

Referees meeting #2

Paul went over his slides. Paul J has updated cut plots as requested. Not all actions have been implemented due to lack of time.

1. He has looked at the number of TOF spacepoints in TOF0 and TOF1. The excess seems to be just noise.
2. The discrepancy between TOF01 MC and data is still there, consistent time offset between data and MC. Paul has replotted with more bins. We discussed taking the ratio of TOF01 (muons)/TOF01 (electrons) and see if that fixes the offset. **Try using the ratio to fix the TOF01 time offset between MC and data.**
3. Study of diffuser radius cut stands.
4. $P_{\text{tof01}} - p_{\text{tku}}$: Paul S points out that we can use $\text{TOF}(\text{electrons}) \cdot c$ as a substitute for the distance between TOF01 in the p_{tof} calculation. **Check if this improves the comparison.**
5. The TKU fiducial cut has been adapted to use the maximum radius in the full tracker volume, rather than the maximum radius in just a tracker station.
6. We noted that the χ^2 in the TKD is higher in MC than in data. Comment was that this is a combination of position resolution and scattering resolution; and we have some uncertainty in the density of the tracker glue. It was not thought to be a significant effect in the recon.
7. Paul showed tracker noise plots. **Paul S asked for the noise plots to have ratios to the total number of events, so that the likelihood of noise can be estimated.**
8. A misalignment in Y TKD between data and MC was noted.
9. MC was noted to be more sensitive to the low pt hole in the reconstruction.
10. It was noted that the p_z in TKD was a bit offset. The means look good but the peak is potentially offset. The impact of p_z is that it gives us confidence in the momentum reconstruction; that p_z is also a measure of the cooling effect. For now we decided it was good enough; we may need to revisit later.
11. Paul checked the difference between dots and lines in beam sampling slide 31. Dots are reconstructed data; lines are MC truth. **Paul Soler asked "Matched optics" to be relabelled as "Reweighted matched optics" or similar.** It wasn't clear that they have the same parent sample distributions.
12. On slide 33 (and others), Chris asked which parent samples were used for the different subsamples. Paul commented that the left hand column was sampled from 4-140, middle 6-140, right 10-140. He has splitted the parent samples into two and generated two different subsamples from each (having different emittance). **Chris noted that the naming convention 4-140 (etc) refers to quad/dipole settings; it might be good not to confuse that convention.**

13. On slide 46 **Dan asked to put a legend on tracker bias and resolution pz to indicate there is a band which gives the resolution.**
14. The Pattern Recognition was noted to behave very poorly in TKD Pz recon (slide 47). It was not clear what the source of the PR. Dan asked what the big picture is for the PR resolution; how is it relevant to the analysis. Paul responded that he was asked to apply a patch on the phase space variables (fix the bias there) rather than the emittance as a whole; so he is trying to understand the source of bias. Dan asked whether the TOF information could be used to extract the track pt. We thought it would be tricky to untangle thanks to the diffuser.
15. Paul showed that the inefficiency in TKU is associated with dead channels. It is significantly reduced when he takes a cut on the truth momentum.
16. Paul discussed the bias in emittance. He reminded us that he has a successful correction for the bias. He tried doing a correction based on the phase space variables instead. We noted the bump in TKD resolutions. There seems to be a consistent picture of misalignment in TKD (also this is visible in the beam distributions). The correction based on momenta was not as successful as the correction based on R matrix. It was noted that the R matrix terms are based on covariance between e.g. p_x and $\Delta(p_x)$; which is probably directly relatable to this linear fit of graph of $\text{mean}(p_x)$ vs $\Delta(p_x)$ shown on e.g. slide 67/68. It might be the same problem explored in a different way. **Chris proposed to show the individual 2D histograms that go into the R matrix as a "half way house"**. Dan added that it would be essential to show that the correction (by extension the R matrix) is stable under systematic effects.
17. We discussed the magnitude of systematic effects. Chris noted that his assumptions for the nature paper were reasonably conservative. One may be able to do a better job.
18. Paul spent some more time on systematic studies. He has discovered his MC samples shown last time were not statistically significant. We discussed how the systematic errors should be done. Chris **commented that Paul should take the subsample and calculate the bias on the subsample** e.g.
[(badly reconstructed emittance of the subsample) - (true emittance of the subsample)] - [(well reconstructed emittance of the subsample) - (true emittance of the subsample)]
19. Dan asked whether the effect of End coil 2 is significant. Paul said not clear.
20. Paul is continuing to look at the effects of the transmission bias. Noted the emittance increase is still there even following an amplitude cut. Dan asked why for samples just considered across the absorber is there still a transmission loss. Paul was not sure. **Dan asked for Paul to study the loss mechanism. Paul Soler asked Paul Jurj to plot the trajectory of the lost particles to find where they go.**
21. We looked at the emittance change plots. **Paul was asked to separate the statistical and systematic errors.** Paul will update the plots when

he has sufficient stats in the systematics. We discussed the statistical significance. **Paul was asked to show 68 % CI for both systematic and statistical OR show 90 % and specifically label that. Not mix 68 % systematic with 90 % statistical.** Paul Soler asked about the bootstrap method relative to John Cobb method. Paul J said yes, the bootstrap method yields a higher error; it is because bootstrap includes some more effects like non-linear emittance growth.

22. We congratulated Paul on starting analysis on 170 data.
23. We liked the look of the angular momentum. There was discussion of the potential to split that into a separate paper, in collusion with Tom.
24. We discussed which journal. Nature physics => high impact factor. PRAB => people read it. Also PRL, and others. TBD.
25. Biggest hole is the systematics analysis. Then next step is the note. Plan for next meeting in one month.