

Dear Editor and referees,

We thank you for following up on the review of our manuscript and for the constructive critique, which helped to improve significantly our paper.

We acknowledge that the draft is not considered for publication in PRL and we decide to proceed with the submission to PRC, as suggested by you.

Nevertheless, we would like to address some of the remaining concerns in the answers below. We hope these might be useful in the review process at PRC.

With kind regards,

the authors.

Second Report of Referee A -- LW16156/Bellini

I am still of the opinion that the paper is insufficiently compelling to publish in PRL. The authors have improved their presentation, which makes the paper easier to digest, but this does not change the principal objection to publication. The 5 points delineated in the conclusions might motivate some technical improvement in coalescence treatments, but none of these points seem likely to generate any new understandings of heavy-ion collisions. Showing that two models give different answers is usually sufficient to warrant publication in PRC, but not PRL -- unless clarifying the differences provides significant insight into a critical issue of the field.

A: We acknowledge the comment and proceed with the submission to PRC. We thank the referee for taking the time to read our drafts.

Second Report of Referee B -- LW16156/Bellini

The revised version of the article is now much improved in its presentation. I still think that this paper is worth publishing in PRL, however, I am surprised that the replies to my points 1) and 2) of my previous report somehow did not make it into the new version.

In reply to my previous question 1) the authors point to a new paper Kai-Jian Sun, B. Donigus and C.M. Ko, arXiv:1812.05175, (Ref. 37), where the measured hyper-triton to lambda ratio is well reproduced by coalescence. In the reply the authors give (sensible) arguments why there is a apparent difference between their result, where the coalescence model give a B_2 which is almost a factor 10 off the data, while in the above work, coalescence seems to work. Unfortunately, this argument never made it into the manuscript. Why not plot another curve in Fig. 2 based on the assumptions of Ref.37. I would assume that the difference between the coalesce and the thermal + blast wave would still be large for small systems, so that the main point of the paper would remain intact. If not, well, bad luck but we still learn

something.

If the result of Ref. 37 is not properly discussed, any reader looking up Ref.37 would conclude that the whole idea is possibly based on some (uncontrolled) model assumption.

I am aware that Ref. 37 appear AFTER the first submission of this manuscript, but since it is now available, it needs to be addressed.

A: While proceeding with the submission to PRC, which has no word limit on the draft, we were now able to include more details on the comparison to the work by Kai-Jian Sun, B. Donigus and C.M. Ko, arXiv:1812.05175. In particular, we have addressed the comparison with their result by including a full discussion on the radius parameterisation in a new appendix and complemented the text with a plot (Fig. 4) that displays the possible radius-multiplicity parameterisations and compares to ALICE HBT data. The choice of adding one appendix (and not include these considerations in the main body of the draft) was motivated simply by the need to maintain a smooth flow of the paper in its main body, whereas the comparison advocated by the referee seemed to us to be more suited for a separate paragraph. A direct comparison in Fig. 2 is not feasible due to differences in the two approaches, as explained in the appendix. In addition to this, we have added to the draft our prediction for the $t/3\text{He}$ ratio, which had been previously sacrificed for space reasons. Our prediction can be tested directly with future ALICE measurements.

Also, the reply to my previous question 2) does not seem to appear in the paper. The argument given by the authors is convincing and should be included in the paper to avoid that other readers ask themselves the same question. This will not take more than a sentence or 2 or a footnote.

A: Thanks for the comment. We have now incorporated a new paragraph in sec. IV. "Production via coalescence could also be investigated by looking at the transverse momentum dependence of the coalescence parameter. However, the advantage of studying these effects as a function of multiplicity/centrality is that system size offers a larger lever arm. For a fixed p_T/A , B_2 changes by about a factor 50 going from pp to central Pb–Pb collisions, whereas B_2 changes by a factor two going from $p_T/A = 0.4 \text{ GeV}/c$ to $p_T/A = 2.2 \text{ GeV}/c$ in most central Pb–Pb collisions [35], and by less than a factor two in the measured p_T/A range in pp collisions [31, 32]".

Finally a cosmetic observation: The sentence in the first paragraph of Section V "The only data point available so far in Pb–Pb collisions is in agreement with the thermal+blast-wave model but differs by 6σ from coalescence, albeit the validity of our assumptions (in primis the usage of a gaussian wave-function)." Seems incomplete. "... albeit the validity of our assumptions..." Is what?

A: Thank you for the valid comment. We have rephrased the sentence to make it clearer.

In conclusion, I think this paper is still worth publishing in PRL, provided the above comments are taken into account and assuming that the approach of Ref. 37 still leads to a large difference in B_2 for

hyper-triton for small systems, as I would expect.

Report of Referee C -- LW16156/Bellini

The article is fine. The topic is not new, but the authors' conclusions are interesting. The topic is important for our understanding of this phenomenon, namely the formation of light (hyper-)clusters from exploding hot dense matter.

Both referees appreciate that, and so do I. Is it a PRL?

A: Well... we still think yes ;-), especially because the measurements we propose here can have a big impact on various fronts, as we discussed in reply to referee A in the first review round. But we also accept the critique and the different opinions of the referees.

The arguments of the referees had clearly been such that there should have been a major revision done by the authors, and I do agree with most of what has been written by these referees.

The subsequent attempt of the authors, namely to satisfy both referees with the modifications they offered, was unsuccessful - now, both referees are clearly still not satisfied with the author's revisions, and I do agree with the referees.

Hence, I conclude that this paper still needs a lot more explanations and clarifications, which can be accomplished best in a full paper with Physical Review.

A: We thank also the third referee for going through all the draft and replies of the first review round. We have now modified the draft to include more details as suggested. We also added a sixth paragraph with an interesting corollary of our study, which can be used to predict the (anti-)triton over (anti-)helium-3 ratio as a function of multiplicity.