Week 31 (parallel + floating MD)

High band-width feedback system studies (W. Höfle, U. Wehrle, SLAC collaborators)

Summary of the floating MD (the time could be fully used, since the LHC was in store from 8:00 onwards)

The first part in the morning was used to install a set of four amplifiers (max 100 W) each in the SPS tunnel below quad 319 and to connect them to BPW.319.01. The amplifiers can be driven by a signal generated in the SPS FC, gated to limit power dissipation.

After testing without beam, the set-up was used to excite transverse oscillations within the bunch and to record the result with another BPW (321). The beam responds (mainly) at betatron lines and the upper synchrotron side band to the betatron line. Chromaticity was adjusted to as close as possible to zero, i.e. up to +1.5 Q' units. The excitation used lasted for two seconds between 4000 ms and 6000 ms after injection and could clearly be seen on the BBQ. Approximately 60 V RF were injected onto each of the 4 PU plates.

The effect could not be clearly seen on the headtail monitor, but clearly observed on the dedicated set-up in Faraday Cage. The excitation frequency was scanned and the carriers that were used were at fRF, 1.5xfRF and 2x fRF (i.e., 200, 300 and 400 MHz).

... to be continued on 17.08.2011

TT20 steering tests and validation of simulations (O. Berrig, D. Manglunki, S. Massot)

The dispersion at the target was measured by changing the energy of the line. The idea of measuring the dispersion by changing all the strengths of the TT20 line (both quadrupoles and dipoles) came from Serge. Gianluigi Arduini was concerned that, since the extraction elements were not changed, the measurements would not give a correct picture. A simulation of the differences will be done and if the difference is small, we can declare Serge's idea as the best for measuring dispersion in the TT20 line.

We also measured the emittances in the SPS ring and in the TT20 line (using the pictures of the TV screens) in order to cross-compare. The preliminary result is that the vertical emittance of the TT20 line is exactly the same as the vertical emittance of the SPS. It turns out that we should assume the screens to show +/-3 sigma (i.e. that the screens are saturated), as this seems to be the value that best fits to the emittance measured in the SPS.

We also took pictures of all the TV-screens in the TT20 line. With those pictures, it seems that the model we use has no serious errors.

We confirmed that we can improve the focusing on the T2 target, and get a 10% improvement. (The more exact value for the improvement still needs to be calculated). We also confirmed that the spot on the T2 target did not move when we changed the energy of the line. This confirms that dispersion is not a problem for focusing on the T2 target.

Gianluigi's recipe for dispersion measurements is the following:

1) Reduce the voltage on the RF so that the momentum spread of the SPS beam is as small as possible. This means that the beam will be extracted in one go.

2) Make a slope on the tune of the flattop of SPS. This means that extraction will be done at practically the same time in the cycle, because whatever the energy of the beam, it will find the resonant tune fast (because the slope is rising).

3) Change the energy of the SPS, by changing the RF frequency (radial steering).

4) Measure the displacement of the beam in the TT20 line, as a function of beam energy. This is the definition of dispersion.

In addition we made the following interesting observations:

A) The display of the steering program changed a lot when we changed the dispersion. If we can find a way to extract the beam positions, we can get a quantitative measure of the dispersion.

B) It is important to have a beam with small emittance for the MD, because it will not saturate the TV screens and also the effect of dispersion is more easily measured.

Some questions:

I) Do the correctors need to be in the knob that calculates the momentum change of the TT20 line? (Probably yes, but is there effect large enough to have a measurable effect? To be calculated)

II) One strength value was found to be wrong, after manipulating some knobs, what happened?

Longitudinal measurements with intense single bunch on nominal optics (T. Argyropoulos)

The goal of the MD was to calibrate the phase between the 2 SPS RF systems (200 and 800 MHz). This is an important issue since this phase defines the symmetry of the potential well and it can also determine the beam stability. Its control is also needed to define the bunch shortening (BS) mode in a more accurate manner. The BS mode is in fact the operation mode for the 2 RFs in SPS thanks to which the longitudinal coupled bunch instability can be suppressed. The calibration is routinely done every year. This year it was found from the measurements that the BS mode phase offset should be set to 242 degrees (in 200 MHz), while we presently operate at 252 degrees. Next goal is check how the change of this phase would affect the stability of the bunch. In the past it was observed that the bunch would be more stable if the phase is shifted by around 10 degrees with respect to the BS mode phase (now 242 degrees)

Week 32 (parallel) + Week 33 (parallel + floating MD)

Extraction of LHCINDIV beam with Q20 with LHCFAST2 cycle with slow acceleration (B. Goddard, H. Bartosik, et al.)

- First, the newly generated extraction bump orbit correction and extraction knobs were checked; one of the knobs was actually incorrect in the database
- After trimming MSE septa and TT60 H dipole values for Q20, the beam was extracted to the TT0 TED; losses in the extraction channel were observed. It was found later that these were related to the extraction bump and transverse beam halo: a bump of 34 mm is needed instead of the nominal 31.1 mm, and losses were then very low with scraping on and injection well corrected. Kicker timing does not seem to be the main issue. Further investigation including aperture scans will follow (need to increase limits on allowed bumper strengths).
- The beam was sent to the end of TI2 (TCDIs moved out). The first shot was lost in the line due to a missing 11 urad/magnet trim in the upstream vertical bend family. Adding the trim used for the nominal optics solved the issue. After a few corrections in the line, the trajectory agrees very nicely with the old references.
- Dispersion was measured in the line - some beating was seen in both planes - this will be revisited when detailed extraction steering is finished
- Kick response data was taken and seems at first glance to fit very well to the model optics
- Optics measurements were made with 7 OTR screens in TI2 and show a mismatch factor (lambda) of about 1.14 in H and 1.08 in V, already pretty reasonable.
- Need to revisit beam quality in SPS - bunch length, transverse profiles and losses in early part of the cycle.
- In conclusion rematched LSS6 extraction and TI2 with Q20 optics looks OK with no major issues seen - many details to investigate still but beam1 could be injected to LHC in this configuration.

Summary of TT60/TI2 setting up studies with Q20 (B. Goddard, H. Bartosik, et al.)

- Scans made of aperture in extraction region for circulating and extracted beam; bump amplitude increased to 37.0 mm (nominal is 36.1). Beam losses on outside of TPSG at 34.5 mm for extracted beam, and for 55 mm for circulating non-extracted beam. Bump increased to 42 mm with beam extracted with no losses seen.
- Losses in extraction channel are thus understood and solved.

- Trajectory in TT60 looks good for 37.0 mm bump, with Sunday's trim to MSE removed (setting now 1.743 mrad per magnet). Also correction of 32 urad to 610337 - should try next time with the nominal settings for MDLH.610104 and 610206.
- Dispersion remeasured to +/1500 Hz in LHC (about +/-1.5e-3 in SPS). See beating (normalised) is now about +/-0.1 m^0.5 in both planes - still seems to be coming from SPS.
- Trim of main bends current improved the energy matching but still issues with the Q20 beam on this new cycle - losses in early part of the cycle, larger emittances, problems correcting TT10, orbit poor.

Tune shift scans with Q20 and Q26 optics (H. Bartosik, B. Salvant)

The coherent tune shift as function of intensity was measured in parallel MD sessions using the MD1 cycle. A 30% higher detuning is expected for the Q20 optics due to the increased β-functions. A tune shift of ΔQ_y~-0.016*I/1e11ppb was measured in a first session end of July with the Q20 optics and injected intensities beyond 4e11 ppb. Similar results have been obtained in week 33. The same measurements were repeated with the Q26 optics, where a few datasets with different beams from the PSB and varying chromaticity were taken. Tune shifts close to ΔQ_y~-0.014*I/1e11ppb were obtained. For both optics, the measured detuning was in all cases higher than expected from the present SPS impedance model. This observation is consistent with measurements with the nominal Q26 optics from previous years.

High intensity 50ns beams on Q26 and Q20 (SPSU team, B. Bahlan)

This MD was strongly affected by the LHC filling, because the LHC damped twice during the allocated 12 hours, and by RF problems at the PS with the 10 MHz cavities. However, some studies on the ZS were carried out in the early afternoon, during which the effects of ZS sparking were studied at injection, during the ramp and at the end of the cycle. It was found that, with the present currents accelerated in the SPS, more than 30kV would be required at the cathode to limit the sparking (actually up to 100kV). During the ramp, it is usually a sparking that causes a voltage drop at the ion traps and consequently strong outgassing due to electron cloud. Before extraction, it is usually the outgassing due to the disappearing beam to induce the sparking. Opening the gap to 28mm avoids the sparking, while for gaps below 20mm sparking happens basically at every cycle. This could be done to the proximity to the cathode, that's why a negative orbit bump could be attempted in the ZS. Longitudinal measurements were taken on the Q20 cycle. Measurements with the electron strip monitors were taken on both cycles, showing very similar electron cloud horizontal distributions for the two beams (expected difference in beam size up to ~25% at the StSt liner location). It was also observed that the half coated liner chamber (only bottom part with a-C) exhibited a larger electron cloud signal than the fully a-C coated liners, but however much smaller than the signal in the StSt liner with multipacting. This suggests that the "equivalent SEY" of the half coated chamber is lower than the 1.7 value required for electron cloud build up in the liner chambers, but is also higher than the close to 1 values of the fully a-C coated chambers. A few measurements on the transverse emittances were taken on 2 batches in the Q20 cycle using the standard acquisition mode on slots 1-900 with the BWS519 wirescanners. The averaged values are ~1.6 μm in horizontal and ~1.5 μm in vertical plane. As in the last dedicated MD, a few measurements deviate significantly from the average values (0.8 μm up to 2.4 μm!). The analysis will be followed up together with the BI experts. Some time was spent on the setup of the orbit in the Q20 cycle as huge rms orbits in the vertical plane were observed last time in the high energy part of the cycle. Similar observations have been made this time. As the correctors are not efficient at high energy anymore, the vertical orbit could not be corrected better than 6 mm rms at flat top. Further investigation needed.

High bandwidth feedback studies (W. Höfle, U. Wehrle, SLAC collaborators, H. Bartosik)

A head tail motion could be excited both with fixed frequency excitation (as on 3.8.2011) and with mode pattern generation. It was noticed that a very low chromaticity is needed. Problems in seeing the excitation on the BBQ encountered during parallel MD sessions using the MD1 cycle the days before are believed to be related to chromaticity settings. Apparently the chromaticity at that time was significantly higher than expected, as demonstrated by a chromaticity measurement using a sweep of the radial steering within single cycles. Switching back to the long LHCMD1 cycle clearly improved the situation. In this cycle the chromaticity was indeed very close to 0 in both planes. Compared to the settings from 3.8.2011, the orbit between BPW321.01 and the adjacent BPV had drifted by almost 1mm and had to be corrected again, as a well centered orbit is needed in the pickup used for the experiment. In general many more orbit fluctuations were observed than on 3.8.2011. Nevertheless, many datasets were taken. Different headtail modes were excited and also a response at the 2Qs sidebands was seen, as was expected. The acquired data are yet to be analyzed.