Long Symmetric Cumulant paper : 1st EB review

Dear Babara

We would like to thank you for encouraging and useful comments.The summary of the changes made in the updated version of the manuscript are provided below briefly.

New version:

Difference between new and reviewed version:

The reply to the comments :

Reviewed version :
https://aliceinfo.cern.ch/ArtSubmission/sites/aliceinfo.cern.ch.ArtSubmission/files/draft/djkim/2017-Jun-13-paper_draft-longSC_EB_R1_v7.1.pdf

Regards,

DongJo on behalf of PC

EB Comment # 1

This is an extremely nice paper. It is very well written and the results and discussion are very interesting. This is significant praise, as it comes from someone who has not previously been following flow correlations closely. There are, however, some problems in the description of the model comparisons. It almost seems as if the figures were updated after the text was written. Also, I think that the discussion needs to return to the question of linear and nonlinear response.
My substantive comments are listed first, then some more minor suggestions.

Best regards, Barbara

1) I think that we would provide a valuable service to the reader if we were to add a sentence or two explaining the source of the non-linear response. I checked, and the theoretical papers are a bit vague on this – perhaps the theorists think it is obvious. The explanation of why they are useful is very nice in the paper already. However, I think an explanation of what is meant by “induced by lower order harmonics” should be added.
R: In order to clarify, we have inserted now a concrete example after the sentence in 77: "For instance, v_4 can develop both as a linear response to ε_4 and/or as a nonlinear response to ε_2^2."

2) The discussion on page 4 is very nice!
R: Thanks! This is also a very encouraging comment, which will always in the future have a bright place in our memories...

3) The statement on line 241 that “the pT,min dependence of NSC(3,2) shows a moderate decreasing trend as pT,min increases” is misleading. I think “decreases” is meant to indicate that the correlation strength becomes more and more negative. However, that doesn’t mean that the correlation decreases – in fact, the anti-correlation increases. Please rephrase to clarify, as the physics meaning is quite different.
R: Thanks, well spotted! We have now replaced in 241 "decreasing trend" with "increase of anti-correlation".

4) The statement in line 325/326 that the correlation between v5 and v3 is **not** described while the v5 and v2 **is* described is too strong. It would be very useful to calculate the chi-squared (with systematic as well as statistical errors accounted for) between the predictions and the data. For SC, they will not be substantially different for v5/v3 and v5/v2, and the text should reflect this properly.
R: We agree that for the other centralities the chi-squared test between these two observables wouldn't differ much.
We have replace “only the correlations between v5 and v2 are well described for all available centralities. On the other hand, for correlations between v5 and v3 the description fails in more peripheral collisions, providing further independent constraints for the models.” to “the correlations between v_5 and v_2 are well described for all available centralities and similarly for the correlations between v_5 and v_3 within the errors.”

5) The next sentence is also not well supported by the data. The anticorrelation is underestimated for semi-central collisions, but not for the most central or 50% centrality bin. The disagreement does not look worse than for the v3/v2 case. I STRONGLY suggest calculating all of the chi-squares and including them in a table. Then the strength of the statements can be calibrated by the values in the table. Strong conclusions should not be drawn from “only by eye” comparisons, as it is straightforward to be more quantitative.
R: We agree with your observation, and now we have extended the sentence 326-328 in the following way "... data significantly." => "... data significantly in midcentral collisions, and it fails similarly for the anticorrelation between v_3 and v_2". On the other hand, we are reluctant to invest more of our time now to perform the suggested chi-squared tests, since in any case we believe that quantitative comparison to the models via such tests would be necessary only at the point when by naked eye we cannot rule out the agreement. That being said, in all phrasing throughout the paper we were careful enough not to make any too quantitative statements on how well the given model describes or fails to describe the data. After the models have been tuned and improved based already on our measurements in this paper, we will gladly perform such chi-squared tests in the follow up studies and comparisons!

6) Line 337: 0-10% should say 0-5%, as the signs of NSC(3,2) agree for the second data point.
R: Thanks, well spotted! It is indeed safe to say that only for the 1st bin, so we have now replaced "0-10%" with "0-5%".

7) Line 338: the centrality dependence of NSC(3,2) is only somewhat weaker (if at all) than that for NSC(4,3), and the spread among the model calculations is similar. I suggest replacing the sentence beginning “The NSC(3,2)…” by “NSC(3,2) and NSC(4,3) are not

very sensitive to the initial conditions or eta/s parameterization used in the model, and are not described quantitatively.
R: Done.

8) Line 347: either the error bars on the calculation points in figure 5E are wrong, or the sentence beginning “The NSC(4,3)…” is wrong. I suggest simply removing the sentence.
R: Thanks, well spotted! We have now dropped this sentence.

9) Default AMPT looks best to describe all NSC in figure 6. The SC’s, however, are not described by any single AMPT set. I think that the paragraph describing the comparisons bin by bin is more confusing than enlightening, and so I strongly suggest removing most of it. The paragraph should consist only of the very nice text immediately following “In summary”, and perhaps be attached to the previous paragraph.
R: We prefer nevertheless to keep this paragraph in its present form. The underlying reasoning is that we feel obliged to comment at least a little bit in the main text on any model or measurements we are showing in the figures. In the case, however, that you would further insist that this paragraph shall be shortened, please let us know concretely which sentence can be dropped, and we will do it immediately!

10) Line 418: The last sentence in the paragraph is confusing. Is the message better described as “Consequently, the observed weak pT dependence may be due to hadronic rescattering. To clarify, the relative contributions to the final state particle distributions from partonic and hadronic stages need further study.”
R: Thanks, we have implemented your suggestion, which indeed reads much better.

11) Line 430: As written, the paper implies that there is something special about the 10-20% centrality range, because this is the only place where there is a clear separation between the hydro calculations with different T dependence of eta/s. On the face of it, this doesn’t make any sense! Are the differences between the two parameter sets actually statistically significant in that bin??? If there is a reason why 10-20% centrality should be so different from 5-10 and 20-30%, we need to explain why to our reader! If there is no good reason for the specialness of the 10-20% bin, then we should not make a point about that bin. If the problem is statistics in the calculation, then we need to generate more statistics.
R: We are not aware of any special role for centrality 10-20% due to physics in this context, and our sentence was just a mere encapsulation of what models have yielded. We agree with you that perhaps it's then safer not to emphasize this centrality, and therefore we have simply blended the sentence 429 "ranges. While the difference..." with the previous one: "ranges, while the difference of the model results for the two parameterizations in most centralities is rather small.”
Based on the discussion with the EKRT authors, they have enough statistics on that bin, it is more problematic if we go to the most central collisions but with these new sets of calculations on the figures, all the calculations are statistically significant.

12) The introduction has a nice discussion about linear and nonlinear response, and how that can be separated in the data by comparing correlations among lower and higher harmonics. After this lovely lead-in, the paper should come back and draw some conclusions about the responses in the discussion and summary sections! It seems that figures 4, 5, and 6 should tell us something about this. The logical place to bring this up could be in lines 434 and 456, rather than just saying “let’s compare to hydro”, which we just got done showing.

R: We were reluctant to insert in this paper more on linear and nonlinear response, since just recently ALICE has published standalone paper only on that (https://arxiv.org/abs/1705.04377). But while the nonlinear ALICE paper doesn't contain pT dependent results yet, it is bit hard to make a connection to it. Also note that pT dependent studies in cited theory papers are premature in flow correlation and therefore we ended up with very naive interpretations.

13) The summary section would be smoother to read if broken into several paragraphs. I suggest beginning a 2nd paragraph with "We have found that" , and starting the third paragraph at "A quite clear separation…"
R: Done. Done.

Minor comments:

Line 41: remove "also" as all the discussion thus far has been purely from theory, whereas the hydro conclusion comes from theory fitted to data to extract eta/s. Even better would be to write "Hydrodynamical simulations constrained by data support the view that eta/s of the QGP is close to that limit." Without the constraint from data, eta/s is a free parameter in hydro simulations.
R: Thanks, we have implemented your suggestion.

Line 174: For clarity, I suggest changing "systematic uncertainty from the centrality determination…" to "systematic uncertainty on the cumulant which arises from the centrality uncertainty…"
R: We have implemented slightly modified: "systematic uncertainty on the symmetric cumulants which arises from the centrality uncertainty…"

Line 232: This sentence has a somewhat suboptimal structure. I suggest replacing ", in order to avoid" by "; this avoids"
R: Done.

Line 243: please add "the" before "pT dependence"
R: Done.

Figure 3 caption: please add "pT integrated" before v5.
R: Taking it into account, we realized we use the notation v_n everywhere without pointing that we have used a different notation in other papers even though it was clearly said that it is equivalent to \cite{Adam:2016izf}.
Now we added a sentence after Line246.
"We use the notation v_n instead of $v_{2}\{2, |\Delta\eta|>1\}$ used in \cite{Adam:2016izf} and they are $p_{\rm T}$ integrated."

Line 303-306: this is a run-on sentence which is hard to follow. Suggest "We compare to event-by-event EKRT+viscous hydrodynamics predictions with various parameterizations of the temperature dependence of eta/s(T); these were shown in Fig.2 of Ref. [41].
R: Done.
Line 333, add a comma before "shown in"
R: Done.

Line 335, add a comma before "but it cannot"

R: Done.

Line 421: add “also” before “compared”
R: Done.

Line 433: add a comma before “as predicted in”
R: Done.

Line 451: “these results presented in this article” should be either “these results” or “the results presented in this article”. I suggest selecting the shorter version, as fewer words are somehow always easier to follow and understand.
R: In 461, we have now replaced “these results presented in this article” with "these results".