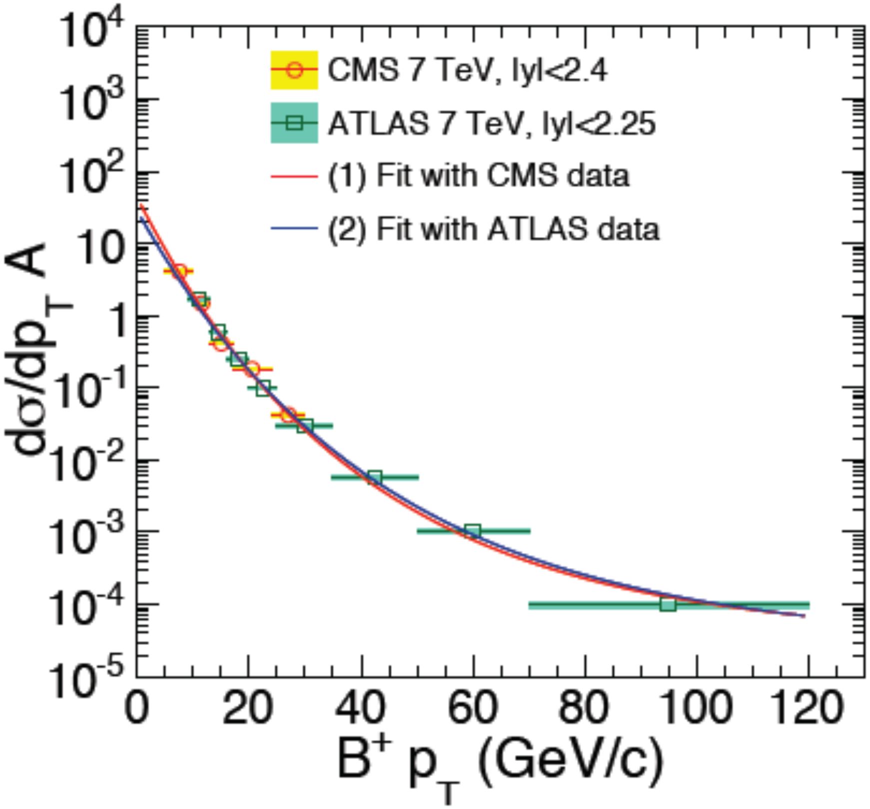
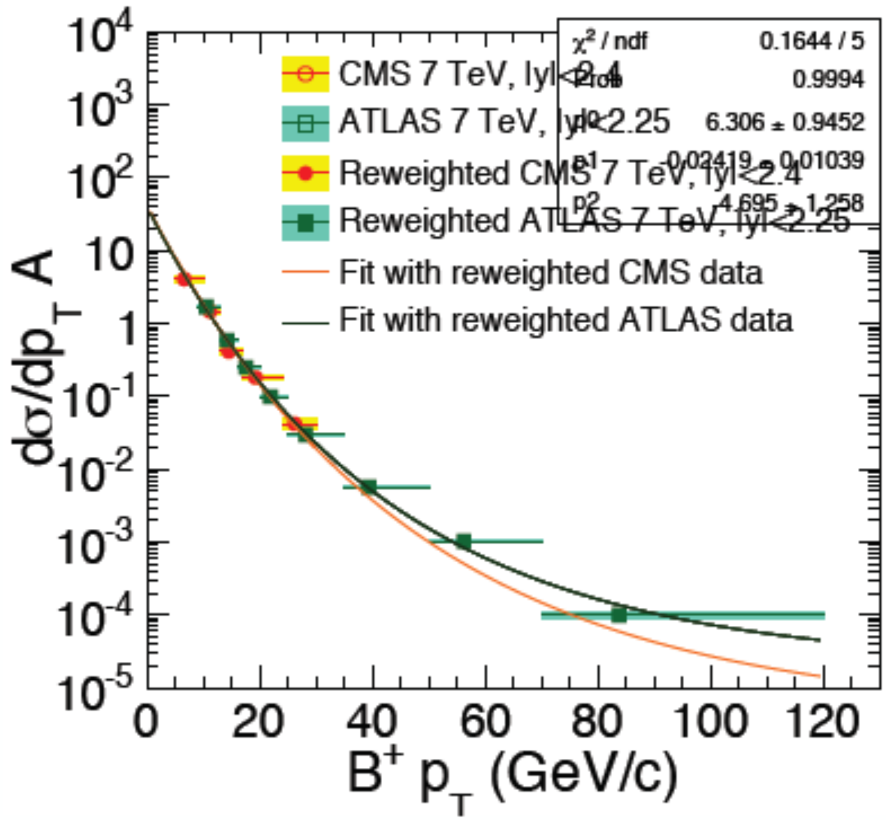
Status report for HIN-14-004 after ARC discussion

Hyunchul Kim
with HIN-14-004 analyzers

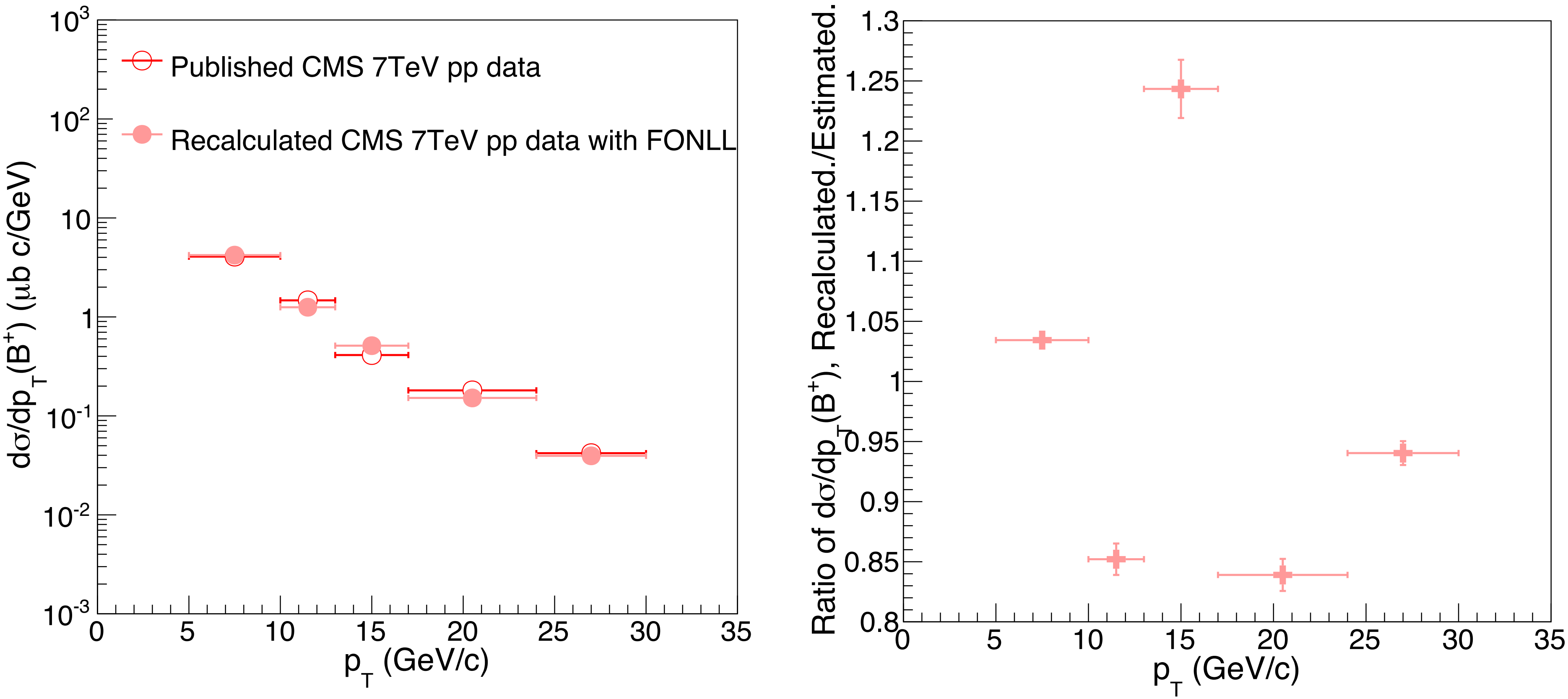
Improvement the strategy





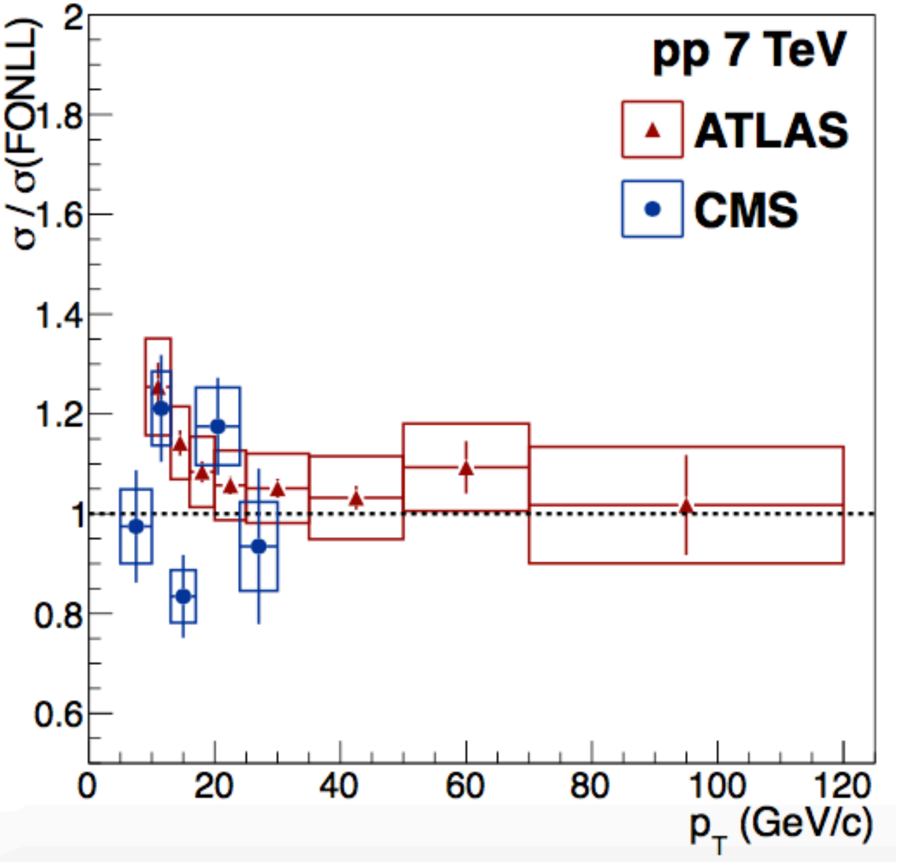
- **Simplify the fit procedure**
  - Fit with CMS, ATLAS exclusively and then merge, refit

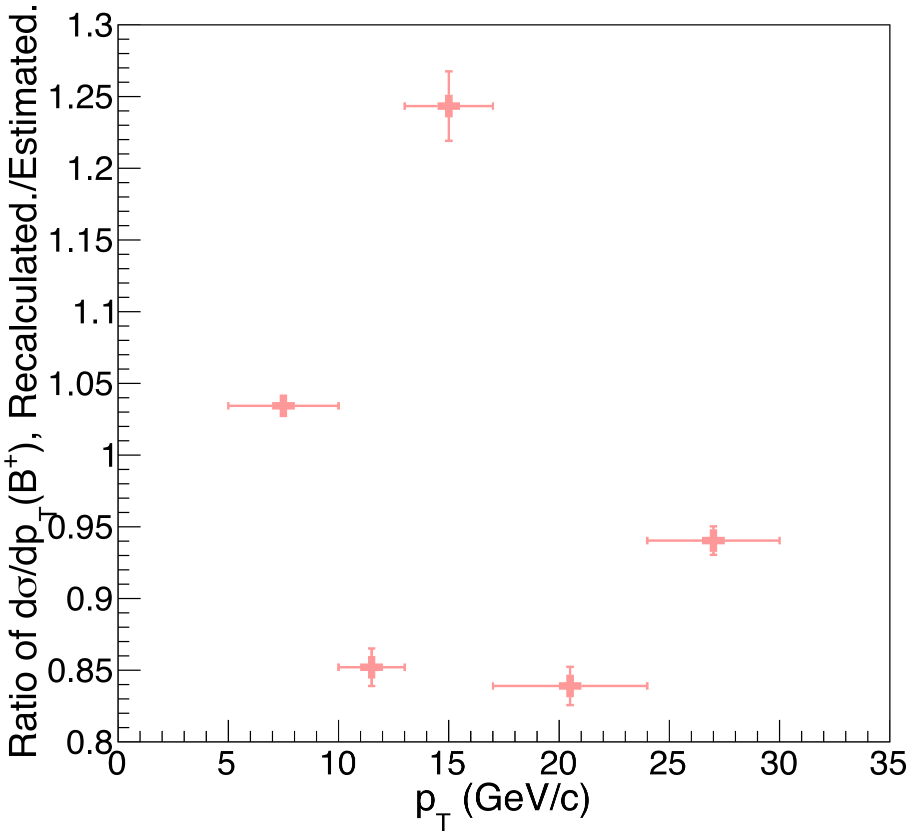
Comparison between recalculated and original



- **Not so much difference?**

Comparison between recalculated and original



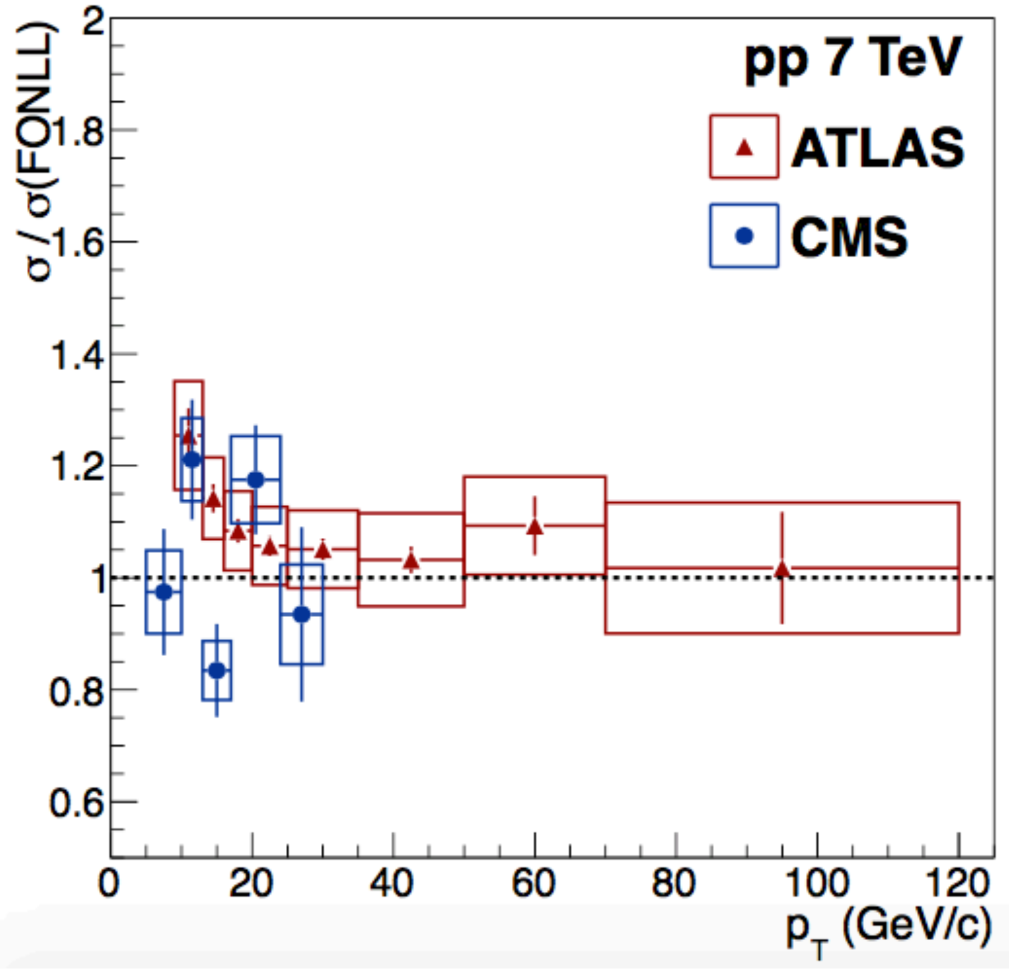


- **Difference between recalculated / published looks in the range of sigma_CMS / sigma_FONLL**

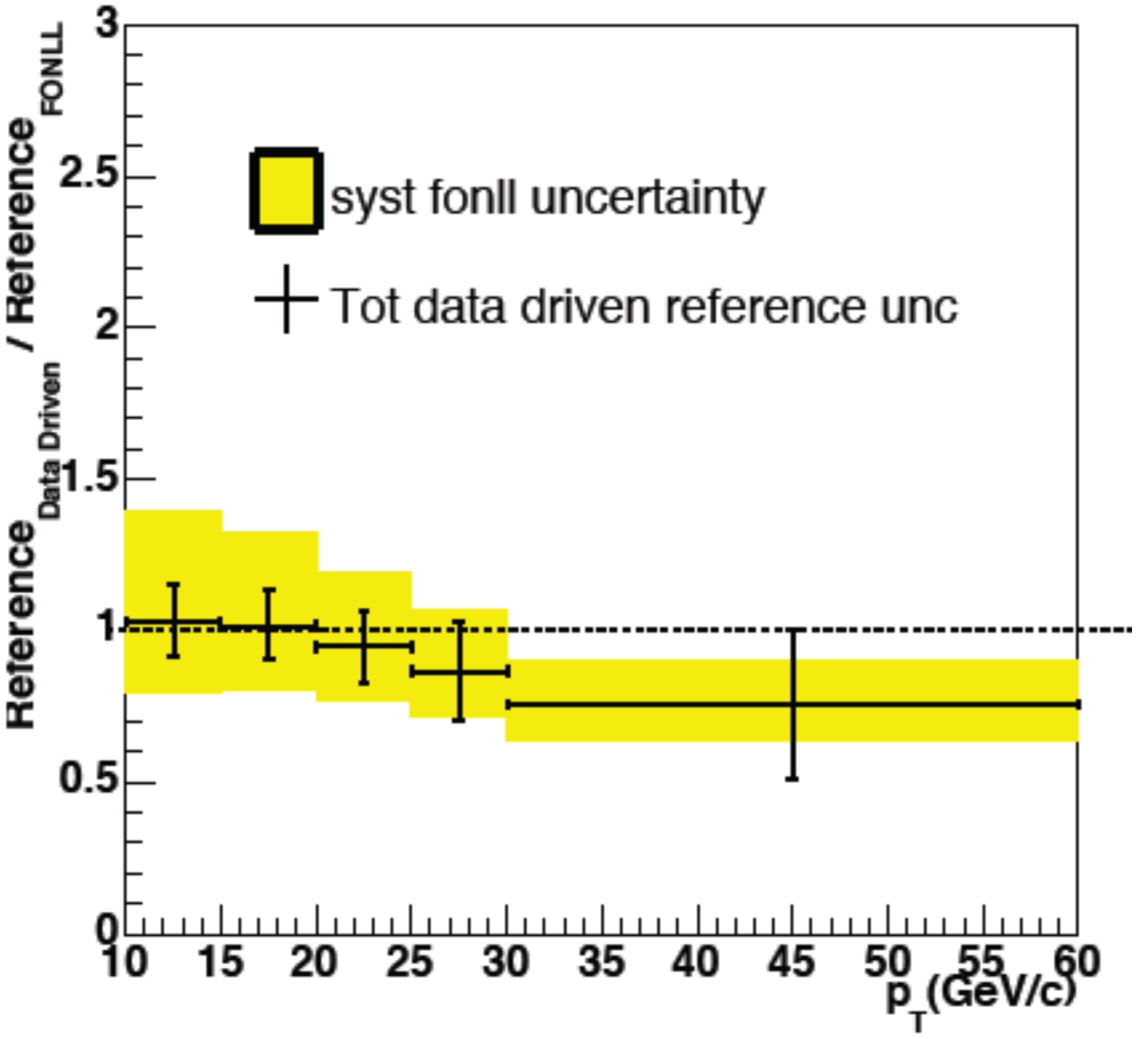
Can we believe FONLL over data?

- **CMS : rather flat with dispersion at lower pT**
- **ATLAS : up to lower pT direction**

- **Hard to use extrapolation on data**

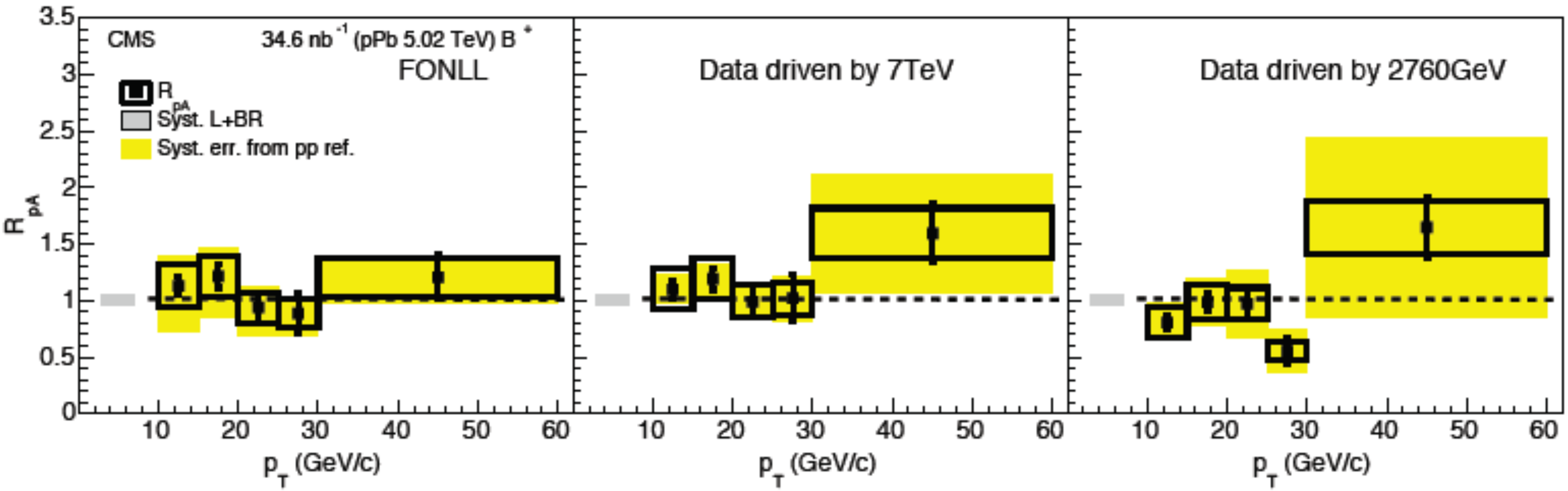


data driven reference vs. pure FONLL



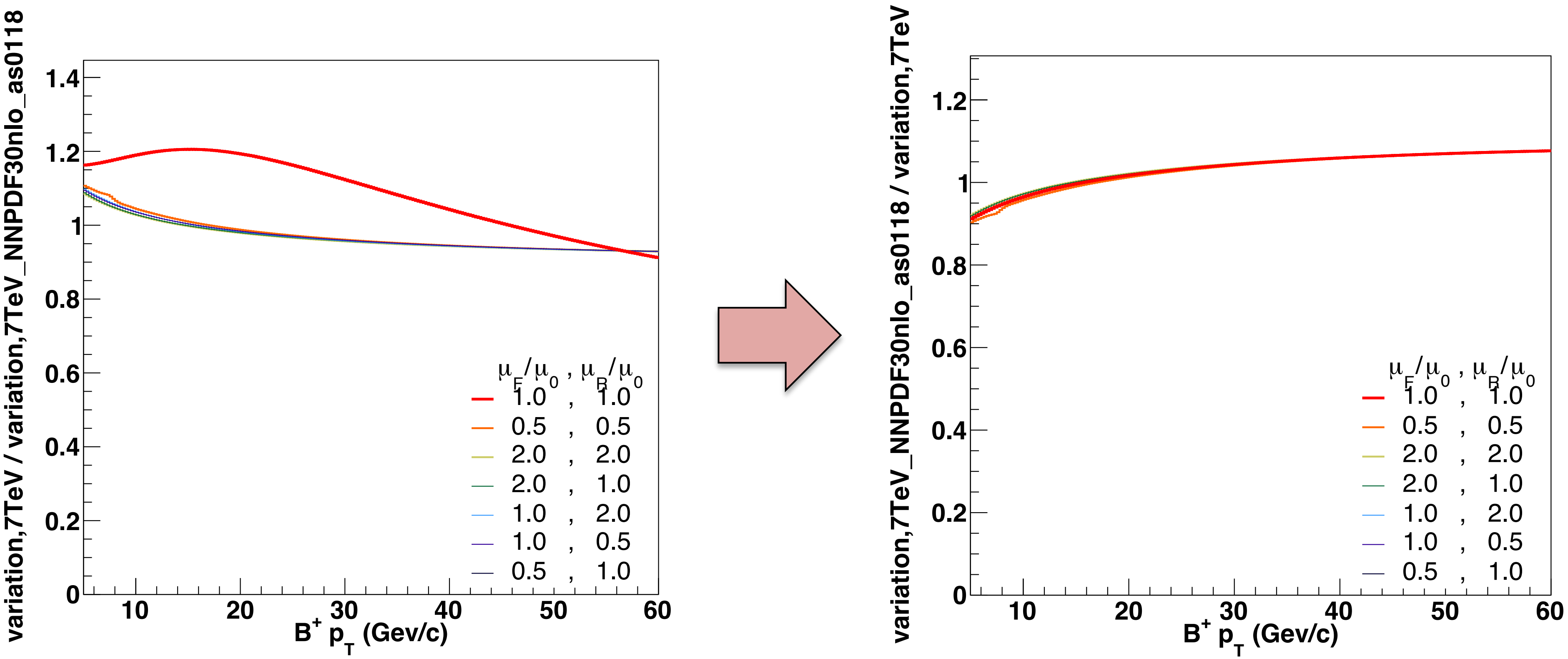
- Flat with uncertainties
- lower pT : pure FONLL uncertainties is dominant
- higher pT : error from data is larger than from FONLL

See results in one canvas



- **No change our physics message**
  - Nuclear modification factor over FONLL assumption is flat within uncertainties

Bonus – Correct bug of FONLL calculator



- **Correct the bug that central value was NLO calculation not FONLL**

Decision about pp reference

- **Conclusion**
  - KEEP the pure FONLL reference
  - Add a sentence (or paragraph) mentioning data-driven reference as a cross-check, but not to display that in our paper
  - Add the detailed contents in AN
- **Reason**
  - RpA central values are not strongly affected, though the uncertainties on the reference might be reduced at lower pT region
  - Data-driven reference need more work to be confirmed
    - Varying the fragmentation functions(PDFs)
      - Corrected the bug by Matteo Cacciari with our report
    - Allowing the ratio of 5/7 TeV as a function of pT to differ from FONLL
  - New reference doesn't change our physics message
  - We don't have enough manpower and time to investigate and convince this reference
- **Strategy**
  - After update the paper and AN with comments about the reference and other issues, we will go to CWR

Actions after ARC discussion

- **Remove the sentence about J/ψ mass constraint**
  - Because of J/ψ mass resolution (15~50MeV), it might not be the real affected cut
- **Add the rapidity range in figure of paper and sentences including rapidity boost in pPb collisions**
- **Update the results with new BR fraction and uncertainties from 2014 PDG information**
- **Keep the results as sigma, not BR x sigma**

Needed todo list for update

- **Paper**
  - Add wording to clarify dominant background source
- **AN**
  - Add the contents on data-driven reference
  - Answers for questions on Tag an Prove parts (and update if needed)
  - Add a sentence with the explanation about the uncertainties on acceptance and efficiency
  - Implement the definition of significance
  - For data/MC comparison, add more words after applying optimized cuts
- **Finalize in this week and circulate again (and then go to CWR)**
- **Twiki : https://twiki.cern.ch/twiki/bin/viewauth/CMS/ARCCommentsHIN14004PaperV0**

Backup

Raphael and Jim's comments for reference

- **Raphael**
  - Ok, since we are probably about to add a reference extrapolated from data, I also wanted to make up my mind about how to present the new reference, before to see it. What I would do is to show the extrapolated reference, together with the full FONLL on x section figures, that is 2 and 4 (left). On figure 2, it will not be readable and thus I suggest adding a subpanel in which we put both the DATA/FONLL and DATA/ Extrap. The DATA/ FONLL being the RpA we have on the other figures, I actually wonder if we should even have the RpA figures at all. I personally like, when we do not have the data to make an RpA, not to show a RpA (as done for the W paper for instance, in which the reference is nevertheless quite well controlled). This is only an opinion, to be discussed.
  - Side note: If we remove the RpA plots, then I'm sure we can add the new reference description and fit in a letter.
- **Jim**
  - I agree heartily with Julia's comment that we should not move forward without a pp data basis, even if this is the first B reconstruction in pPb collisions.

For implement of background study

- In the paper you describe briefly what MC you use to study background. I am missing the discussion of sources of peaking B background, particularly present under the B0. Keep in mind, that dependent on the source of background due to limited knowledge of the production strengths(fragmentation ratios), there is quite an uncertainty in such contributions. I suggest for completeness that you name the dominant sources that you identified and present a systematic uncertainty associated with these estimates
- As a minor point: I think your best candidate selection is based on highest chisquare probability (line 87)

For J/ψ mass constraint

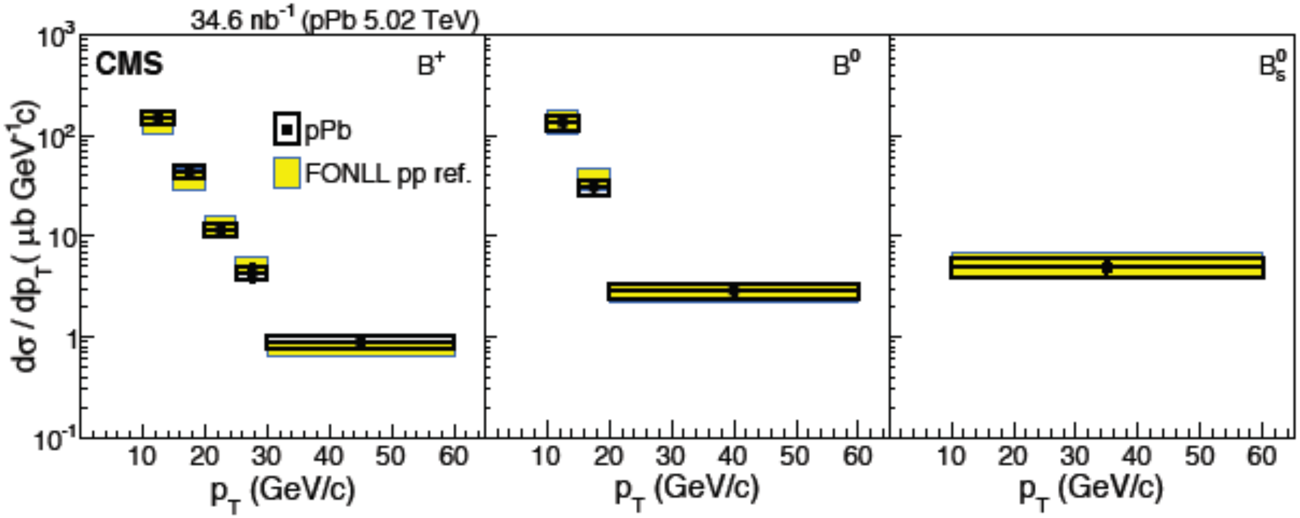
plied in the B^+, B^0 and B^0_s analyses respectively to reduce the combinatorial background. A kinematic fit is performed with the two muons and the tracks, constraining the invariant mass of the J/ψ candidates to be equal to the world average value [43]. Various selections are then

- **1/ (line 82). Though it probably does not matter much, isn't constraining the invariant mass of the J/psi candidates (that are already within 150 MeV maximum away) to the PDF mass bizarre? The real dimuon measured mass should not be the J/psi mass, even for J/ psi particles, since we are dominated by resolution. Constraining it does not seem to me like "adding information". It would, if we had not already made a strict cut before, but we explicitly say we do. I think a referee could be surprised by that, and I suggest removing this technical detail, which I'm sure does not change anything in the analysis. (but we can discuss it)**

For description of rapidity range with boost

- **2/ Eq. 1 and Fig. 2. We do not divide by Delta y here, which we do in other papers. This is fine, but then, we have to be very clear on the figures, what is our rapidity range. Thus add on Fig. 2 |ylab| < 2.4 -> But this brings me to another important comment, which is that you did not describe the pPb boost anywhere, which should absolutely be done. At line 135, suddenly appear ycm, and it is not related to ylab. Look our past pPb paper, and pickup the boost description there.**

$$\left.\frac{d\sigma^{B}}{dp_T}\right|_{|y_{lab}|<2.4} = \frac{1}{2}\frac{1}{\Delta p_T}\frac{\left.N^{B}\right|_{|y_{lab}|<2.4}}{(Acc \times \epsilon)\cdot BR \cdot \mathcal{L}}.$$



For BR

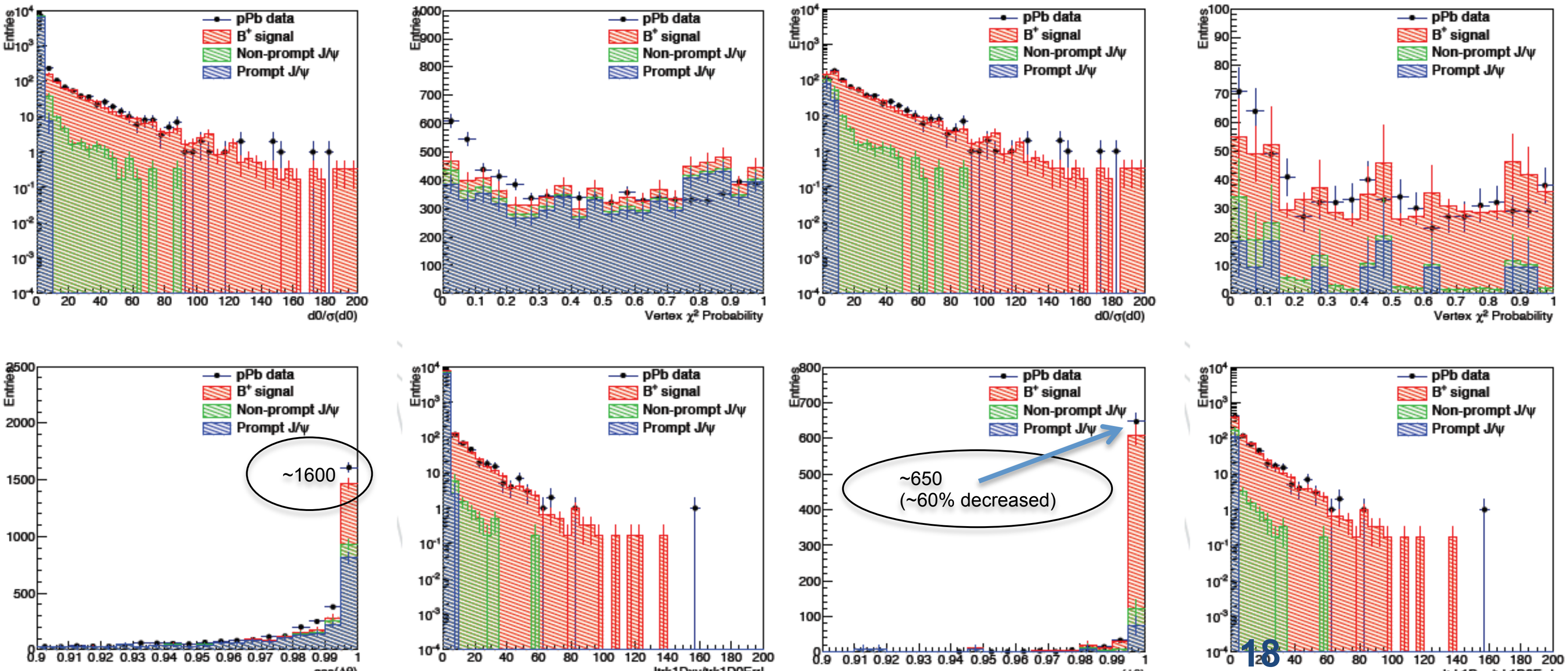
as the sum in quadrature of the different contributions. The global systematic uncertainties on the integrated luminosity measurement (3.5% [53]) and on the B meson branching ratios (3.2%, 4.6% and $^{+22}_{-24}$% for B^+, B^0 and respectively [43]) are also considered.

- **3/ (line 120) I thought we discussed that already. What is the point of adding the BR in our cross section, in general? Shouldn't we show sigma x BR ? This is in particular stringent when one of the BR have such large (20%) uncertainty... This being said, the BR I find for Bs -> J/psi phi is (1.07 pm 0.09) per mil, with the subsequent decay also well known. Where does this +22% -24% come from?**
- **A : Those larger error(+22%,-24%) from Bs was from PDG in 2013. In recent version, that is significantly reduced(please see below PDG value). All the results for Bs is updated with new BR already. We will fix it.**

Γ_{27}	$B_s^0 \to J/\psi(1S)\phi$	$(1.07 \pm 0.09) \times 10^{-3}$	1588

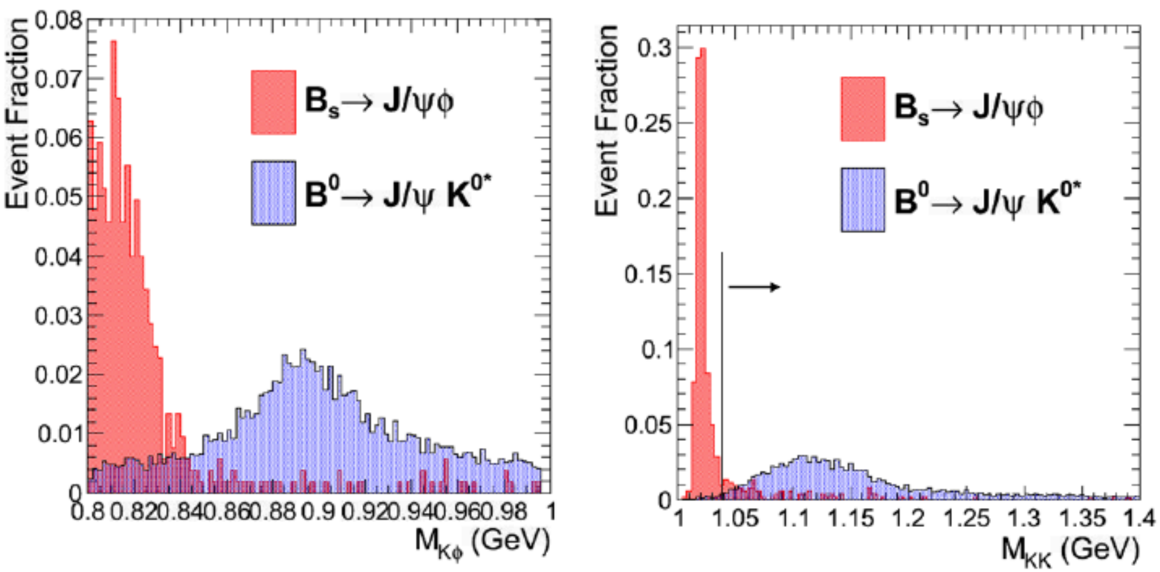
For Data/MC comparison

- **Between Fig. 21 and Fig 22, meson cuts are applied. This cleans up the discrepancy in cos(Delta theta) and improves vertex chisq, but removes 60% of the apparent signal peak in the cos(Delta theta) plot. There is no discussion of this in the AN. What cuts produce such a big effect?**
- **A : Most of removed components is from prompt J/ψ. Prompt J/ψ components is cut by d0/d0Err condition(top,left plot). So we think d0/d0Err would produce this effect.**
  - Add more explanation and documentation



For quality cut for track

- **The cut optimization seems to be arbitrary. The phi rejection cut, displayed in Fig. 23, shows what appear to be equal populations of B_s ->J/psi phi and B^0 -> J/psi K* in the cut choice plots. However, in data the cross sections for these two channels are not equal. Shouldn't the cut be optimized for a signal retition/expected background rejection basis?**



ϕ rejection: $M_{KK} > 1.04$ GeV

A : Yes it is a bit arbitrary in that sense. But we don't think varying the cut again is a good idea since we have finalized the full analysis with this cut. Additional variation may cause some slight bias. The signal inefficiency introduced is small.

Figure 23: Invariant mass of the track-track systems of B^0 candidates under the $K\pi$ (left) and the KK (right) mass hypotheses for genuine $B^0 \to J/\psi\, K^*(892)^0$ decays and wrongly reconstructed $B^0_s \to J/\psi\, \phi$ decays.

For significance of quality cut

- Some of the statistical significance plots used to justify cut selection look very strange to me, e.g., Fig. 28, 29, and 30. There are variables shown for which the significance changes plotted vary at the 0.1% or less level. How are these significances defined, and how does one decide when a given change is significant? Usually, these plots would be presented with an arrow to indicate where the cut is placed.
- A: The significance is from s/sqrt(s+b). TODO: add more detailed description

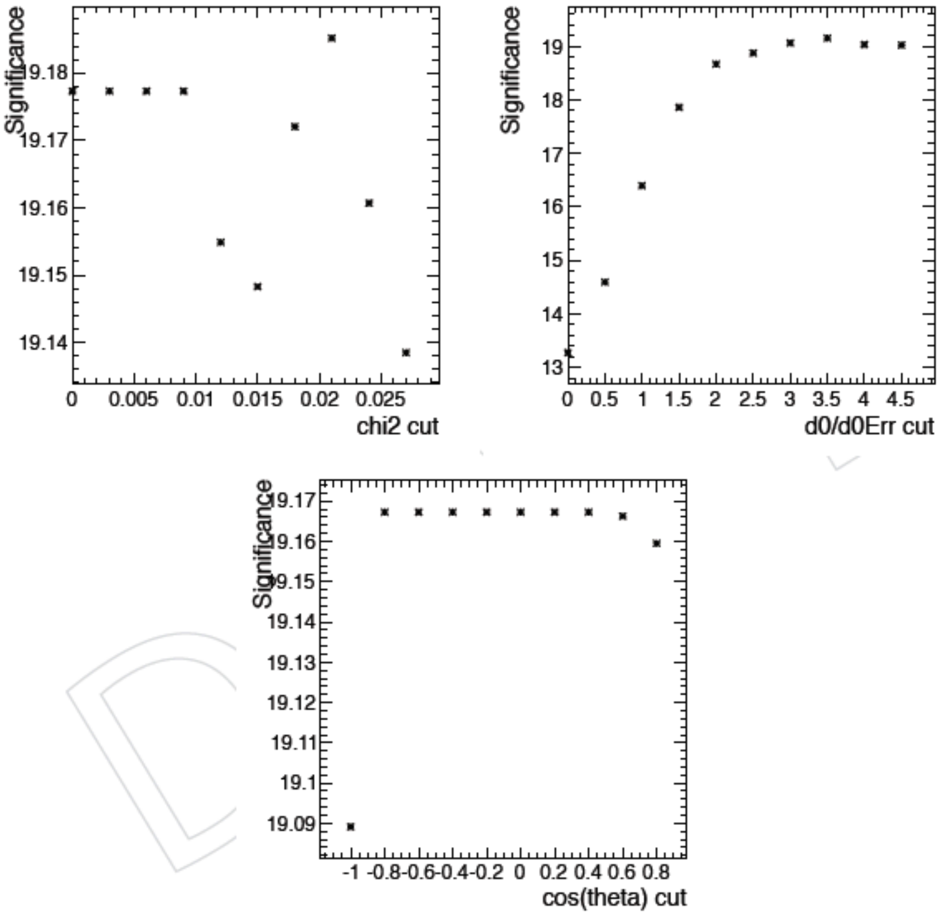


Figure 28: Significance as a function of cut in B^+.

For background estimation

- Section 5.2 states that a linear background function is justified by data and MC. The MC plots in Appendix C have smooth background on the high-mass side that extrapolates under the peak and matches the low-mass contribution from non-prompt J/psi. HOWEVER, partial reconstructions do not seem to be included in the MC, and the fit procedure seems to be based on making a linear fit only to the high-mass side. Because of partial reconstruction contamination of the low-mass side of the B peak, making a linear background fit to both sidebands is generally viewed as incorrect in hadronic B-decay analyses. In Fig. 31, 32, and 34, the linear background does not represent the high mass side well at all. It is pulled up above the mean level on the high side and does not agree well with the points on the low side. The effect is the UNDERESTIMATE the B yields by an amount that exceeds the statistical uncertainty quoted on the yield. I think that what has been done is wrong.
- A : Partial reconstructions are included in the MC which create the peaking background in the low mass region. Are you referring to additional peaking background in the high mass region?

For acceptance

- Section 6 describes the acceptance calculation. Tables 12-14 quote acceptance measurements with an uncertainty. What does that uncertainty represent? The MC statistics? I am not prepared to believe without a great deal of persuasive discussion that you know the acceptance in any bin for any state at the 0.1% level. Are these uncertainties ever used in any calculation?
- A : Yes, the uncertainty in those table is just statistics from MC. The reason of low statistical uncertainties of 0.1% level is we generated 100M events for this study. And this uncertainties is not used for any calculation not only for main results and but also for systematics

Table 12: Acceptance value for B^+ with rapidity and p_T binning

$y(J/\psi)$	$p_T(J/\psi)$ [GeV/c]	Acceptance
$-2.4 \leq y < 2.4$	$10.0 \leq p_T < 15.0$	0.246 ± 0.003
	$15.0 \leq p_T < 20.0$	0.424 ± 0.007
	$20.0 \leq p_T < 25.0$	0.513 ± 0.013
	$25.0 \leq p_T < 30.0$	0.647 ± 0.019
	$30.0 \leq p_T < 60.0$	0.721 ± 0.021

For efficiency

- Same question for the uncertainties quoted on the efficiencies. What do the quoted uncertainties represent? Are they used in the cross section error propagation? If you generated an infinite number of Monte Carlo events, would these uncertainties go to zero?
- A : Same as acceptance parts. Also those statistical uncertainties is not used for main results. So as we thought if there is infinite number of MC, we expect those uncertainties would be go to zero.

Table 15: Overall efficiency in different channel for candidates in different p_T, y_{CM} binning in pPb MC.

- Channel/p_T	10–15	15–20	20–25	25–30	30–60
$B^+ \to J/\psi K^+$	0.372±0.003	0.453±0.006	0.492±0.009	0.524±0.014	0.512±0.014
$B^0 \to J/\psi K^*$	0.184±0.004	0.253±0.006	0.285±0.007		
$B^0_s \to J/\psi \phi$	0.284±0.003				

For T&P

- I don't understand the role of data uncertainties in the systematic discussion. In Fig. 50 and 51, are the uncertainties in each data point too small to be shown? These uncertainties influence what one can conclude from the plot.
- A : Even if higher pT region, statistical uncertainty is up to 0.3%

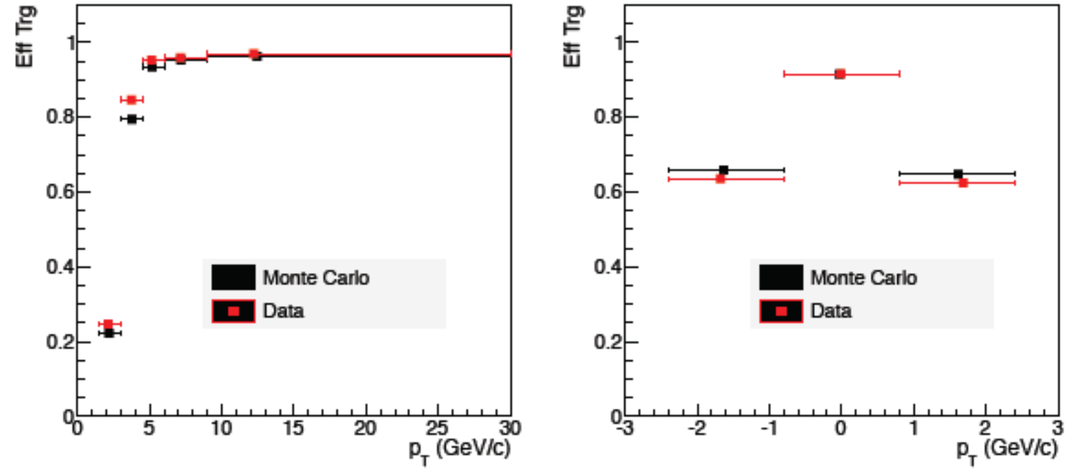


Figure 50: (Left) Muon trigger efficiency as a function of the muon transverse momentum in the eta range $|\eta|$ <2.4. (Right) Muon trigger efficiency as a function of η in the p_T integrated range 10-60 GeV/c.

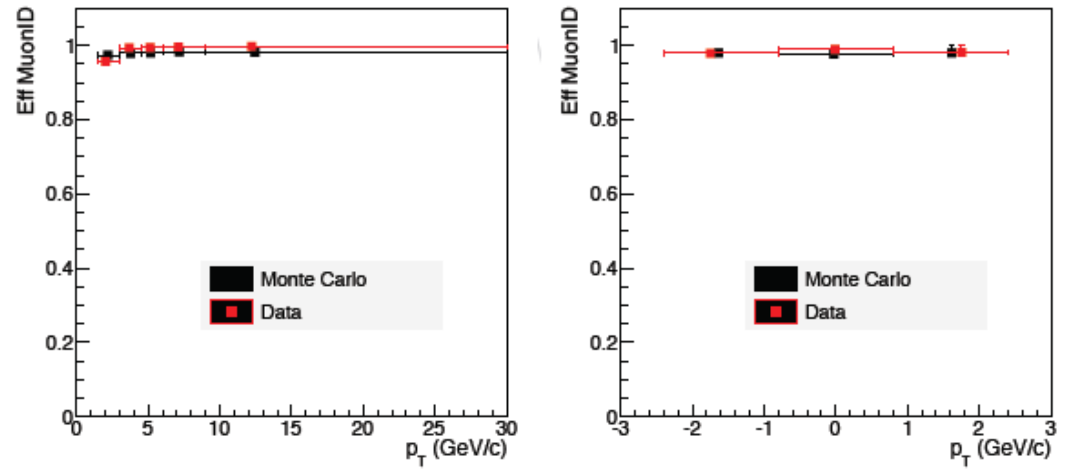


Figure 51: (Left) Muon ID efficiency as a function of the muon transverse momentum in the eta range $|\eta|$ <2.4. (Right) Muon ID efficiency as a function of η in the p_T integrated range 10-60 GeV/c.

For T&P

- In a related vein, I don't understand the systematic uncertainty assessments in Tables 19-21 because there are no uncertainties on the mean values. Take the following example. I do a T&P study of data in a bin with two events. I find one of them, so = 0.5. MC with 100K events says that the expected efficiency is 0.487. Does this mean that the T&P systematic is .013? Of course not! You can only test the T&P to a level limited by the data uncertainty. In my opinion, all these systematics are wrong.

Table 19: Trigger efficiency systematic uncertainty due to Tag and Probe correction for B^+ vs p_T

p_T bin (GeV/c)	(10,15)	(15,20)	(20,25)	(25,30)	(30,60)
$<\epsilon^{Data}(\mu_1,\mu_2)>$	0.972	0.990	0.992	0.997	0.994
$<\epsilon^{MC}(\mu_1,\mu_2)>$	0.962	0.985	0.988	0.995	0.992
$<\epsilon^{MC}>/<\epsilon^{Data}>$	1.011	1.005	1.004	1.002	1.002
Syst. Uncert.	0.011	0.005	0.004	0.002	0.002

Table 20: Trigger efficiency systematic uncertainty due to Tag and Probe correction for B^0 vs p_T

p_T bin (GeV/c)	(10,15)	(15,20)	(20,60)
$<\epsilon^{Data}(\mu_1,\mu_2)>$	0.973	0.986	0.995
$<\epsilon^{MC}(\mu_1,\mu_2)>$	0.962	0.980	0.992
$<\epsilon^{MC}>/<\epsilon^{Data}>$	1.012	1.006	1.002
Syst. Uncert.	0.012	0.006	0.002

Table 21: Trigger efficiency systematic uncertainty due to Tag and Probe correction for B^0_s vs p_T

p_T bin (GeV/c)	(10,60)
$<\epsilon^{Data}(\mu_1,\mu_2)>$	0.979
$<\epsilon^{MC}(\mu_1,\mu_2)>$	0.970
$<\epsilon^{MC}>/<\epsilon^{Data}>$	1.009
Syst. Uncert.	0.009