Thank you very much for your thorough review of our work. I greatly appreciate the time you took to complete this. I apologize for the delay in addressing these comments, and thank you for your patience. I will submit, together with the new draft, a document highlighting the changes made to the text. Much effort was also spent in cleaning up the figures, but these changes are not included in that document. Please contact me or Tom if you have any questions or additional comments. Thanks again.

Cheers,
Jesse

- In the papers about "non-traditional" femto that we're currently publishing, a point is made that we can do the measurement in pp collisions due to the smallness of the source. It would be good to make some mention in the introduction about why we expect to be able to measure the interaction and scattering parameters in PbPb collisions too.
  - Can you elaborate a little bit on what you would like to see? For this analysis, we were not sure at the beginning whether or not we would be able to measure the scattering parameters, i.e. it began as an exploratory analysis.
- ~~throughout the paper, be consistent about using AK or A-K (for all femtoscopic pairs)~~
- ~~lines 42-43 : remove one "not" in "not previously not known"~~
- ~~lines 70-71 : the inclusion of "central, semi-central, and minimum bias" doesn't really make sense unless you explicitly introduce the idea of triggered data (since central and semi-central events are part of any minimum bias sample)~~
  - Removed
- ~~line 73 : define the z position as being along the beam axis~~
  - Removed "z", included "along the beam axis"
- ~~lines 74-75 : "the event must contain at least one particle of each type from the pair of interest" sounds like a tautology, at least for the same-event correlations, because if an event does not contain at least one of each particle type then it automatically doesn't enter the correlation. If this is an additional condition that you impose on the events in the mixing pool, then this should be specified in the section about event mixing.~~
  - Yes, this is an additional condition imposed on the events in the mixing pool. This information has been moved to the section about event mixing.
- ~~line 83 : I think the specification of the pseudorapidity of the TPC and TOF is not necessary, since you don't use tracks outside of $|\eta| < 0.8$ regardless of the PID. Instead, the η acceptance should be stated elsewhere where the general kinematic cuts are described.~~
- ~~line 96 : established on → established based on~~
- ~~line 101 : is "methods" the right word here? All the PID is based on Nσ cuts~~
  - Additional methods → Rejection procedures
- ~~line 108 : maybe add "due to their decay topology" after "V^0 particles", otherwise the definition sounds arbitrary~~
- ~~line 110 : "exposed" is not the right word here~~
  - are also exposed to a minimum requirement of their impact parameter… → must also meet a minimum requirement on their impact parameter..

- ~~Table 1 : for consistency in the notation, you can remove the – and ± signs in $N\sigma e^{-}$ and $N\sigma K^{\pm}$ in the "electron rejection" section~~
- ~~Table 2 : for consistency in the notation, format N_{σ_{TPC}} as N_{σp,TPC}~~
- ~~Figure 1 : I think that in ALICE publications we normally draw invariant mass plots with points including statistical error bars, not a connected line (but you could double check other papers)~~
  - It actually seems that the connected lines plots are standard for invariant mass plots, at least within the femtoscopy group (e.g. https://arxiv.org/abs/1903.06149, https://arxiv.org/abs/1506.07884v2, https://arxiv.org/abs/1805.12455v2). I'm not sure why this is, but I am happy to change the style if you believe points are most appropriate (the error bars of which are smaller than the marker size). For now, I will leave it as is.
- ~~Figure 1 : It would be good to specify in each figure the pair (πp or ππ), either in the x-axis as m_{πp} and m_{ππ}, or elsewhere~~
- ~~lines 162-163 : I don't understand the sentence "The constraint values used coincide with the values at which the average separation correlation functions stabilize to unity" (what are average separation correlation functions?)~~
  - The average separation correlation functions are formed exactly as the normal relative-momentum correlation functions, except they are binned in the average separation instead of k*. This is constructed, for example, between the proton daughter of the Lambda and the pi+ daughter of the K0s.
  - This piece of information does not seem vital, and it is somewhat difficult to explain concisely. Therefore, I have decided to remove the sentence. If you disagree, and believe it should not be removed, I will include it.
- ~~line 171 and Eq. 1 : should it be written $dN/d^3p_a d^3p_b$ or $d^6N/d^3p_a d^3p_b$? (similarly with dN/d^3p_a or d^3N/d^3p_a)~~
  - Sometimes I see these superscripts suppressed, but I have now included them for clarity.
- ~~line 194 : is pair transverse momentum (...) integrated → is integrated in pair transverse momentum~~
- line 195 : limited data → limited statistics
  - I believe the guidelines advise the use of data over statistics (https://twiki.cern.ch/twiki/bin/view/ALICE/GuidelineEditing)
- ~~lines 196-199 : the comment about the magnetic field seems out of place, would it be better to go in the systematic uncertainty section, or some other section which is more explicitly about the experimental setup?~~
  - I have moved these sentences to the "Summarized correlation function construction" section.
- ~~line 212 : "are the analytic functions" → what analytic functions?~~
  - Removed "the", i.e. now reads "are analytic functions". A reference to the paper containing these functions is also now included, for the interested reader.
- ~~line 221 : finally observed → observed~~
- ~~lines 222 and 285 : it's always dangerous to write "assumed" in a scientific paper. Was this assumption checked? I could easily envision that it's not true, for example if the kaon is actually a misidentified pion, and you're measuring a Aπ pair~~
  - The idea is that the net contribution from all of these different systems is assumed to average to unity. We expect some systems to contribute a positive signal and some a negative signal, with the net effect being zero.

- I'm not sure I follow why assumed should be removed. If an assumption can be checked, and is checked and found to be true, then it seems it is no longer an assumption. The problem here is that there is no simple and straightforward way to check the assumption, as we do not know exactly which systems enter our analysis, and even if we did, we would not know how the correlation functions for each system behaves.
- In any case, I am not an expert at writing publications, and certainly want to reduce the chances of hiccups in the future publication process, so I have changed "assumed to" → "taken to".

- ~~line 223 : pairs whose members originate as daughters from resonances → pairs in which one member originates from a resonance decay (right?)~~
  - Right. This has been changed to "Pairs in which at least one member originates from a ~~resonance~~ particle decay" to be as clear as possible. Note, as suggested by another reviewer, the word "resonance" will likely be relaxed to "particle" or "particle and resonance", as, for example, Xi- isn't typically described as a resonance.
- ~~line 228 : finally measured → measured~~
- ~~the notation of Eq. 6 and Eq. 7 is not consistent, because in Eq. 6 $\Sigma_{ij}\lambda_{ij}$ does not include the primary ΛK pair but in Eq. 7 it does (this is clarified in the text at line 235). Perhaps it would be clearer to just define $\Sigma_{ij}\lambda_{ij}$ + λ_{ΛK} = 1, where ij ≠ ΛK. Furthermore, would it be possible to remove the additional symbol λ', by just including λ_{fit} in Eq. 6 explicitly?~~
  - This is a good point about the inconsistency between Eq. 6 and 7. Looking throughout the rest of the paper, I loosely use λ_{ij} to mean both the primary ΛK contribution and the residuals. Therefore, it seems it may be best to write Eq. 6 as $C = 1+\Sigma_{ij}[C_{ij}(k^*_{\Lambda K})-1]$, as to state in the text that the primary ΛK component is included in ij. I will change it as such, but please let me know if you believe your proposed revision makes more sense.

  - Yes, it would be possible to remove the additional λ' from Eq. 6, but I think this ($\lambda_{Fit}\lambda_{ij}$) would look a little confusing to femtoscopists who are used to seeing a single λ. For now, I will keep this as is, but am willing to change it if you think it best.
- ~~The fitting is mentioned many times throughout this paper, starting with line 237, but what is missing is an explicit statement "we fit the experimental correlation function".~~
- ~~line 241 : daughter → "daughters" or "daughter system"~~
- ~~line 244 : it would be nice to have an example or examples like this ("e.g. ΣK") come earlier, in the paragraph around line 218, to help the reader~~
- ~~lines 253-256 : I didn't understand this section where line 253 says "experimental Ξ^- $K^\pm$ data are used" and then line 256 says "instead of using the experimental data"~~
  - One may obtain consistent results using either the experimental Ξ^- $K^\pm$ data or modeling the Ξ^- $K^\pm$ system with the Coulomb-only scenario. I have changed to wording to hopefully be less confusing, but please let me know if it needs further clarification.
- ~~line 254 : Ξ^-K^+ the system → the Ξ^-K^+ system~~
- ~~line 263 : p-p → pp~~
- ~~in the paragraph around line 261, I would suggest making it clear that the reconstruction efficiency that we care about is that of the daughters under study, not the parent particles. For example, when discussing the contribution of ΞK to the AK correlation function, the reconstruction efficiency of the Ξ is not relevant~~

but the reconstruction efficiency of the secondary Λ is.

- line 270 : if possible, try to avoid using two different definitions of the word "primary" in one line
- line 282 : τ_{max} is not defined, it should be in line 280
- line 287 : included → includes
- line 292 : 1.0 → 1
- Table 4 : is there any reason why the λ values for the different species (i.e. ΛK^+ vs anti-ΛK^-) should be different? Or are the deviations just due to the statistics of Therminator?
  - These are just deviations due to the statistics of THERMINATOR.
- lines 313-314 : "The behavior of the non-femtoscopic background is needed" is an odd formulation, actually what we need is an understanding of the non-femto background
- lines 317-320 : why is it necessary to describe something (weighting pairs by the wave function amplitude to introduce femto effects) that is not done in this analysis? I think these few sentences can be removed
- line 327-328 : I don't understand the phrase "application with the experimental data". Maybe this sentence is meant to say that before fitting the experimental data, the coefficients of each polynomial are fixed by fits to the Therminator background.
- line 333 : I think the sentence "the non-femtoscopic background correction was applied as a scale factor" is unclear. It's applied as a multiplicative factor to the entire correlation function during the fitting process, right?
  - That is correct, and the wording has been changed.
- line 345 : scattering parameters unique from each other → unique scattering parameters
- line 347 : add R after "common radius parameter"
- line 352 : remove "and"
- No critique here, I just wanted to say I like Sec. 3.6. After a long detailed description of a complicated fitting procedure, I found the summary really helpful!
  - Thank you!
- remove and rephrase "residually"
- throughout the paper, and particularly in Sec. 3.6 it is not always clear whether the words "correlation function" refer to the experimental data points or to the fit function. I would prefer to keep "correlation function" referring only to the experimentally measured points, and find some other name for the fit function, but one could also rephrase things in a different way.
  - I have rephrased things slightly, but please let me know if you have further suggestions or think more thorough rephrasing is needed.
- Eq. 13 is a redefinition of $C_{fit}(k^*_{rec})$ from Eq. 10
  - I have removed the "fit" subscript from Eq. 10
- Fig. 2 caption : I suggest combining the last two sentences: "This polynomial is shifted and scaled to match the experimental data and is drawn as a solid curve"
- Fig. 2 caption : define what the rows are (centrality bins)
- Fig. 2 : maybe instead of "ALICE bgd. fit" in the legend, what about "scaled bkd. Fit"?

- Changed to Scaled Bgd. Fit

- ~~Fig. 2 : it looks like only the lowest k* bin has systematic uncertainties, or are they just smaller than the markers in all other bins?~~
  - Yes, they are smaller than the markers in all other bins.
- ~~line 391 : & → ⊕ (for consistency with other places in the paper)~~
- ~~line 393 : receives → has~~
- ~~in the figures, Λ is sometimes written in bold and sometimes in normal text, and is written in a different font than the rest of the text~~
- ~~line 407 : the ΛK^- → ΛK^-~~
- ~~line 409 : remove "effect of the"~~
- ~~lines 416-418 : make the typography of the pair types consistent with the rest of the paper (K^-–p → K^-p or pK^-)~~
- ~~line 418 : could be due to an effect arising from → could arise from~~
- ~~line 423 : a type average → an average~~
- ~~Fig. 5 : are the points from the ΛK analysis artificially offset in m_T for visibility? If so, it would be good to state this in the caption, or even remove the offset because none of the other systems are offset~~
  - No, they are not offset. The average m_T value changes slightly with centrality (phase space changes while the cuts remain the same).
- ~~line 427 : remove "extracted"~~
- ~~line 434 : I'm not sure how "good global m_T scaling is defined", so maybe just "global m_T scaling"?~~
- ~~line 438 : how → why~~
- ~~line 442 : I found the phrase "not the pair sources" to be confusing, although I understood what was meant later in the paragraph. However, I'm not sure that expressing the difference between, for example, KK and ΛK femtoscopy, in this way is effective. Femtoscopy is always in some sense a pairwise effect, and so in the case of identical particle femtoscopy it's just that the single-particle source is the same as the pair source, right?~~
  - Essentially, yes. For identical particle systems, the pair source radii are related to the single-particle source sizes by a factor of √2
  - I have tweaked the wording here a little bit to hopefully be clearer.
- ~~Table 6 : & → ⊕ (for consistency with other places in the paper)~~
- ~~line 483 : for each system → present in each system~~
- ~~to make the final sentence of the paper stronger, and so that all the major conclusions of the paper are included in the summary, you could add "which is confirmed by the spherical harmonics decomposition of the correlation functions" at the end~~
- ~~line 595 : transition → transformation~~