that he would not expect any changes from the machine, in principle. **F. Antoniou** clarified that, in the plots, the dependence is not on the pile-up but on the crossing angle.

X. BUFFAT: LONG-RANGE AND HEAD-ON BEAM-BEAM: WHAT ARE THE LIMITS?

The observations and experience with long-range beam-beam effects during 2016 were recalled. The limitations were probed in MDs, which showed an onset of losses below 8.6 σ. In August, the half crossing angle in physics operation was reduced from 185 μrad to 140 μrad, corresponding to a reduction of normalized beam-beam separation from 10 σ for a 3.75 μm emittance to 9.3 σ for a 2.5 μm emittance. After this reduction, a smaller luminosity lifetime and larger losses were observed, which could be mitigated by a tune optimization. Furthermore, asymmetries were observed in the long-range tune shift in IR1 and IR5, which are yet to be understood. Other tests were carried out to investigate the feasibility of luminosity leveling using the parallel separation, and no detrimental effect was observed on the beam quality. Finally, the limitations from head-on beam-beam were discussed, and it was concluded that this is not a limitation with the present machine and beam parameters. The presence of noise together with head-on beam-beam might, however, fully or partly explain the observed emittance growth in physics that was not predicted by the luminosity model in the previous talk. A further optimization of the ADT might be a possible way to mitigate the issue.

Discussion:

O. Brüning asked whether, during the studies with changing crossing angle, the losses were predominantly halo or core particles. **X. Buffat** replied that this could be seen well when looking at the luminosity data which indicated that losses occur in both the tail and core.

O. Brüning asked in view of HiLumi LHC, how much the presented tests with large beam-beam parameter would be impacted when adding the long-range beam-beam. **X. Buffat** replied that the efforts are currently focused towards understanding the impact of the head-on collisions and that they will move to adding also the long-range interactions only later. He added that when doing leveling, if only one experiment decided to start leveling, he would recommend to go for leveling in both planes right away, even if this meant some extra work.

H. Burkhardt remarked that the head-on beam-beam tune shifts had rather high values in the high-beta run without long-range interactions and that limitations were encountered there. He concluded that losses were then present even in absence of long-range interactions. **X. Buffat** replied that this was difficult to analyze due to the collimator scrapings. **H. Burkhardt** replied that they had data also without scraping so that one could check this in more detail.

M. Lamont asked whether one should say that a separation of 9.3σ is actually pushing the limits. **X. Buffat** explained that this was not the case. One can observe some losses during the first hour only. He pointed out that one does lose some margin in this case, however.

S. Redaelli added that from the global losses there is no worry for the machine and pointed out that one could not speak of a detrimental effect from these losses for the machine. **X. Buffat** explained that indeed one starts to see losses when there are set-up problems but these do disappear once the machine has been optimized.

Y. Papaphilipou pointed out that the strategy of first going with larger crossing angle and moving to low emittances allowed them to gain some freedom in order to first explore margins and optimize setups and then to start tightening these margins stepwise.

F. Zimmermann: asked for a confirmation about the impact of the 9 nm noise at the IP to explain the emittance growth. **X. Buffat** explained that this is true if this noise occurs at the level of the bunch spectrum and reminded that this is indeed in agreement with the expectations from the LHC design.

L. CARVER: INSTABILITIES AND RF HEATING: ARE WE STABLE AND COOL?

The performance in terms of collective effects in 2016 was found to be very good, with beams reaching 1.4 times the LHC design brightness used routinely. Some parameters, such as octupoles, chromaticity and coupling, had to be further adjusted during the year to suppress emittance blowup at various phases in the cycle, and the important role of linear coupling was further explored. The role of Q” as a further knob affecting beam stability has been tested at flat top. Some instabilities were observed, however, some did not significantly impact performance, and the ones that did could be cured. Furthermore, measurements of impedance over the year were shown, as well as tests of beam stability for various settings during the cycle.

The results are well understood except at the end of the squeeze, where more studies are needed to clarify the required octupole current. Extrapolations to 2017 were performed and it was concluded that the collimators could be further tightened without stability issues, e.g. inserting the primary collimators to 5 σ and the secondaries to 6.5 σ. For beam-induced heating, no limitations were encountered in 2016 and it is not expected that further limitations arise in 2017. However, any unknown non-conformities might change this.

Discussion:

O. Brüning asked why we needed LIU if we are already able now to inject beyond HL-LHC brightness bunches. **L. Carver** replied that LIU was required for high brightness bunch trains.

L. METHER: ELECTRON CLOUD IN 2016: CLOUDY OR CLEAR?

The electron cloud effects gave in 2016 the main contribution to the heat load in the arc. This was not limiting the performance, since the limit from the SPS beam dump was more constraining. A weak conditioning was observed, with a total decrease of the heat load by about 20% over the year. The accumulated electron dose on the beam screens in 2016 is estimated to have been four times larger than in 2015, which, based on lab measurements, should suffice to fully suppress the electron cloud effects, which is however not observed. The unexplained difference in observed heat load between sectors stayed similar to 2015. Experimental tests showed that a hybrid injection scheme of 8b4e and BCMS could suppress the heat load significantly, which might be needed in Run 3 and for HL-LHC. For 2017, if BCMS beams are used, it is not expected that the performance will be limited by the heat load. Several open points remain for further studies, such as the difference between sectors, the evolution of scrubbing, the disentangling of heat load contributions from different elements, and further improvements in the simulations.

Discussion:

G. Trad asked whether scrubbing with doublets was planned. **L. Mether** said this question would be addressed in detail in the talk by G. Iadarola later in the workshop.