Minutes of LIU-PSB Beam Dynamics WG #11 written by D. Quartullo

Participants

S. Albright, F. Antoniou, F. Asvesta, C. Bracco, G. P. Di Giovanni, A. Garcia-Tabares Valdivieso, A. Huschauer, E. Koukovini Platia, M. Kuczyński, B. Mikulec, A. Oeftiger, D. Quartullo, G. Rumolo, B. Salvant, A. Santamaria Garcia, P. Skowronski, C. Zannini.

Approval of Minutes

Minutes are approved.

Horizontal instability studies at 160 MeV mimicking the future injection energy in the PSB (E. Koukovini Platia)

After-talk discussion:

- B. Mikulec comments that the last day to do the measurements for the termination of the extraction kicker is the 12th of November and asks the speaker if she assumes that the needed remaining measurements could be done by that date. The speaker answers that it should be decided in the following days.

- S. Albright asks the speaker what tune should be used in operation at 160 MeV, considering the showed results. The speaker answers that there is clearly a set of tune values that should be avoided but unfortunately these values are exactly the ones desired. C. Bracco asks if they refer to the nominal injection. E. Koukovini Platia replies positively. C. Bracco comments that there is an alternative working point for the injection at 160MeV, namely Q_H=4.43.

- C. Bracco asks the speaker if turning the transverse feedback on could cure the instability. The speaker answers that there is a worry that the feedback is blind at the very beginning of the injection (few first turns) due to saturation of the BOSS unit, caused by then non-perfectly closed injection bump. In that case, if the tune related to the rise time of 0.6 ms is considered, then the feedback cannot help in these few number of turns. Therefore everything depends on the future feedback reliability and the future injection bump, in other words if the feedback is active from the very beginning without any saturation of the BOSS unit, then it will suppress the instability.

- G. Rumolo comments there are two problems with the feedback at injection. The first one concerns the fact that the longitudinal distribution changes significantly during the current RF capture process and this could confuse the transverse feedback. The second problem is that the BOSS unit is saturated in the first milliseconds. He adds that the real point is if in the future it will be the same or not. Likely the first problem will be avoided through direct injection of the bunch inside the RF bucket, as for the second issue of saturation the prediction is more difficult. He adds that the fact that the feedback works immediately is very important, otherwise there is a strong limitation on the feasible working points. The speaker confirms, saying that if one

chooses an alternative not planned value for the tunes, then resonance losses might occur which cannot be cured with feedbacks.

- G. P. Di Giovanni asks if the PS experiences a similar case since the system is the same. A. Huschauer answers that the injection could still be done without feedback. However the comparison cannot be done since the values of chromaticities in the PS are relatively higher and in general the injection procedure is different.

- B. Mikulec suggests that during the commissioning phase, lower intensities with an alternative working point should be chosen and then slowly the desired working point should be approached when the orbit will be closed at injection (no saturation).

- B. Salvant asks why the instability depends so strongly on the tune and which source could excite the instability. The speaker answers that the source is not yet found but the strongest suspect is the extraction kicker with its external circuit. B. Salvant asks the speaker if other sources different from impedances could be the cause and if PyHEADTAIL simulations have been performed to benchmark the measurements. The speaker answers simulations have not yet been run but are ongoing. The speaker observes that the RF cavities have been excluded as source of the instability thanks to the MD at the beginning of August. G. Rumolo adds that focus should be put on the terminations of the extraction kickers. S. Albright asks B. Salvant it the length of the cables have been included into the ECR, B. Salvant does not know it. It has been confirmed later that the cables will not be changed.

- F. Antoniou asks the speaker if she sees any differences between her observations and the ones that were found in the past. The speaker answers that this is the first time measurements have been performed on the 160 MeV plateau (in the past they were done using an ISOLDE cycle during acceleration), however similar characteristics have been identified if close to natural chromaticities, in terms of rise time and number of head-tail modes. A clear dependence of the rise time versus horizontal tune has been found thanks to the latest measurements.

- F. Antoniou asks what sources could cause this instability. C. Zannini answers that the kicker could be responsible, however it is very likely that a combination of elements lead to the instability.

Status of optics measurements (P. Skowronski and A. Garcia-Tabares Valdivieso)

Slide 7:

- B. Mikulec observes that the numbers shown represent not the average beta-beating but the average increases in beta-beating.

Slide 8

- P. Skowronski says that the beta-beating error bars will be not less than 3% unless substantial improvements are made. G. P. Di Giovanni asks if the error bars can be reduced through statistics. P. Skowronski answers that should be tried.
- F. Antoniou asks if the stability of the Q3Q5 plays a role, the answer is positive (he refers to slide 6).
- G. P. Di Giovanni asks what the plots on the bottom represent in slide 6. P. Skowronski answers that they refer to beta from amplitudes data, while the plots on the top refer to beta from phase more commonly used (cotangent equations).

Slide 10

- P. Skowronski comments that the seen phase beating is not localised at the wire-scanners and injection area but it is localised within 3 sectors. He believes it cannot be an accident. He asks the participants if someone knows what can be the cause, nobody answers.

Slide 13

- G. P. Di Giovanni observes that, even if there is the puzzling vertical beating in the 2nd half of the rings, at least there is no beta beating around the wire scanners, the speaker confirms, adding that there is no beating also in the ejection region.

Slide 16

- B. Mikulec asks the speaker if the cycle measured is a copy of the LHC cycle, the answer is positive. She observes that there should be an orbit correction and that dispersion should be negligible. The speaker confirms, saying that the correction is 3-4 mm. However**,** he says again that measurement results show dispersion and the first thing is to confirm those.

After-talk discussion:

- The speaker asks the participants when the saturation kicks in, what are the saturation modes and at what energy one should expect to see saturation. A. Huschauer answers that measurements of the B-field could be used to have a first approximation.
- F. Antoniou asks if it is worth the effort to try improving the calibration of the BPMs. The speaker answers that the calibration that Jeroen did brought the error from 60% to 20%. He is afraid that to go even further could be very challenging and it should be verified that is feasible.
- S. Albright refers to slide 11. He asks the speaker if there are elements around the region where the beta-beating is seen that could correct the beating itself. The speakers answers that the corrector magnets are in series on opposite sides of the machine therefore there is no simple way to correct this beta-beating.
- B. Mikulec says that the beta-beating at the wire-scanner location seems to be small. The speaker remarks that it is true up to 10% (1 sigma). The beta-beating seems small also in the extraction region, and therefore the two main questions to be answered have been treated.

- B. Mikulec asks the speaker at which level the correction of the vertical beta-beating at injection after LS2 should be done. The speaker answers from 3% to 5%, observing that 5% error bar is already incompatible with 20% beating assuming enough correctors.
- B. Mikulec asks that a lot of work should still be done and that priorities should be fixed before the end of PSB operation in November. The speaker answers this is only the first iteration and that trying to correct the beating at 160 MeV will be a good exercise to understand the amount of correction that could be applied. The speaker says that a proper study and manpower is needed to check all different plausible beta-beating patterns and how to correct them having the 4 corrector magnets.
- Finally the speaker says that for the time being the beta-beating is not big, the question being now if the phase beating can be brought to zero. Any additional beating will derive from the injection and this can be cured by dedicated corrector magnets for local compensation.
- G. P. Di Giovanni asks what the speaker refers to when the term saturation is used, since saturation is seen in the bending of the external rings but not the internal, however the beating is seen in all the rings (the strongest is even seen in ring 2). The speaker suggests that there may be some special elements, like sextupoles, running for some bumps at the end of the ramp. G. P. Di Giovanni observes that the beating is not seen at 1 GeV but only at 1.4 GeV. F. Antoniou suggests that a thorough study of the chosen configuration should be performed in all four rings. She adds that she applied the correction in ring 2 for the Q3Q5 and new measurements could be useful.
- S. Albright asked if the radial loop was on during the measurements, the answer is yes. He doubts that there could be some unwanted steering, however the speaker confirms that he saw a flat orbit without driftings. Indeed he has measurements with and without radial loop and there are no changes.

Phase Alignment in Ferrite and Finemet Cavities (M. Kuczyński)

Slide 15: The speaker says that, even if the algorithm he created works well with beam profile shaped with the second RF system in the bunch-lengthening mode, the zeros of the function theta are meaningless in this case. S. Albright thinks that those zeros could still give a good approximation of the synchronous phase.

After-talk discussion:

- A. Santamaria Garcia asks if the phase obtained as an output from the algorithm has been used with a measured symmetric beam. S. Albright answers that during the present week several MDs are planned which will show if the algorithm effectively works.

- A. Santamaria Garcia asks if the proposed algorithm is expected to be used in operation or in post-processing. S. Albright answers that the idea is to make it usable in operation but it would be used once at the start of the year or after a cavity break when a cable is changed. A. Santamaria Garcia observes that usually more numerical tools are used than analytical at CERN. S. Albright answers that he did a numerical version of the algorithm in the past and this new

algorithm to compute bunch length and synchronous phase could be a valid alternative. He remarks that the results obtained by the speaker specifically for bunch length calculation are very positive, since the current convention (Foot Tangent Method) is not ideal. He thinks that this new method could even substitute the current one if made more suitable for operation purposes. In that case, one has to think how to implement it into the tomography algorithm.

Actions

1) **Data post-processing: losses on the 2D tune diagram, space charge tune shift and PSB resonances (E. Koukovini Platia)**
2) **PyHEADTAIL simulations: Multiturn wakefield to identify the source of the instability from the developed PSB impedance model (E. Koukovini Platia)**
3) **MDs: Termination of the extraction kicker (E. Koukovini Platia)**
4) **Measure at 160 MeV other rings with Q3Q5 and Q4Q4 right after (P. Skowronski and A. Garcia-Tabares Valdivieso)**
5) **Try to improve and stabilize the beam (P. Skowronski and A. Garcia-Tabares Valdivieso)**
6) **Selecting best shots, more statistics (P. Skowronski and A. Garcia-Tabares Valdivieso)**
7) **Attempt phase (thus beta) corrections at 160 MeV (P. Skowronski and A. Garcia-Tabares Valdivieso)**
8) **Measure at 1 GeV to see how the beating evolves when approaching the flattop (P. Skowronski and A. Garcia-Tabares Valdivieso)**
9) **Try to bring Q3Q5 to 1.4 GeV: additional, independent, measurement to understand the source of the beating and check if BPM calibration factors are energy dependent (P. Skowronski and A. Garcia-Tabares Valdivieso)**
10) **Change the measurement technique for the synchronous phase (M. Kuczyński)**
11) **Create map between (α,φ_{s0},Θ_2)→ (φ_s , λ) (M. Kuczyński)**
12) **Take better measurements to evaluate the azimuth and delay compensation for the low level RF (M. Kuczyński)**