Measurement of the relative branching fraction of $\Lambda_b^0 \to \Lambda_c^+ \overline{D}^{(*)0} K^-$ and $\Lambda_b^0 \to \Lambda_c^+ D_s^{*-}$ decays – WG review round 2

Based on v1r1, Jan. 16 2023.

Comments from Liupan An

Q Fig.6(b) If I understand correctly, the fractions of all components in simulation are chosen according to the AmpGen model, which shows discrepancy with real data. Could you add the corresponding distribution from simulation here? Also, for my curiosity, how did you extract the Run2 data distribution? Besides, I think it is also helpful to add a comparison of the M(LcDK) distributions from 1) Run2 data; 2) AmpGen model; and 3) simulation to validate "the shape of the partially reconstructed Lb->LcD*K decay is not sensitive to the detailed composition of various amplitude models".

A Fig 6b) (also a) to some extent) doesn't have that much relevance for the Analysis and describes intermediate step (that was important when developing the analysis). The mix of components was a pure guess, that allowed us the use the MC a bit more efficient, as we would have larger weights when using phase space (and thereby less effective statistics). The data for that plot was coming from a narrow region around the D^* partially reconstructed component with a missing π^0. Mindaugas has made the more accurate version of this plot with the ECC method in the $\Lambda_b^0 \to \Lambda_c^+ \overline{D}^{*0} K^-$ note. I'll work on implementing the plot you request, which will supersede Fig 6a).

Q Table 12 The N(D0pi0)/N(D0gamma) ratio seems to not be fully consistent with the known value. Since you mentioned that residual efficiency effects are negligibly small, is there any other effect I forget to consider?

A The efficiency correction is close to 1 sigma upwards. So it pushes the PDG value 1.83 ± 0.07 to around 1.89 (with the BESIII value only, we'd end up around 2). Taking systematics into account, we'd probably get around a 1 sigma pull. The systematics include the combined D^* shape that uses the PDG branching fractions of D^* as generator level input (fit F). In that way, also the efficiency correction is taken into account. Should there be further checks on this?

Q Table 15 I think it will be helpful to add the level of agreement (XX sigma) wrt the nominal result for each variation.

A You are right, the uncertainties quoted there are not really meaningful and we will replace it by a pull w.r.t. the nominal result.

Comments from Lorenzo Capriotti

Q For your kind-of partial wave expansion (the box/hills/horns method) you say that you - correctly - propagate the limited MC size error by Gaussian-constraining the (analytical) parameters obtained from the calibration fits. I see no mention of this for the KDE method, do you implement something in that regard (or did I miss it in the note)?

A In the way they are set up, there are only “1.5” free parameters left, as the endpoints are fixed from kinematics. The remaining parameters are the resolution, which I count as $1/2$ a parameter as it is shared with all other partially reconstructed components (we are far enough from the LcDK/LcDs threshold to not have a mass dependence), and the slope/ξ for Box/Hill or Horns. While the ξ parameter is left floating, the slope of the Box shape is constrained from fitting the efficiency as a function of mass with a linear function. We can explain this a little better in the note. The variation of KDE components is covered by the systematic uncertainty on the fit model. Did you have something else in mind that we could try?

Q Regarding the comment on adding the individual efficiencies: my goal is not to learn something but rather to check if the calculations are done correctly, including error propagation. This is part of the review process and that is linked to one of the general comments I made last time - that quantitative information was mostly missing in the previous iteration of the note. From my experience it is not so uncommon to find mistakes in calculations during review. I understand that this info might be difficult to obtain - as well as more numerical info such as individual fit results including the calibration on MC, for example, which I would also have liked to see - due to the structure of your workflow. So, if adding the individual efficiencies is something that can be done in reasonable time, please add them- If it takes too long - and you can reasonably decide what "too long" means - then it is fine to leave it as it is.

A I can try to dump them from the workflow and put them in an appendix. Please let me know if that’s a good solution. However, the robustness checks, in particular the 4 different selection strategies and the 3 different weighting methods, make me very confident that we didn’t miscalculate. Also I calculated uncorrected efficiencies by hand several times to see whether the numbers after weighting and combination are in the same range.

Comments from Zan Ren

Q L.131 where does the number 5.4 fb^{-1} comes from? We usually use 6 fb^{-1} as a rough value to the outside, even calculate from the sum of Table 13, it shouldn't be this number, either.

A Good spot, it should be 5.7(17) fb^{-1}. 5.4 is without 2015.

Q L.471f Could you please show some details on where does the number "2%" come from? If this is a highly-credible value, then the relative yields between the $\bar{D}^0\pi^0$ and $\bar{D}^0\gamma$ modes will be easy to constrain, why didn't you fix the relative branching fraction in the default fit?

A If we use the patch mentioned in L.475, we can trace how many $\bar{D}^0\pi^0$ and $\bar{D}^0\gamma$ decays have been reconstructed. Since we know the generated number, it's trivial to calculate the ratio of reconstruction efficiencies.

Q L.477ff Have you considered the uncertainties of this reweighting?

A The effective statistics of the MC sample is degraded by applying weights. It is included in the MC statistical uncertainty together with the kinematic weights.

Q Fig. 6 I think it would be more convincing if you draw the $M_{\text{inv}}(LcDK)$ distribution in simulated samples, with or without the D_{s1}^* resonances, or by changing their relative fractions of them, to show that the DK amplitudes have negligible effects the the partially reconstructed $M_{\text{inv}}(LcDK)$ shape.

A Liupan had a similar comment. We added a plot similar to what you requested.

Q Fig. 8 I am wondering if the gen-level fast-simulated distributions can be a perfect-enough description of the single-charmed decays? Can you compare it to the full-simulation (after your nominal selection criteria)? By searching Sec 5.1, I found that the systematics of this kind of line-shapes are not considered.

A There seems to be a misconception. The single charm decay is a fully reconstructed one and does (almost) not depend on its decay dynamics. I'm writing almost, as the $K^-K^+\pi^-$ system can marginally shape the slope on the wider distributions (c) and (g). Those restrict the $K^-K^+\pi^-$ mass range to the one used for 3D fits. However, in 3D fits we account for this background in a data-driven way, namely by fitting the diagonal band you see in (f) (and less pronounced in (b)). In the final fits, we cut tighter in the $K^-K^+\pi^-$ or $K^+\pi^-$ mass to select D_s^- or $\bar{D}^0$. As you can see in (d) and (h), there is almost no plateau left which could be shaped by dynamics in the $K^-K^+\pi^-$ system.

Q L. 579 Why the uncertainty due to assumptions in normalisation of single-charmed decay should be statistical uncertainty instead of the systematics?

A In an ideal world, you would be able to fit the normalization and get the uncertainty estimate and the impact on the parameter of interest directly – thus account for it as statistical uncertainty. We try to do the next best thing: get and propagate it from the 3D fit. Why would you want to count it as a systematic uncertainty?

Q Table 12 For the fit results, could you please add the covariance matrix of the nominal model as an appendix. Or a least tell the readers which two floating variables report the maximum correlations and at what level.

A Done.

Q Fig. 22 I only see one figure here. So where is sub-figure(b)(c)?

A Thanks! That's a copy-paste error from the previous version.

Q Table 14 Could you add one as the last row to show the total systematic uncertainties?

A Prefer to keep as is.

Q L. 952 I assume the GBReweighter is used. Please specify the package.

A Yes, you are right. Citation added.

Q Fig. 43&45 The normalisation step doesn't take effect for these figures.

Q Fig. 44&46 The last two sub-figures are empty. Then the titles of some x-axes should also be fixed.

A Both well spotted. Fixed now.