I have addressed many of your comments and concerns, but there are still some pieces I need to work on. That which still needs addressed is shown in blue. Specifically, the introduction and results need some attention, and I will try to shorten and refine the text overall. I plan to submit another version of the paper towards the middle/end of next week. The main purpose here is to show new results, which are described below. Therefore, I suggest you wait to scrutinize the text and wording until the next version.

Also, I am now generating systematic uncertainties for the new fits. The systematic uncertainties currently in the figures are those from the previous fit, which I expect to be similar to those of the new fit.

KEY
My comments
Done/implemented
Needs to be done

Main updates in analysis:

I am now able to use the THERMINATOR 2 simulation to model all backgrounds. Previously, I was forming my background correlation functions only with particles marked "primary" by the generator. This was limiting my statistics, so I am now using all particles. With the better statistics, a much better match to the LamK0s experimental data is possible.

I have slightly tweaked how I fit the THERMINATOR 2 backgrounds. I am still fitting the simulation data with a 6^{th}-degree polynomial; however, I now fix the linear term to zero. The background distribution should be continuous and symmetric about k*=0; therefore, the first derivative should be zero at k*=0, which is the motivation for setting the linear term equal to zero. I suppose all odd terms should be set to zero. We can discuss this option, but for now only the linear term is set to zero. Furthermore, the same arguments which explain the suppression in the background signal at intermediate-k* values also imply that there should be an enhancement at low-k*. Setting the linear term equal to zero has the added effect of maintaining the background enhancement at lowest-k* (with the poor LamK0 statistics, sometimes the fit algorithm would result in a suppression at low-k*; see the right panel of Fig. 2 in the previous draft of the paper).

I also slightly tweaked how I utilize the background fit from THERMINATOR 2. Previously, I allowed for a simple scale factor to match the extracted THERMINATOR 2 background fit to the data. Now, I have also added a linear term, which allows for vertical shifts and makes the quality of the fit much better. In other words, I obtain the 6^{th}-degree polynomial fit to the simulation data, let's call it ThermBgdFitPoly. Then, with the data I use DataBgdFit = par[0]*ThermBgdFitPoly + par[1], where par[0] and par[1] are free parameters.

As you can see in the new version of Fig. 2, the match to the data is much better, and we are able to use the simulation to model the backgrounds in all systems.

Still To Do

Continue to shorten and refine text

Introduction
Discussion of LamK correlations
References to theoretical papers

Results/Conclusions

Calculate V0 resolution

Vary cTau for systematics
Vary lambda_ij parameters for systematics

C11 from THERMINATOR 2 for various muOut values compared to data

Sum the pair and conjugate in CF figures?

DCA in terns of sigma?

General Comments

Although we mentioned clearly that the text is often too long and verbose, many parts
Are still left unchanged. For this reason we took the time to suggest which sentences might be deleted, since they are unnecessary. Please try to modify the text accordingly.

I apologize that I did not remove everything you wanted. I did not realize that you wanted so much removed, and I only removed and/or shortened what was explicitly mentioned.

The main still existing problem of this paper is the dependence of results on how residual correlations are accounted for. In this version of the draft, it is postulated that only deposits with c * tau less than 10 fm are taken into account. Authors should explain why this criterion is correct. Otherwise, I expect questions to this point during next rounds of discussion of the paper.
The results are stable with respect to the value of cTau used. Whether I choose cTau= 6 fm, 10 fm, 100 fm, the results hardly change.

The results do change when setting cTau below ~5.4 fm, in which case the K* and Sigma* no longer contribute to the "primary" category, and are included in the residuals treatment. However, such a situation is unrealistic, so the differing results are not a concern.

Concerning your answers to our comments here some remarks:

General remark about the fit: Why do you fit the part-part and antipart-antipar correlations separately? The interaction should be the same and you would gain in statistics if you fit the sum.

The particle pair and it's conjugate certainly should have the same interaction, and therefore share the same scattering parameters. The pair and conjugate systems are not fit separately, they are fit together with a common parameter set. I will try to make this more explicitly clear in the text.

Physically combining the data by summing numerators and denominators, or by forming a weighted average of the correlation functions, would not gain us any statistical advantage. In essence, this would decrease the statistical error bars but would also decrease the number of data points.

General remark about the residuals method: it is fine to show only the 3 residuals method but you should verify the systematics you obtain on the scattering parameters if you vary the cut off from 10 fm to 7-8 or 11-12, since this number is not fixed.
You consider K* and Sigma* decays to contribute completely to the primary but in fact it is not very clear when femto stop to 'act'. After 5 6 7 fm?
This is why we absolutely need this systematic variation.

I will include this in the systematics. I agree, it is not completely clear that K* and Sigma* contribute only to the primary category. The question is how many K* and Sigma* survive until after kinetic freeze-out? With mean proper lifetimes of ~4-5 fm/c, I expect most to have decayed, and therefore contribute to the primary category.

As long as the cut value on cTau is above ~5.4 fm, so that the K* and Sigma* contribute to the primary category, varying cTau does not change the lambda values significantly. I will include the effect in the systematics. Varying the cTau value varies the relative contributions between the primary and other category. The table below shows the lambda_Primary and lambda_Other values from a LamKchP analysis for the tree values cTau = [6, 10, 100 fm]. The fit results extracted with these three different sets are all consistent.

cTau (fm)	Primary	Other
6	0.485	0.242
10	0.509	0.218
100	0.547	0.182

lambdaFIT: the motivation for the variation of the Lambdafit linked to the non Gaussian source is really confusing. This parameter does not account at all for non Gaussian shape or for the outward elongation of the source. Either you test a different source shape, which although is complicated in the Lednicky model or the variation of the parameter is completely arbitrary.
Also because a real systematic variation would be to vary the single contribution of the secondaries by keeping the sum of the secondary constant and by fitting the lambda_fit parameter you modify the primary to secondary ratio but not the single contribution of the secondaries. The variation of the secondary to primary ratio could also be a systematic check (variation of 10% maybe) but the fit of lambda_fit does not make much sense to my opinion.
On the other hand you demonstrate in the analysis note that the results do not change even if you keep it constant to 1.
I would suggest to reformulate the variation in the text more as a systematic variation.

The motivation for lambdaFit is not to account for the non-Gaussian nature or the outward asymmetry in the source. The lambdaFit parameter balances the combined contributions from the primary and residual correlations with that from the “Other” and “Fakes” categories. Therefore, leaving the lambdaFit parameter free allows these contributions to vary somewhat. Of course, the lambdaFit parameter does have some restrictions on the values it can assume. Basically, lambdaFit*(lambda_Primary + lambda_Sig0K + lambda_Xi0K + lambda_XichK) should never be greater than one.

As you will see, with the new background treatment the lambdaFit values are between about 0.8 and 1.1, which I believe are acceptable values. Forcing all lambdaFit parameters to be 1.0 only changes the results slightly. If possible, it is probably easiest for me to present the results during a video conference so we can discuss them in real time and converge on the final procedure.

LK0s background : Why should only the LK0s background fail? It looks like there is a bias due to the selection cuts and it is very suspicious that the Therminator shape has the same shape as the data. Is it possible that also the signal suffers from this BIAS?
How does the LK0s system respond to the Stavinskiy method? In the AN fig 15 it looks like that the correlation can be flattened out. What happened if you sum part-part and antipart-antipart and you fit this correlation function ? Do you get the same scattering parameters.
In general we need more statistics for the THERMINATOR2 background maybe. Would this be possible?

I’m not sure what exactly you mean by bias. The LamK0 background differs from those of the LamKch essentially because of the upper pT cut in the Kch selection. This effect is visible in both the data and the simulation.

I believe that, with the improved THERMINATOR 2 statistics, the simulation can now be used to model all of the LamK systems. Again, the improved statistics come from using all particles, instead of just those marked as “primary” by the simulation.

The Stavinskiy method does flatten out the LK0s system, but not as well as the LamKch systems.

As explained earlier, the pair and conjugate systems are already fit together.

Source Discussion: the paragraph at the end of section 4, where the explanation of the radii on the mT scaling plot is put forward, leads to qualitative explanations. The text is too long and should be shortened but any chance you can include numbers that show that the difference seen in Figure 7 really comes from the two different sources?
Some more quantitative statement would be nice if possible.

Abstract:

It is too long, we suggest to get rid of some text:

L10: , characterization the pair emission’ -> ‘characterization of the pair emission’

L12 : , The fit assumes a common radius and lFit parameter for each centrality bin, shared across all L K pair systems’ -> delete

L 17-18 ‘In the experimental data, a striking difference is observed in pairs with low relative momenta
(Kk_ .100 MeV) between the LK+ and LK correlations, and the LK0
17 S system exhibits features between the two’ -> delete

ll.17-19 It should be mentioned that the negative and positive scattering lengths mean attractive or repulsive interaction, respectively, since these signs of the scattering lengths in femtoscopy are opposite to those usually used in the scattering theory.
Explicitly mention this later, when the complex scattering amplitude is introduced around Eq. 4, and in the Results section. I changed the text here, and instead of stating the sign of Ref0, I simply state that LamK+ is repulsive and LamK- & LamK0 are attractive.

Introduction

L27 : ‘two(or many)-‘ -> ‘ two (or many)’

L39-40: , In many pair systems, the contributions to the correlation function from quantum statistics and/or the Coulomb interaction overwhelm that of the strong interaction, making it difficult to extract scattering information.’ -> delete

ll.43-50 Please, add a reference to theoretical papers where lambda-kaon (or similar systems) were discussed. I think the introduction devoted to kaon-lambda correlations is still too poor. There is no physics discussion about the difference of correlations in three systems (LK+, LK-, LK0s). Even in the abstract, it is written more about that.

I do not know the exact mechanism driving the differences between the three systems. So, and discussion of this different would be speculation.

L49-50: references missing

Section 2

lines 90 and 92: N sigma - N is italic, but in line 101 and in whole Table 1 is not.

line 95: Table 1 -> Table~1 (in latex mode)

page 4, Table 1: line with p < 0.65 GeV/c is somewhat in the middle of two lines - maybe it should belong to line with TOF and TPC available? i.e. the bottom part of the table could be more readable.

L105: mention [arXiv:1709.01731]

L 109- 113: ‘L(L¯) and K0S particles are electrically neutral, and cannot be directly detected, but must instead be reconstructed through detection of their decay products, or daughters. In general, particles which are topologically reconstructed in this fashion are called V0 particles. The decay channel L ! pp was used for the identification of L hyperons (and, similarly the charge-conjugate decay for the L¯identification), and K0S ! p + p for the identification of K0S mesons.’

Replace with

‘L(L¯) and K0S particles are reconstructed through their weak decays: L->ppi (AntiLambda –>) and Kos-> pipi. The obtained candidates are denominated as V0 particles.’

L115: ‘ To construct a V 0 particle, the charged daughter tracks must first be found’
➔ delete

l140-141 , candidates, the candidate with the smallest DCA to
the primary vertex is kept, while the others are removed. ‘
➔ change into
, candidates, only the candidate with the smallest DCA to the primary vertex is kept’

l145: normally the resolution can be expressed as the difference of the measured mass with the nominal PDG value and the sigma of the Gaussian fit can be quoted as the resolution.
I have not seen this before, but I can certainly calculate this.

page 6, Table 2: 3.8 MeV -> 3.8 MeV/c^2; DCA among daughters should be in standard deviations, check for example table 1 in https://alice-publications.web.cern.ch/node/4103 or https://arxiv.org/pdf/1512.07227.pdf or https://arxiv.org/pdf/1204.0282.pdf or table 2 in https://arxiv.org/pdf/1307.6796.pdf
I have never seen it expressed like this in femtoscopy papers. I’m not sure what exactly this means, so if you want me to express the cut in terms of the standard deviation we will need to discuss it.

page 6, Table 3: 13.677 MeV -> 13.677 MeV/c^2 ; 2.0323 MeV -> 2.0323 MeV/c^2; DCA among pi pi daughters in sigma
Again, in femtoscopy papers I have not seen the DCA expressed in terms of sigma.

for all three tables: this is really very negligible detail, it is just a matter of taste - e.g. p < 0.5 and p > 0.5 - you can add = to one of < or > (just to include also very improbable possibility in computing that something can be exactly 0.5.. :-))

page 7, fig. 1: some people still use B/W printer for reading articles, I would suggest to keep line colours, but to change line styles (dashed, dotted, ...) to have it distinguishible on B/W paper. Same can be applied for figures 4-6, if you do not want to change them, just put "(colour online)" into figures caption.

Section 3

L174-176 : , Equation 2 reveals the limitations of femtoscopy; at best, it can probe the distribution of relative positions of particles with identical velocities and total momentum P as they move in an asymptotic state [1]. Therefore, the entire size of the source is not measured, but rather the “regions of homogeneity” [14].’ -> delete

l183: Nmix not defined before

l.188 There are three different centrality ranges (0-10, 10-30, 30-50%) used to construct the numerator of the correlation function. Why do you use the same centrality range (5%) for mixed events to construct the denominator of the correlation function in all three centrality bins?

The purpose is to mix only events of like centrality. I suppose variable bin sizes could be used, but it is not immediately apparent what the size of the bins should be.

However, it is clear that we should not use one single bin for each centrality range (I'm not sure if this was your question?). Take, for example, the 10-30% centrality range. The numerator of the correlation function consists of an average of all events within the 10-30% range. We want our reference distribution to reflect this same situation, but without femtoscopic effects. If we were to mix a 10% central event with a 30% central event, the resulting mixed-event would not look like either. In such a case, the denominator distribution would be the average of these incorrectly mixed events.

line 192-193: I do not understand "instead of the center of the TPC" - I assume after the correction particles from both mixed events share same primary vertex..?

Maybe we can speak about this in person, as I am not describing this well. The point is to align the event vertices before mixing. We would not mix two events with primary vertices which are separated in space, as, for example, their tracks will start with non-zero separation, and will therefore be biased to larger average separation values. By measuring all tracks with respect to the primary vertex of the collision (via the vertex correction procedure), this places the primary vertex at the origin of each reference system, therefore causing all mixed events to have aligned vertices.

In any case, the sentence has been removed together with the others in your following comment.

l190-194: , After the event binning, a vertex correction is applied to each event, in which the primary vertex position is subtracted from the TPC track positions for each particle in the event. Subsequently, the tracks in an event are all measured relative to the event vertex, instead of the center of the TPC. This effectively gives each event the same primary vertex position, which is important for the implementation of the average separation cut in mixed-event pairs.' -> delete

l 197: 'should be the same' -> is comparable

l204-209: Is it necessary to show the wave function and effective range approximation for f? Can't you summarize in 2 lines the approximations made to obtain formula 6 and also omit formula 7?All of this is rather standard in femtoscopy and this paper is already long enough.

L220: , obtaining a pure sample of primary LK pairs is impossible to achieve;' -> delete

l.226 Here feed-down channels are chosen without any explanations. Previously there were two options accounting for residual correlations. Please, give an explanation here.

L 227-228: you mention here K* and Sigma* within the other feed-down sources but both are considered to be source of primaries later on.
This is not consistent. Differentiate between weak decays and strong decays mentioning that the latter still contribute to the primary correlation function and eventually only distort the source.

L249: as mentioned already here the Lambda parameters are addressed differently than before (lambda and LambdaAB instead of lambdaij). This is not consistent and a bit confusing. Please make it homogeneous.

Eq. 11 is redundant. The text explains it already.
Removed both Eqs. 10 and 11.

l.258 It should be mentioned here what are the contributions from the efficiency of reconstruction and from the yield of particles? How are they related to each other?

l.261. Why $c\tau < 10$ fm ? We know from previous version of the draft and from the Note that it is important for the extracted parameters.
Hopefully this is explained better now. As long as K* and Sigma* contributions are included in the "Primary" category, the exact value of cTau

L259-262: move this remark above in the text, at line 226 where you discuss what is a primary and what is a secondary

l.263-269 The discussion of the pair purity is not clear. Why the purity of a pair of particles is discussed under the assumption of the ideal identification of cones. In reality, the purity values for lambdas and kaons are on the same level.
I have adjusted the lambda parameters so now lambda_Other = 1-PairPurity = 1 – Purity(Lambda)*Purity(K).

L272-273: 'However, little is know about the interaction between the particles in 273 the residual pairs of this study, and assumptions must be made. For this analysis'
replace with :
'Since the interaction between these particle is not known '

l278-281: 'For residual pairs affected by both the strong and Coulomb interactions, things are a bit more complicated. This is due to the fact that, for the case of both strong and Coulomb interaction, there no longer exists a nice analytical form with which to fit. Generating a correlation function including both is also time consuming, as described further in Appendix B.'
➔ delete

l282: ' in this case, there is no need to make any assumptions about scattering parameters or source sizes' -> delete

l 283: Consist -> Consistent

l287-288: 'the strong interaction is necessary for the fine details. As these correlations are run through a transform matrix, which largely flattens out and fine details, a Coulomb-only description should be sufficient.' -> delete

l293 'Smearing of the momentum typically will result in a suppression and broadening of the signal.'
-> delete

l.304: this is a bit misleading since the background is for us most important at small k* and not at large k*, please reformulate.

L306: ‘not due to any interesting physics’ -> delete

307-317: this is too long and with many repetitions. Shorten it.

L318: The THERMINATOR…

l.318 It should be mentioned here (or somewhere else) that the Therminator simulation does not include femtoscopic effects.

l.321 Therminator is in green color. I suggest changing the scale for Fig.2. Now the Y axis scale is the same for all three centralities. However, the effect of baseline is different for each centrality bin. The scale is reasonable for 30-50% centrality and the scale is too small for 10-30 and 0-10%. In addition, there is a problem with the normalization, e.g., K0sL in 30-50% centrality.

L320-321 ‘After issuing each simulated event a random Y EP , THERMINATOR 2 did an exceptional job of describing the LK data. Furthermore, the simulation showed the non-femtoscopic background affects the correlation function as a separable scale factor.’ -> delete

l323: simulations are in green not in gold. Revise also the caption of Fig. 2 accordingly.

L329-330 ‘At the time of the fit, the polynomial used to correct each correlation function could only be adjust by a simple scale factor to best match the data.’

What does this sentence mean exactly? Why at the time of the fit?

I mean that the background is fixed before the fitting of the signal region. I have reworded the sentence, and hopefully it makes more sense now.

L332: since you still use the linear form for the LK0s background you should show it in Fig 2.
We are now using a polynomial form with THERMINATOR 2 for all backgrounds.

Is eq. 13 necessary? Would the reference not be enough?

l.335-341. Again, I do not agree that you keep this method (rotation) in the paper. Initially, this method was proposed for one-event two-pion femtoscopy where there was no alternative (pi+pi- CFs are strongly distorted by resonances, the contribution of mini-jets and so on).
I believe the inclusion of the method is interesting because we show its effect with a different application/intent than proposed. This information could be of use to a researcher in the future.

L363: ‘the raw data is never touched’ -> delete

L 380 381: ‘In order to understand the systematic uncertainties of the data, the analysis code was run many times using slightly different values for a number of important cuts, and the results were compared.’ -> delete

In the discussion of the systematic errors also a variation of the limits of ctau for the primary (default 10 fm) should be included.

page 13, fig. 2: gold -> green

page 15: Table 5: 3, 4, 5 mm -> 0.3, 0.4, 0.5 sigma (or standard deviations)
page 15: Table 6: 3, 4, 5 mm -> 0.3, 0.4, 0.5 sigma; Max. DCA of K+- - in cm
Again, in femtoscopy papers I have not seen the DCA expressed in terms of sigma.

l.364-378 I would add the final formula for the fit function.
It is somewhat difficult to write a single formula, as the generation of the fit function has several steps (generation of primary component, generation of residuals which are run through transform matrices, momentum resolution procedure, non-femtoscopic background correction).

Section 4

L 399: as mentioned before: why don't you fit the sum of LK+ and AntiLK- and so on? The strong interaction is the same and also the background looks the same.
This would reduce your very large statistical errors.
I would also show only the fit of the sum in the paper.

As stated previously, the systems are fit together. They are fit simultaneously with the same fit function. Explicitly summing their numerator and denominators before the fit will not enhance the statistics.

Fig 6: why are the d0 errors for LK- so much larger now than in the previous version oft he paper?
Because the lambdaFit parameter is allowed to be free here, instead of being restricted as before.

l.404-406 This piece is a repetition of what was said above.
Talking here about overall normalization parameter N, not lambdaFit.

l.409 It would better to use the same functional form of the baseline (linear fit to the data) for all systems and to use THEMINATOR to estimate systematics.
We now use the THERMINATOR 2 simulation to model all backgrounds.

l.401-404 The scattering parameters are the same for conjugate pairs. What is the reason for separated fits of conjugate pairs? I suggest combining such pairs.
There are no separated fits for conjugate pairs. These are already combined in the fit process. I will try to do a better job of making this more clear.

l.411 There are no printed values in Figs.3-5. In addition, I did not find the information about chi^2/ndf.
This information was asked to be removed from the figures.

l.416 Does theory predict no difference between three pairs? Please, add some explanation about it.

L421 :' Each of the three systems has scattering parameters unique from the others.' -> delete

about the radius and lambda parameters for the 0-10% centrality:
you write in your comments to the IRC:
'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).'
This effect shows to my opinion that it does not make any sense to leave lambda_fit free. The effect obtained on the different radius is artificial and too large radii are obtained. So please really consider at least imposing some constraints of the values of lambda_fit, since 1.4 seems rather unrealistic.
It will be easiest to use video chat to converge on our final fit procedure.

l.424 Past studies of kaon-proton... Please, add a reference to the Kp study.

l.413-429 do not contain any discussion about effective radii values. Please, add some text.

lines 425-429: the K^+ - p combination is repulsive (according to not cited past studies). While we can interpret attraction in LambdaK^- as similar to attraction in pK^- (due to similar quark composition), we need strange quarks in order to explain the repulsiveness between Lambda and K^+. Can we also somehow explain the repulsiveness in LambdaK^+ according to repulsiveness in pK^+? I apologize, if it is trivial (I can imagine Coulomb repulsion for pK^+ but I guess this was also subtracted?).

I do not follow. I do not know of any simple way to explain LamK from the pK measurements, aside from stating LamK+ is repulsive as is pK+, and LamK- is attractive as is pK-. I do not understand why you say LamK- (uds + $\bar{u}$s) is similar to pK- (uud+ $\bar{u}$s), but that LamK+ (uds + u$\bar{s}$) is not similar to pK+ (uud + u$\bar{s}$)? Maybe we can discuss this through video.

lines 436-438: the LCMS and PRF are not defined (they were in intro in previous draft)

L450 452: ' In general, each single-particle source will have its own size, shape, and space-time position within the produced medium, which is unique from its paired partner.' -> delete

line 454: also also -> also

page 19: fig. 6: for draft printed on B/W printer, the markers are hardly distinguishable - change the marker styles.

l460: can you add a reference here?

l.470-471 Is it possible to estimate the difference of the emission time of K and Lambda from C11 (Fig.8)?
No, I do not believe this is possible without a full fit.

l.477-484. It seems that the mT-scaling violation is the most significant for 0-10% centrality. There is no discussion about it. Please, add some sentences.

line 506: Lednicky -> Lednický; Lyuboshits -> Lyuboshitz (according to original preprint from inspirehep)

line 558: Lednicky -> Lednický

line 574: LambdaK+- correlations -> LambdaK+ (i.e. no K-, or write conjugate as well)

Summary

The obtained values of the scattering length and effective radii should be mentioned in the text of Summary.

Section 5

Appendix

Relative Emission Shifts with THERMINATOR 2.

It would be reasonable to add C11 functions for different source shifts in the outward direction and compare these functions with the results shown in Fig.8.
OK, I can work on this.

Figs. with data: It is necessary to add "ALICE" and/or collision system/energy somewhere on the pads.

Figs. 3-5 "Preliminary" should be removed

L575: Looking at the analysis note I have the impression that the statistics is a problem. But other than that I dont see what you write that the Stavinskiy method does not work for LK0s. Why dont you look at the some of part-part and antipart-antipart?
I have removed the sentence. The Stavinskiy method does not flatten out the LamK- as much as the LamKch systems, but that fact is probably not worth stating here.
Pair and conjugate combined for simplicity

In the Appendix l.573-575:
"The results of correctly implementing such a procedure are shown in Figure A.1. The figure shows that the Stavinskiy method does a very good job of ridding the ΛK ± correlations of their non-femtoscopic backgrounds."
I think we will not see "a very good job" if we compare Fig.2 and Fig. A.1 with the same scale on Y-axis (CF in the range of 0.96-1.05). If the authors insist on the presence of this method in the paper, then it is necessary to make a correct comparison of the data and baseline. In Fig. A.1, you need to add a baseline from the Therminator and make the scale convenient for comparison. In my opinion, it will become clear that the rotation method describes the baseline much worse. In addition, it is not suitable neither for LK+- nor for LK0s. This method cannot be used to estimate systematic uncertainties as it was done in first ALICE publications on pion correlations

(https://arxiv.org/pdf/1012.4035.pdf
https://journals.aps.org/prd/pdf/10.1103/PhysRevD.82.052001).
I do not understand why any sort of THERMINATOR simulation is needed here? The point of the procedure is that it flattens out the background, which it clearly does. The method is not supposed to match any baseline, it seeks to remove it. The demonstration is not only to show how it affects the background, but also how it affects the signal region. Therefore, I do not want to zoom in on the Y-axis so much that we lose the low-k* signal region.

It may be easiest to discuss this in real time.