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.

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.

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.

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 infact it is not very clear when femto stop to 'act'. After 5 6 7 fm?
This is why we absolutely need this systematic variation.

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.

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 backgorund maybe. Would this be possible?

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 sourcese?
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
(k_ .100 MeV) between the LK+ and LK□ correlations, and the LK0
17 S systemexhibits 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.

Introduction

L27 : 'two(or many)-' -> ' two (or many)'

L39-40: , In many pair systems, the contribu39tions to the correlation function from quantum statistics and/or the Coulomb interaction overwhelm that 40 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.

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($\bar{L}$) and K^0_S 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 V^0 particles. The decay channel L ! pp□ was used for the identification of L hyperons (and, similarly the charge-conjugate decay for the $\bar{L}$ identification), and K^0_S ! p+p□ for the identification of K^0_S mesons.'

Replace with

'L($\bar{L}$) and K^0_S 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.

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

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

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 176 “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?

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..?

l190-194: , After the event binning, a vertex correction is applied to each event,
191 in which the primary vertex position is subtracted from the TPC track positions for each particle in the 192 event. Subsequently, the tracks in an event are all measured relative to the event vertex, instead of the 193 center of the TPC. This effectively gives each event the same primary vertex position, which is important 194 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 consisntent. 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 differentely thant before (lambda and LambdaAB instead of lambdaij). This is not consistent and a bit confusing.
Please make it homogenous.

Eq. 11 is redundant. The text explains it already.

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.

L259-262: move this remark abvoe 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.

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 compli279 cated. This is due to the fact that, for the case of both strong and Coulomb interaction, there no longer 280 exists a nice analytical form with which to fit. Generating a correlation function including both is also 281 time consuming, as described further in Appendix B.'

➔ delete

l282: 'in this case, there is no need to make any assumptions about scattering
283 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 288 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 YEP, THERMINATOR 2 did an exceptional job of
320 describing the LK data. Furthermore, the simulation showed the non-femtoscopic background affects the
321 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?

L332: since you still use the linear form for the LK0s background you should show it in Fig 2.

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).

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 381 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 standar deviations)
page 15: Table 6: 3, 4, 5 mm -> 0.3, 0.4, 0.5 sigma; Max. DCA of K+- - in cm

l.364-378 I would add the final formula for the fit function.

Section 4

L 399: as mentioned before: why dont 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.

Fig 6: why are the d0 errors for LK- so much larger now than in the previous version oft he paper?

l.404-406 This piece is a repetition of what was said above.

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.

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.

l.411 There are no printed values in Figs.3-5. In addition, I did not find the information about chi^2/ndf.

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
422 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.

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?).

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
451 own size, shape, and space-time position within the produced medium, which is unique from its paired 452 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)?

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.

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?

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 $\Lambda K^{\pm}$ 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).