LHC Studies Working Group
Notes from the meeting held on 26 October 2012

The meeting was dedicated to a review of the studies performed in MD block #3 and a first look at MD block no 4. Agenda and slides can be found at: https://indico.cern.ch/conferenceDisplay.py?confId=213404

1. Introduction (F. Zimmermann)

F. Zimmermann opened the meeting. He pointed out that so far only 5 MD notes had been published for 2012. P. Baudrenghien pointed out that the RF group had published 3 of these 5 notes and he asked if as a reward some extra time could be granted. S. Redaelli commented that there were 3 notes from collimation ready to be published.

2. High beta* development (R. Tomas)

R. Tomas reviewed the high beta* MD. K modulation had been applied at beta*=1 km. The quality of the data will allow determining the beta* with a resolution of about 5%.

3. Optics during ramp and IR nonlinearities (R. Tomas)

R. Tomas first reviewed the optics during ramp. He then reported on the IR checks. Skew sextupole polarity checks had been done. Sign conventions of skew sextupoles and normal sextupoles agree with MAD (for the arcs the skew sextupoles do not have the same sign convention). Between 450 GeV and 1.3 TeV the largest optics changes are observed. Afterwards the beta beating stays rather constant. Similar results are found for both beams. Chromatic coupling was measured versus beta*. For 40 cm beta* the chromatic coupling increased. The coupling was 0.0024 for dp/p=0.001 at beta*=0.4 m. F. Zimmermann asked for the size of the coupling at dp/p=0.000. R. Tomas replied typically a few 1e-4, but could be as small as desired. Experimental values are much smaller than expected from the model. Correction with the skew sextupoles was computed and compared with the model. Both model and experimental corrections work, but the model includes other requirements, which explains the larger KSS strengths. For beam 1 IR a3 and b3 correction in IR1 look nice, but b4 correction does not look so nice. A 5 mm misalignment in RCOX.L1 could explain the observation. The b3 component is affected by the b4 correction. Beam 2 IR1 corrections show no problem. The IR5 results are not yet understood. The first measurement of amplitude detuning with ac dipoles were performed. IR1 and 5 corrections are included. An example for beam 2 showed a natural amplitude detuning about 100 A of MO. F. Zimmermann asked about the source of the amplitude detuning. R. Tomas responded that it was due to a combination of sources (e.g. some of the arc octupole circuits not working, some from the IR nonlienarities). A. Burov asked about the sign of the detuning. R. Tomas replied that this could be checked. W. Hofle asked about the nominal sigma and the amplitude. R. Tomas explained that a nominal sigma is 3.75 micron and the amplitude refers to the steady-state oscillation. The beam 1 H amplitude detuning is about a factor 3 weaker than for beam 2. In summary, IR5 needs further studies. The natural detuning is not negligible and different between the two beams. N. Mounet requested measurements at the flat top. This could be combined with MQY calibration MD. The total time needed would be 4 or 5 hours. A. Burov commented that the H detuning is focusing for both beams.

4. Longitudinal impedance and phase module calibration (J. Esteban Muller)

J. Esteban Muller recalled that in the previous MD impedance measurements gave different results for the two beams, and also between different modules. Calibration and measurements at 4 TeV had been needed. The MD featured 2 fills with 8 bunches at 450 GeV and 1 fill with 3 bunches at 4 TeV. Various measurements were done, e.g. the total voltage was changed to disentangle the impedance contribution and the phase-module effect. There is a dependence of the bunch phase reading on the bunch length and perhaps also on the bunch intensity. For longitudinal impedance only 3 bunches were injected due to limited remaining time. The bunch length target for the longitudinal emittance blow-up was set to 0.8 ns. Loss of Landau damping was observed when the phase loop was switched off for the bunch with 1e11 p, stabilizing at 0.95 ns length. E. Metral asked about the longitudinal emittance. P. Baudrenghien replied that 1.2 ns corresponds to 2.5 eVs. From the peak detected Schottky monitor, the quadrupole line frequency does not shift with bunch length within the resolution of the measurement. Transverse tune shifts were also measured, but still need to be analyzed. The systematic error of the phase measurement is 0.2 degrees, but if the impedance model is correct a precision of 0.1 degree or less is required. Relative phase measurements (e.g. for TDI impedance measurements) and large phase shifts (e.g. due to electron cloud related loss of energy) are correct. Replying to a question of F. Zimmermann, E. Shaposhnikova explained the problem of the peak-detected Schottky measurement; the plan was to use beam transfer function techniques instead.

5. Cogging for 2-beam impedance MD (P. Baudrenghien)

P. Baudrenghien described the rephasing tests during the two-beam impedance MD. The motivation and idea for the 2-beam impedance MD were reviewed. Beam 2 was to be rephased clockwise in multiple of one quarter of a turn. However, the MD suffered from little availability (e.g. time was needed for transfer line steering and a sector tripped), so only the rephsasing had been tested. A dp/p=1e-4 corresponds to a 12 Hz change in RF frequency. The acceptable momentum offset limits the maximum frequency change. Additionally, acceleration and deceleration need to be adiabatic. A first test at injection was done with 6+6+32 bunches in each ring. A time of 12.5 minutes was needed to move by a quarter turn. No losses, no lifetime degradation and no increase in abort gap population were observed. Rephasing at 4 TeV showed a very short drop of lifetime at the start of the frequency ramp. At injection, the bucket height is 8e-4 dp/p, while at the top energy it is only 4e-4. A decision was taken to reduce the rate of frequency change to 1 Hz/s to be adiabatic at 4 TeV. W. Hofle asked whether the derivative of the frequency change should also be made smooth. P. Baudrenghien explained that this effect was negligible. S. Redaelli asked why the particles were lost so quickly in IR7. P. Baudrenghien replied that this could be due to the motion of the bucket which pushes the particle energy upwards. P. Baudrenghien stressed that his team was ready for the next 2-beam impedance MD. It would take 2 second to move 1 beam by 1 RF period. E. Shaposhnikova pointed out that when the bucket is moved one loses the particles which had been coasting before.

6. Voltage modulation over one turn (T. Mastoridis)

T. Mastoridis summarized the cavity phase modulation MD. At present the LLRF is setup so to have less than 1 degree of phase modulation under nominal conditions. Eventually at higher intensity the klystron saturation level would be reached with this setup. The solution is to accept cavity phase modulation by the beam (beam loading). This should not shift the collision point in IPs 1 and 5. The first test in MD#2 had delivered promising results, but the use of Matlab code instead of firmware calculations had made the algorithm very slow (or unstable when pushing the gain). For the MD#3 study new firmware had been developed (5-6 orders of magnitude faster). Unfortunately, due to a problem with the beam dump system for ring 1, only beam 2 was available, and for 1.5 hours only. The algorithm was tested with 150 and 654 bunches at injection. The cavity phase modulation reached approximately a quarter of the theoretically estimated value of 35 degrees for the half-full ring case. Reduced gain settings were used for observation purposes. Settings of the algorithms will be optimized based on the MD results and an operational version will be deployed soon. Replying to a question of F. Zimmermann, T. Mastoridis explained that the time had been too short for the algorithm to converge to the final value, which explained the missing factor 4.

7. Emittance preservation (M. Kuhn)

M. Kuhn reported on the emittance preservation MD: the goals were to check the effects of the 50 Hz lines, the ADT gain, the wire-scanner PMT saturation, compare with experiment measurements (e.g. ATLAS and CMS luminous region data, LHCb SMOG). For now wire-scanner data were shown, averaged over 6 bunches of a batch, measuring beam sizes versus time at injection. A clear effect was observed in the vertical plane although the tune was not moved there. An unexpected beam dump occurred during the first ramp due to wire scan losses. The wire scan interlock threshold was reduced to 3.6e12 p and also the beam intensity (less intensity per bunch and one less 6-bunch train). J. Wenninger commented that wire scans at the same intensity had not dumped the beam previously. M. Sapinski added that the effect was still not understood. In previous experiments it was detected that the beam size in all planes shrinks except for B2H, while in this MD the behavior was reversed. The lowest emittance was seen for bunches with high ADT gain. Best wire-scanner settings are being investigated. W. Hofle explained that the gain had been changed batch by batch before the start of the ramp. S. Redaelli asked how the emittance for the sacrificial batch could have gone down at the start of collision for about 30 minutes. Only the core emittances were shown. Emittances from ATLAS and CMS are 10-20% larger than from wire scan. Wire scanner data were also compared with SMOG. Qualitatively the results are consistent. W. Hofle remarked that when repeating this study one should separate ramp and collision. S. Redaelli remarked that the BSRT beam 1 should be reliable at 4 TeV at least.

8. Collimation hierarchy MD (G. Valentino)

G. Valentino recalled the planned studies: loss maps during the ramp, hierarchy limits at 4 TeV, speed of recovery from hierarchy breakdown, and impedance studies. In less than 4 hours more than half of the studies were done. S. Redaelli remarked that the collimator impedance measurement is important and including set up would need 5-6 h. N. Mounet commented that the impedance measurement itself does not take more than 1 h. Loss maps were taken during the ramp. The 7 TeV nominal settings at 4 TeV showed poor hierarchy. Collimators were realigned using automatic alignment tool. G. Papotti asked about results from loss maps on the ramp. S. Redaelli answered that at first sight they looked good, but expressed concern about the resolution for losses in the arcs and pointed out that it would be useful to have more precise settings for the ADT. Replying to J. Wenninger, S. Redaelli also commented on the prospect of going to very tight settings: a 1 sigma retraction between primaries and secondaries shows not so good hierarchy, but then the question is how quickly the hierarchy can be recovered. In this perspective, the realignment could be redone every 3 months instead of a year, for example, allowing gaining some margin. J. Wenninger remarked that setting up with 50-ns beam would remove systematic errors due to the BPM calibration. S. Redaelli remarked that a test with one 50-ns beam would be of interest. W. Hofle added that the ADT blow up is designed for acting on the full physics beam. Replying to E. Metral, G. Valentino confirmed that 40 collimators had been aligned in 50 minutes (for the nominal settings in mm) and added that starting from relaxed settings a full setup would take 5 hours (i.e. like at startup after a shutdown).

9. UFO time scale quench test preparation (M. Sapinski)

M. Sapinski recalled that the ADT is used to create very fast losses on the timescale of UFOs, but quenches are not allowed in these tests. ADT losses in MD#2 were still a factor 5-10 too slow. Ideas to kick the beam first with the MKQ were not successful in getting fast enough losses. In the end, the amplitude of the ADT was increased by increasing the window length for gating (with respect to the 50-ns settings, possible as single bunches were in the machine), and this achieved fast enough losses that did not require the MKQ. W. Hofle commented that the combination of the ADT and orbit bumps had not been explored in the experiment, M. Sapinski added that all collimators but one jaw had been retracted, creating a very clear aperture limitation at that location.

10. Injection studies (L. Drosdal)

L. Drosdal reported that LHC injection losses and profiles with different SPS scarping could only be done in H plane. The TCDQ impedance measurement was done and the data analysis is ongoing. Matching monitor tests were not successful, also because the matching monitor could not be commissioned during the BI MD at the beginning of the MD block. The automatic TCDI set up was yet again not tested.
Beam scans were performed in the SPS. Test was done 3 intermediate bunches (50 ns). With scraping in the SPS the beam profile in the LHC is much cleaner and more Gaussian. The emittance was reduced when the scraper was further in. Injection losses were measured as a function of SPS scraping. Scraping should be set further in than the TCDI position and before cutting into the beam core. SPS scraping could not be performed in the vertical plane. To a first look the scraper seemed to get stuck, but later investigations indicated that it had simply reached the end of its possible movements (could have scraped further by applying an orbit bump at the scraper location). The TCDI setup is the next highest priority for the injection studies.

11. Single-beam instability and TCDQ impedance (N. Mounet)

N. Mounet showed the results of the TCDQ impedance MD. Tune shift was measured versus TCDQ movement. The measured tune change is of order 1e-4 in agreement with the model. The beam became unstable when moving too close.
The second study was the single-beam instability with the new octupole polarity. There were two fills. The first fill allowed both flat-top and squeeze measurements. With the squeeze one needed more than 510 A to stabilize the beam. At the flat top 300-208 A stabilized. In fill 3146 with similar conditions the threshold was 6 times smaller. This seems to point to the non-reproducibility of these studies. Possible explanations are longitudinal or transverse tails. An example instability image was presented. A. Burov highlighted that volatility is everywhere, and probably it is not possible to find a threshold function matching all the data. G. Arduini commented that on the other hand the timing of the instability is very reproducible.

12. Beam instrumentation (F. Roncarolo, E. Calvo)

F. Roncarolo reported that the aggressive BI plan was only partially completed, and BGI and matching monitor developments were skipped altogether. Additionally, only after the MD a fault in the BSRT focusing system was discovered. It was highlighted that for the BSRT studies the requested small bunches from the SPS were not obtained. E. Calvo commented that only one beam was available for the BPM interlock studies. The MD objective was to investigate the cause of the problem of the p-Pb MD with the IR6 interlocked BPMs. Reflections were confirmed for the two BPMSB type monitors of B2, but not seen in the BPMSA type. They started to disappear when the fat pilot was scraped down to 1.4e10. Replying to a question by G. Papotti, E. Calvo elaborated that the only proton beam had caused the beam dumps (while the other ring was not tested for the same problem). BPMSB channels acquired 3 bunches when there were only 2 present. It could be inferred that the reflection is attenuated by about 27 dB and was delayed by 356 ns with respect to the main signal. Many glitches were seen on the bunch counter, which still need to be investigated. The beam position reading diverges before reaching the interlock level. Solutions for proton-ion run are that one can modify/remove the attenuators or eliminate reflection if its source is found. W. Hofle suggested to try and deduce the location of the fault by the 356 ns delay. F. Roncarolo remarked that for the BSRT a series of investigations was proposed for MD4, mostly to study the issue of heating.

13. Collide & squeeze (J. Wenninger)

J. Wenninger recalled the aim of the MD is to collide all along the squeeze to gain head-on beam-beam tune spread for stabilizing the beam. For practical reasons, beams were put into collisions at 9 m (after the Q change). The MD ended a bit early as there was not enough time for a second cycle. The clear luminosity dips indicate that the orbit had moved more than hoped for at the IPs. Around 3 m it was not easy to join up the references with the optimized orbit obtained in the first MD iterations. Luminosity evolution versus beta* was shown, and it was in agreement with the expected values taking into account a 20% beta-beating. The initial half separation was shown for the various steps, it was quite good between 2 m and 0.9 m. The shape of many luminosity knob trims look similar for MD#3 and the beta* leveling study from 3 m performed in MD#2, except for an overall offset. Separation ran away below 1 m, mostly in CMS. This is not good enough for the long-term stability. The earlier study showed a good reproducibility over 2 weeks. Data was acquired to compare the IP offset from relative positions inferred from IR1 BPMWF BPMs. For making this the operational procedure, more time would need to be invested in the orbit setup during commissioning: e.g. 2-3 extra orbit cleaning cycles at the startup would be needed. Possibly the settings could be played one more time to re-check the long term reproducibility. It would be even more interesting to operating one week for physics in this configuration.

14. MD#4 schedule (F. Zimmermann)

F. Zimmermann presented the updated LHC schedule for the rest of 2012 and a preliminary draft MD#4 schedule. The scrubbing run is postponed towards the end of the 2012 proton run and followed by the 25 ns physics run and MD studies (week 49-50). MD#4 is planned for week 48. Elena Shaposhnikova commented that a test of bunch flattening due to RF phase modulation was an LMC action of high priority.

15. Impedance localization at injection (N. Biancacci)

N. Biancacci first described the motivation of the study, which aims at localizing transverse impedance sources. The method is based on measuring the current dependent phase advance: 8 bunches of different intensity would be excited with the AC dipole. The TDI would be moved to see its impedance effect. B. Salvant remarked that one could also move the secondary collimators. J. Wenninger commented that 15 minutes was optimistic for injecting the different intensities. F. Zimmermann suggested alternatively injecting a single intense bunch and re-using this bunch after scraping. E. Calvo pointed out a potential problem with different bunch intensities. B. Salvant remarked that the resolution can be adjusted for each bunch excitation.

16. Beam-beam studies (X. Buffat, S. White)

X. Buffat described the MD on beam-beam with noise. At injection, 9 bunches per beam with different collision schedules are used. White noise will be injected at various magnitudes, and the ADT gain increased. W. Hofle remarked that IBS and power converter ripple were larger at injection than at the flat top. On the other hand the damper could be stronger at injection.
S. White presented the plan for an MD on beam-beam & impedance. Simulation results were shown which indicate mode coupling. The same phenomenon was simulated for the long-range effect. Stable and unstable regions can be defined as a function of ADT and octupole strength. F. Zimmermann commented that the planned studies were too long to be fit into a 10h block and some prioritization should be devised.

Date for next meeting to be decided; invitations and agenda will be sent in due time.

Frank Zimmermann

List of participants

ARDUINI	Gianluigi	BE-ABP-LIS
BAUDRENGHIEN	Philippe	BE-RF-FB
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Excused: M. Giovannozzi, M. Zerlauth.