Comments to version 4.6

Hannes (1. Dec)

- DESY affiliation wrong
  - corrected
- 43 this reference does not tell anything, remove.
  - removed
- 44 non-perturbative or nonperturbative
  - Changed to nonperturbative everywhere
- 49 Ref 4,5 are very old, the early papers… we should refer to P8 etc.
  - changed
- 52 add photon TMD and 4 flavor paper
  - these two references moved to line 88 " …along with TMD distributions [18}").
- 70ff "The results … do not aim …" replace with: "The results aim to provide …"
  - changed
- 113ff section numbering is wrong
  - corrected
- 190 ff: should change a -> a etc
  - changed
- 195: "... plays the role of nonpert …" -> " … is the nonpert… "
  - changed
- 207: add "A detailed discussion on the role of soft gluons and the nonperturbative Sudakov form factor is given in Ref. arXiv 2309.11802"
  - done
- 211 space PB -approach
  - changed
- 447 add: "The new TMD distributions are available in TMDlib and TMDplotter"
  - LF: add including the new q_s value
  - changed
- 466 "...give the effect of …" " … describes the non-perturbative …"
  - changed
- 489 remove "These other …."
  - changed
- 496 remove: "It will be …. "
  - changed
- There are double references
  - changed
- Unify references

- done

Mees van Kampen (Dec 4)

Thank you for this draft, it looks very good. I have read it. Here are a few comments:

- Line 33, PBmethodology needs a space.
  - Ok, changed
- I find the notation of the evolution variable as q' not very instructive since it looks (already) as a momentum scale, while only with an ordering relation we give it the appropriate physical interpretation. I would call it \mu', in DGLAP style.
  - It is because q is a vector
    - Include k and q are 2-dim vectors
- In line 231, in parentheses it says "compare Eq. (1)". With what should Eq. (1) be compared? To me it is not clear why there is no intrinsic kT distribution for charm quark by looking at Eq. 1. I think the comment in parentheses in lines 247 and 248 is better and applies here as well.
  - What should be compared, is that the intrinsic kt comes only, when there is a starting distribution, this can be seen in Eq1.
    - Removed the reference to eq.1 (since there it is also not clear that heavy flavors are not included)
- In Fig. 4, I think that the blue curve (without intrinsic kT) is not correctly shown in the ratio plot since it deviates strongly from the red uncertainty band and falls within the band in the ratio.
  - It is the ratio to each of the central values, changed. Insert a sentence to indicate that the blue line has no uncertainty
    - Drop removed, new plots

Francesco Hautmann (Dec 5)

- below is a revised abstract

  The Parton Branching (PB) method describes the evolution of transverse momentum dependent (TMD) parton distributions, covering all kinematic regions from small to large transverse momenta k_T. We perform a detailed investigation of the PB

TMD methodology at next-to-leading order (NLO) in Drell-Yan (DY) production for low transverse momenta.
This region is very sensitive both to the contribution of the intrinsic motion of partons (intrinsic k_T) and to the resummation of soft gluon taken into account by the PB TMD evolution equations. We present the extraction of the nonperturbative ``intrinsic-k_T'' distribution from recent measurements of DY transverse momentum distributions at the LHC across a wide range in DY masses, including a thorough treatment of statistical, correlated and uncorrelated uncertainties.
We comment on the (in)dependence of intrinsic transverse momentum on DY mass and center-of-mass energy, and on the comparison with other approaches.

- LF: the weak point of the first parag of the abstract is that it gives the impression that we determine the TMD in an absolute way, while we could have made different choices of values of q_o or have chosen more complicated functions and it would still describe both DIS and DY data. What is nice in our choice is that it is economical and intuitively nice. But it is not the only way.
- Changed the sentence: A study applying the PB-method shows the importance of very soft gluons to both the integrated as well as TMD parton densities.
- Otherwise left the abstract as is.

- typo: "soft gluon" - - > "soft gluons" in the 3rd sentence of the abstract.
  - Hm,... didn't find this
  - LF: (in his abstract suggestion)

- below are comments to section 1. F
  - - insert general TMD reference
    - \cite{Angeles-Martinez:2015sea} in the sentence at line 61-62 (for instance, insert the reference after TMDs, or at the end of the sentence)
      Ok, changed
  - - insert NP sudakov reference
    \cite{Hautmann:2020cyp}
    at line 74 after "nonperturbative Sudakov one"
    Ok, changed
  - - line 104: "dependency" - - > "dependence"
    Ok, changed

- - beginning of line 114: remove "in Sec. 3" (this is still in Sec.2)
  Ok, changed
- - end of line 114: Sec. 4 - - > Sec. 3
  Ok, changed
- - line 116: Sec. 5 - - > Sec. 4
  Ok, changed
- - insert sharper punctuation and slight rewording in the sentence at line 44-47:
  The very low p_T region of the DY cross section is sensitive to the contribution from the non-perturbative transverse motion of partons inside the hadrons; additionally at low transverse momentum multiple soft gluon emissions have to be resummed; at larger transverse momenta perturbative higher-order contributions become dominant.
  - Ok, changed

- small corrections in Sec. 3:

- - line 304: insert a comma after "parameters"; also put the footnote after "parameters" (rather than at the end of the sentence); to avoid repetition, reword "illustrating the importance of evaluating . . . " - - > "underlining the role of evaluating . . ."
  - Ok, changed
- - line 311-313: the phrase "through the TMD multi-jet merging technique" was inserted at the wrong place. It should be removed from line 311 and should be inserted instead in line 313 after "matrix element".
  - Ok, changed

- a typo in Sec. 4:
  line 451 - "used to describe" - - > "used it to describe"
  - Ok, changed

Louis Moreaux (annotated pdf file):

- 332-334 “In order to avoid sensitivity to the collinear parton distribution, we normalise the prediction in each mdy-bin ot the integral of the measurement in that mdy-bin for the range in pr(ll) under consideration (shown in Fig. 7).”
  - -> “NO”

    - Sentence removed
- Comment on bottom p8
    - I did not understand his comment and what he wants to change
- P11, footnote: “bug in the treatment of QED radiation in Rivet”
    - Bug in Rivet or our routine?
        - It was a bug in Rivet how to treat radiated photons when using Pythia6. I guess, how we write it is ok.
        - Give Rivet version, from when on this is correct
            - Corrected in version 3.1.8
            - Changed
- More experimental in the intro
    - Hm, don’t see which experimental infos are missing
- Cross talk between section 2 and 3
    - It is the discussion on nonpert Sudakov and intrinsic kt
    - Give a sentence befor the conclusion
- 96-97: sounds like the conclusions of the paper;
    - 168ff “All PB TMD parton distributions (and ..)
        - Sentence at a wrong place
            - Moved to the para above
- Fig1 Format style error
    - corrected

Natasa Raicevic:

- I see that Marius Ambrozas is not in the acknoledgement as we discussed at some point.Please, add him.
    - Ok, changed

Ola Lelek:

Physics:

- 128-129: at the initial evolution scale we do not have heavy flavors. How could they have intrinsic kthen? Am I missing sth?
    - Reference to eq 1 removed
- 207-208: My question about references 41 and 42: isn't the inconsistency there originating from the inconsistent zmax between forward and backward evolution? I.e. PDFs are obtained with DGLAP with zm=1 and PS is done with dynamical zmax? What if both PDFs and PS would have dynamical zmax, would it still be inconsistent in the sense of references 41 and 42?

    - It seems so. In Ref 41 only the inconsistency of the pdf with the parton shower is discussed, and in ref 42 a method is proposed to correct the hard scattering to include this effect. I am not sure, whether this will solve the problem, and whether with the method of 42 one has still no dependence of intrinsic kt on sqrts, since the non-pert Sudakov has a scale dependence and resummation….
- 210: instead of saying "not part of the collinear calculation" isnt it better to say that since in eq. 4 we have normalized gaussian, dependent only on kt, it does not affect collinear distribution?
    - I don't think it is because of the normalized gauss, it is that the kt does not play a role in the collinear distribution, no matter whether the gauss is normalized or not.

- Fig2:
- 213-214: blue is PBset2 without intrinsic kt and red is PBset2.
    - Yes, right, corrected
- 217: blue curve -> purple curve
    - Yes, right, corrected
- 218-219: whats the sense of talking about distributions without intrinsic kt, if its important both for the models with and without non-perturbative sudakov? I.e. how good description of low pt region in DY do you get with PBset2 and qs=0? Not good at all I believe. And if you include physical, reasonable intrinsic kt then both models, with and without non-perturbative sudakov, have reasonably looking TMDs and can describe the data.
    - The reason to show curves w/o intrinsic kt is to illustrate the importance of the non-pert Sudakov.
- 220-221: in my opinion Fig. 2 shows very clearly that also perturbative region of the TMD, i.e. k_T>q0, is affected by the choice of zM.
    - Perhaps yes, but the largest effect is in the low kt region.
- Fig.2: why is charm on this picture? It just shows that since we do not have charm at mu_0, it doesn't have intrinsic kt contribution? See my comment to line 128-129.
    - Yes, true, it is just an illustration. But it is important to show, because charm and bottom contribute quite significantly to Z production.

- 227: it falls so low only because you show it without intrinsic kt. BTW, notice that, even at large scales and with zm=1, the effect of intrinsic kt is still very visible at large x
    - Yes, exactly, but we do not show it at large x
- 229-231: why is it interesting and not obvious?
    - There could be still an effect, since the charm is generated by gluons, and gluons have intrinsic kt.
- from Fig.2 we can of course see that there is a difference between fixed zm and dyn zmax (as from fig.1) but we cannot say which model is better based on this figure (and I believe this was the initial idea behind inserting it?)
    - The whole argumentation is, that using zdyn only is just wrong, because major parts of the pdf, which is needed for proper subtraction in the NLO xsection, is removed. So it is not a question which approach is better, but which one is wrong.
- 241: its not only the low kt region which is affected by the choice of alphas (the differences are the biggest there but also higher kt region is affected, especially for gluon or larger x)
    - Yes, but here we concentrate on the low kt region. The effect in the high kt region is smaller.
- Fig 3 and its description in the text: the same comment about charm as above
    - See comment above.
- Fig 3 and its description in the text: you don't show ratio, and if you would do it for larger x, e.g. x=0.05 or 0.1, or for gluon, you would see differences between PBset1 and PBset2 also for higher kt. Its just because you show log-log axis, no ratio and particular selection of flavor and x that you see a difference only at very small kt
    - Yes, a ratio plot would be misleading. Here we see differences of orders of magnitude.
- Fig4: again my comment that for very large x you would see an effect of intrinsic also at high scales (but of course this does not affect DY at CMS so for justifying the fact that at high scales at CMS we are not sensitive to qs this plot is good)
    - Again, we want to concentrate on the essential points. You are right that there are also effects at large kt… but they are smaller and not the topic of this study.
- Fig.4: It should be explained what the ratio shows. Its not blue/blue and blue/red. Its blue/blue and red/variation (down, up and central

value) of intrinsic kt in red, thats why both central values of red and blue are exactly at one. Then, what exactly do we learn from this ratio part of the figure? Is the envelope really needed?
    - Yes, very good point, it will be explained what the ratio shows. The point to show the ratio is, that the uncertainty does not cover the difference to the curve without intrinsic kt.
- 366: extract independent values of what?
    - Yes, we should say of q_s, corrected
- 371: why "reduced"? because pt is cut?
    - Ahh, we should say, that reduced chi^2 is chi^2/ndf, good point, corrected.
- 383: "followed by the two regions around it"--> isnt precision of the fourth mass window better than the third one?
    - Good point, yes. We should say by the regions around it (remove the two), corrected
- Fig.11 description around lines 425: Fig 11 shows also phenix and E605 what is not mentioned in the text
    - Yes, we should mention Phenix and E605 in the text, changed
- Fig 11 caption: shaded band shows chi2 variation of one unit for each data set --> shaded band shows chi2 variation of one unit for each data set + and from the step size in the qs scan?
    - No, it is chi2+1. In the uncertainty there is the step variation included.

Notation:

- 144-146: two different notations, A_a(x,k,mu_0) and A_{0,a}(x,k_T^2,mu_0) (i.e. the subscript 0 and bold vs scalar notation for kt).
    - Yes, changed to |{\bf k}|
- Eq.4 is k_T vector or scalar? If scalar then replace |k_T| --> k_T
    - Changed
- PB-NLO-2018 Set2 or just Set2? these two are used interchangeably in the text throughout the paper
    - Changed to always PB-NLO-2018 Set2
- 221: qt> q0 --> k_T > q0.
    - Yes, changed

Typos:

- 33: PB methodology
    - changed
- 151: branching variable --> branching variables

    - Right, we have q and z as variables, changed
- 238: too large a cross section --> too large cross section
    - We believe the wording is correct
- 371: shown in Fig.
    - Yes, changed