From: **Max Klein** mklein@hep.ph.liv.ac.uk
Subject: [h1zeus-herapdf-nnlojet] comments max klein
Date: 27. August 2020 at 20:35
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MK

Comments on V0.5 - Max Klein

Dear Friends

many thanks for this paper, another culmination of decades of work on PDFs.

I am sorry that this comes late but had told Matthew I may have difficulties to comment on the short deadline set.

It is very important to see that jets and inclusive DIS are compatible, and the small DIS alphas value remains a puzzle. We saw that in 2000 when he had made a huge effort to extract the coupling, by reducing the PDF influence with a minimalistic parameterisation and when we studied for very long how to use BCDMS which now isn't so important owing to our jet data [that paper has been the most cited by H1 hep-ex/0012053] .

I find the name HERAPDF2.0Jets NNLO rather 'barock' not nice. I understand the history and the desire to provide all details with the name, but are they all needed? what about HERAjets and HERAincl, these names will be used in PDF context only anyhow.Did we have a version 1.0 for/with jets, not sure; the order in QCD could be provided in text and captions as is done already (fig 2 e.g.) This could also be done, for example, in fig 3.
I expect you do not change this, cna live with that, but wanted to state it.

Figures:

2b,c it would be more instructive if you chose the same y scale, say 0-15
for both plots. to use 0-50 makes c) even more shallow than it is wrt b)

Fig 3: you plot xf(x,Q^2) which was fitted using Q2 as the factorisation scale.
It is in my view better to write Q2=10 GeV2, even if previously we did it like this,
while before that time we wrote Q2=.. Q2 is used is in many areas and
DIS papers before and after.

more important: a really nice plot and result!

caption: delete at the value muf= 10 (sounds like that has sth to do with alphas)
and it is in the plot. (here and elsewhere).

Fig 4: delete: the PDG value and muf=10.. (PDG may change and is not important here)

Fig 6 caption: .116 --> .1155

Fig. 7 dashed --> dotted

    this gives the impression as if nothing happened through jets, but it did
  - by fixing alphas to 0.1155 instead of 0.118, so we know better.

  - a particular question regards the gluon at large x. refering to an email
    by Mandy and Katarzyna from 14.8., they said :
it does seem that the jets make the high-x gluon a bit softer (as well as reducing uncertainty)

 the plot then attached compared xg inclusive with xg with jets. but the scale was chosen such that the blue line ran out at x=0.5. I propose to make and include a plot log xg (from 10-4 to sth like 20) as function of linear x (0-1) with these two gluons. then we would see a real difference. the large x behaviour of xg is very important and hard to get in inclusive DIS due to the valence (non-singlet) dominance.

Fig 8 not sure I like this: it shows 4 times that jets have no influence on the
    gluon uncertainty (very little) - is that the message?

Fig 9/10: if the cross section is indeed for a wide bin, then you do not plot
    dsigma/dpt but some delta sigma/delta pt. It would be much better to
    apply a bin center correction (probably in Q2 and pt) and then indeed
    plot a differential cross section. or it needs a different notation I think.

Fig 11 caption: this is not a 'differential cross section' cf the remark above. or?

    you have a jet x setion d2sigma/dptdQ2 what is exactly the cross section
    you normalise this to, what is sigma_NC?
    and then what is the unit of sigma/sigma_NC?

    you write only datafitted are shown, how does the fit describe the other data?

why do you show this ratio?

I think one should also include a plot with the inclusive data and how they are described. after all: that is a fit to both inclusive and jets. in the inclusive we had two puzzles: how we describe the lowest x data and how we reproduce the FL turnover. A kind of final, overall fit, I think, cannot be presented without illustrating the quality of the description of the NC data also.

your fig 2 indicates that the low q2 puzzle may have been gone??,
at least alphas is stable with q2min. since the alphas for q2 >3.5
is the same as for q2 >10, two questions: how does the uncertainty
reduce with lower q2 (one only sees this a bit in fig 2) and why
do you still limit the data to q2 >10 instead of 3.5 as we did
previously, do you have reasons to do that other than inclusive history?
after all, one of the indications of new lox physics was the chi2 variation
with q2min and one is interested to know whether including jets affects this
and how (maybe that is discussed and i overlooked it, sorry in this case)

Appendix A
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just reading this, it is not clear why that is here presented. IF you want to do an alphas discussion in earnest, then all the points here listed need to be really discussed/evaluated. My impression is that this 1 page list of subtleties is better either left out or indeed seriously discussed, but that could lead to 10 pages and change the paper (and its schedule).

Text

I am unable to comment on the text in detail for lack of time. I understand the EB meets tomorrow. Thus a few quick observations only, I will be glad to read a next version more carefully/thoroughly.

l32 i would not talk about a family here, maybe set is better

l33 were based but still are, no? MSbar

l35 represents the completion --> complements
    [never they this is the end]

l37/38 delete An -- family.
    l38 when --> since

l50 delete highly or indicate why they are not consistent
    [what is meant here H1-ZEUS or/and inclusive+jets?]
    then say that because of the consistency we can use a straightforward chi2=1 criterion to define the experimental uncertainties

l51 I do not understand the statement that jet and inclusive data are consistent at NLO (or NNLO). if we want to say they can be consistently described in NNLO QCD then this belongs to the conclusion, not the introduction.

one expects at the end of the intro a guide about the paper structure and not the LHC philosophy which I would put to the intro beginning.

typo: the the line 234

l236 if the minimum would not have been the fit result, one would worry,
    so why do you write this?

l269 i do not understand the 'impossible comparison' logics: even if the scales were treated differently (which is anyhow more art than science) shouldn't one be able to compare the results, uncertainties etc?

i notice that then indeed you compare all (284/5) which is good

sorry to stop here. many thanks again for so much and good work!