Long Symmetric Cumulant paper : 2nd IRC review

We would like to thank all IRC members for the prompt reviews on the paper draft as well as for many great suggestions. The replies to all IRC comments and an outline of the changes made in the updated version of the manuscript are provided below:

Paper draft location :

Reviewed version :

<https://aliceinfo.cern.ch/ArtSubmission/sites/aliceinfo.cern.ch.ArtSubmission/files/draft/djkim/2016-Dec-08-paper_draft-longSC_v4.3.pdf>

New version:

**Reviwer : Alice Ohlson**

Two general suggestions/comments:

- Like I wrote in my previous comments, be very careful about stating

that a value is increasing when it's actually getting more negative

(i.e. decreasing!) or about saying that something more negative is

"larger" than something less negative.  The strength of the

anticorrelation may be increasing (larger), but the actual numerical

values are decreasing (smaller).  I notice this in line 229 but I think

it happens elsewhere too.

R : We changed to “Both NSC(3,2) and NSC(4,3) are getting more anticorrelated toward peripheral collisions with the similar magnitude.”

And remove ‘and a monotonic increase is observed” in line 227.

- I suggest defining a quantity p\_T^{min} at line 231 so that you can

then use it throughout this paragraph were applicable. For example, in

line 234 you can write 0.2 < pTmin < 0.7 to be more clear.  Also you can

change the legends in Fig. 2 to say e.g. pTmin = 0.8 GeV/c instead of

0.8 < pt < 5 GeV/c (this will reduce the text on the figure).

R : good suggestion, we modified the text and figures accordingly.

* line 81: magnitude -> magnitudes
* R : done
* line 82: is sensitive -> are sensitive
* R : done
* line 109: alone are -> alone is
* R : done
* line 116: of heavy-ion -> of the heavy-ion
* R : done
* line 149 (and perhaps elsewhere): pseudo-rapidity -> pseudorapidity
* R : done, only here
* line 153: taken for -> included in
* R : done
* line 192: correct for -> avoid
* R : done

- lines 193-194: I suggest "resolution, a hybrid track selection

utilizing SPD hits and/or ITS refit tracks combined with TPC information

was used."

R : done

* line 195: The systematic -> A systematic
* R : done
* line 199: and it is -> and is
* R : done
* Fig. 1 caption: Systematical -> Systematic
* R : done
* line 241: NSC(m,n) -> NSC(4,2)
* R : done

- lines 260 and 315 (and perhaps elsewhere): initial condition ->

initial conditions

R : done and found one more place, fixed now.

* line 305: remove "observable"
* R : done
* line 308: to model the initial stages -> in models
* R : done
* line 309: use -> measurement
* R : done
* line 310: The better -> Better
* R : done

- Figs. 3, 4, and 5 need a bit more work on the plotting, some ticks

labels are cut off, points cover up 10^-6, the exponents along the axis

change, and parm->param in the legends.  Also, all the text needs to be

made larger.

R : parm->param fixed, we have improved the figures as you said.

* line 355: As it can -> As can
* R : done
* line 355: for "param1" -> for the "param1"
* R : done
* line 355: latex ''->``
* R : done

- line 356: from the hadronic to the QGP phase occurs at the lowest

temperature, around 150 MeV

R : done

- line 357: characterized by a moderate slope in eta/s(T) which

decreases (increases) in the hadronic

R : done

* line 358: with the parameters for -> in
* R : done
* line 358: of phase -> of the phase

R : done

* line 359: latex ``->''

R : done

* line 359: already with -> by
* R : done
* line 359: measurement -> measurements
* R : done
* line 361: remove "the transition towards"
* R : done
* line 363: the data -> the anticorrelation in the data
* R : done
* line 364: a failure of constant -> the failure of a constant
* R : done
* line 370: quite clear -> clear
* R : done
* line 386: Except for the
* R : done
* line 392: a AMPT -> an AMPT
* R : done
* line 412: "for NSC(3,2) there is no pt dependence in the centrality"
* R : done
* line 413: seen for -> observed in
* R : done
* line 414: or the centrality increase -> or centrality increases
* R : done
* lines 415 and 417: Fig. 8 and Fig. 9 -> Figs. 8 and 9
* R : done
* Figs. 8 and 9 captions: cut -> cuts, are ->is
* R : done
* line 420: magnitude of the data for both -> magnitudes of both
* R : done
* line 425: remove "from the data"
* R : done
* line 430: In case -> In the case
* R : done
* line 434: with two -> with the two
* R : done
* line 435: parameterizations -> parameterizations of eta/s(T)
* R : done
* line 435: magnitude -> result
* R : done
* line 436: centralities -> centrality range
* R : done
* line 436: underestimate in -> underestimate it in
* R : done
* line 436: add comma after "However"
* R : done
* lines 437-438: calculations between -> results for the
* R : done
* lines 438-439: between two parameterizations are -> between the two are
* R : done
* line 439: in 10-20% where -> in the 10-20% centrality range where
* R : done
* line 445: correlation -> correlations
* R : done
* line 448: the non-flow -> non-flow
* R : done
* line 449: free from -> independent of
* R : done

- The sentences at liens 466-467 and 470-472 say almost the same thing,

so I suggest incorporating the first one into the second.

R : done

**Reviewer : Sudhir Raniwala**

Here are some suggestions. Some of these may reflect my ignorance. Please take what you like.

1. In general, define once and for all that in the present work, SC(m,n) is considered as higher order if m + n >= 7, and lower order if m+n <= 6.

R : Probably that would be overdoing ?

2. Do we want to write $\eta/s(T)$ or $\frac{\eta}{s}(T)$ ?. The former indicates that entropy (alone) is temperature dependent.

R : We used this in the short paper and the theroy papaer uses this convention. Since it was defined clearly, we think the current form is ok.

In addition to minor edit comments, some comments are interspersed in the following, and may be of greater relevance.

Line 17: and the temperature dependence 🡪 and to the temperature dependence

R : I am not sure ?

Line 19: Correlations between the magnitudes of v2, v3 and v4 🡪 Correlations of v2 with v3 and v4

R : we think the current form is ok.

Line 31: has been 🡪 was / is (In general, ‘has been’ and ‘have been’ are not good in a science paper, imho).

R : done

Line 41: close to that limit 🡪 close to the lower bound.

R : done

Line 41-42: We can remove the sentence ‘This may have....goals’.

R : OK.

Line 58: Remove the word ‘respective’.

R : we think it is ok with it.

Lines 61-63: This appears to be too much blah blah. What is the extent of variation of eta/S with temperature? How does this variation affect vn values? A more specific statement may help.

R : We don’t think so. This statement shows where we are in our field. That is why this paper is important.

Line 78-79: “Therefore, the higher .....harmonics” 🡪 The nature of the (non)linear response will manifest itself as correlations of higher harmonics( n > 3) with lower order harmonics.

R : We think the current form is better.

Line 86: parts of 🡪 features of , or aspects of

R : We think the current form is better.

Line 99: explained 🡪 detailed

R : We think the current form is better.

Line 104: What remains..... 🡪 It is imperative to discern the temperature dependence of eta/S of the QGP.

R : ??

Line 105-106: were studied in [] 🡪 were studied in reference []

R : conflict with llya’s comments. We will leave it as it is.

Line 107: has just been 🡪 was recently

R : done

Line 107: Can remove “in Ref”

R : we keep “in” as 105 but remove “Ref”

Line 108: remove “only”

R : we think we can keep it.

Line 114: remove the word “development”. “Therefore their “ 🡪 Their

R : We think the current form is ok.

Line 114-118: Needs to be rephrased. Am sorry I can not give a good suggestion.

R : We think the current form is ok.

Lines 128-134: There is something that I am missing, as also mentioned earlier. NSC are SC divided by mean-squares of vm and vn. SC contains <vm^2> and <vn^2>. The language conveys that if we do NOT leave a gap while calculating SC, it is OK, but it is necessary while calculating NSC. So the second term in SC will have a different value with and without the eta-gap. What am I missing here ?

R : Yes, you are right but that effect is rather small and assigned as a systemactic uncertainty.

Line 138-139: “The observed centrality ......... “. Let me consider a limiting (extreme) scenario. The model is ‘useless’,and is not able to reproduce any feature of the data. Then eta/S or its temeperature dependece become completely irrelevant. This was one of the concerns that I had expressed earlier, the goodness of the model to explain the distribution of v\_n.

R : We are not sure what you mean by the goodness of the model. The models cited here have their own limitations. Good desciption of vn in various models can’t gurrantee that they are correct or not and rather we have found that vn themselves couldn’t constran model parameters which are described in the introduction with the references. That was why SC has been quite interesting and we do observed a clear seperations of the model parameters. Once we understand model parameters better, also vn desccription in the model will be improved.

Line 173: How come systematic uncertainty on different data points show such ‘uniform’ behaviour. Are we considering the maximum systematic error here?

R : do you mean why we are adding in quadrature ?

Line 176-178: There will be an auto-correlation effect here. The magnitude of fluctuation is related to the multiplicity. And if the track multiplicity is used for centrality determination, then it will bias the systematic error.

R : This is what we studied and assigned as a systematic error.

Line 193-193: Known to ALICE, but may require elaboration for the general reader.

R : We have repharased to " resolution, a hybrid track selection

utilizing SPD hits and/or ITS refit tracks combined with TPC information

was used.” Based on Alice’s suggestion.

Line 211-213: What is being said here, please, in reference to Fig. 1.

Suggestion: For all centralities, data for SC(m,n) shows a negative correlation if m – n =1 and a positive correlation if m – n > = 2.

R : we think the current version is ok.

Lines 221-224: I have missed the relevance. What is the issue? What is being said, really?

R : We are describing what we observed.

Line 225-226: linear or non-linear is irrelavant because centrality interval is a more of a convenience. Linear and non-linear would make more sense if the figure is plotted as a function of N\_part of impact parameter. Please rephrase.

R : We are not sure why N\_part is better here. We are just describing the observed centrality dependence.

Line 242 -244: Can I conclude that the results are ‘insensitive to pT of particles’ or claim that the results are robust against changes of pT.

R : what do you mean by “insensitive” ? There is an expectation that the pT spectra would change at the freeze-out from hydrodynamic calculations. Also see L245-247.

There is alice paper on this subject.

Lines up to 247: The SCs change with pT because vn change with pT and therefore it is important to see whether the model reproduces the pT dependence of vn. This is another reason in support of the earlier question about necessity of model reproducing the gross features.

R : similar answer from us as Line 138-139

Line 259-261 ‘ and given initial conditions later’.....? Please rephrase.

R : We think this is ok.

Line 271-272: “ ...non-vanishing initial local flow velocities.....”. What is the relevance with the present work?

R : We describe the initial conditions of AMPT which has a different implementation.

Line 274-276: Is there an eperimental evidence to support that the process of thermalisation has started, but equilibrium has not been reached?

R : Not at all. A very strong assumption in hydrodynamic models is that the local equilibrium is achieved.

Line 276-277: The AMPT addresses such non-equilibrim many body dynamics with includes both initial partonic....

R : We think the current version is OK.

Line 294: Missed the emphasis here....are we saying that lower radial flow is able to reproduce the charged particle flow, but not able to reproduce the identified particle flow results?

R : No, it says disagreement “is responsible for the quantitative disagreement [82].”

Line 304-305: “This observation......density” – How? Why ? What ?

R : The disagreement between the models and the data will help models to be improved.

Line 335-336: Obviously I do not understand this. How can difference in individual results explain the difference in correlations ?

R : This came from the relation between SC and NSC via <vm^2><vn^2>.

Line 338: Remove “the”.

R : done

A general comment for section 6.1: One needs to read it multiple times to derivee the conclusion on what observables and what experimetnal results validate show which physical properties? Before writing the text, it may help to make a table (or an n-dim matrix in one’s mind) to make the statements and conclusions crisp.

R : We tried to summarize them in L395-410. We think it is ok.

Line 346: “give better constraints” 🡪 further constrain

R : We think this is ok.

Line 355-356: Please rephrase.

R : we have repharased it to “As can be seen in Fig.~1 from Ref.~\cite{Niemi:2015qia}, for ``param1'' parameterization the phase transition from the hadronic to the QGP phase occurs at the lowest temperature, around 150~MeV. This parameterization is also characterized by a moderate slope in $\eta/s(T)$ which decreases (increases) in the hadronic (QGP) phase.”

See the Alice’s comment on this

Line 411: You may want to change the title considering that we are restricting to v2 –v3 and v2-v4, these are lower order as per the proposed defintion of m+n < = 6. Also, since v3-v4 is not being discussed, the present title may mislead.

R : This is what llya suggested. We think this is ok.

It may be good to reduce the blah blah in the following:

Line 466-467: Can we specify what contraint?

R : this is combined to Line 470-472

Line 470-472: Which specific properties of the model?

R : the initial conditions and $\eta/s(T)$ being said.