Commnets on “Quarkonia suppression in PbPb collisions at $\sqrt{s}_{NN} = 2.76$ TeV”

GENERAL PROBLEMS WITH MANUSCRIPT

I. FIGURES

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Report of the Referee – CX10370/Kumar

First of all, I would like to apologize to the authors for the delay to produce this report.

This paper discusses the important topic of quarkonium (J/psi, Upsilon, Upsilon') suppression in PbPb collisions at the LHC. The suppression is calculated in a model which include various hot medium effects (gluon dissociation, recombination, pion dissociation) in addition to nuclear PDF corrections. The results are compared to ALICE and CMS measurements.

I list below several comments and points which should be addressed before I can recommend the paper to be published in Physical Review C.

III. NUCLEAR PDF (NPDF) EFFECTS

1. The authors use only the central set of the EPS09 parametrization instead of calculating the EPS09 uncertainty. Therefore it is difficult for the reader to assess the uncertainty coming from nuclear PDF corrections at the LHC. If it is numerically too difficult to compute the EPS09, they could use for instance another nPDF set, e.g. the recent DSSZ.

   We have this uncertanity band from the original calculations. We will include it in to the calculations of CNM effects.

2. There is little detail on how nPDF effects are computed. For instance, it would be interesting to know what is the hard scale used in the calculation. In particular, does it depend on pT? Also, it is not clear to me how the heavy-quarkonium production cross section is computed. I understand that heavy quark production is computed using Refs. [26,27] but one needs a specific model in order to compute the production of Q-Qbar bound states. Is it CEM?

   I have to search these details from Ramona latest papers.

3. The nPDF effects seem to have a small but visible dependence on the centrality of the collision, however the default EPS09 set has no impact parameter (spatial) dependence.

The authors should clarify the origin of this Npart dependence. I am also quite surprised to see that in the CMS kinematics, pT $\geq$ 6.5 GeV, the nPDF effects lead to an enhancement (due to anti-shadowing), I was not expecting this because of the rather small values of x_{Bj} typically probed at the LHC.

We have to find some reasonable response for this comment. Probably we have to scale it with T_{AA} instead of N_{Part}

4. This is a detail but it is more appropriate to mention 'nuclear PDF effects' instead of 'shadowing' which only applies to the small-x depletion of the PDF in nuclei (one can see the effect of anti-shadowing and the EMC effects in Fig. 5 left and Fig. 6 right). This is a very good suggestion. On figures we mentioned CNM Effects (Cold Nuclear Matter Effects) probably we should chnage the title of section 2.

IV. PION DISSOCIATION

1. The authors use an arbitrary cross section of 1 mb independently of the pion energy. They mention that this cross section is small, however this is very similar in magnitude to the gluon-quarkonium cross section, see Fig. 2. In order to have a consistent framework, the authors should rather use the pQCD quarkonium-pion cross section, directly accessible from the convolute the quarkonium-gluon cross section (Fig. 2) with the gluon distribution in a pion, see Refs. [16,33,34] (see e.g. Ref. [34] in which the quarkonium-pion cross section has a rapid variation with the pion energy).

   These are the referrences

   - G. Bhanot and M. E. Peskin, Short Distance Analysis for Heavy Quark Systems. 2. Applications, Nucl. Phys. B 156, 391 (1979).
   - F. Karsch, M. T. Mehr and H. Satz, Color Screening and Deconfinement for Bound States of Heavy Quarks, Z. Phys. C 37, 617 (1988).
   - F. Arleo, P. B. Gossiaux, T. Gousset and J. Aichelin, Phys. Rev. D 65, 014005 (2002) [hep-ph/0102095].

2. Apart from the energy dependence, the magnitude of the pion-J/psi and pion-Upsilon cross section should not be the same, the latter being smaller than the former by a factor 4-5.

   This will be automatically solved if we are able to use the p_T dependent cross-section.

3. I fail to understand to understand why pion dissociation is much less effective for Upsilon than for J/psi (compare Fig. 6 left and Fig. 7 left), since the cross section is taken to be equal. Is it due to the different p_T spectrum?

   probably yes to be checked

4. Also, what is assumed for the quarkonium(2S)-pion cross section? It should be significantly larger than the quarkonium(1S)-pion cross section. As a matter of fact, the comover effect seems more pronounced on 2S states (compare Fig. 7 left and right) but nothing is said on the comover effects on 2S in the model.

   right now we are using same cross-section for both states. It is mass effect. Mass of Υ(2S) is significantly larger than Υ(1S).

V. HYDRODYNAMICAL EVOLUTION

1. The authors assume a transverse expansion of the medium, see Eq. (8). In non-central collisions, however, the expansion should depend on the azimuthal angle, leading to non-negligible elliptic flow. I think the authors should comment on that.

   There will be azimuthal-anisotropy with respect to reaction plane. But this effect will be averaged out because large number of collisions. We do not consider this effect.

VI. COMPARISON TO DATA

1. In order to have a meaningful comparison to data, I think the authors should try to quantify the uncertainty of their calculation. For instance, the most important effects in their model are gluon dissociation and recombination, which crucially depends on the quarkonium-gluon cross section. How would the predictions vary if they change the magnitude of this cross section (which is poorly known) by, say, a factor 1.5 or 2?

It can be implemented easily by hnad. We should check the effects on the final R_{AA}. We will check it by multipling by a factor of 1.5.

2. Similarly, the magnitude on the medium energy density could be varied in order to give a feeling to the reader on the corresponding uncertainty on R_{AA}. Clearly the calculations are done for one set of parameters and assumptions, it's difficult to know how the predictions would change when those are varied.

   We calculate the initial temperature using measured charged particle density and assumingτ_0. We can change it to 0.2 and 0.4 fm and see the effect. May be we can put a uncertanity band for this.

3. In this respect, many effects are included... but with uncertainty which are not quantified. Since the effects of nPDF or comover affect R_{AA} by at most 20%, I think it would make more sense not to include them at all. In particular, the authors include effects with magnitude 10-20% while other cold nuclear matter effects such as energy loss in nuclei prove much stronger (see e.g. 40% effects on J/psi in arXiv:1407.5054) but are not included.

   Have to be seen in the reference.

4. Also, the authors do not discuss the dissociation of heavy-quarkonium due to the Debye screening of the heavy-quark potential at finite temperature, which is historically the main effect discussed in Ref. [1]. Do they consider it to be negligible? This point would need to be discussed.

   There are two alternative mechanism for quarkonia dissociation, color screening and gluon dissociation. Since the gluon spectrum becomes harder with temperature so some effect of color screening is already included. We are varying the $\sigma_{\rm Diss}$ to include the corresponding uncertanities.

5. Fig. 6 left and Fig. 7 left are a bit misleading because data points corresponding to two different kinematical regions are included, but only one calculation is provided and it is not possible to know which region it corresponds to.

   We can include rapidity dependence of energy density by including rapidity dependent gluon distribution.

6. The authors present a model for Υ(2S) for not for ψ(2S). I understand that for charmonia, and in particular 2S states, the pQCD cross section may not be valid but still it would be very interesting for the reader to know the model predictions. In particular, the CMS experiment reported on a very interesting behavior of $\psi'/\mathrm{J}/\psi$ in PbPb collisions, and it seems natural to wonder whether or not the possibly large recombination in the ψ' channel could compare to these data.

   The ψ(2S) cross-section is not reliable so model prediction does not have much significance.

VII. COMPARISON TO DATA

1. Finally, many details or references are missing. In particular, why $\mathrm{R}_{0-5\%}$=0.92 R_{Pb}? What is the value of a_m? Which values are taken for m_c and m_b? It is also not clear to me why they need to compute the entropy S(tau) in Eqs. (10,11), I probably missed the point.

   We will give the value of a_m. It is 5. We will give calue of m_c and m_b. $\mathrm{R}_{0-5\%}$=0.92 R_{Pb} is calculated by assuming that in head on collision (b=0) the R is equal to the R_{Pb} which correspond to $\mathrm{N}_{\mathrm{Part}}$ = 416 (To be checked).

2. On page 6, the reference to an experimental paper on dN/deta is missing.

   It is very surprising that we miss this reference. We will give the refernce of ALICE and CMS. It is given on one place already for ALICE. We will also include the CMS also on both place.

3. On Fig. 3, what is the rapidity of the quarkonium states? Is it mid-rapidity? Maybe it would be more relevant to draw curves for a given quarkonium energy E instead of binning in pT. It would also be interesting to have the equivalent of Fig. 3 and 4 for 2S states.

   This is also realted to the forward rapidity question. We can include the rapidity dependence by including y dependent gluon distribution and by putting $\mathrm{p} = \mathrm{p_T}$ coshy in the formula of dissociation rate.

   see ref. arxiv:1005.1208 by authour U. Jameel and DKS.

$p^2 = p_T^2 + m_T^2 \cdot \sinh y$

In conclusion, it would be valuable to address/answer these various points in a revised version of the manuscript before I can recommend its publication in Physical Review C.

All the points mentioned in the reports are adressed. Please find the revised article attached.