Thank you all for your detailed review and prompt comments. I apologize for my delayed response, I am in the middle of preparing for my PhD defense, which is scheduled for April 5 (which is also why the first draft read somewhat like a thesis). I believe I was able to address most of your comments and concerns. Along with this document, I will upload a new version of the analysis note, with the most recent results and some additional comparisons between different fit methods. The paper is admittedly still a little rough in terms of wording, and I will continue to refine things.

There were a few points which, if you deem necessary, will have to wait until after my defense. Most notably, if a purity figure for my Kch sample is required, similar to that done in the charged kaon femtoscopy paper, I will need to complete this after my defense. I have never made such a plot, but believe it will involve slicing my dE/dx distribution into different momentum slices, and fitting each with a collection of Gaussians to extract the Kch purity. My Kch cuts are very similar to, and slightly more strict than, those used in the recent kaon femtoscopy paper [arXiv:1709.01731], so I expect my purity plot to be very similar to theirs.

The comments which were implemented are shown in green. I add any additional comments of my own in this scarlet color. Any blue or black color is still left over from the original version sent to me.

For the results included in the first version of the paper, the lambda_{Fit} parameters were limited to a maximum value of 1.1. For the 0-10% centrality, the fit reached this maximum value.
After speaking with Tom, we feel that such an arbitrary limit on the lambda_{Fit} parameters should not be imposed. Allowing the parameters to be free only changes our extracted fit results within error bars. Most notably, for the 0-10% centrality bin, the lambda_{Fit} value is 1.40 (instead of 1.1) and the radius is 6.24 fm (instead of 5.81 fm).

As this lambda_{Fit} parameter has been a continued source of confusion, we are open to discussing possibly fixing all lambda_{Fit} values to unity in the fit. With such a restriction, the extracted fit parameters do not change much, as you can see in the "Fit Method Comparisons" section of the analysis note (Fig. A.2 in Appendix A.1). Again, most notably, the 0-10% radius is decreased from ~6.24 fm to ~5.10 fm. This is exactly as expected, as the lambda_{Fit} parameter and radius are tightly correlated, and forcing the lambda_{Fit} to decrease also decreases the radius. Our main motivation for allowing the lambda_{Fit} parameter to be free is to help the fit procedure when considering the source is not perfectly Gaussian, it is not centered at 0 in the outward direction, as well as other approximations involved in constructing the fit function (e.g. all the approximations that go into the treatment of residuals). However, I am not sure if allowing these lambda_{Fit} to run free are useful in better describing the experimental data, or are simply introducing confusion into the interpretation of our results. In either case, the extracted scattering parameters (the main focal point of the analysis) are mostly unaffected, and the result of higher than expected radii still holds true.

One major change that I propose is completely removing any mention of the 10 residual method. This only adds confusion to the paper, and is really not the correct method. When moving out to 10 residual contributors, we include the Sigma* and K* resonances, which have proper decay lengths of 5 fm and 4 fm, respectively. Most of these will decay before kinetic freeze-out, and Lambdas and Kaons originating from these resonances should therefore be considered as primary.

If we decide that the 10 residual treatment should still be included, I believe the lambda parameters will need to be altered to better describe the situation of most of these resonances decaying before kinetic freeze-out, with only a fraction surviving until after. Lambdas and kaons born from these resonances decaying before kinetic freeze-out should contribute to the primary lambda component, while only those born after should contribute to the appropriate residual lambda component. I'm not sure exactly how we would estimate the number of Sigma* and Lambda* resonances surviving until after kinetic freeze-out, and I think this would be more trouble than it's worth.

I am happy to meet through video to more thoroughly discuss the points presented above, although I would prefer this to be anytime after April 5.

https://alice-publications.web.cern.ch/node/5115
Comments/suggestions by Laura, Konstantin and Marek

Abstract:
General Comments:

In general, the draft of the paper is written well enough.
The draft is slightly overloaded with details that can be reduced.

Maybe too many technical plots: We risk to overload the paper with redundant details.
In the description of the PID and V0 cuts, partially too verbose, maybe reduce a little the text for sake of simplicity. The text is nicely written but it reads a bit as a PhD thesis, normally journal articles are more compact.
Also, the plural form ' we ' is used throughout the manuscript, but the impersonal form is better in papers. Please change.

From the point of view of the analysis, the discussion of the 3 and 10 residuals should be modified. First the discussion should be more clearly explained in terms of short lived resonances (K* Sigma*) and their contributions and not only via this nomenclature 3-10 residuals, since it is misleading.
Since the results of the scattering paramters are very different for the 2 approaches, we think that both should be discussed in the paper. Indeed, it is not very clear if kaons and Lambda stemming from short lived resonances feel the same final state interaction that primary particles do.
See discussion above. In short, I believe the 10 residuals case is incorrect, and should not be included in the paper (it is still instructive to keep it in the analysis note, currently shown in Appendix A.3).

I remember that during the development of the analysis you carried out also some systematics studied for the two cases with and without short lived resonances. It would be necessary to present these systematics as well.
The plot comparing the 3 and 10 residuals case is located in the "Fit Method Comparisons" section of the analysis note (Fig. A.1 in Appendix A.1). If we decide to keep the 10 residuals case, I agree that such a plot should be included.

The scattering parameters are particularly different in the case LambdaK0s.
This is maybe also due to the fact that you use the linear extrapolation for the background evaluation and not Therminator.

This is another point which we don’t understand. First of all the figures that show the comparison between the measuread and simulated correlations in Fig 4 display and agreement, so we don’t understand why you write that for LK0s therminator does not work.
Moreover, since the LK+ and LK- backgrounds are very similar in each centrality bins, one could use them or the average of them for LK0s and not this linear fit.
The figures in the analysis note suggest indeed that because of the slightly larger background obtained with the linear fit for LK0s and the smaller lambda parameters in case of the 10 residuals, the two effects concur to produce different scattering parameters for K0s (in the sign) between the 3 and 10 residuals cases.
For this reason we recommend to drop the linear fit and either use Therminator also for K0s or take the LK+ LK- bacgkrounds.
The updated figure, with the zoomed-in y-axis, highlights the LamK0 disagreement more clearly. Basically, with the LamK0 system, the shape of the background from the THERMINATOR simulation does not match exactly that of the data. If you normalize the THERMINATOR to match the background region, the it underestimates the signal region. If you normalize it to the signal region, it overestimates the background region.

Furthermore, for the 30-50% centrality bin, the fit to the THERMINATOR simulation dips below unity. The arguments for collective flow together with event mixing leading to the non-femtoscopic background imply that the contribution should be above unity in this low-k* region. I could artificially force the fit to be above unity at low-k*, but this seems a little bit invasive.

Unfortunately, the backgrounds for the LamKch systems are different than those in the LamK0. This is demonstrated in the “Non-femtoscopic background” section of the analysis note (Fig. 26 in Section 5.5). Therefore, it is not possible to simply use the average of the LamKch simulation for use with the LamK0 system.

Since the K0s are a mixture of kaons and antikaons, we expect the scattering parameters to be a kind of average of the one obtained for LK+ and LK-.
It seems also that the results go into this direction.

From the analysis note we also know that the lambda parameters obtained in the case of the 10residuals are not 1. On the other hand, if we understand correctly the analysis, this parameters concerns only the coefficient that multiplicate the sum of all secondaries in eq. 15, lambda_fit.
In this case, since the lambda_KL (genuine) varies between 3 and 10 residuals, it is also normal that the lambda parameter varies.
So, we are not sure that this is a problem.
The improper treatment of the residuals, i.e. using 10 contributors, places less emphasis on the primary interaction (as demonstrated through its reduced lambda value between the two cases). More emphasis is placed on the residual contributors, whose signal is effectively flattened after being run through the appropriate transform matrices. Therefore, we are left with a lot of mostly flat residual contributions, and a reduced emphasis on the primary interaction. I believe these two effects account for the larger lambda_{Fit} parameters, and the (mostly) larger in magnitude scattering parameters, preferred by the fitter for the case of 10 residual contributors. Again, a comparison plot showing the difference between the 3 and 10 residual contributors cases can be found in the “Fit Method Comparisons” section of the analysis note (Fig. A.1 in Appendix A.1).

Detailed Comments:
Abstract:

It should be mentioned that the experimental correlation functions are fit with the same radius and lambda parameter for all three pairs in each centrality bin.

L 11-14
maybe shorten to:
Extensive studies with the THERMINATOR 2 generator allow to quantify the non-femtoscopic backgorund, mainly due to collective effect, with unprecedented precision.

l.11-14 There is an information about the simulation of the background with the THERMINATOR model but nothing said about the residual contribution.

l.15-16 It is noted that there is a difference between ΛK+ and ΛK- pairs but nothing said about the ΛK0s pair.

L18-19 :
The underlying cause dictating this interesting difference in the strong force between the two systems is not completely understood.
This is not a statement. You should write that the different arises from the different quark content from which the strong interaction depends upon. Maybe remove this sentence and leave only the next one.

L22-23:
We understand.. -> We interpret this effect as the separation.…

Introduction:
General Comments:

The first section is really general and does not mentions Lambda-Kaon explicitely. Then you jump to the mt scaling and different radii.
The sequence is rather abrupt and a story is missing.
The scope of the analysis is to measure the not-known scattering parameters of Lambda Kaon pairs and also to study the mt dependence of the extracted radii. This should be the beginning of the introduction.
Lambda-kaon scattering parameters are not known at all. Here you can also mention that Kaon(anti)-proton scattering parameters are measured and a different behavious is found (the strong part of the K-p nteraction is attractive and K+p interaction is repulsive), hence what you are doing here is extendeing Low energy QCD meaurements (Check the introduction and references there in the pK- paper by Ramona and Co https://alice-publications.web.cern.ch/node/4742)Then you explain why it is important to study the mt dependence, introducing the universal
scaling that is apparently broken by Lambda-Kaon pairs but for which you find an explanation.
And then you start with line 52 where you summarize the results.

Another point: you write several times that ‘ it is not completely understood’ why LambdaK+ has different scattering parameters than LambdaK-, but this sounds negative. This is your fundings,hence it is a new information for the Low energy QCD sector.

Mention also what does it mean to have positive or negative parameters (attraction or repulsion) already in the introduction.

Concerning the separation in space-time: maybe you can anticipate that you discuss this difference in the paper

The introduction is admittedly still a little rough, but I have made some significant changes. I took tried to focus more on the most important and interesting aspect of this analysis, the extraction of the scattering parameters. I mentioned the Kp data, but will have to add the appropriate references after I have some time to explore the literature. The first paragraph broadly introduces femtoscopy, and explains how it is typically used. I took out all mention of mT scaling in the introduction, and only allude to it in a general manner by mentioning the momentum and species dependence of femtoscopic measurements revealing the collective nature of the system.

I'm sorry this is still not polished, I will work on this in the coming weeks, as I am desperately needing to return focus to my defense.

Detailed Comments:

L33-34: Non-identical particle pairs-> Lambda-kaon pairs

L38: and therefore deconfined QM : Im not sure that flow can be defined as that. I would delete this part

L 42: how picture -> how the picture

L58: We find the -> We find that the
Removed

lines 58-71: I would not reveal main results in the Introduction. I think in the Introduction we should write what we are going to measure, but it would be to soon to write what we have actually measured. I would move this part (if applicable) to Results/Summary part.
Removed

L 62-63: are these positive and negative real scattering parameter implying attractive or repulsive interactions?
Removed

l.65-66 Again, similarly to the abstract, the interaction of ΛK0s pairs is not discussed.
Removed

L72-75: maybe it is premature to discuss the common source here. You can discuss this later in the text.
The introduction should be more general.
I thin it's important to introduce the shorthand notation here, which is used throughout the paper. So one sentence was kept describing this

L 81: mention the content of the appendixes.

line 83: I would also mention year of data taking (2010 or 2011)

Section 2:

l. 93 the TPC only and constrained to the primary vertex. A minimum requirement on the number of reconstructed TPC clusters was imposed
The number of clusters should be mentioned here.

Table 1: why are the nSigma cut varied depending on p? Can you add a sentence?

Fig.1 dE/dx – It will be reasonable to change this figure from dE/dx to the purity of the charge kaon versus momentum as was done in the charge kaon femtoscopy papers. In this case, the reader will better understand why the N_TPC is set to unity at the momentum range 0.4-0.45 GeV/c and why N_TOF decreases with increasing momentum.
In addition, the purity should be different for different centralities.
For now, the dE/dx plot has simply been removed.
As stated in the introduction of this document, if we deem these plots necessary, they will have to wait a couple of weeks until after my PhD defense. My Kch cuts are very similar to, and slightly more strict than, those used in the recent kaon femtoscopy paper [arXiv:1709.01731], so I expect my purity plot to be very similar to theirs. Maybe it is suitable to simply reference that paper in ours?

line 107: the charged Kaons are accepted until 1.5 GeV/c.
I would say in interval 1.0-1.5 GeV/c kaons start to overlap with protons (for dE/dx in TPC). Is the value 1.5 GeV/c still safe, or did you try to reduce the contamination for protons as well (as you did for pions and electrons)?
The TOF is used with the TPC at momenta above 0.45 GeV/c. With the TOF detector, the kaons and protons are still well separated, even at 1.5 GeV/c, so this value is still safe. I believe this is pretty standard for femtoscopy with kaons, and the kaon femtoscopy paper referenced above uses the same interval. The exact nSigma cuts are slightly different than in that publication, and were taken from a presentation for our AliFemto group given by Konstantin (https://indico.cern.ch/event/533150/). No, we did not try to reduce the contamination for protons as well, as they are not a major source of contamination in our analysis.

L118-120 and formula 1: too detailed. Just mention the purity after you have explained that you rely on Montecarlo for the PID.

L121: remove

Fig2: maybe also redundant

L131-133: The daughters... imposition. -> cut the sentence

L138: Why is the minimum tranverse momentum cut reducing the fake contamination? Which fake you mean here? Single fakes or K0s fakes to Lambda and viceversa?
Sentence removed, as it was not really necessary. The cut is to remove the contribution from interactions with detector material, and also to avoid low tracking efficiencies of daughters at low pT

L139-141: too long description of maximum DCA for V0. Keep it shorter.

L 143-145: same here.. Maybe remove the following sentence:

We want the V0 candidate's momen144 tum to point back to the primary decay vertex, and therefor a small qpa; we achieve this by appointing a minimum value on cos(qpa)

L146-150: On occasion, L(L¯) particles are misidentified as K0 146 S, and vice versa. To attempt to remove these contam147 inations without throwing away good candidates, we impose a set of misidentification cuts. The intent of these cuts is to judge whether a candidate is more likely a L(L¯) or a K0
148 S, and are implemented as described below. For a given V0, we calculate the mass assuming different identities (L, L¯, K0 149 S) of the candidate;

Simplify in:

In order to remove the contamination to Lambda and K0s due to misenditifaction of the protons and pions for each V0 the mass assuming different identities (..) of the candidates is calculated.

L152-154: maybe footnote?

lines 157 and 168: why choosing 9 MeV/c^2 ? Wouldn't it be better to change it to more general value such as 3 or 5 sigma?
It should not make a big difference for low pt, but have you run your code with a new V0 finder/vertexer? more at https://github.com/alisw/AliRoot/pull/864
This value was selected to be wide enough for use with both Lambdas and Kaons. The exact value used is not terribly important, as long as the peak is included, and was imposed mainly to speed up the running process (i.e. why waste time looking at candidates which are nowhere near the mass peak, and therefore certainly not misidentified Lam/K). This is just the first step in the multi-step misidentification process described. In hindsight, I should have probably chosen 10 MeV/c^2 to have a nice round number. You can find the effects of these misidentification cuts in the "V0 Selection" section of the analysis note (Fig. 2 and 3 in Section 3.3). Instead of simply throwing everything out within some m_inv region (throwing some of the baby out with the bathwater), we use the multi-step process to keep the good V0s contained under the peak, shown in the figures in the Analysis note.

No, I have not run the code with the new V0 finder/vertexer. I can try to incorporate this into my code if you think it will be necessary/useful. I'm not sure exactly what is involved in injecting this functionality into my framework.

in Table 2 and 3: Cosine of pointing angle seems to be too strict, have you tried to loosen it while maybe introducing a new cut like - V0 transverse decay radius? Are chosen cut values based on some asumption, like desired S/B ratio? But, if you are happy with you final V0 statistics, then it should be fine.
These cuts were adopted from the K0sK0s and Lambda-Lambda femtoscopic analyses, where the values were chosen taking both statistics and purity into account. I have not tried loosening this cut while introducing a V0 transverse decay radius cut. Of course, I am always happier with more statistics. I can try to implement such a procedure, but my feeling is that it will not substantially increase my statistics. Please let me know if you believe it will.

lines 185-188: I had to re-read those sentences (including corresponding part in Sec. 2.3) in order to understand the difference. Could it be written more clearly?

in Table 3: what is the reason for asymmetric mass interval - is the mean of the mass peak shifted w.r.t. PDG value?
This was an embarrassing mistake. The interval was centered on the mass of the charged kaon, not K0. The correct interval has been implemented, and this small shift has no effect on the final results.

l.168 Why the rejection criteria for the mass difference is equal to 9 MeV/c 2 for both K and Λ? It seems that the widths of the invariant mass distribution are different.
See response above for comment on lines 157 and 168. In short, the value of this cut doesn't make a huge difference, as long as the mass peak is contained, as this is just step one in a multi-step misidentification process.

L 159-164 and 170-171 are not necessary, get rid of them

L 173: At this stage, we have a collection of V0 candidates satisfying all of the aforementioned cuts. However, this collection is still polluted by fake V0s, for which the daughter particles happen to pass all of our cuts, but which do not actually originate from a V0. Although the two daughter particles appear to reconstruct a V0 candidate, they are lacking one critical requirement: the system invariant mass does not match that of our desired V0 species (these can be seen outside of the mass peaks in Fig. 3). Therefore, as our final single-particle cut, we require the invariant mass of the V0 candidate to fall within the mass peak of our desired species. Note, however, that some fake V0s still make it past this final cut, as their invariant mass also happens to fall without our acceptance window.

This I would modify completely into:

The resulting invariant mass for Lambda and Kos is shown in figure ... A mass precision and resolutios of .. and .. are obtained for L and Kos, respectively .A final cut on the invariant mass is applied to enhance the purity. The cuts are shown in table…
I have modified the lines into a much shorter version. However, I am not sure exactly how to calculate the mass precision and resolution. If you would like these numbers to be included in the text, we can discuss more about this calculation.

L 181:186: reduce to 1 sentence and move before the discussion of the invariant mass distribution

L 189 190 and equation 2: remove

lines 192 and 195 - construction of distribution before m_inv cut - this information is there twice.

l.194-196
It is vital that this distribution be constructed immediately before the final minv cut, otherwise it would be impossible to estimate the background.
This phrase contains unnecessary details and may be removed.

Up to line 200: shorten mentioning here the final purities only.

Line 202-206: shorten to:
In order to reduce the contamination to the two-particle correlations due to split or merged tracksand pairs sharing daughters, two main pair cuts are applied: a shared daughter cut, and an average separation cut.

L 209-210: remove text in the brackets.

L 211-213 This mistake.... -> remove

Fig 3: Labels and legend in the figures are too small. Remove purity label. Enlarger the Sign and S/B labels.

Caption Fig 3: reduce the text.
Remove: immediately before... bin). Remove last sentene about purity, discuss it in the text.

L 221-228: shorten this part.

Section 3:

l.234 The formula for k* is needed here.

L239: Enture > entire

L245: Ideally, ...[1]. -> remove.

l.247 by forming mixed-event pairs[we would propose to add here the reference to the original paper by G. I. Kopylov, Physics Letters B 50(4):472-474 · June 1974 https://www-sciencedirect-com.ezproxy.cern.ch/science/article/pii/0370269374902639?via%3Dihub]

l.248-250 The rotation method is not used in this paper, therefore I propose to not mention it and remove it from the appendix.
We would prefer to keep this method in the paper. This method is not well documented elsewhere. It is difficult to find a proper description, and I cannot find anywhere documentation showing the effect of implementing such a method. This method is elegant in its simplicity, and does a good job of reducing the effects from the non-femtoscopic background. I believe, therefore, it should at least be brought to the attention of the community. I can include a plot comparing our normal results to those obtained when implementing this process, if this would make the discussion more relevant.

L 251-253: In forming the reference distribution, it is important to mix only similar events; mixing events with different phase-spaces can result in an unreliable reference, and can introduce artificial signals in the correlation function. Therefore, in this analysis,

Simplify to:

In order to mix only similar events

line 255: I am not sure whether non-ALICE reader would understand Nmix variable, could you define it?

line 256: Also, could you elaborate more this vertex correction? Is it applied only for events in mixing procedure? How the binning of mixed events is done (2cm bin width for primary vertices) when z=0? I guess I am missing something here.
Better explanation added in the text.
Are OmegaK residual correlations negligible since they are not mentioned in the text?
Correct, the number of LamK pairs originating from OmegaK is below even that of Xi^{-}K^{*0}

L260: I understand that you consider the kt integrated functions because of the statistics limitation but
You might add here a sentence motivating that the kt dependence of the three LK combinations shold be the same and hence the integrated analysis is ok

Eq 6: redundant. If you mention a weighted average, it is clear what is meant here.

Eq 9: drop it

L. 284: ' in case of no residual correlations'
I don't understand this formula with lambda. If there are no RC then lambda is 1 and then generic formula does mean anything anymore. Why did you write it like that?
I have removed this equation from the text, as I agree it was confusing. Basically, I was trying to explain the case where one does not explicitly account for residual contributions in the fit. In other words, if one broadly splits up the collection of pairs into femtoscopically correlated and non-correlated collections, as assumes the non-correlated pairs contributions average to unity, then the extracted signal will be decreased by this lambda factor.

L286: Why include the QS story with formulas and so on if in the end you are treating indistinguishable particles? I would stick to the correlations you need in the description.
Removed discussions of QS for identical particle studies

L 291-293: I would here emphasize the fact that aside fake contributions you have contributions from resonances. It is not very clear in the current formulation.

L 300: It is not true that residual correlations are only important if the parent correlation is large. You can have a shallow correlation for the parent but a large contribution that will automatically dump the lambda parameter of your genuine correlation.
I would get rid of this list of 3 itmes. It seems confusing to me.

L 303-304: remove up to ' our fit'.

L 304: ' may be combined' is combined out of..

L. 310-311: 'Lambda parameters... are normalized to unity:
Actually is the sum of all lambda parameters Lambada' _{ij} + lambda' _KL that should be unity or?
The text is not clear here
Text made more clear. Sum of all lambda'_{ij} equals unity, where ij also includes primary KL.

L 322-328: the choice of 3 and 10 contributors is not clear at all here. You should explain why you can reduce to 3 at all. It is also not clear what it means, ‘ reduce to ctau<4 fm for consistency’. The role played by short-live resonances should be emphasized.
As explained in the introduction to this document, I think it is most appropriate to exclude the 10 residuals case from the publication.

l.332-333
The particle yields can be estimated using THERMINATOR 2 simulation (Ni Tj HERM), while the reconstruction efficiencies (REi j) are estimated with MC HIJING data, which has been run through GEANT to
It should be explained why the yield was not estimated with HIJING.

l.335 What does the first row in the table A.1 mean (ΛK+ 0.527 and so on)? Are these direct pairs?
These are primary pairs (i.e. direct pairs, and pairs originating from short decays). I will make the table more clear.
Why is the value of other pairs greater in case of 10 residuals?
This is due to the tightening of the proper decay length in moving from 3 to 10 residuals. In decreasing the proper decay length from 10 fm to 4 fm, Lam/K born from resonances with decay lengths between 4 fm and 10 fm are sorted into the “Other” category instead of the “Primary” category (with the exception, of course, of those born from Sigma* and K* resonances, which are sorted into their appropriate category).

I have assumed we will include only the case of 3 residual contributors, making the table much smaller, and have included it in the text, instead of the Appendix.
Of course, if we decide to keep the case of 10 residuals, we will again need the large table.

Table A.1 : see general comments

L 346:351: too poetic, shorten please.(avoid things as ‘ the world in which we live is not perfect’)

L374:377: remove it. We don’t need a generic description of how Montecarlo is used to extract the momentum resolution.

line 387-389: the difference is not due to any interesting physics - I guess the checks were done in analysis note?
This check was done using THERMINATOR simulation by altering the K0s cuts to match more closely the Kch. An example figure is shown in the “Non-femtoscopic background” section of the analysis note (Fig. 27 in Section 5.5).

Just to better understand the figure 4: the data points are with mixed events binned also in Psi_EP?
No. These are binned just as in the analysis, without and Psi_EP binning.

L399-407: I would get rid of this paragraph since normally in papers one only explain the strategy that is finally used.

I kept one sentence stating that we tried EP binning without any success. It seems that many readers would arrive at this as a solution to the non-femtoscopic background issues, so I felt it necessary to state that we pursued that direction.

Fig 4: Improve these figures. 1) the y axis should be all zoomed in (in case you can rebin the first two bins). 2) all labels and legend text should be a factor 3 larger 3) get rid of the 'therminator' label in each panel.

Fig.4 It is reasonable to change the gold color to the dark green one.

L. 420 421: The description of the LK0 421 S is good at a qualitative level, but not quantitatively good enough to be utilized in our fit. As such, we use a linear form to model the background in the LK0
422 S system

I don't understand at all why you write that Fig 4 shows that the Therminator background is not good enough for Lk0s. Also, why a linear form? You wrote on the paragraph before that you fit Therminator with a polynomial function.

This was explained in the General Comments section, above. Basically, THERMINATOR misses slightly the shape of the LamK0 background, and cannot simultaneously describe the low-k* and intermediate-k* regions. It also predicts a background signal below unity in the 30-50% centrality bin.

A linear form was used in the K0sK0s analysis included in the "One-dimensional pion, kaon, and proton femtoscopy in Pb-Pb collisions at 2.76 TeV" paper [arXiv:1506.07884]. Also, a linear form seemed to be the most fair, and least invasive option. I could use a different form, but I would need to restrict the fit to some specific region, and possibly impose some constraints on the fit parameters, to extract the correct form. I would need to re-run the check with the most recent fit procedure, but in the past I found that the exact form of the background made little difference.

L426:432: Im not sure that we need to show the Stravinkiy method. One can add a line mentioning that the method was used as a cross check but I wonder whether we can remove this from the appendix. And from this text.
We would like to keep this method in the text if possible, as it is not well documented, and many in the field are not aware of its effects. To be sure, many understand a background can be built using such a method, but not as many as aware that it can be used to reduce the non-femtoscopic background.

L 441,442: remove

L 446: the overall lambda parameter here is lamda_fit?
Do you mean that this is the same factor for all centrality classes or what? The sources of secondaries are different for the three LambdaKaon combinations.
We mean the lambda_{Fit} parameter is shared between the analyses for each centrality bin. The individual lambda_{ij} for each system are shown in the table. The text has been updated.

L470: why is the background, that is independently determined via Therminator and also fix to it, multiplied by the correlation? Should not you sum it?
Maybe you can include in the systematic errors evaluation the case where the background is summed to the femto correlation.

I believe the background should be applied as a scale factor, not as an additive factor. Imagine an extreme case of multiple background effects coming from multiple sources, each contributing an effect below unity. If one applies all of these contributions as a sum, the correlation function can be negative, which is un-physical.
In any case, in the analysis note ("Non-femtoscopic background", Section 5.5, Fig. 29), I found that applying the background as a scale factor gave nearly identical results as applying it as an additive term, where the term to be added is (1-Bgd).

line 472: Section 3.7: are also purities for K0, Lambda and charged K (or maybe fake candidates) somehow included in the final systematic uncertainty?
No, they are not. How should they be? Can you please elaborate?

L 473 478: I have seen in recent CR1 that people tend to ask the logic of systematic cuts variation.
Personally I don't care and I find the table sufficient but it could be beneficial to explain the systematic cuts in text maybe.
OK. I'm not sure exactly what should be included in the text? The values were chosen as reasonable variations of the parameters used. Maybe we can discuss this.

Section 4:

Fig 5 Horrible as the others ☺
I would not include the radius and lambda parameters in the legend. I would label the different lines.
I would also not include here the scattering parameters since you have later dedicated plots.
I also made this three figures, instead of crammed and combined into one.

l.500 The black solid curve shows the primary (ΛK) contribution to the fit
Does the black curve mean that all lambda for the residual function were set to zero?
No. The black curve shows 1 + lambda'_{LambdaK}C_{LambdaK}. This is better explained in the text now.
Why is the biggest difference for 30-50% centrality?
This is because the non-femtoscopic background is the largest in the 30-50% centrality bin.
In lines 452-453: the cases of zero and ten residual contributors were also investigated, but the case of three contributors was deemed most reasonable
It will be interesting to see the third curve corresponding to the case of ten residual contributors.
I propose the case of 10 residuals be excluded from the publication.

l.501 the green curve shows the fit to the non-femtoscopic background, and the purple curve shows the final
What was used as a background in the end (green curve)? Was it a result of using the THEMINATOR model or an approximation of the data?
The polynomial fit to the THERMINATOR data was used for the LamKch systems, and a linear form was used for the LamK0 system.

l.509-511 The ΛK0s real(f0) is close (the same within errors) to ΛK-. Please, give a comment here.

Fig.6 Left panel: change square symbols to circles in the legend on the left. The legend on the right is not clear. I can guess that it corresponds to the theoretical prediction mentioned in the caption. Please, check it.

line 500: boxes represent systematic errors - there are no visible boxes in figure 5 - are they smaller than markers?
They are visible in the lowest k* points, and are smaller than the data elsewhere, although still visible through the width of the box.
Figure 6: There are no green and yellow points showing theoretical predictions.
Sorry, that was an old description with a new plot. It was meant to say cyan and magenta, not green and yellow.
Figure 7: x-axis label - is it an average pair transverse mass?
Yes, this is better explained in the text now.

L 515: here you can discuss the repulsive character of LambdaK+ and attractive character of LambdaK- in parallel to the K+N and K-N interactions. Those are also repulsive and attractive, respectively.
As I suggested for the abstract and introduction this shold be mention also before.

On the other hand although, K+N has a very small imaginary part, since absorption is not occurring and K-N has a large imaginary part.
Here you see for all three channels a seazible imaginary part and you could discuss it a bit.
I need to familiarize myself better with the KN results before being able to comment anything worthwhile here.

L 543-550: Maybe we have to move the spherical decomposition part to the main body of the paper in a reduced form.
Moved into end of Results section
In this section you might consider repeating that the difference between Lambda and Kaon sources comes from flow.
I tried to emphasize this more. Also, I have results from a numerical integration method which also support the picture of increasing radii with increasing mu_{Out}. See, for instance, Figs. 37 and 38 in Section 7.1.2 of the analysis note. Do you think these demonstrate the effect of increasing \mu_{Out} more simply/clearly than the THERMINATOR 2 method currently in the Appendix? Or, should be keep the THERMINATOR 2 demonstration?

Section 5:

Appendices:

Typos:

line 21: grammar purists only: minus sign is hyphen (should be in math mode) - same can be applied for the rest of the draft (S=-2, 0-10%, ..)
line 36: pion -> pions
line 43: corrections, and -> corrections and

We prefer to keep the Oxford comma

line 56: Lednicky -> Lednický

line 80: (I am not native speaker, maybe it is wrong): estimation systematic uncertainties -> systematic uncertainties estimation

line 89: lease -> least

line 111: the the -> to the

line 122: You might consider changing V0 to V 0 (i.e. to use superscript) throughout the text, just to avoid confusion with V0 as detector. There is no strict rule for this (you can keep it if you want), but we prepare a paper in Strangeness PAG, where V-shaped decay is denoted as V 0 . :-)

line 157: c in italic

line 209: does "ex." mean "for example"? Please, check whether using "e.g." is more correct.

line 239: enture -> entire

line 244: equal -> equals

line 260: perhaps change both ++ and -- to latex math mode?

line 288: the spins are switched (should be 1/2 for Lambda and 0 for K)

line 585: Lyuboshitz -> Lyuboshitz,

line 598: hijing -> HIJING (according to the title in the paper)