Opponent's review of doctoral thesis of Ing. David Horák

„Measurement of ρ^0 photoproduction at high energies with the ALICE detector"

In this thesis author presents results of his work at the ALICE experiment on the topic of particle production in ultra-peripheral nucleus-nucleus collisions. In this case a production of ρ^0 meson is measured in Pb-Pb and Xe-Xe collisions at energies of 5.02 and 5.44 TeV per nucleon pair.

The thesis is rather long, but written in excellent English without notable grammar mistakes and hence reads very well which adds to the overall good impression. The text is well structured into 5 chapters and numerous appendices. Although the text is long it is in accord with the amount of presented information.

First chapter gives quite comprehensive overview of the experimental facility and data processing framework as well as discussion of planned upgrades. Second chapter describes theory and models of the particle production and accompanying processes in the ultra-peripheral collisions. This is in my opinion a chapter where additional text would be helpful. The actual physics motivation for performing this measurement, such as the predicted gluon saturation, is only touched upon. Similarly it would be interesting to discuss if and how the model predictions differ. Actually from the results section it seems there is not a large difference between the models in the region which is accessible by this measurement. Are there actually significant differences between the predictions of individual models which would be interesting to explore in the future measurements especially given the recent and foreseen systematic errors? Third chapter is a nice overview of previous results providing a useful framing for the results obtained in the next section.

The most important and longest of all is the fourth chapter which describes in detail authors own work on analyzing the data and obtaining the final physics results. The analysis procedures used for both data sets (Pb-Pb, Xe-Xe) are described in great detail clearly demonstrating the huge amount of work, depth of understanding of the technical aspects of the analysis as well as authors attentiveness to details. I should mention that on multiple places the technical detail may not be very accessible to non-members of the ALICE experiment when for example details such as pass or train numbers are mentioned (page 51). The thesis clearly demonstrates how well the analysis is under control. I especially like how the systematic errors are treated using multiple methods to crosscheck on the same source of the uncertainty. On couple occasions the author even comes up with new approaches to access the systematics. This makes the obtained results very solid. The last concluding section is a bit brief. It seems that the author had perhaps run little tired. There are some interesting topics mentioned such as the influence of shadowing and possible future measurements which could have been discussed little more. I have couple questions on the author which are included in the appendix regarding the analysis and results.

In summary the author has presented in my opinion indeed well written thesis on a very interesting topic which lead to two major paper of the ALICE collaboration. This is an excellent result which will be noticed by the scientific community. There is no doubt that author obtained original and valuable results of his own and demonstrated that he became an expert on the presented topic. He has definitely fulfilled the criteria for successful defense of the thesis.

Prague 14.4.2021 RNDr. Petr Chaloupka, Ph.D.

Appendix – Questions:

At one point in your analysis you combine data for positive and negative rapidity. Have you considered analyzing them separately and compare for systematic differences?

Since you vary size of binning of you data to check for systematic errors would it be possible to perform unbinned likelyhood fit and eliminate this source of systematic uncertainty?

On page 85 about obtaining number of candidates. I do not understand what you mean by setting N=0 in eq 3.4 and why do you restrict the integration limit for the BW part of the Sodingen function.

Looking at results on page 92 it seems that the STARlight model does systematically worse then the other models. However this models is the only one employed for all the simulations used to obtain corrections and normalizations. Did you try to use any other model (is it possible?) and could the disagreement between data and the STARlight model lead to any systematic errors in corrections.

The previous STAR results are compared to two models (GM and GDL, page 47) and GM seems to describe the data. Have you tried to compare your results to theses models as well?