Status of the machine studies

LIS, 20/08/07

- General comment (from MD users):

- Setting-up time usually much longer than expected which reduces the effective MD time.

- Until the new engineers in the SPS are fully trained one old SPS expert should be available for difficult operations like a new cycle/new coast, this would also speed-up the training of the new ones.

1) Optics study at transition in the PS (S. Gilardoni, 18/06)

- Measured: orbit during transition, dispersion measurements, transition crossing without triplets, without all gamma jump quadrupoles and without PFWs.
- Next steps: After the data analysis a second iteration may be required also to check behaviour of the radial loop. Missing from the wish list not yet measured the optics with triplets powered with different currents.

2) Study of the scaling of the e-cloud instability threshold with energy (G. Rumolo, 08-26-28/06)

- FastBCT, WSs, LHC-BPM, Q-meter and HT-monitor used and all worked correctly.
- We observed a beam vertical instability both at 26 and 37 GeV/c. At both energies there seems to be a well defined threshold in vertical chromaticity (as value needed to stabilize the beam) and the instability appears both with high and low current (nominal LHC intensity and half this value). The intra-batch pattern of the Delta_y signal, as seen from the LHC-BPM, suggests clearly a coupled-bunch instability at 26 GeV. This is less evident at 37 GeV/c. However, in both cases, the instability affects the tail of the batch. Measurements carried out before and after the scrubbing run do not exhibit significant differences.
- Next step: Re-do the measurements using the intermediate plateau at 55 GeV/c and 4 batches of LHC beam. The instability observed during these tests can be arguably ascribed to electron cloud. With 4 batches we should encounter the e-cloud single bunch instability.

3) SFTPRO trajectory correction in TT2-T110 by using the ERDs DFA242 and DFA254 (A. Franchi, 28-29/06)

- An algorithm and a routine for the computation of the best corrector setting was successfully tested. This MD followed up and completed a similar MD carried out on May 23&25, 2007. Some discrepancies between the computed setting and the one found by trials and errors in CCC do not occur anymore.
- Next step: Implement both the algorithm and the routine in the CCC.

4) Optics measurements of AD beam in TT2 with QKE58 off (E. Benedetto)

- 2nd (23/06)
  - The MRP at extraction was not correct (discovered on 27/06/07).
  - We always need somebody in the Central Building to adjust manually (in local) the beating for the phase synchronization.
  - 1) check if there is a variation of the beam position, when varying the beam intensity from Booster (from 500e10 to 1500e10 (QKE58 on) -> Yes, there is! 2) measurements with QKE58 off, low intensity from the Booster (500e10) -> found almost the same horizontal Dispersion (~3m) as with QKE58 on, which sounds strange!
  - Next step: Check if there are losses at extraction, which can explain the results.
- 3rd (28/06)
  - The MRP at extraction is not correct: the orbit has a negative offset, therefore one would need a larger bump to extract the beam, but it is already close to the maximum value. For the moment we took measurements anyway by adjusting the KFA71.
  - We always need somebody in the Central Building to adjust manually (in local) the beating for the phase synchronization.
  - Measurements with QKE58 off, very low intensity from the Booster (230e10) and KFA71 adjusted to reduce as much as possible the losses in 16 -> The horizontal Dispersion is 3.37m, so it is a bit larger than what found in the previous measurements, where we had losses and beam scraping before extraction. It goes in the right direction toward what expected (for the LHC beam we had D_h~4m with QKE58 off)).
  - Next steps: 1) Adjust the extraction condition (-> Thu 5/07/07) and re-do the measurements with QKE58 off. 2) TT2-FTA matching for QKE58 off.

5) Non-linear chromaticity measurement in the SPS (LHC cycle) (R. Thomas, 05/07)

- 1. PLL could not get set-up before the MD. 2. BPMs operated in 1000 turn acquisition mode reported fake data. 3. Large and probably fake spikes in the closed orbit measurement that might have prevented a good orbit correction.
- The non-linear chromaticity has been measured for a good fraction of the LHC cycle. However some segments remain unprobed due to the fact that the MultiQ application was used for the tune measurement and it blowed up the beam size. This forces the rejection of parts of the data. Also an ununderstood jump of the vertical tune is observed at 10s.
- Next steps: Measure again the non-linear chromaticity for the full cycle using the PLL. The PLL should not blow-up the beam size and should make the measurement faster. The possibility of acquiring 1000 turn BPM data for all SPS BPMs should also be investigated.
- New requirements: Set-up PLL and hopefully a working BPM system.

6) System test, calibration and verification of the PLL phase advance. First test on LHC baseline Q' measurements (slow and small dp/p modulation), verification of side-exciter principle for continuous Q' measurement without radial modulation (R. Steinhagen, 11/07)

- Most of the beam diagnostic was not available during the regular and coast cycle to cross-check the beam conditions: BCT (both), fast BCT (both), SPS orbit acquisition (coast), wire scanner, LHC bunch-by-bunch BPMs, BOSC, ... (OP confirmed that the timing was not setup properly for that cycle) -> No verification of PLL emittance blow-up, actual excitation amplitudes, no dp/p measurements possible for Q' cross-calibration. Due to various small issues, we required in total more time to setup the machine than foreseen which also reduced the effective time available for our measurements. PLL phase noise was higher than last year and thus the slow dp/p RF modulation was either too fast (1Hz) or too small to be detected by the PLL. BTF was not reproducible (large systematic phase changes, additional non-tune resonances) within one fill and from one fill to the next -> May have also implications for the AC-dipole operation. Parameters could be changed only from one "virtual coast cycle" to the next -> No slow ramps or on-the-fly Q'/Q"/Q"' possible.
- First order calibration and locking of the PLL has been successful (-> PLL HW/mapping OK). Last year's side-exciter results could be qualitatively reproduced. First limits of the method's sensitivity to non-linear amplitude detuning using sextupoles and octupoles were established and showed that in case of the octupoles, mainly the upper side-exciter phase seems to be affected. The BTF function showed increasingly non-tune (coupling resonances) with increasing beam coasting time.
- Next steps: Setup a slow RF dp/p modulation (together with Thomas B.) with a modulation frequency of ~0.2 Hz and dp/pl_{max} \approx 1e-4. Verify the source of the PLL phase noise and non-reproducibility of the BTF. If possible, we could/will use real-time or direct hardware inputs to the RF and magnet systems in order to be able to make in-cycle changes.

7) Slow controlled longitudinal emittance blow-up (LHC test-bed) (T. Bohl, 11/07)

- Results for slow emittance blow-up with shaped noise and production of flat bunches were good and confirmed the simulations - the test to produce hollow bunches was not completely conclusive, it showed that improved hardware is needed.
- Next steps: Continue studies in future with higher intensities per bunch and more bunches.

8) Optics and Dispersion measurements of LHC beam in TT2-TT10 and 1st turn SPS, with optics 2007 (QKE58 off) (E. Benedetto, 16-17/07)

- Problems with TT2-TT10 couplers and with BTV1024. LHCINDIV, 1 bunch (Booster Ring # 3), I=~9e10, was fine. Locally tuned the 40MHz cavities for low intensity beam and removed the protection in order to be able to vary their frequency in a range of ~2kHz.
- 1) Couplers in TT2-TT10: 1a) we could not make them work with 15ns long bunches (SPS pick-ups are fine); 1b) they didn't work either when we kept the bunch rotation (~4ns) -> prepulse Delay was not set to 2 (was it the only problem?). 2) OTRs: 2a) BTV1024 CCD camera is broken -> E.Bravin will change it during access Wed.18; 2b) filters still need to be changed via Nodal. 2c) doubts on BTV1018 images -> to be checked w. E.Bravin. 3) SEMgrids in TT10 were not acquiring (maybe they where not inside the beam, even if I put them). 4) SEMwires in TT2: the beam is very large. 5) RF issues, it took time to understand that: 5a) when/if switching off the bunch rotation and the 40/80MHz cavities, need to disable PAX.R2VMOD3 only, to keep 10MhZ on and the syncro working; 5b) Need to locally tune the 40MHz cavities for low intensity beam; 5c) Need to remove 40MHz-cavities protection in order to be able to vary their frequency by ~kHz.
- 2nd iteration of measurements with LHC-type beam, but with the new optics 2007 (matching with QKE58 off). We decided to use LHCINDIV (El=0.35eVs), instead of MD4 (old MESPS, El=~0.2eVs), because MD4 is not yet ready in the SPS. The original planning was: 1) Calibration of BPM with LHCINDIV (w/wo bunch rotation) -> NOT DONE; 2) Dispersion measurements wo bunch rotation -> NOT DONE (could not make

Couplers TT2 acquire w. 14ns long bunch); 3) Profile measurements with OTR and SEM -> NOT DONE (btv1024 CCD camera was broken); 4) Dispersion measurements w. bunch rotation (4ns long bunch) -> DONE, BUT couplers in TT2 not working. Preliminary results: horizontal dispersion @ begTT2, measured with BPMs in SPS only!!!! is D_h(sps)=2.8m -> prediction and last year results were D_h=~4m (last year results, using only BPM in the SPS showed D_h(sps)=3.26m)

- Next steps: Do points 1), 2), 3) and re-do point 4) by checking that the Couplers work.
- New requirements: LHVINDIV or MD4(preferred). The 4 points can be done all in one MD or even separately: Point 1) <20min and the help of Jorg; Point 2) 1.5h and check Couplers with 14ns-long bunches!!!; 3) <1h; 4) 1.5h and support from RF, probably it will work without problems if we correctly set the prepulse delay of TT2Couplers.

9) Prototype verification of a delayed MHS clock system for groups of 10 MHz cavities for LHC type beams (M. Schokker, 18/07)

- Longitudinal set-up is due to introduction of extra delay in the excitation path of the 10MHz cavities completely different. This necessitated the set-up of new longitudinal parameters.
- Proven is that, with new hardware which is now in the prototype stage, we can change the phase of each 10Mhz cavity group. This gives more control over various RF gymnastics with in particular the triple splitting for LHC protons and the splitting for LHC ions.
- Next step: Start the series production of the prototype module and commissioning of the system preferably before shutdown.
- New requirements: When the series production is ready we would like to commission the system, meaning that we need a period (4 hours) without LHC (TSTLHC25/TSTLHC75/LHC25/LHC75) or AD beams. In this period we will set-up the AD beam with the new system. After this we need 1 more day without LHC operation to set-up the LHC beams.

10) BBLR (U. Dorda, 24/07)

- Sometimes no beam from PS. tune and orbit measurements often problematic. For some times could not bump beam into one direction.
- Loads of interesting data (chromaticity scan, distance and current scans) at 2 energies.
- Next steps: higher energy, tune scan, chromaticity scan with compensation.
- New requirements: need access before MD in week 34 to rotate wire compensator.

11) SPS: Achieving nominal longitudinal beam parameters (longitudinal emittance blow-up) (T. Bohl, 25/07)

- We did not get the required beam (1-4 batches, nom. intensity, accelerated till end of flat top) and could only start at 0355H with the beam which was available. So we started 5 min before the end of the allocated MD time and had to finish within half an hour.
- Only for a short time the batch could be accelerated to flat top because of MKD out-gassing problems. New software and hardware was tested. Both worked as expected. However, due to unsuitable beam conditions the longitudinal emittance blow-up parameters could not be optimised.
- Next step: MD has to be repeated with suitable beam.
- New requirements: We request 4 hours for the long MD of week 35. Earlier dates are not possible due to absence of key people.

12) Setting-up of the nominal LHC beam in the SPS (G. Arduini, 25-26/07)

- We had "more or less" the required beam conditions: the transverse emittance of the beam at extraction was larger than nominal (see logbook). Informed the PS crew and R. Steerenberg. Although we agreed that we could continue to make measurements with that. For follow-up. There were several trips of the 40 and 80 MHz cavities. There was also a problem with the definition of the cycles (the length of the SPS cycle was specified to be 36 BP: the same of the scrubbing run). It took a bit of time to redefine the sequence. When we tried to get the spare 80 MHz on to study the impact of the bunch length on beam capture Heiko obsered that the bunch length was varying along the batch at PS extraction. This needs to be addressed and parallel MD time has been allocated for next week (Thursday).
- On the basis of the results of the previous MD in week 27 (where Rogelio & others) found a sudden jump in the tunes in the flat-bottom (10 s) we found that there were some problems with the HW compensation trims: a trim was done starting at 10 s. As a result of the fact that the HW compensation trim programme allows to measure and do trims at points which are not multiples of 6 ms there were points at positions which are not multiples of 6 ms. This took most of the morning to be cleaned up. One of the main quadrupole power supplies (QF2) was found to oscillate in the middle of the ramp. The problem was tracked back to a regulation card and was fixed in the afternoon. So the damper set-up could start only at ~17:30 (instead of 14:00). As in the

previous MD we could not accelerate even one batch to the end of the cycle because of the outgassing of MKDV1 (beam dump) kicker. This problem has been mentioned at the APC and it requires follow-up with the kicker people. The setting-up of the damper took longer than expected (~until 23:00) also because we had some stops due to power converters (in total more than one hour). The LHC BPM was not working. The reason is unclear (Lars and Verena are informed) but for the time being the acquisition of this device is not at all reliable. For next MD we are planning to have alternative solutions to analyze the data if we manage ate least to have the front-end working.

- The transverse damper has been commissioned. The losses at low energy were pretty high (10-15 %) and we observed that the beam was getting unstable and losses were observable in particular after the second injection. There was no way to stabilize it with chromaticity. The instability was depending on the batch spacing. Already moving the batch by ~2 microsec was sufficient to eliminate the losses. Measurements with the e-cloud detector revealed that there was a clear correlation between batch spacing and e-cloud signal (no surprise) but also that there was a suden increase in the e-cloud signal approximately 6 s after the first injection and this need to be understood. As mentioned above very little tools were available last week to check why the beam was unstable.
- Next steps: Due to the problems encountered and taking into account the availability of people we have decided to continue the high intensity LHC beam during the MD in week 32. We will run the 270 GeV coast cycle with high intensity to verify the behaviour of the LHC beam at low energy (Wofgang is away then for 3 weeks) and to complete the setting-up of the cycle (tune, chromaticity) instrumentation verification in particular when in coast (according to Lars not all was working in coast last time). Possibly we will try to make some measurements for the energy dependence on the e-cloud instability (if this work we can give up some time later). The last 4 hours of the MD will be dedicated to start the studies for the abort gap cleaning when Wolfgang is still around. The MD on chromaticity measurement has been delayed.

13) MTE, measure of non-linear chromaticity with PFW in 3 currents mode (S. Gilardoni, 07/08)

- All measured whished so far has been taken, analysis pending.
- Next steps: To be repeated once in PFW in 5 currents mode.

14) PS CT loss displacement using the QKE16CT (S. Gilardoni, 01/08)

- Uncorrect magnet polarity.
- Loss pattern not as expected due to wrong polarity of the quadrupoles. During same MD series of measurement for the Gamma transition study has been done also.
- Redo the same with the correct magnet polarity. Recabling to reverse the polarity at the power converter level already requested.

15) Commissioning of PS electron cloud experiment with LHC25 beam during the SPS scrubbing run and data taking with various clearing electrode voltages and magnetic fields (T. Kroyer and E. manher, 12-13/06)

- No direct beam intensity readings available on the afternoon of June 12.
- Commissioning successfully finished, several strong indications for electron cloud effect during the last ~40 ms found: vacuum pressure rise, electron current on the shielded pick-ups, clearing current on the stripline electrode. Furthermore it was found that electron cloud effect can be suppressed by putting an appropriate positive or negative negative voltage on the stripline electrode. In some cases, the electron cloud effect was enhanced with the magnetic field on.
- Next steps: To be discussed. Interesting topics would be the dependence of the electron cloud effect on intensity, filling pattern, bunch length etc.

16) Setting-up of the cycle with acceleration to 55 and 270 GeV/c with LHC beam (G. Arduini, 08-09/08)

- We had frequent bunch length variations of the PS beam. Even after RF expert intervention we measured longer bunches than in the past (> 4ns) instead of ~3.8 ns. It seems also that the Longitudinal feedback is not ON this year. Frequent trips of the 40 MHz cavities are observed (due to marginal tuning - according to Erk Jensen). They have request the implementation of an automatic tuning system (for the moment w/o/ success). Transverse emittances were still larger than nominal at PS extraction (see logbook). The values measured at 5:51 were probably measured with 24 bunches (12 at the beginning and 12 at the end of the batch). Problems with the triple splitting observed in the afernoon (see logbook).
- The plateaus at 55 and 270 GeV/c were not exactly flat (the momentum values are different at the beginning and end of the cycle). We have found that some of the timings (e.g. start time of the first plateau) are not multiple of 6 ms and as a result of that all the points in the second ramp are not at multiples of 6 ms. Probably it would be good to keep the rule of having the points at multiples of 6 ms only at least in the generation phase. We have also seen that whenever we trim the MBI current at the injection plateau the last but one point of the

cycle is not trimmed accordingly and we get a "ramp" just before the injection plateau. The pre-injection ramp-down/up is not present on this cycle and for that reason we have a drift of the magnetic field. We observed also that the orbit acquisition was not working above 20 s. S. Jackson fixed the problem but with a patch that might not work systematically. Problems in the acquisition of the Head Tail Monitor when more bunches are present. IPM-H only partly tested (w/o magnetic field). IPM-V not giving any profile. We discovered that the interval of time between 2 acquisitions for the SEMCLOUD is not exactly what is required, furthermore there is an offset between the time returned and the real time. Giovanni and Rama calibrated it (need to follow this up with Lars and Jörg). Error in the table for the skeleton of the orbit correction (initially it was not possible to correct the orbit at high energy). Fixed by Fabio. Need to be verified. We lost 2 hours due to a compensator problem.

- Tuned the cycle with up to 2x72 bunches. Still larger capture losses than usual (due to longer bunches from the PS?). Tool to tune the injection plateau as a function of the number of batches is now available (Mathematica notebook). Understood the origin of the e-cloud enhancement after 6 s (Elena Shaposhnikova). Due to the uncaptured beam. Probably it was not observed before because the bunches were shorter. Measurement on the V-ECI dependence on energy taken (G. Rumolo).
- New requirements: IPM and HT monitor need to be tested with multi-bunch.

17) PS-SPS longitudinal transfer optimisation (E. Chapochnikova, 13/08)

- All morning was spent for setting-up for high intensity LHC beam in the SPS. Finally, increased transverse damper gain helped to reduce losses on the flat bottom. The PS (H. Damerau and M. Schokker) were able to produce short and long (nominal) bunches as required in our MD.
- Comparison of flat bottom and capture losses for single batch with different intensities and bunch lengths give following results: beam loss now are very close to the 2004 loss for total intensity of 870E10 (maximum intensity in 2004), but they practically double for 10% more intensity injected. For long bunches flat bottom losses are higher and for short bunches capture losses are higher. Short bunches give also less total loss for lower intensity (2004 max).
- Next steps: Probably we need to optimise voltage programme and WP for this very high intensity again. Otherwise we are using what was optimum in 2004 for lower intensity. The same for short bunch option.

18) Hardware commissioning with LHC beam in the SPS (T. Bohl, 13/08)

- Due to delays for the scheduled MD we did not get the time for the h/w commissioning.
- New requirements: The request for 2 hrs with LHC beam is maintained.

19) Measurement of the single long bunch spectra with RF off (E. Chapochnikova, 14/08)

- Measurement of longitudinal emittance was not possible (tomoscope problem).
- We observe a new high frequency resonant peak in bunch spectra which was not there even in 2003, after the installation of 5 MKE kickers. This could be a reason of mw instability and long. emittance blow-up observed at 26 GeV/c flat bottom in measurements with RF on.
- Next steps: After analysis of existing data, these measurements could be repeated for longer bunches or smaller longitudinal emittance. The search for the source of this impedance is also very important.

20) Measurement of the SPS transverse impedance (H. Burkhardt, 27/07-06-07/08)

- Full set of data at 26 and 37 GeV for transverse impedance.

21) Abort gap cleaning tests in the SPS in view of LHC (A. Koschik, 09/08 dedicated – 23/07 and 03/08 parallel)

- Single bunch 1e11 on MD cycle was fine in the second parallel MD, but a bit unstable in the first one. 2. In the dedicated MD we had the LHC type coastable beam (26-55-270GeV) and used 1 batch with 2x12 bunches.
- Problems with BPMs in the SPS: In some sectors the BPM readings are not reliable, hence orbit correction only possible to some extend.
- Aperture/Beam Losses: Contrary to simulation/aperture model the beam losses (after deliberate excitation) occur mostly in LSS3, which is not understood. Had to steer beam close to TIDV to accumulate losses there.
- AbortGapMonitor was not working, hence no measurements done with it.
- Set-up of beam conditions always takes longer than expected.
- Automated logging of wall current data would be very helpful!
- Analysis still in progress:
    - Beam cleaning was shown to be dependent on the excitation programme and the chromaticity setting as expected from simulation.
    - Beam loss rate showed to be in the order of the ones predicted.
    - Beam loss location not very well defined in the SPS.

- Next steps: Thorough analysis and comparison with simulation. Had no time during MD to check with octupoles switched on, hence another parallel MD time slot is desired => Parallel/Parasitic MD time, single bunch.

22) Study of the fast vertical instability at SPS injection with bunches of low longitudinal emittances (B. Salvant, 02/08)

- Data to be analyzed.
- Decoherence time often bigger than 3000 turns, which was very good for these kind of measurements.

23) Measurement of the low frequency part of the SPS longitudinal impedance: preliminary results => Presented already at APC 03/08 (E. Chapochnikova)

- Comparing with the 2006 data, it seems like all methods point to an increase of the slope by ~50%, which translates into an estimated increase of the low frequency impedance by about the same amount instead of the expected ~10% decrease. The quadrupole oscillation frequency does not depend much on the initial bunch length for fixed current. However, the bunch length at 600 ms in the SPS (and therefore the longitudinal emittance) was lower in 2007 and dependent on intensity.
  The changes in the SPS during the 2006/2007 shutdown (G. Arduini and N. Gilbert) do not justify the observed increase of the impedance. More MDs are foreseen this year, including
  - measurements with larger emittances and, possibly, a larger intensity range
  - measurements with long bunches (20 - 25 ns) with RF off
- They were limited in the MD to 6e10 ppb (hard limitation coming from the production of the MESPS beam. Since 2004 the MESPS beam is produced in h=1 in the PSB and larger longitudinal emittances (~0.2 eV.s) are produced, whereas it was done on h=2 in the past).

24) Measurements on the parallel MD cycle "LHC pilot" => In the SPS supercycle during days this week and the next one

- This SC is a possible option for 2008 for the LHC start-up, allowing permanent beam availability for the LHC but in addition continuous FT beam, so all users do profit. Purpose of the current tests is to study and optimise the ramp of the LHC pilot beam and see how much beam and intensity can be sent. We already accelerated 1 bunch until top energy (~1E11 p/b, ~ 1 microm transverse emittances). Next step: Go to 12, then 48 and finally 72.
- The meas. of the radial position along the cycle revealed that the rms radial position drifts from ~ 3 mm to ~ 8 mm between 2000 and 3000 [ms] (~ 2000 ms corresponds to ~ 200 GeV/c) and then come back to ~ 3 mm from 3000 to 4000 [ms]. This problem does not appear on SFTPRO, which led Jorg and Karel to the idea that it could be due to dipolar errors amplified by the 1 / sin [Pi*Q] term. Reminder: the LHC beam uses a low tune Qh =26.13, whereas SFTPRO uses a high tune 26.62 (almost everywhere except at the end for the slow extraction where it is slightly increased to 26.66). The amplification factor for LHC vs. SFTPRO is ~ 2.3. This has been confirmed on the MD LHC cycle by increasing the horizontal tune to 26.40, which reduced significantly the orbit deformation. Should the alignment of the SPS magnets be done with LHC beam which is more sensitive? Which energy?...
- Checks of all the wire scanners started => A program will be written to have directly the transverse emittances in the CCC.

25) PFWs in the PS

- MDs required by Yannis in the PS for the PFWs => We discussed with Yannis and Christoph who agreed that it is important and he informed the users (yesterday) that there might be additional PS dedicated MDs in the future… Reminder: There are only 2 PS dedicated MD left: Next week 34 (23/08), which is already full and the people who could perform this MD will not be ready, and week 41 (11/11) where some time has already been required for the PS nominal ions.

26) Ions in the PS

- The "early" LHC beam has successfully been injected in the PS last Friday, one week-end before schedule.
- Check of the TT2 during next week 34 (23/08) MD.
- 2 problems:
  - The stripper in TT2 moves slower than it used to; it is hoped that the specialists from AB/ATB will be able to correct this before or during the technical stop on August 28th.

- 1 power supply has to be adjusted to go down to 12 A for the low beta => Not ej. in TT2 => 1 hour intervention requested for the next MD on week 34 (There will certainly have an access also for the cabling of the sextupole in SS1 for another MD).

27) Scrapers in the SPS

- Still in the phase of setting-up before making MDs.

28) LHC 75 ns

- PS dedicated MD panned next week 34 (23/08).