Dear PRL Editor,

We would like to thank PRL for considering our manuscript.
We have addressed the comments of the referees as outlined in our replies and as a consequence, the text has largely improved.
Thanks to the constructive comments of the referees, we realised that the first version of the paper was not clear enough to allow for a broad audience to understand the two employed models and our intentions. Hopefully, we have succeeded in addressing the concerns of the first referee in the replies and in the improved draft.
Following the encouraging review of the second referee, we believe that the paper should still be considered for publication in PRL. We have addressed his reservations in our replies and with modifications to the text. The paper is of interest for a broad community, as outlined in the replies to the first referee. Moreover, the appearance of more recent publications (following ours with their appearance on the preprint server) addressing nuclei and hyper nuclei production shows the relevance of these studies for the debate that is ongoing in the community.

Shall this still not be convincing enough for the editor and the referees in this second iteration, we would also accept a submission to Physical Review.

With best regards,
the authors

Replies to referees

Re: LW16156
Testing production scenarios for (anti-)(hyper-)nuclei and exotica at LHC energies
by Francesca Bellini and Alexander P. Kalweit

Report of Referee A -- LW16156/Bellini

R: I recommend against publication of the manuscript in PRL. The topic is certainly of interest, although to a small community.

A: We do believe that the paper is of interest for a large community that spans several fields of physics:

- nuclear physics (it addresses the special nature of hypertriton and uses state-of the art knowledge of nuclear properties)
- heavy-ion physics (it addresses the "anti-nuclei puzzle" as described in the manuscript)
- hadron spectroscopy (the hypertriton could serve as a test case for new studies on X(3872) and hadron-molecule states in general)

- dark matter searches and astroparticle physics (production mechanisms of anti-nuclei need to be understood for the determination of the secondary cosmic ray background for space-based experiments like AMS-02, GAPS, ...)
- nuclear astrophysics (understanding hyper-nuclei production and properties adds to the study of the hyperon-nucleon interactions that are relevant to understand and model the equation of state of neutron stars)

R: The techniques are rather standard, and the ideas have been discussed previously. Non-zero-extent wave functions have been applied to deuteron production, but to my knowledge this is the first time I've seen it applied to A>2 coalescence.

A: While the techniques employed in our study have been discussed previously, we have extended and combined them such that for the first time a direct comparison of the thermal and coalescence models is allowed as well as a direct comparison to data. Moreover, we present for the first time a consistent picture across different collision systems (pp, p-Pb, Pb-Pb), and we do extend the approach to the A>2 (hyper)nuclei, as also acknowledged by the referee. In this respect, the latter is an element of originality in our approach.

R: Unfortunately, the paper is not well written. I had to re-read several sections numerous times to understand what was meant by the various formalisms. The model descriptions "thermal", "blast-wave" and “thermal+blast-wave" seemed to be applied to the same line in Figure 3 depending on what was being read in the manuscript. The ordering of the various subsections made it hard to follow where the authors were going, or to discern the main point of the paper.

A: We first thank the referee for pointing out that the text could benefit from clearer explanations. This gave us indications to improve the flow of the paper and to revise it to make it clearer. We now label consistently the statistical thermal approach combined with the blast-wave model as “thermal+blast-wave” (see the updated text for details). We also would like to clarify here the following point about the models: the thermal model cannot – alone – be compared to the pT-dependent coalescence description because it provides only predictions for pT-integrated yields. A thermal particle production implies a Boltzmann distribution of the momenta only for a static source, which is not the case of the rapidly-expanding system produced in heavy-ion collisions. The thermal model (i.e. the GSI-Heidelberg implementation used in our paper) needs to be complemented by a hydrodynamic description of the rapidly expanding source. To that end, we chose to use the Blast-Wave model, which has been proven to describe reasonably well the measured momentum distributions of protons. Therefore, the label “thermal+blast-wave" refers to the combination of the two model: the pT dependence is modelled by the blast-wave whereas the normalisation (i.e. the pT-integrated yield) is taken from the thermal model.

R: The conclusions were insufficiently compelling to warrant publication in PRL, even if the paper was rewritten. Stating that one model fits the data better than another is publishable, but not as a PRL.

A: We thank the referee for the straight criticism. However, we would like to strengthen our position with respect to our conclusions by stressing that conclusion point 2 has far reaching consequences: the success of the thermal model might imply that nuclei are formed as compact multi-quark states, thus reshaping our present understanding of particle formation in QCD. Moreover, we believe that conclusion point number 3 establishes a clear experimental path that, as stressed in the paper, is going to drive a significant part of the LHC experimental program for the next 10 years. We have added a fourth point in our conclusions to put our study into the context of the future experimental programme.

R: If the paper was rewritten and submitted to PRC, it would benefit from a more physical comparison between models. Foregoing the finite-extent-wave-function corrections, coalescence and thermal models should provide identical answers apart from a modest Boltzmann correction, exp(-B/T).

A: Indeed, our paper is the first one that systematically tries to address precisely this point: for small nuclei such as deuteron and 3He, both models give identical answers (see Fig. 3) and this also indicates that finite-extent wave-function corrections to the models are small for ordinary nuclei. However, it is exactly these corrections that lead to large differences for the hypertriton.

R: Additionally, in this paper, the coalescence prescription used a Gaussian assumption for the spatial shape of the phase space density. By comparing the phase space distributions (for a given momentum) in the blast-wave to that assumed in the coalescence prescription, one should be able to understand why the two models differed so greatly in the lower panel of Figure 3. Was the problem that the shape of the phase space distribution was non-Gaussian, or was the Gaussian radii incorrect? Aside, I assumed both models used the same non-zero-extent wave function corrections.

A: First of all we would like to remind that the thermal model does not have any non-zero-extent wave function correction. Similarly for the blast-wave model. Only coalescence takes the finite extent of the wave-function into account. One might argue that the current thermal model implementation should be extended to consider non-zero-extent wave function corrections, but existing studies (e.g. our reference [45] by Stoecker, Vovchenko) then showed that the model breaks down entirely, thus leaving data unexplained completely.
As we argue in the paper, the difference between models for the hypertriton originates from the large extent of the hypertriton wave-function which enters in the coalescence probability but not in the thermal model yields.

R: Finally, I list two physics questions the authors should consider should they rewrite the paper for PRC.

1. Many of the hadrons come from longer-lived decays. How does this affect the various B_A parameters?

A: We thank the referee for this very interesting question, which we did not address in the paper due to space constraints. While the coalescence probability itself does not depend on the overall proton or neutron abundance, the nucleons from decays might enter the experimentally measured nucleon spectra and thus the experimental BA which is derived from them. We would like to address the question by separating the case of strong decays (i.e. Delta resonances, primarily) and weak decays (i.e. Lambda, etc…).
Since weak decays happen much later than the nuclei formation time, they do not play any role. In addition, weak decays are experimentally subtracted from the measured proton and anti-nuclei distributions we considered (see the cited ALICE publications for the details).
The influence of the strong decays depends on the two production scenarios considered here. The thermal model takes into account strong decays of hadronic resonances into proton and neutron. There is no significant heavy resonance decaying into (anti)nuclei. For instance, 4Li decays into 3He+p but its production rate is suppressed by a factor 300 (in PbPb) to 1000 (in pp) with respect to the production of 3He.
In the coalescence picture employed by our study, nuclei formation occurs at the kinetic freeze-out. Hadronic resonances that decay before kinetic freeze-out feed the nucleon sample that is available for coalescence. Resonances that decay after kinetic freeze-out do not participate in nucleus formation. The only relevant resonance in this context is the Delta resonance, which decays into nucleons with a lifetime of the order of 1 fm/c. Higher mass resonances provide a negligible contribution with respect to the Delta. If the hadronic phase (as in the systems under study here) is at least as long as the Delta resonance lifetime, then the nucleons from decaying Deltas are available for nucleus production and are appropriately accounted for.

2. For coalescence, the HBT radii used should be those that characterize the phase space distribution in the pair frame, not the lab frame. In the pair frame, Rout is larger than Rside by an additional gamma factor. This could increase the product of the three radii by nearly a factor of 2.

A: We agree with the referee. This is one of the reasons why we do not explicitly rely on the experimentally measured HBT radii for the determination of the coalescence volume but we constrain it with the measured deuteron B2 instead. As a matter of fact the only information we take from the HBT study is the increase of the coalescence volume with the cubic root of the charged particle density, which we assume as linear for simplicity. We have rewritten chapter 3 accordingly, and it has now become chapter IV.

Report of Referee B -- LW16156/Bellini

R: In this paper the authors study the coalescence model for the production of light (anti)-nuclei in heavy ion collisions. The authors emphasise the special role of the hyper triton since, due to it every small separation energy and associated large size, it provides the most sensitivity for testing the coalescence model. The authors imply a simple model for the nuclei and use the quantum improved formulas of Ref. 7 to calculate the coalescence parameter B2 for various system. They argue, more or less correctly, that a measurement of the hyper-triton for smaller colliding systems would be very helpful in testing the coalescence model and/or the structure of the hyper triton.

A: We thank the referee for his nice summary of our work and the encouraging feedback.

R: In principle this paper would potentially be suitable for PRL is the following comments are properly addressed:

1) The authors admit that they use a simple (oscillator) model for the nuclear "wave function" i.e. rms. While this is probably fine for d and He, it is by no means clear if it will not be misleading for the hyper-triton. As shown in Ref. 2 the hyper-triton is closer to a "deuteron"+ weakly bound Lambda than the simple three baryon oscillator state the authors assume. This is also demonstrated in Ref. 33, where the authors of that paper are able to reproduce the measured hyper-triton yield by talking into account, in addition to three baryon coalescence, the deuteron - Lambda coalescence. Incidentally, the authors of Ref. 33 state that by using p+n+Lambda coalescence only they get about 50% of the measured hyper-triton yield. The present paper, on the other hand, seems to be almost a factor of ten below the data (see Fig. 3). This obvious discrepancy needs to be addressed. In addition, the authors also need to provide an estimate how the dependence of the source size R changes if one were to include the aforementioned "deuteron"-Lambda coalescence.

A: We do not agree with the conclusions of reference [33] because summing up the two scenarios (d+Lambda and p+n+Lambda) looks like double-counting to us. As a matter of fact, one of the authors of [33] has recently released a paper in which the two mechanisms are presented as alternative scenarios:
Kai-Jian Sun, B. Donigus and C.M. Ko, arXiv:1812.05175.
In this new paper (released after the appearance of our study on the preprint server) also an estimate of the deuteron-Lambda coalescence as a function of system size is presented, leading to similar observations as ours in terms of system size dependence. The only difference between the two studies is given by the source volume parameterisation:

- It is assumed as pT-independent in the study of Ko et al., while we do assume pT dependence
- in this new paper the authors are able to describe the hypertriton to 3He ratio in central collisions because a much larger volume of the source is assumed (at dN/deta ~ 1000, they assume 8.3 fm compared to about 3.9 fm as in our paper) and the pT-integrated yield is considered instead of a pt-differential study as in our work. It must be noted, that the effect of the wide hyper-triton wave-function is pt-dependent as the volume of homogeneity (or coalescence) volume is pt-dependent as known from HBT studies.

For completeness, we added a reference to this study.

2) The authors concentrate on the system size and argue for measurements with smaller systems. However, coalescence being a phase space effect can equally be tested by looking at the momentum dependence of B_2. Or in other words, it is known from HBT measurements the the effective source size (region of homogeneity) drops with increasing momentum. Thus one would expect that a similar effect as shown in Fig. 3 should also be seen if one looks at the momentum dependence of the coalescence parameter B2. The authors need to discuss this and give credible arguments why the momentum dependence is less suitable (if this is the case) (Incidentally the measured pt dependence of the the B2 parameter shows already this effect).

A: We agree with the referee that the momentum dependence is also a crucial and very interesting element in the discussion presented here.
Our choice here was motivated by the fact that we seem to be more sensitive in the system size study, going from pp to Pb-Pb, because the lever arm is larger. This can be illustrated as follows: for a fixed pT/A, e.g. 0.75 GeV/c as in fig. 3 of the paper, B2 changes by about a factor 50 going from pp to central Pb-Pb collisions. Considering the pT/A dependence as measured by ALICE in PRC 93 (2016) 024917, in most central Pb-Pb collisions, B2 changes only by a factor 2 going from pT/A = 0.4 GeV/c to pT/A = 2.2 GeV/c. Similarly, in pp collisions B2 varies by less than a factor 2 with pT/A in the range measured by ALICE (see PRC 97 (2018) 024615).
A more detailed study including pT dependence (of the coalescence parameters as well as of the source size) will be the the topic of the next, more extended paper.

3) The authors use a blast wave model for to calculate the spectrum for the thermal model. What are the blast wave parameters used? They should be provided.

A: As we indicate in the text of the paper, the Blast-Wave parameters are taken exactly from table 5 of the ALICE paper cited as [14] (PRC 88 (2013) 044910). These are measured in fine centrality bins. We interpolate linearly between bins.

4) In the introduction the authors list three approaches, (a)-(c), to address the production of light (anti)-nuclei. They miss a fourth, and likely most realistic one, which has recently been developed by Oliinychenko et al. (arXiv:1809.03071). In this approach, which was restricted to deuteron, the destruction and production of deuteron after chemical freeze out was studied in kinetic theory and the measured B2 parameters were well reproduced.

A: We apologise for the omission of this interesting reference in our paper, which was not intentional, but simply driven by the fact that the paper of Oliinychenko et al. appeared on the preprint server only after ours, but still before our submission to PRL. We have now added the reference and a short description as well.

5) Section V needs to be rewritten. After reading this several times I still have no idea what these models A and B are supposed to be. Also, the average reader will not necessarily know what INEL>0 is supposed to mean.

A: We thank the referee for the constructive criticism. We agree with this comment and we have now rewritten the section aiming at more clarity. In this process, several details that were not so relevant for the final choice of the parameterisation, were sacrificed for the sake of clarity.