Summary of the 2-day internal review of LHC performance limitations (linked to transverse collective effects) during run I (CERN, 25-26/09/2013)

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Summary

In this note the 2-day internal review of LHC performance limitations (linked to transverse collective effects) during run I, which took place at CERN on 25-26/09/2013 (https://indico.cern.ch/conferenceDisplay.py?confId=267783), is summarised and the next steps to prepare the machine set-up in 2015 are discussed.

1. Introduction

In this review, only the limitations due to transverse collective effects were discussed. The goal was to review all the instabilities observed until now and check / update (after systematic data analysis) our preliminary conclusions of last year (i.e. end of 2012). The 2012 run was devoted to the LHC exploitation but also to explore the LHC performance limits. The following machine and beam parameters were reached: a maximum beam energy of 4 TeV, a minimum betatron function at the Interaction Point (IP) 1 and 5 ($\beta^*$) of 60 cm, a bunch spacing of 50 ns (the 25 ns bunch spacing has been studied during some Machine Development - MD - sessions), a maximum number of bunches of 1380, a maximum number of particles per bunch of $1.7 \times 10^{11}$ p/b, normalized rms transverse emittances at the start of the fills of $\sim 2.5 \mu$m ($\sim 2.2 \mu$m also reached), which means that the bunch brightness was more than 2 times larger than nominal. This is therefore a great result with a lot of promises for the LHC, however several types of instabilities perturbed the intensity ramp-up and transverse coherent instabilities have been observed more or less at each phase of the cycle over the last three years: at injection, during acceleration at $\sim 2$ TeV with a nominal single bunch (without Landau octupoles in 2010), at flat-top (before the betatron squeeze), at the end of the squeeze, in adjust and in stable beams. All the observed instabilities could be cured except the instability at the end of the squeeze which was still present at the end of the run and which represents therefore a potential worry for future operation at higher energy, higher beam intensity and high beam brightness.

The usual knobs, which are available to damp the transverse coherent instabilities, are:
1) Transverse tunes (and tune splits between the 2 beams).
2) Linear coupling between the transverse planes.
3) Chromaticities (value and sign).
4) (Landau) octupoles (value and sign).
5) Transverse damper, called ADT, (gain and bandwidth: either not fully flat / bunch-by-bunch or flat / bunch-by-bunch).
6) Bunch length and / or longitudinal profile.

The initial recommendations at the beginning of the 2012 run (based on single-beam impedance considerations) were:
1) ~ 1-2 units of chromaticities (Q’), with the request to test the negative sign during some MD sessions.
2) ~ -450 A in the focusing Landau octupoles (as ~ -200 A were used at the end of 2011 and the transverse impedance should have increased by a factor ~ 2.3 due to the tight collimators setting, according to the impedance model).
3) ~ 10 cm rms bunch length (i.e. ~ 1.35 ns at 4 σ) instead of the ~ 9 cm (i.e. 1.2 ns at 4 σ) used in 2011 for beam-induced RF (Radio-Frequency) heating reasons (it should be also better for the single-beam instabilities, even if this original hypothesis was found dubious later taking into account the Q” (introduced by the Landau octupoles) effect, which can reduce the longitudinal tune spread [1].
4) Reduced ADT gain (as much as possible to avoid unnecessary noise).

During the 2012 run it was decided at some point to change the sign of the Landau octupoles as beam-beam and octupoles fought against each other, reducing the available tune spread for Landau damping and a series of instabilities were observed in the horizontal plane during the squeeze and when going in collision in IP1 and 5. Furthermore, new values for the ADT gain, the chromaticities and the Landau octupoles were suggested after a new analytical approach [2]. It was then decided to run with about maximum ADT gain (i.e. with a damping time of ~ 50 turns), high chromaticity (~ 15-20 units) and high focusing Landau octupoles current (~ + 500 A). In parallel, many studies have also been performed with beam-beam: stability diagrams with both octupoles and beam-beam, mode coupling with both impedance and beam-beam, etc.

At the end of last year, the “clear” observations for the instability at the end of the squeeze were the following:
1) It is observed only with 2 beams.
2) It is observed only for β* smaller than a few m.
3) It affects only a few bunches at the very end of bunch trains.
4) Increasing the octupoles current helps.
5) Increasing the chromaticities helps.
6) It is very reproducible and mostly in the vertical plane of Beam1.
7) Once in collision, no instability is observed anymore.

The goal of this review was to check if after detailed analysis these conclusions were still valid and if other “clear” observations / correlations appeared. It was also the occasion to discuss all the other instabilities in order to see if and when they could become really worrying. Nicolas Mounet reviewed the single-beam instabilities and impedance related MDs, Xavier Buffat the instabilities in adjust and in stable beams, Tatiana Pieloni the instability at the end of the squeeze, Elias Métral the cogging (also called “2-beam impedance”) MD, Giovanni Iadarola the electron cloud effects and Kevin Li the instabilities at injection. A kind of global picture was then proposed at the end by Gianluigi Arduini and Elias Métral. Finally, Werner Herr discussed the experiments required in 2015 and the possible next steps, while the concluding remarks were made by Oliver Brüning.

One of the main problems of 2012 was that several things were changed at the same time when the sign of the octupoles was changed. Indeed, we moved from negative to positive
detuning with amplitude (i.e. from negative to positive current in the focusing Landau octupoles) on 07/08/12 (fill #2926) and more or less at the same time the chromaticities were increased and the gain of the ADT in the vertical plane was increased. Let’s call this global change (positive current in the focusing Landau octupoles, high chromaticity of ~ 15-20 units and high damper gain in the vertical plane with ~ 50 turns damping time) the “Middle of the Year Change” (MYC).

2. Single-beam instabilities and impedance related MDs by Nicolas Mounet

The “clear” observations of single-beam instabilities are:
1) Significant particle losses in a timescale of seconds to minutes.
2) Several instabilities without transverse emittance blow-up (because the beam was rapidly saved).
3) Activities on the BBQ (Base-Band tune measurement with direct diode detection) and also on the ADT (but less sensitive).
4) With the ADT on, still some signal observed on the ADT pick-up.
5) Normally, absence of the 1 - 2 qy line (which has been observed in some instabilities, see later) but it was also seen once on 10/10/12 (on the flat-top with the ADT on and after the MYC).
6) With the ADT off and before the MYC, a coupled-bunch pattern was observed and the end of the batches lost more.
7) With the ADT on and before the MYC, no clear coupled-bunch pattern and less clear loss pattern. It seems to be more a single-bunch instability (the higher the brightness the worse).
8) Before the MYC, observation of an almost straight unstable line in the spectrogram (which depicts the evolution of the unstable betatron frequencies vs. time).
9) After the MYC, observation of several lines, sometimes curved towards higher tunes (called “hockey sticks”, see example in Fig. 1).
10) With ADT on and after the MYC, the tails of the batches were more critical than the head.
11) There is a case of an instability with high chromaticity (~ 15-20 units) and a high positive value (500 A) in the focusing Landau octupoles (see Fig. 1).

The remaining issues are:
1) The MDs were done on Beam2 only, whereas the instability at the end of the squeeze was mostly in the vertical plane of Beam1 after the MYC.
2) The impedance model (and in particular the related tune shift) was not studied at high chromaticity, which was used after the MYC.
3) The impedance model is still under development.
4) A better understanding is still needed about coherent modes and stability diagrams in the presence of the ADT. The ADT has been described until now by a simple bunch-constant wake. How close to that model is the real ADT? This is a question about the ADT, not about the beam dynamics, whose main remaining problem is the longitudinal-to-transverse Landau damping.
5) Concerning the “hockey sticks”: is this the combined effect of particle losses (lower intensity implies a positive tune shift in the vertical plane) and positive detuning with amplitude (which implies positive tune shift with the increasing amplitude of oscillations)? If we look at the particle losses, what is the expected tune shift? In the case of the negative focusing Landau octupoles polarity, the detuning with amplitude was negative and compensated the positive detuning with intensity reduction. Are they of the same order of magnitude?

In summary, several features observed are similar to the ones observed with the instability at the end of the squeeze (see later: tails of batches more critical; hockey sticks).
Several features observed are similar to the ones observed during the cogging MD (see later: the 1 - 2 q_y line can be observed; beam could not be saved by octupoles once unstable). Finally, the single-beam intensity stability limit for negative focusing Landau octupole polarity is higher than for the opposite polarity.

Figure 1: MD during which the beam became unstable in the vertical plane of Beam2 when the focusing Landau octupoles current was reduced to 300 A, and it was then very difficult to re-stabilize it afterwards as one can see that with 500 A the beam was still unstable.

3. "Snow-flakes" instabilities and instabilities during adjust by Xavier Buffat

The “clear” observations are:
1) The stability of beams colliding with an offset is critical around 1-2 σ full separation. This was observed both in operation and in dedicated experiments. Furthermore, several models confirmed this observation, with 3 mechanisms which are critical in this area:
   a) A large chromaticity variation is expected for beams with a separation smaller than ~ 2 σ (see Fig. 2).
   b) There is a minimum of beam stability for beams separated by ~ 1.5 σ (see Fig. 3).
   c) The coupling of beam-beam and impedance modes is critical around 1-2 σ (see Fig. 4).
2) Figure 5 shows the evolution of the intensity of Beam1 vs. the number of the fill along the year, where it can be seen that a “golden period” existed just before the Technical Stop (TS) 2 whereas after the Technical Stop 2 it was never possible to recover such good conditions. The “main” change, which occurred 4 fills before the Technical Stop 2, is a change of the filling pattern on 15/06 (between the fill #2733 and the fill #2734). In this period, there was also a complete measurement of the chromaticities. A good measurement of the chromaticities was done on fill #2717 and corrections were applied on fill #2718. The horizontal chromaticity was found to be close to zero at the end of the squeeze and it was increased by 2 units. Several fills were then observed without any instability till fill #2731.
3) Before the MYC, instabilities were observed in adjust, in stable beams and during the squeeze:
   a) They were mainly horizontal in adjust.
b) Many of the instabilities caused a beam dump (the Beam Loss Monitor - BLM - threshold in the Interaction Region - IR - 7 was regularly increased).

c) It depended on the filling scheme and on the ADT.

d) The “snowflakes” (instabilities observed in stable beams) disappeared before the MYC.

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**Figure 2**: Chromaticity changes due to beam-beam when going into collision (IP1 only and IP1 & IP5).

**Figure 3**: Stability diagrams for the nominal bunches with both signs of the focusing Landau octupoles and beam-beam, when going into collision.

4) After the MYC, the instability at the end of the squeeze was mainly observed.

5) A possible explanation of the fact that the horizontal plane was more critical before the MYC might come from the chromaticity control (see Fig. 6).

The remaining issues are:
1) As the 3 mechanisms are predicted to be critical in the same range (~ 1-2 σ full separation), it is difficult to disentangle between the 3 mechanisms. Dedicated MDs are needed to try and deduce the dominant effect(s).
2) Is the positive octupole polarity beneficial for this type of instability? The lack of observations of instabilities in similar conditions except for the sign of the octupoles prevents any conclusion on the matter. Dedicated MDs are also needed. For completeness, it is worth mentioning that a test at the end of fill #3143 was done with 260 A in the octupoles and with a

![Figure 4: Mode-coupling in the presence of both impedance and beam-beam.](image)

![Figure 5: Evolution of the intensity of Beam1 vs. the number of the fill during the 2012 run. All the fills with the beam mode adjust have been considered. In blue: BBQ activity during the collision beam process. In red: BBQ activity only during the collision beam process.](image)
chromaticity around 5 units, so with quite different settings from the usual ones. There was no instability when the ADT was on, and an instability appeared only when it was off. It has been described in some detail in an MD note.

3) Did the polarity of LHCb and ALICE spectrometers play a role in the instability? Concerning LHCb, the spectrometer polarity was changed several times during the year, no correlation with the instabilities could be found, while in the case of ALICE, the polarity was changed only once at the MYC, preventing any conclusion.

4) We should try and produce the corresponding plots of Fig. 3 for the different families of bunches having different patterns of beam-beam head-on and long-range and add the corresponding most unstable coherent modes. In the plot it is assumed that we are collapsing only in the separation plane but in fact we are also collapsing the separation in the crossing plane. What happens for realistic evolution of the crossing?

In summary, the situation after the MYC was fine. However, if we come back to the negative polarity for the focusing Landau octupoles, one should have several means to fight against the instabilities (high chromaticity; asynchronous collision process to avoid the minimum of stability in both transverse planes and thus benefit from Landau damping sharing; etc.). We should model this situation for realistic collapse schedules to support this statement.

![Figure 6](image_url)

**Figure 6:** Measured chromaticities vs. fill number (the black dashed line represents the MYC).

### 4. End-of-squeeze instabilities by Tatiana Pieloni

The “clear” observations are:
1) A typical spectrogram is shown in Fig. 7, where several lines spaced by the synchrotron tunes are excited, but it has been recently observed that a similar picture can be obtained with a single beam (see Section 2).
2) More instabilities have been observed after the MYC.
3) Very reproducible at the end of the squeeze after the fill #2980.
4) Clear pattern after the MYC whereas it was less clear before the MYC.
5) Mostly in the vertical plane for Beam1 after the MYC (the plane changed with the MYC).
6) More critical at the end of the batches, which is similar to the single-beam case.
7) Several fills have been tested with some tune splits (between the 2 beams) and the loss pattern changed (at least in some cases, where the unstable bunches moved towards the bunches at the centre, as expected in some models). Do we need some larger tune splits to conclude? What about the sign?

**Figure 7**: Example of spectrogram during an instability at the end of the squeeze.

The remaining issues are:
1) The change from the negative to the positive sign of the focusing Landau octupoles current should provide more tune spread at the end of the squeeze, at least for the nominal bunches (see Fig. 8). In this case, why then the remaining instability was observed at the end of the squeeze? Where are all the coherent modes? What about the PACMAN bunches (next step)?
2) What about the chromaticity variations due to the beam-beam long-range? This still needs to be estimated.

**Figure 8**: Evolution of the stability diagram during the squeeze for both signs of the focusing Landau octupoles (i.e. for the cases before and after the MYC) and for the nominal bunches only.

In summary, the situation was not fine with the positive current in the focusing Landau octupoles after the MYC. If we will come back to the negative current in the focusing Landau
octupoles, one should have a very small tune spread at some point for some time but as one should have been better with the positive sign (for the nominal bunches) and it did not solve the problem, the situation is more involved and not understood yet. The detailed simulation at 4 TeV and 7 TeV needs to be done to see what should be expected at 7 TeV and when. Furthermore, the same thing should be done also for 2011 to compare, as also there a minimum should have been crossed at some point but no instability was observed (it is true that the conditions were exactly the same and for instance the impedance of the collimators was smaller). This should be done for different classes of bunches (i.e. with different long-range and head-on beam-beam patterns) and the coherent tune shifts should be plotted as well. Is this including also the effect of the squeeze in IP2 and 8? As, after the octupole polarity change the unstable bunches were observed to be those at the end of the trains, it might be good to look at those diagrams for these bunches in particular and compare with the stability diagrams for the stable bunches.

5. Cogging (2-beam impedance) MD by Elias Métral

The motivations of the proposal from S. Fartoukh and F. Zimmermann were the following:
1) Much more Landau octupoles current seemed to be needed at the end of the squeeze with 2 beams compared to 1 beam. A coupling effect between the 2 beams was then strongly conjectured.
2) This effect could be driven by the beam-beam long-range interactions and / or by one or several 2-in-1 structures shared by the 2 beams [3], to be identified (such as the Y-chamber, the TCTV in IR8, the TDI in IR2 and IR8 or any other objects or vacuum pipes located between the two D2 recombination dipoles of the LHC experimental insertions), and magnified during the squeeze.
3) In both cases, a tune split between the beams, with the appropriate sign (depending on the sign of the actual incoherent tune spread seen by the unstable beam), shall definitely improve the situation, and, if not, invalidate the assumption of a coupling effect between the 2 beams.
4) In all cases a clear understanding of the situation, or at least an empirical cure, remained mandatory to prepare the Runs II (post-LS1) and III (HL-LHC) of the LHC, when the machine will operate at higher energy, with more impedance (tighter secondary collimators, higher β-functions in the experimental IRs, DS - Dispersion Suppressor - collimator or crab-cavities for the HL-LHC) and with similar or even higher beam brilliance, including the 25 ns beam being targeted by the LIU project (called h9 beam).

The procedure foreseen to identify and possibly localise in the LHC ring any coupling effects between the 2 beams was to displace Beam2 clockwise via the RF (cogging) in order to generate machine configurations where the 2 beams do not see each other in any of the 4 experimental IR’s (called IR0), or meet only in IR1/5, or only in IR2, or only in IR8. In each of these configurations, the aim was to reduce the Landau octupoles currents and / or the linear chromaticity till an instability starts to be visible on the BBQ. Assuming the Landau octupole / Q’ threshold would be found to be the same in the various above configurations, a high-Q transverse mode would be highly suspected. In this case, the procedure would consist in a very slow RF scan (0.25 Hz RF trim applied to Beam2), operating close to the instability threshold in terms of Q’ and Landau octupole current, and trying to observe any periodic behaviour on the life time and / or the BBQ, and then hopefully be able to give a first estimate of the frequency of this mode. The filling scheme consisted of the nominal filling scheme (50ns_1374_1368_0_1262_144bpi12inj) where only the first 150 nominal bunches were kept for both beams, called 50ns_150b_TwoBeamImpMD (the first 6 bunches, which are necessary for transfer line steering, followed by a single SPS injection of 4 PS batches).
The formula to compute the time $\Delta t_{IP2}$ needed to go from the case with 2 bunches colliding in IP1 and 5 only to the case with the 2 bunches colliding in IP2 only (see Fig. 9) is given by (assuming a procedure infinitely fast compared to the time it takes for the beams to move from the initial situation to the final one, which is the case)

$$\Delta t_{IP2} = \frac{1}{4 f_{rev}} \left| \frac{\eta \Delta p}{p} \right|.$$ 

Making the numerical computation, gives $\Delta t_{IP2} \approx 12$ min 30 s, assuming $f_{rev} = 11.245$ kHz, $|\eta| \approx a_p = 3.2 \times 10^{-4}$ and $\Delta p / p = 9.3 \times 10^{-5}$ (corresponding to $\sim 12$ Hz in the RF frequency). An overview of the MD is shown in Fig. 10.

**Figure 9**: (Left) layout of the LHC and (right) principle of the cogging process.

The machine conditions were therefore the following: 78 bunches ($= 6 + 2 \times 36$) / beam, 1.5E11 p/b, 1.5 – 2 µm (from SPS), 4 TeV, end of squeeze (0.6 / 3 / 0.6 / 3 m), all settings nominal (~ 500 A in the focusing Landau octupoles, $Q' \sim 16$, ~ maximum damper gain). A trim of – 12 Hz was done for Beam2 and therefore the later moved clockwise with respect to Beam1. It is worth mentioning that the best way to see that is to look at the “LHC Configuration” on the LHC vistar and in particular at the BPTX, which measures the difference in the arrival time of Beam1 and Beam2 at IP1 for ATLAS and which is calibrated to be 0 when Beam1 and Beam2 are in the bucket #1. If we want to move by a quarter of a turn, we should therefore reach a value of $\sim 87$ µs / 4, i.e. $\sim 22000$ ns. Note that the dephasing between the 2 beams cannot be seen on the large screens of the LHC island in the CERN Control Centre.

The “clear” observations are:
1) It was not possible to re-stabilize Beam2 (after the first instability in the IR15 configuration when reducing the focusing Landau octupoles current) when coming back in the octupoles current. This is similar to a single-beam observation discussed above.
2) However, the BBQ signals disappeared when the cogging started (see Fig. 10). A detailed analysis showed that this happened really at the beginning of the process and not when the two beams did not see each others. In fact, due to the high chromaticity, the change of energy (to do the cogging process) made a tune split of $\sim 1.5 \times 10^3$, which might have been enough to decouple the 2 beams. It seems indeed to show that a coupling between the 2 beams existed (at least in this unstable situation). The tune split had the same sign as compared to the one
proposed during the tests where the vertical tune of Beam2 was increased by $+3 \times 10^{-3}$ during part of the squeeze. The tune split during the cogging test was in both planes, with the same sign and approximately the same amplitude, while during the tune split tests the tune split was applied only in the vertical plane.

**Overview**

**Figure 10**: Overview of the cogging MD with in particular the first instability, which appeared when the Landau octupoles current was reduced and which could not be stabilized again by coming back in the octupoles current. The BBQ activity disappeared when the cogging process started (at the very beginning of it).

The remaining issues are:
1) There was a large spread in initial transverse beam emittances in Beam1 (larger than a factor of 2).
2) No measurements of transverse emittances were available for Beam2.
3) A large transverse emittance blow-up was already observed on Beam1 after the first instability.
4) Why didn’t we see the effect of the tune split at ~ 09:30 (see Fig. 10)?
5) Why didn’t we cross the compensation point between beam-beam long-range and octupoles when we scanned the negative values of the focusing Landau octupoles current? Is it the effect of the transverse emittances?

6. **Electron cloud effects by Giovanni Iadarola**

The “clear” observations are:
1) There is an e-cloud build-up in the inner triplets at injection and 4 TeV, but it should be stressed that it appears only with two beams with 50 ns bunch spacing (see Fig. 11). The MDs with one beam demonstrate that the heat load with only one beam is fully compatible with the
impedance-induced RF heating alone. No big change is observed during the ramp and the squeeze.

2) Simulations show that:
   - The average e-cloud density on the beam is strongly dependent on the beam position with respect to the e- stripes.
   - The field at the beam location is determined by the whole distribution (not only by the local density).
   - In the triplets, the peak of e-cloud density (integrated along the triplets) is in the middle of the batch and not at the end.

5) From full HEADTAIL simulations for the SPS (including the realistic distribution of the electron cloud and the pinch during the bunch passage, and therefore with all nonlinearities), the e-cloud instability scales with the square root of the betatron function.

This cannot explain the instability at the end of the squeeze for the time being.

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**Figure 11:** Average e- density on Beam1 along the IR1 triplet for different secondary emission yields.

7. **Injection instabilities by Kevin Li**

The “clear” observations are:

1) In 2011, with the 50 ns bunch spacing beam:
   a) During the scrubbing run, the instabilities observed could be compatible with e-cloud and / or the impedance, some bunches having brightnesses larger by 50%.
   b) The b3 decay was not compensated yet. This was done after our request to control the chromaticity within ~ 2 units. FiDeL [4] was on in April but it was maybe not fully operational at that time.
   c) On 09/06/11 it was decided to introduce some current in the Landau octupoles current to try and suppress some instabilities which were observed from time to time on some batches, during or right after the injection. The focusing Landau octupoles
current was then set to -6.5 A in (fill #1865). The situation was fine afterwards (for both 2011 and 2012) and the current was never optimized as there was no real need.

d) The possible explanation is that it was mainly due to e-cloud.

2) In 2012, with the 50 ns bunch spacing beam, changing the sign of the focusing Landau octupoles current (i.e. going from –6.5 A to +6.5 A) worked well and here again the situation has not been optimized as there was no real need.

3) In 2012, with the 25 ns bunch spacing beam:
   a) The first problem occurred on 01/10/2012 and then during the scrubbing run.
   b) The instability has been cured by changing the current in the focusing Landau octupoles to -13 A.
   c) All hints again towards e-cloud instabilities.
   d) Reminder: One might have hysteresis issues at low octupoles current, and we should therefore be very careful.

   The remaining issues are:
   1) The current in the Landau octupoles should be optimized at injection to try and avoid possible issues with the dynamic aperture.
   2) What about the sign of the Landau octupoles current? It is worth reminding that the amplitude detuning is
   a) Negative when the sign of the focusing Landau octupoles current is negative.
   b) Positive for space charge.
   c) Positive for the beam-beam head-on.
   d) Negative for the e-cloud.

8. Global summary?

   The instability at the end of the squeeze (which remained at the end of run I) is not understood yet and it remains therefore a potential worry for future operation at higher energy and higher beam intensity and brightness. A. Burov developed a nice model to try and explain this instability with a 3-beam model [5] but at the moment it is not possible to conclude about the consistency with the observations. The e-cloud density in the triplet has been estimated (see Section 6) and all the information should be now available to refine this model. Furthermore,

   New interesting results have been revealed during this review:
   1) Several features observed with 1 beam are similar to the ones observed with the instability at the end of the squeeze (tails of batches more critical; hockey sticks).
   2) Several features observed with 1 beam are similar to the ones observed during the cogging MD (the 1 – 2 qy line can be observed also with only 1 beam; the beam could not be saved by increasing the Landau octupoles current to its initial value). Only two cases with 1 beam showed high thresholds for the Landau octupoles, as it was presented during the review. This cannot be considered as reproducible, and very likely it should not have the same origin as the one leading to the very reproducible instability at the end of the squeeze.
   3) If we assume that both tune split and cogging (giving also a tune split) studies had some effects (as it seems to), it would mean that the 2 beams had some coupling. Some models indicated that beam-beam should not be a problem in the presence of the ADT (assuming a perfect model for the transverse damper). What happens if the ADT is not behaving as we think? Several effects can be looked at: (i) the limited resolution of the ADT; the FIR (Finite Impulse Response) filter to adjust the damping limits the range of tunes that can be damped; the effect of internal bunch motion, which is currently being investigating and the frequency response (single-bunch, bunch-by-bunch, or frequency dependent gain). Simulations should
study the impact of these different effects. Furthermore, are the models including the information on the real configuration in IP1 and 5 as well as in IP2 and 8?

4) We said that a tune split is not needed, as the ADT should do the work. What if the ADT is not behaving as we think? A tune split is certainly much simpler. More simulations should be performed to make some predictions about the required tune split in both planes. The next question would then be: can we run with large (how large?) tune splits from the point of view of incoherent effects?

9. Next steps for understanding and preparation of the machine set-up in 2015

As concerns the simulations, which should be done in order to prepare the beam studies and machine set-up for 2015, the stability diagrams and coherent tune shifts for the different families (i.e. with different beam-beam head-on and long-range patterns) of bunches for the 2011 ($\beta^* = 1$ m), 2012 and 2015 configurations should be computed:

1) The squeeze process for both octupole polarities and different values of the chromaticity for high and low damper gain should be implemented. This should include the squeeze in IP1/2/5/8.

2) The collision beam process assuming realistic functions for crossing and separation bumps collapse used in 2012 for both polarities of the octupoles and different values of the chromaticity for high and low damper gain should be used. This should include the collision in IP1/2/5/8. Do we see any issue with the crossing angle in ALICE from the simulations? We should include the variation of chromaticity as a function of the separation.

3) The effect of the tune split should be simulated. Which sign and amplitude? Both planes or single plane? Can we run the LHC with a “large” tune split from the point of view of incoherent effects?

4) What is the effect of a staggered collision process? In which order: 1/5/8/2 (8 and 2 might still be separated)?

Remaining data analysis:

1) What was the change after Technical Stop 2? Did we see an effect of the change of filling pattern just before the Technical Stop 2?

2) Did we analyse the ADT data that we saved at the end of the squeeze? The instability was there all the time and we should be able to see it at least in some fills because we saved very often the ADT data during the squeeze.

3) Detailed comparison between the instabilities with and without tune split using all the available information (BBQ, ADT data, Fast Beam Current Transformer, transverse emittances).

4) Do we have fills where at the end of the collision process we ended-up with a separation of 1-2 $\sigma$ in IP1/5 after the Landau octupole polarity change? Was the beam unstable? We should look at the ratio of the luminosity with respect to the peak for all the fills after the polarity change to find possible candidates.

Requirements in instrumentation:

1) These have been discussed during the review for the instrumentation required for instability measurements [6]. Do we have additional requests?

2) Faster scan of the transverse emittance with the BSRT (Transverse synchrotron light monitors)?

3) BGI (Beam Gas Interaction) monitor?

4) Schottky?

5) BTF?
Requirements in logging: the information from the Fast Beam Current Transformer is vital for the understanding of the instabilities. We should therefore try and avoid the compression of the data.

Required input:
1) What should be the values of the chromaticities?
2) Expected ADT strength and performance in general?
3) We can assume a maximum current of 590 A for the Landau octupoles current. Will this be enough? Which sign should be used?

Measurement to be performed in 2015 (MDs):
1) Measurements with single beams:
   a) Single bunches? What will we measure? In which conditions? Aim? Impedance characterization? How many shifts?
   b) Many bunches? 50 ns? What will we measure? Which machine conditions? Aim? How many shifts?
2) Measurements with both beams and many bunches:
   a) By which measurements should we start first? Should we try and have first the results of the simulations discussed above to guide us in the definition of the MDs?
   b) Effect of the Landau octupoles on the instability at injection before and after the scrubbing run?

Mitigation measures:
1) Dependence of the chromaticity on beam separation for different $\beta^*$ in IP1/2/5/8 and on crossing angles? Can we reduce this change by keeping $\beta^*$ in IP2/8 higher? Is the spurious dispersion playing a role in the difference between Beam1 and Beam2?
2) Should we use additional octupoles (RCO, RCOX)? Can this help?
3) Can we retract some collimators to reduce the impedance of the machine?
4) For the long term, a new idea has been proposed by A. Grudiev to try and add more Landau damping in the transverse planes thanks to an RF quadrupole (if needed) [7]. These studies are ongoing.

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References


