DEBRIEFING AND FOLLOW-UP OF THE LPL REVIEW

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=> LPL (LHC Performance Limitations during run I) review on 25-26/09/13: https://indico.cern.ch/conferenceDisplay.py?confId=267783

- **Debriefing**
- **More detail of the cogging MD**
- **Follow-up**

DEBRIEFING (1/4)

- Any other comments?
- Comments from OliverB
  - Proposition to repeat again during the post LS1 operation
  - Difficulty to disentangle the effect of many changes at the same time
  - Did we learn anything about the data logging from this review exercise that would facilitate this (e.g. flags?)
  - What is the cause for the discrepancies of the machine and our impedance model (e-cloud?) => More MD studies
  - Additional beam diagnostics functionalities: Schottky, emittance, e-cloud Monitor, Head-Tail monitor, BTF measurements etc
  - Is the damper working as we expect it (emittance growth due to damper and beam-beam)? What is the limit for damper gain?
  - Do we have a preferred Octupole polarity for post LS1 operation?

DEBRIEFING (2/4)

- Expected stability thresholds for post LS1 operation for a given Q', octupole powering and damper setting?
- Strategy for intervention if we observe instabilities after LS1?
- Ecloud in triplet magnets => beam screen design for HL-LHC!
- Instability during going into collision stopped with octupole change. Benefits of larger stability diagram with positive octupole did not help EOSI
- Ecloud activity in common beam pipe and triplet magnets: is this a problem for the triplet heat load? => Not an issue due to NEG coating
- Do we have filling patterns for optimum scrubbing and for more stable physics?

DEBRIEFING (3/4)

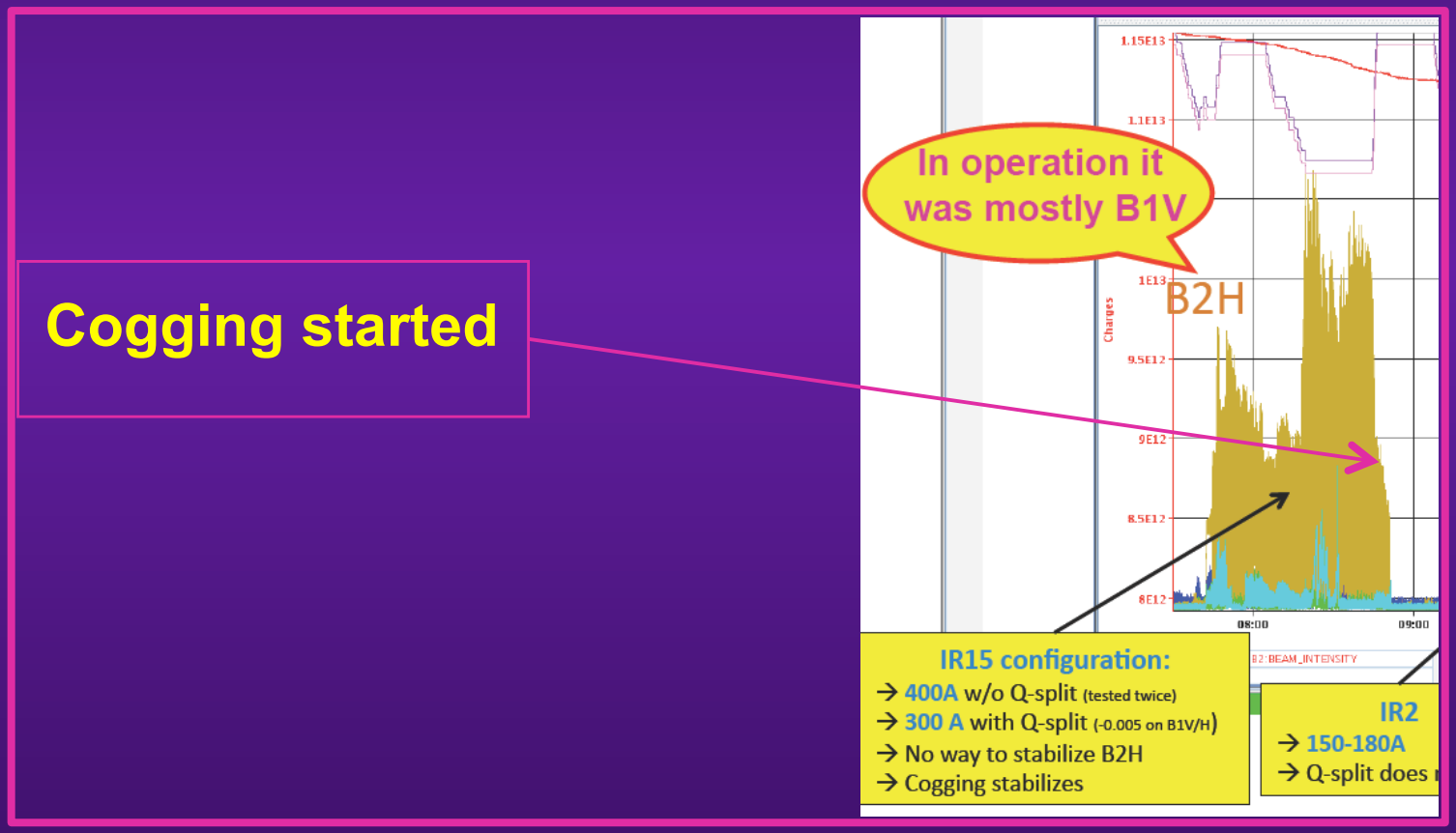
- The “Golden fills” of 2012 were obtained < MYC and it was after very careful chromaticity meas. and close to 0 (see summary of GA)
- Reminder: Tune split (between the 2 beams) => 2 effects: tune + split
- What could have been done during 2012? => Change the octupoles at injection to check the ecloud hypothesis
- Comment from SF about the other source of octupoles discussed at Evian => 1 plane only and degrade the other plane as shared by the 2 beams
- IR2 and IR8 => Was critical this year due to 3 m whereas it is 10 m in design and if we go to 10 m then the chroma effect should disappear
- IR1/5 => SF mentioned the spurious dispersion from the field imperfections in the magnets, which can be of any sign, different for both beams and both planes

DEBRIEFING (4/4)

- WH reminded sudden losses in 2010 without BBLR
- SimonW proposed to implement a BTF meas. as in RHIC where it is always used
- Some comments from AlexeyB => Will discuss them after
- Future MDs
  - Really go step by step and better structure the MDs
  - Publish the notes in time
- Why no pb in 2011?
- RodericB => To cure the instability pb, we could imagine retracting some collimators in IR3

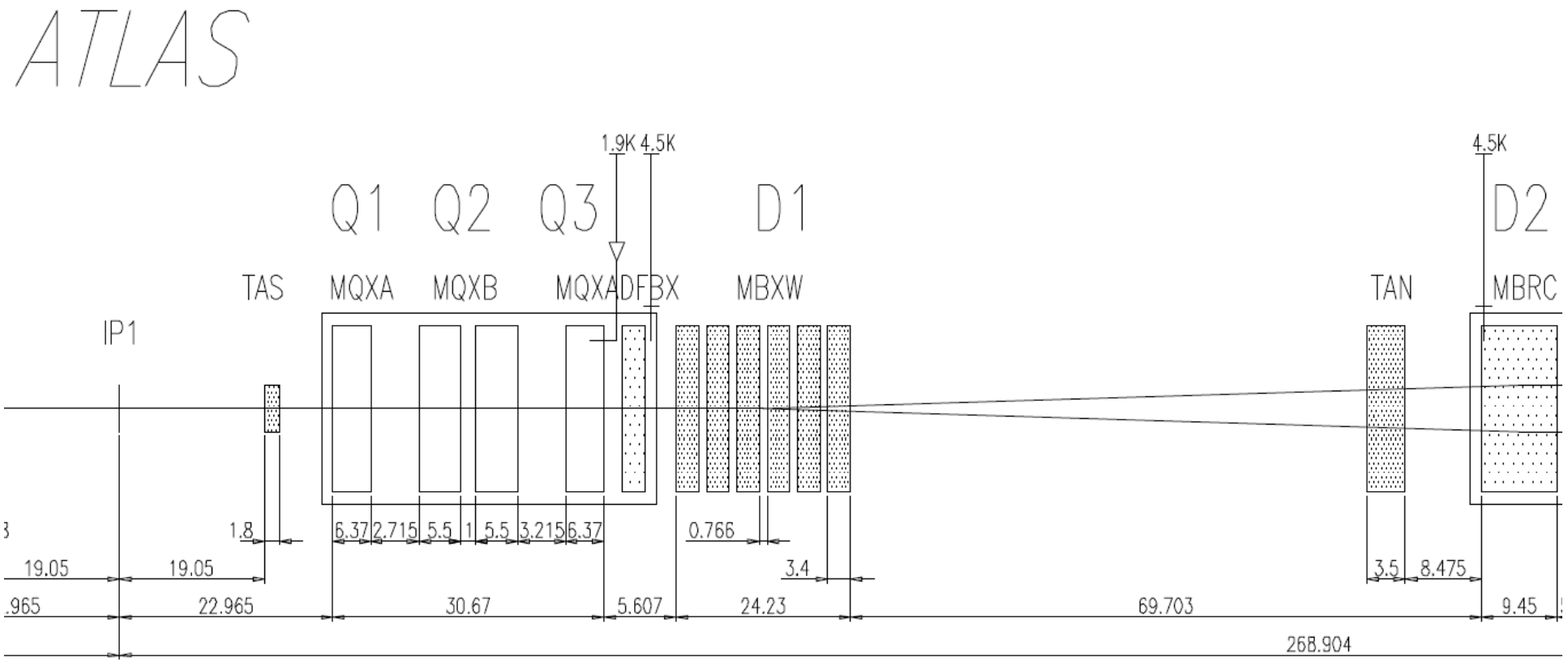
MORE DETAIL ON THE COGGING MD (1/7)

- **Important question: Did the B2H signal disappear when the cogging process started or when the 2 beams did not see each other anymore? => Mode detailed analysis**



MORE DETAIL ON THE COGGING MD (2/7)

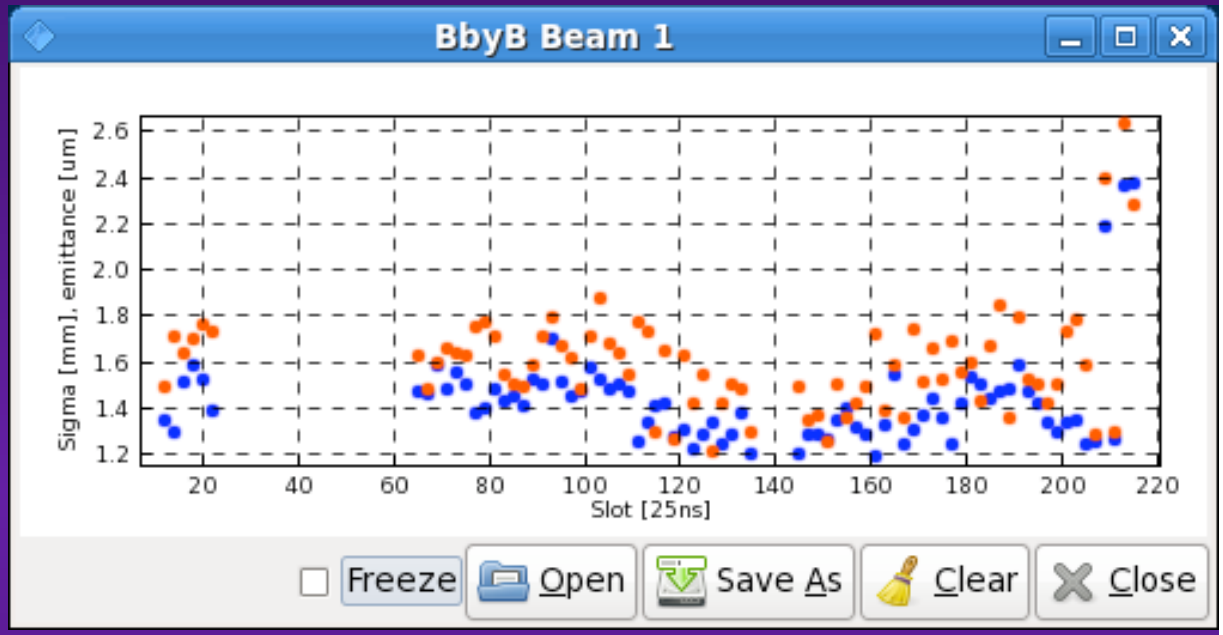
- **Total length of the common region around IP1 (from D2 to D2) = 2 d ≈ 306.4 m ⇔ 1022 ns**



d = 22.965 + 30.67 + 5.607 + 24.23 + 69.703 ≈ 153.2 m ⇔ 511 ns

MORE DETAIL ON THE COGGING MD (3/7)

- **Total length of 1 beam**



  - **Beam length considering all the bunches (i.e. also the 6) ≈ 200 × 25 ns ≈ 5000 ns = 5 μs**

  - ***Beam length considering only the 2 trains (i.e. without the 6) ≈ 150 × 25 ns ≈ 3750 ns ≈ 3.8 μs***

MORE DETAIL ON THE COGGING MD (4/7)

- **Total time shift needed (called Δt) between the 2 beams such that the 2 beams do not see each other anymore (starting from the situation where the 1st 2 bunches of B1 and B2 collide in IP1)**

=> Δt = Length of the common region around IP1 + length of 1 beam

≈ 1022 ns + 5000 ns

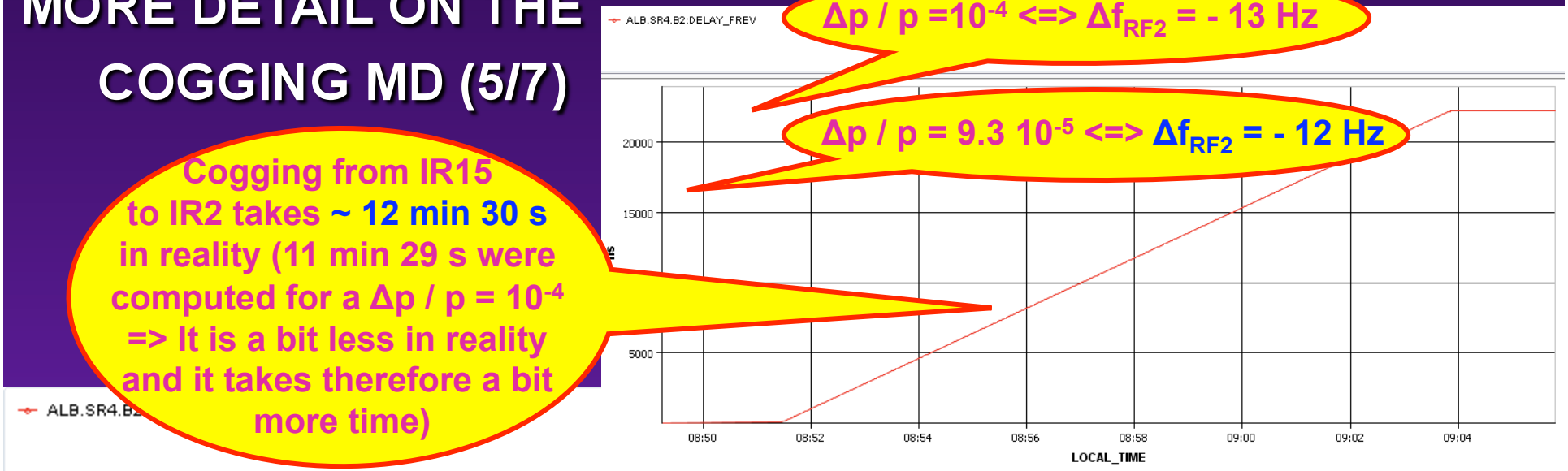
≈ 6.022 μs

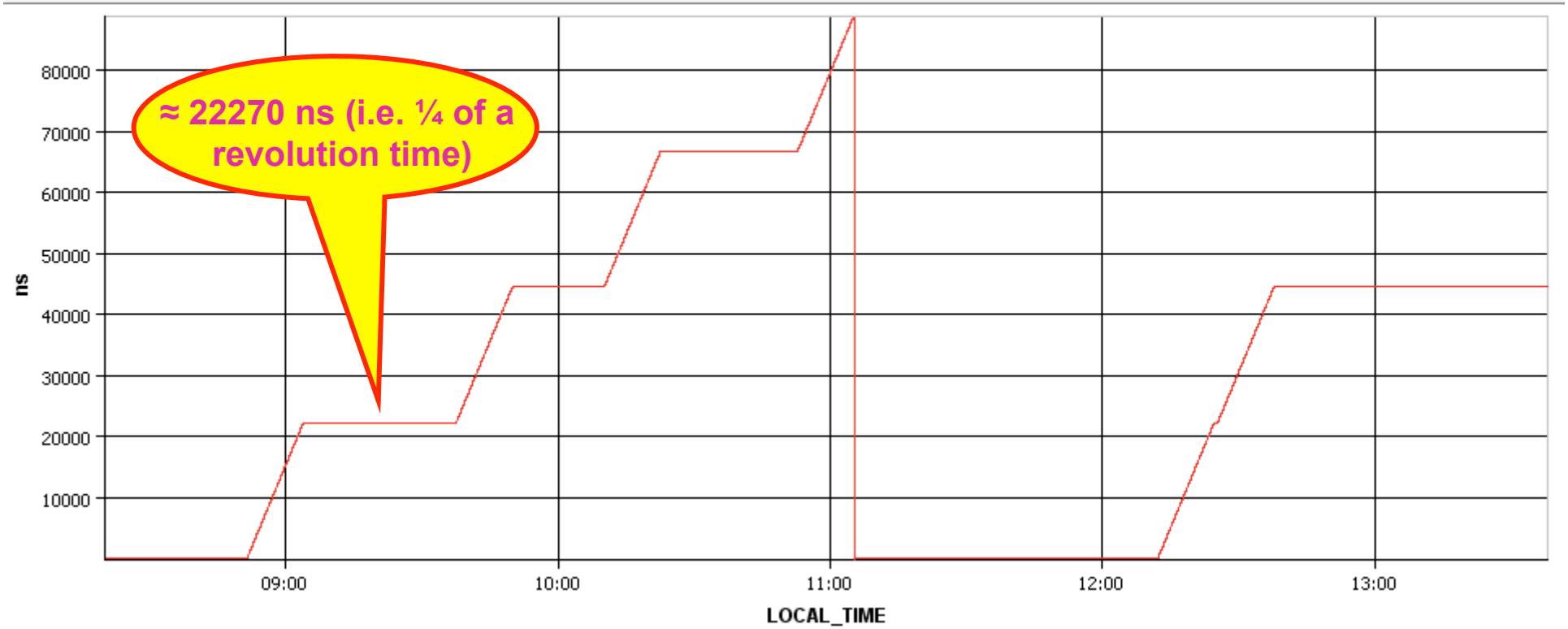
MORE DETAIL ON THE COGGING MD (5/7)

Cogging from IR15 to IR2 takes ~ 12 min 30 s in reality (11 min 29 s were computed for a $\Delta p / p = 10^{-4}$ => It is a bit less in reality and it takes therefore a bit more time)

$\Delta p / p = 10^{-4} <=> \Delta f_{RF2} = -13$ Hz

$\Delta p / p = 9.3\ 10^{-5} <=> \Delta f_{RF2} = -12$ Hz



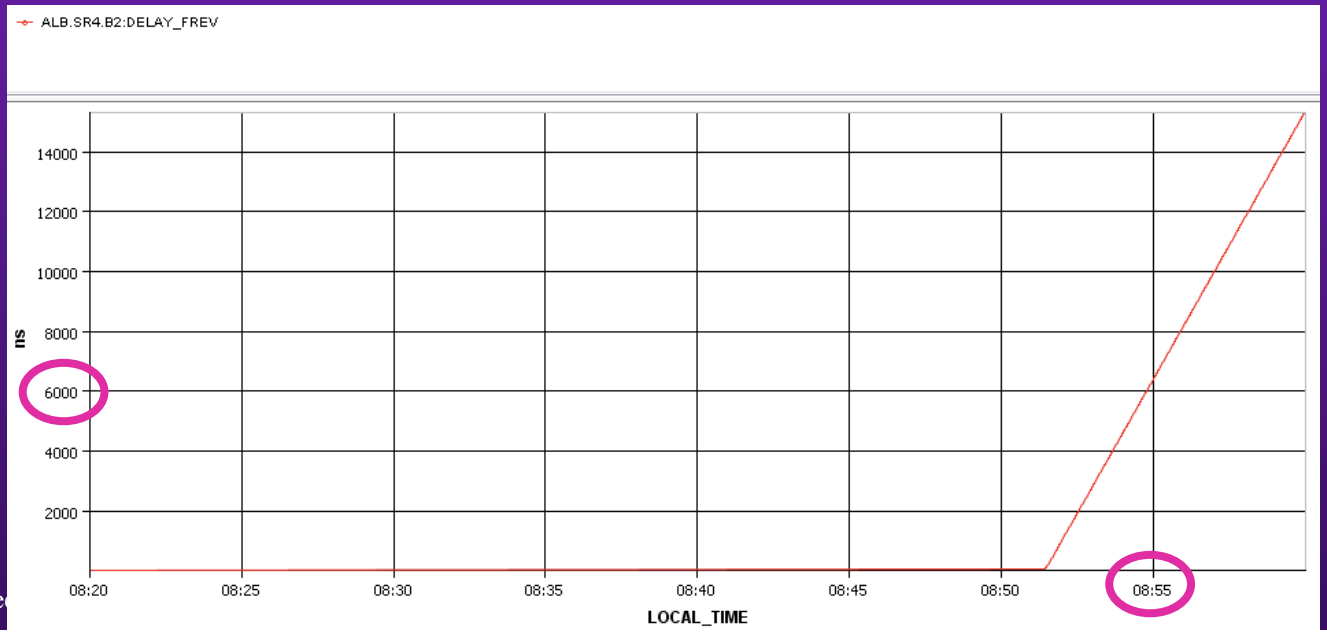


MORE DETAIL ON THE COGGING MD (6/7)

- The time needed (called T_{needed}) such that the 2 beams do not see each other anymore (starting from the situation where the 1st 2 bunches of B1 and B2 collide in IP1) is therefore

=> $T_{needed} \approx (6022 / 22270) \times 12$ min 30 s ≈ 3 min 20 s

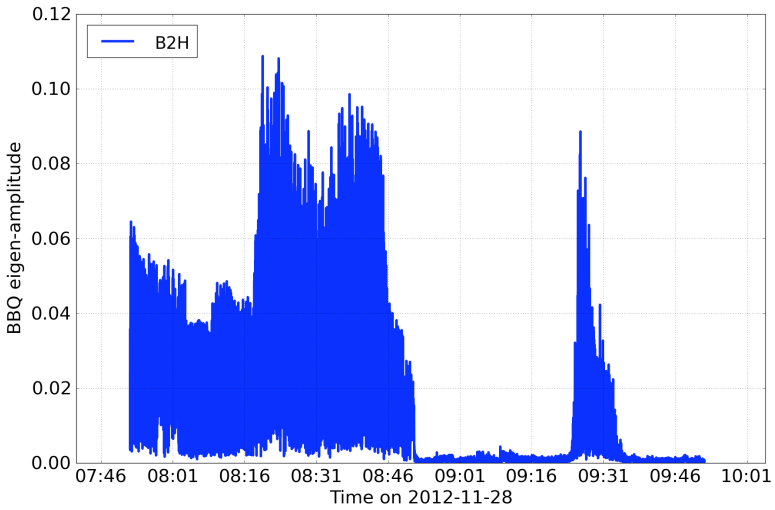
=> We would expect that the 2 beams do not see each other at time ~ 08:55 (as we started the cogging just before 08:51:30)

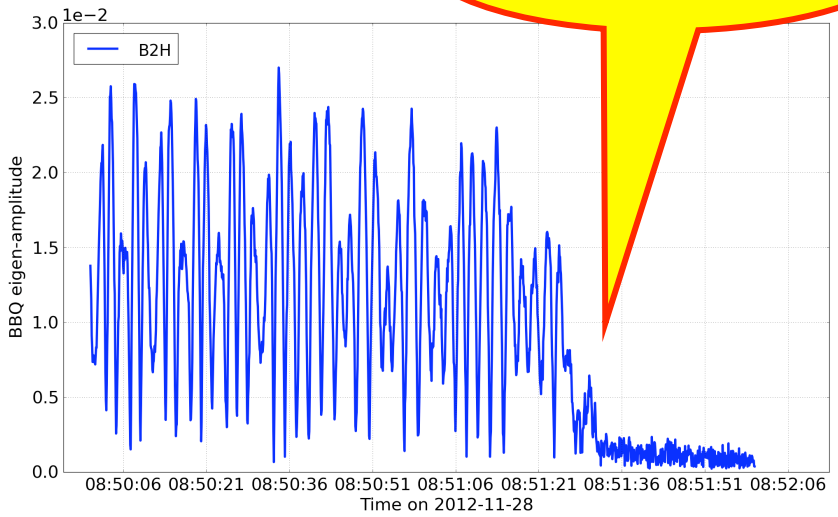


MORE DETAIL ON THE COGGING MD (7/7)

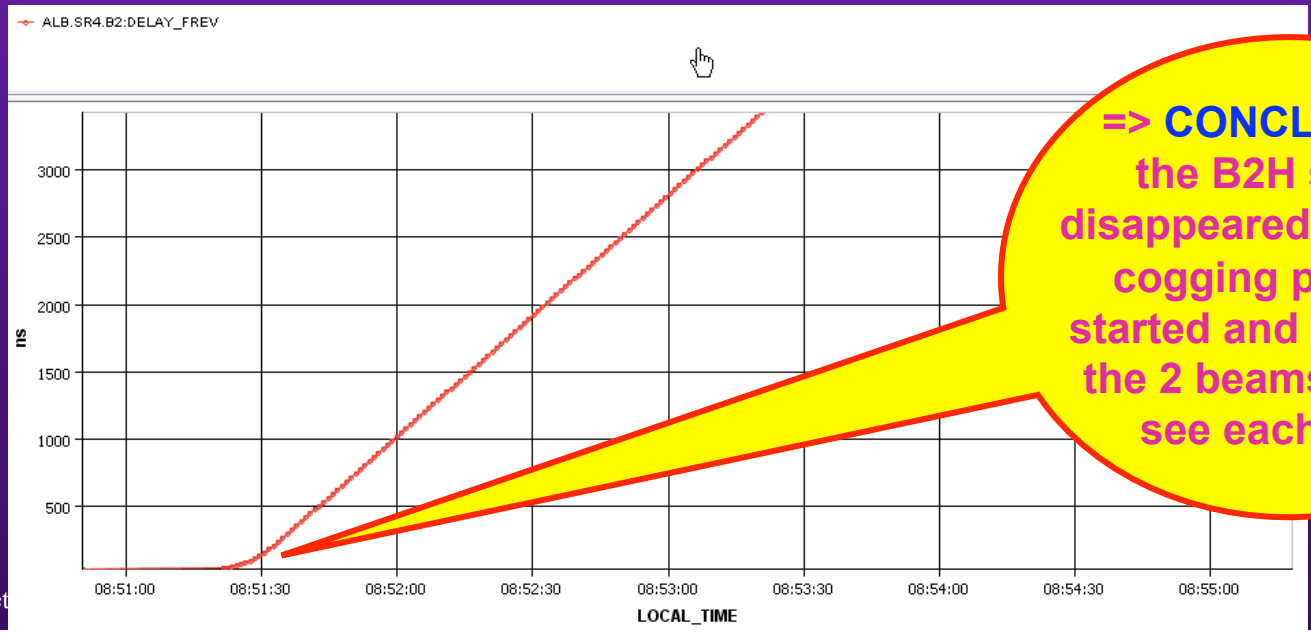
- **B2H BBQ signal**

Nicolas Mounet





Signal disappeared at ~ 08:51:30



=> **CONCLUSION:** the B2H signal disappeared when the cogging process started and not when the 2 beams did not see each other

FOLLOW-UP

- Need to understand why we saw nothing in 2011, whereas we reached ~ 1.4E11 p/b, 1380 bunches (but smaller collimator impedance and $\beta^* = 1$ m instead of 0.6 m)
  - What do we see in AlexeyB's model?
  - What about the BB and octupoles compensation?
  - Etc.
- Effect of the filling pattern (see change in July)?
- Are the spectrograms and loss patterns really very similar in most of the unstable cases observed in operation or not?
- Clarify the situation with tune splits (looking at all of them) => Show the spectrograms (and loss patterns) of all the cases with tune split at the EOS (as it seems that tune splits had an effect => Is it confirmed?)
- Study in detail the compensation between BB and octupoles (for LOF < 0) for post LS1 operation, compared to 2011 and 2012