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:
https://twiki.cern.ch/twiki/pub/ALICE/PtDependentStandardCandles/longSC_v5.1.pdf

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 suggested.

- 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 : In L209-210 they are defined clearly. Probably this math is not needed.
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 [40] and the theory paper [30] used 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 : we think the current version is ok.
Line 19: Correlations between the magnitudes of v2, v3 and v4 → Correlations of v2 with v3 and v4
R : we think the current version 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 : we think the current version is ok.
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 fine with "respective".

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. Our short SC paper was addressing eta/s(T) for the first time based on the experimental data.
Also based on the cited paper which studied the effect of eta/s, there is x 2 difference on eta/s value based on vn comparisons to the data by using different initial conditions.
A large uncertainty on IC and corresponding eta/s varies by x2, Refs. [4,24,25].
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 version is ok.

Line 86: parts of → features of , or aspects of
R : We think the current version is ok.
Line 99: explained → detailed
R : We think the current version is ok.

Line 104: What remains..... → It is imperative to discern the temperature dependence of eta/S of the QGP.
R : We think the current version is ok.
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 version is ok.
Line 114-118: Needs to be rephrased. Am sorry I can not give a good suggestion.
R : We think the current version 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 systematic 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 description of vn in various models can't guarantee that they are correct and rather we have found that vn themselves couldn't constrain model parameters. These facts are described in the introduction with the references. That was why SC has been quite interesting and we did observe clear separations of the model parameters. Once we understand model parameters better, vn description in the model will also 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 them 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 rephrased to " resolution, a hybrid track selection
utilizing SPD hits and/or ITS refit tracks combined with TPC information
was used." Based also 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 : There is an expectation that the pT spectra would change at the freeze-out from hydrodynamic calculations. Also see L245-247. As you said, there is no hint of changes pT_min up to 0.7GeV/c but we start to see the changes >0.8 GeV/c.

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 the current version 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 (IC) of AMPT which has a different implementation than the other IC. This should be stated clearly because it could effect the results and their implications.

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 should be 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 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. We don’t want to overdo it. The disagreements between the models and the data are quite visible with SC observables, which tells us that the results can help to constrain model parameters. Which parameters and how much .... ? We can’t solve it by ourselves at this moment; we leave these kinds of works to theorists.

Line 346: “give better constraints” → further constrain
R : We think the current version is ok.
Line 355-356: Please rephrase.
R : we have rephrased 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.

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.

Reviewer : Ilya Selyuzhenkov

https://twiki.cern.ch/twiki/pub/ALICE/PtDependentStandardCandles/longSC_v5.0.pdf

l7
The correlations between event-by-event fluctuations of anisotropic flow harmonic amplitudes
->
The amplitude correlations between event-by-event fluctuations of anisotropic flow harmonics

R : We think the current version is ok.

l12
flow), as well as the transverse ... are presented
->
flow) is presented. The transverse ... is also reported.

R : done

l18
favored regardless of initial conditions and the AMPT initial condition
->
favored independent of specific choice of initial conditions in the models. The calculations with the AMPT initial conditions

R : done, yields to yield accordingly.

l20
This might be an indication of possible viscous corrections to
->
This might reflect viscous effect in

R : We think the current version is ok.

l21
Question: Why only "at hadronic freeze-out"? Why not during the whole evolution? I think it is better not to limit these effects to freeze-out.

R : The viscous correction is an additional correction (or term) to the viscosity of the QGP phase at hadronic freeze-out. In general \eta/s in the QGP phase is dominant contribution to the final state particle flow. But we don't know yet how much viscous correction will be even though we know there should be some. We said higher order harmonics are more sensitive to the viscos correction at freeze-out and it's p_T dependence is important to help to understand it. It is based on the cited theory papers [36,37]. For example, D. Teaney, PhysRevC.86.044908(https://arxiv.org/pdf/1206.1905v2.pdf) page15 for higher order harmonics and also other papers. We mentioned it briefly at line 65 (The relative contributions from the partonic and hadronic phases) and here we focus only on the contribution from the hadronic phase. If there is any, there will be a change of momentum distributions at the freeze-out and flow harmonics and their correlation will be smeared (see Figs. 20,21 from ref. [30])

l23
the results presented here
->
the presented results
R : done

l28
"collective and anisotropic flow" - anisotropic flow is also "collective" effect, so "and" does not read well here. Suggest rephrasing:

This matter exhibits strong collective and anisotropic flow in the plane transverse to the beam direction, which is driven by anisotropic pressure gradients, resulting in more particles emitted in the direction of the largest gradients.
->
The matter produced in a heavy-ion collision exhibits strong collective radial expansion. Due to anisotropic pressure gradients in the plane transverse to the beam direction, more particles emitted in the direction of the largest gradients which result in anisotropic transverse flow.
R : done, remove "which".

l31
This has been predicted
->
The measurements are well described
R : done

l32

These calculations also demonstrated that the .. is close
->
The experimental data favor the .. to be close
R : We think the current version is ok.

l35
For instance, one
->
One
R : done

l41
Remove "This may have important implications for other fundamental physics goals" - This generic sentence adds zero information to the paper.
R : done

l45
is traditionally
->
is nowadays
R : done

l45-56
Ref. [14]-[15] does not introduce the notion of vn having corresponding Psi_n
=> Add more recent references where it is discussed.
R : add one more reference, Phys.Rev.C58:1671-1678,1998

l59
Difficulties in extracting η/s in heavy-ion collisions can be attributed mostly to the fact that it ... for comparison
->
Until recently, the difficulties in extracting η/s in heavy-ion collisions were attributed mostly to the fact that extracted value ... in calculations
R : we think the current version is ok.

l61
Viscous effects also reduce ... elliptic
->
Viscous effects reduce ... anisotropic
R : done

l61
Viscous effects also reduce the magnitude of the elliptic flow.
->
This sentence does not belong here. It either need to be extended or removed.
R : we think the current version is ok.

l62

Furthermore, the magnitude of η/s used in hydrodynamic calculations should be considered as an average over the temperature evolution of the expanding fireball as it is known that η/s depends on temperature.
->
It is now understood that the temperature dependence of the of η/s should be considered in the hydrodynamic calculations.
R : we think the current version is ok.

l64-66
"Therefore, both the temperature dependence of η/s and the relative contributions from the partonic and hadronic phases should be understood better to quantify the η/s of the QGP."

This is a repetition of the previous two sentences and can be removed
R : we think the current version is ok.

l67
An important input to the hydrodynamic model simulations is the initial distribution, ... which is ... the probability distribution
-> This is again about initial conditions which was introduce earlier, no need to repeat
->
The initial distribution ... is ... the distribution
R : we think the current version is ok.

l 69
This initial energy density profile can be quantified by calculating the distribution of the spatial eccentricities
->
It is quantified by the spatial eccentricities
R : we think the current version is ok.

l73-74
There is experimental and theoretical evidence
->
There is an evidence
R : we think the current version is ok.

l76-83 Have you synchronized this discussion with the outcome of the discussion with PC+IRC for related paper on non-linear response (You Zhou is its PC chair)?
I think it is important to write this part such that it reflects the fact that both linear and non-linear terms are misleading and in reality both have non-linear contributions.
R : we think the current version is ok. Yes, we are aware of it (DongJo is one of PC of that paper). l76-83 is correct "Harmonic v4 and higher order flow coefficients can arise from initial anisotropies in the same harmonic". Confusion has arrived while we decomposed it w.r.t v2 and v3 also with the assumption. i.e v2(v3) $\propto \varepsilon 2(\varepsilon 3)$ Also it was not stated clearly in the current

paper draft. Also it was known that hydrodynamics is non-linear theory and the linear term in that paper contains non-linear contribution but they should be small. There should have been clearly said the assumptions in the draft.

l84
Hence, studies of the higher order (n > 3) to lower order (v 2 or v 3) harmonic correlations
->
Studies of the higher correlation between higher order (n > 3) and lower order (v2 or v3) harmonics
R : done , studies of the correlations between higher order ($n>3$) and lower order (v_2 or v_3) harmonics

l85-86
viscous correction to the momentum distribution at hadronic freeze-out
->
Same question as in the abstract> Why only "at hadronic freeze-out", not during the whole evolution?

R : See the reply for l21.

l87
Recently, the ALICE Collaboration measured for the first time the Symmetric 2-harmonic 4-particle Cumulants (SC), new multiparticle observables which quantify the relationship between event-by-event fluctuations of two different flow harmonics [40]
->
The first results for a new multiparticle observables which quantify the relationship between event-by-event fluctuations of two different flow harmonics, the so-called Symmetric 2-harmonic 4-particle Cumulants (SC), were recently reported by the ALICE Collaboration [40].
R : done

l94
higher order Fourier harmonics
->
higher order harmonics
R : done

l96
We also include extensive comparisons ... calculations.
->
We also present a systematic comparison ... calculations.
R : done

l100
theoretical calculations. Various theoretical models
->
model calculations. Various models
R : done

l103-124
This is basically a repetition of what was written in the introduction. I see no reason to repeat the messages which were just discussed.
I suggest to:

(a) merge this part with the text in the introduction at l87 and start Sec. 2 right from the definition of SC observables (l125), i.e.
The SC observables are defined as
->
The Symmetric 2-harmonic 4-particle Cumulants (SC) are defined as

(b) merge Sec 2 and Sec 1, and move "outline" i.e. l94-l101 after lines 143.
R : we think the current version is ok.

l128
cumulants reflect
->
cumulants has an advantage that it reflect
R : we think the current version is ok.

l132
For the two
->
For the product of
R : we think the current version is ok.

l136
However, the
->
It was found that the
R : done

l139
indicating clearly that
-> (either indicating or clearly shows, but not a mix of two)
indicating that
R : done

Fig. 1 caption
Systematic errors
->
Systematic uncertainties
R : done

l127
However, unlike ... The NSC(4,3) magnitude...
->

Unlike ... This demonstrates the advantage of using the normalized SC observables in which the correlation strength between flow harmonics is not hindered by the differences in magnitudes of different flow harmonics. The NSC(4,3) magnitude...
R : done

l220
Fig. 1b
-> should the brackets be added here and in other places?
Fig. 1(b)
R : done

l230
we systematically vary
->
gradually change
R : done

l232
Various minimum pT cuts from 0.2 to 1.5 GeV/c are applied
->
The minimum pT cut is varied in the range from 0.2 to 1.5 GeV/c for values of pT = 0.2, 0.8, 0.9, 1.0, 1.2, and 1.5.

Question: do we really need so many steps shown in Fig. 2? Maybe it is enough to have them all shown in Figs. 8-9
R : we think the current version is ok and these show all the data points we have. In this way it is much clear to show both SC and NSC. Also p_T dependence can be seen clearly. Figs.8-9 show only NSC.

l237
dependence of the vn values
->
dependence of the individual flow harmonics <vn>.
R : done

l244-245
These observations are strikingly different from pT dependence of the individual flow harmonics...
->
Actually I do not completely agree with this assessment. Can this be due to increase in non-flow correlations at high pT? The published sigma_vn/vn are obtained with VZERO event plane (much larger eta gap).
It would be good to provide an explanation for why this is happening.
R : non-flow correlation should be included in the systematic error. Also we tested with lowering pT_max we still see the same trend. Hydrodynamic model calculations also show the similar trend. We didn't know if this trend can be seen in the hydro until we receive the hydro result.

Comments arrived at 12.21.2016 for sec5.6.
Dear PC, EB, All,

while trying to compile my detailed comments for Sec. 6 (i.e. model comparison) and re-reading it together with Sec.5 (results), I arrived to the feeling that these two section needs substantial restructuring, including the figure rearrangements.

My main worry is that despite that after the first discussion with the IRC and restructuring the paper draft had significantly improved, these main sections of the paper (Secs. 5&6) are still very hard to read and to comprehend the details of the data to model comparison. This obscure the resulting physics message inferred from the measurement of different order flow harmonic correlations and from the model comparison.

R : We are not sure what kind of physics messages you are referring to, we think they are clearly stated. Currently we know that theory itself has it's own limitations. We don't want to overdo the work. We have compared our data to the existing models and tried to convey what we have learned.

To address this issue I have a proposal for restructuring which is outlined below. I realize that to implement it will take time for PC and more than one iteration with IRC will be required, but I am afraid that without this the paper draft will not be shaped to the level of being ready for the first round of the Collaboration Review.

R : Please clarify the first question so that we know what we are heading for.

Giving that the QM2017 time is approaching fast and also considering the coming Christmas and New Year holiday period, the chain of iterations "IRC->EB->CR1->IRC->EB->CR2->IRC->EB->archive" will not converge by the beginning of February. For this I ask the EB for advise. My personal view is that we should not sacrifice the quality of the presentation of the paper due to QM rush. One option to proceed would be to release the higher harmonic results as preliminary and submit the paper (hopefully very shortly) after the QM.

R : We don't agree with it, no submission, no results in QM. There is no reason to show it in a conference while the submission is a month away. We are not sure why we want to do that. Data is final, why we want to label it differently before and after QM.

Here is the proposal for restructuring of Secs. 5&6:

1. Arrange and focus on physics discussion by two categories "centrality dependence" and "pt-dependence" which will become two main sections instead of the current structure. This will require (among

other things) to move Fig. 2 and text on lines 230-246 to what is now Sec 6.3 on line 410

2. Combine Figs. #3-#7 into 3 new figures (2 columns [SC and NSC] x 5 rows [2-3, 2-4, 3-4, 2-5, 3-5]) - one per each set of model comparisons (Fig. #3 [EKRT+viscous hydro], #4 [2+1 hydro], and #5 [AMPT]). Fig. #3 will require to include as the top panels the comparison to 2-3 and 2-4 with EKRT which is in the short SC paper but currently missing in the long paper (I think this should be included for completeness).

The arguments for combining the figures are:
- Frankly, I do not understand why to separate the discussion of 2-3-4 and 3-4-5 harmonic correlations. We do not do that in Fig. 1 and I think we should follow this for model comparison, i.e. just have an extended "replica" of Fig.1 with 2x5 panels for each of the three groups of model comparison.
- Combining will avoid some repetitions which are currently present in Secs. 6.1 and 6.2
- It will give a better overview of data to model comparison for all harmonic combinations in one figure, which should improve significantly the readability of the paper in addition to reducing the high number of figures.

3. The model description will stay as it is now, but will be presented not as a separate Sec. 6, but as a part of Sec. "centrality dependence" and discussion of the results.

R : We don’t see why this is needed. We think in the end everything looks same to us. We think 2x5 panels are very hard to follow in general.

Another general comment is about model description on page 8 (l247-295). The way it is presented now is very dry and leaves an impression of "boring" description of many knobs and switched of many different model parameters. I do not have a very clear idea how to improve this yet, but one option which PC can consider would be to compile a table with columns for different models their parameters stages (initial, QGP, hadronic) and in a separate column indicate whether a given model can reproduce the data and up to what precision

R : We think that would be too much for the experimental paper. We are not sure what extend we have to compile. Model implementations are not very straightforward. Even what to and how to compare are challenging tasks to the theorists. In this section, we just give a brief idea on the models and with the given citations. One can check the details of the calculations based on the cited papers.

Regarding the summary of the observation on page 13, lines 395-409 -

it is very nice that such a list suggested by the IRC has been added.
My feeling is that it should be moved later in the text after
discussion of the pT dependence, extended with observations from the
comparison with the pT dependence of SC/NSC observables, and
essentially land just before the summary section.

R : We think the current version is ok. L395-409 is for integrated SC and NSC. pT dependence has its own section. Right after those, we have a summary of the results.