1 MaHaiLong's questions

- 1. Sec. 1.1. You use long subsection to explain how difficult to distinguish f0(980) and a0(980), then you use combined amplitude to describe them in your amplitude analysis. I am not very sure whether it is necessary to mention this in INTRODUCTION. Should it be better to directly mention it in your amplitude analysis part?

  We explain the difficulty here to derive the definition of S(980), the shape of which is extracted in Sec. 4 and then used in amplitude analysis part in Sec. 5.

- 2. Figure 22. The left sub-figure seems not very good. You can see that the data points of Ds peak are almost on the fitted curves. Have you checked whether smearing Gaussian improve the fit?

  Figure 22 on page 33 has been updated.

- 3. Line 16 on page 30. You use DIY MC to estimate detection efficiency based on your amplitude analysis results. Have you examined whether there will be some uncertainty due to the fluctuations of your fitted results? If it is negligible, it is better to mention this in your systematic part.

  See Sec. 6.4 on page 33. The systematic uncertainty of Dalitz model is added.

- 4. Systematic uncertainty of BF measurement, or Table 14: Can the systematic uncertainty of ST Ds yields can be really negligible? Especially for the uncertainty due to background fluctuation?

  See Sec. 6.4 on page 33. The systematic uncertainty of ST Ds yields due to the signal shapes, background shapes and fit ranges has been updated.

- 5. Sec. 6, BF measurement part: You obtain the DT yield by 2D fit to M_{inv}^{tag} vs. M_{inv}^{sig}. Why need to obtain ST yields and efficiencies in given mass windows ?

  We do not do 2D fit to get ST yields. For Cat. B (Both the signal modes and the tag modes are $D_s^+ \to K^+K^-\pi^+$.), we fit the average mass of Ds at signal side and tag side. For tag mode $D_s^- \to K^+K^-\pi^-$, we do not constrain the mass window when obtaining the ST yields and efficiencies.

- 6. Table 11:Please mention the BFs of sub-decays are not included in effs.

  See Sec. 6.1 on page 32. Table 11 has been updated.

- 7. Have you checked whether your obtained BF of Ds → a0(980)pi agrees with the one obtained in the Ds → etapi+pi0 measurements? It is better to mention this situation in the summary part.

  See Sec. 7 on page 39. The comparisons of $\mathcal{B}(D_s^+ \to S(980)\pi^+)$ in this analysis and $\mathcal{B}(D_s^+ \to a(980)\pi^+)$ in the $D_s^+ \to \pi^+\pi^0\eta$ measurements is updated in the summary part.

- 8. Summary: In summary and future paper, it should be valuable to provide the BFs of sub-decays. And then, it is better compare some VP decays, e.g. Ds → K*K and phipi, which can be predicted by theory.

See Sec. 7 on page 39. The BFs of sub-decays has been added in the summary part (page 36).

- 9. others. L8 on page 3: meson $\rightarrow$ mesons Table 12, the 2nd row from bottom: pi $\rightarrow$ π.

  Grammar errors mentioned has been modified.

2 Jim Libby's questions

- 1. no systematics are considered in the MIPWA analysis. However the statistical variation of the results is the largest systematic uncertainty on the S(980)pi FF,for example, which is one of the key results. Systematics can come from data-MC agreement for the BDT output and other efficiency effects, the background subtraction, the ansatz of the fit i.e. we know that there are two contributions to S(980); what are the results with a fit with two poles below thresholds with different widths? How would these ansatz affect the amplitude analysis.

  Systematic uncertainties in the MIPWA analysis have been added on page 18. And we also add the results of the attemption to distinguish $a_0(980)$ and $f_0(980)$ in Appendix A. The results show that we cannot measure $a_0(980)$ and $f_0(980)$ very well, which is the key motivation to do the MIPWA analysis.

- 2.the reasoning for the different selections for the MIPWA and amplitude analysis is not clear. (The double tag for the BF measurement is well motivated.) In the amplitude analysis the 0.4% background that is neglected is given as the motivation. But the single-tag sample is also very pure $74/18600 = 0.4\%$! However, I am not sure how this 74 bkg events quoted in the text is calculated exactly, as it is 1/10th of the background in the stat box of Fig. 9. Taking the statistics at face value, surely exploiting the better statistics in the ST sample would be beneficial and extract the S(980) parameters directly from a fit to this sample. This all needs to be clarified.

  Sorry I wrote the number 74 by mistake. The mistake has been corrected on page 14. The number of background should be 735.7 ± 30.0 in the stat box of Fig.9. The background ratio of the data sample (in the low end of K^+K^- mass region) used in MIPWA analysis should be $735.7/(18590.6+735.7) = 3.8\%$. In addition, the background ratio in the low end of K^+K^- mass region is much lower than that of the whole K^+K^- mass region. So in the amplitude analysis part, we use the double tag to ensure lower background ratio.

- 3. Fig 1: is there a good reason why the colour-suppressed K*pi diagram exceeds the contribution of the colour favoured external emission phi/f0 diagram? Both are Cabibbo favoured.

  See Sec. 7 on page 39. The comparison of $\mathcal{B}(D_s^+ \to \bar{K}^*(892)^0K^+)$ and $\mathcal{B}(D_s^+ \to \phi(1020)\pi^+)$ between this analysis and some theory predictions is listed in Table 21. Although $FF(D_s^+ \to \bar{K}^*(892)^0K^+)$ is larger than $FF(D_s^+ \to \phi(1020)\pi^+)$, $\mathcal{B}(D_s^+ \to \bar{K}^*(892)^0K^+)$ exceeds $\mathcal{B}(D_s^+ \to \phi(1020)\pi^+)$.

- 4. P5 line 9: f0 $\to$ S and P10 line 9: vote $\to$ veto.

  The errors has been corrected.

- 5. P11 line 8: how are the BDT selection criteria chosen?

  We choose the BDT selection criteria because, after applying the BDTG cut, we can get a relatively pure sample (background ratio less than 5%)

and Cat. A and Cat. B for MC and data have almost the same purity, which is good to subtract the background.

- 6. P12 et seq: it would be good to have shape comparison between data and MC for the inputs and outputs of both BDTS. Superposition of the plots are required to see if there is any significant difference.

  See Sec. 4.2 on page 15. The shape (background subtracted) comparison between data and MC for the inputs and outputs is shown in Figure 11. There is no significant difference in the low end of K^+K^- mass region.

- 7. Fig. 8 on page 14: are the undulations in the background introduced by the BDT or existing from partially reconstructed events? Seeing this distribution before application prior to the BDT criteria would answer this.

  Figure 1 shows that the BDTG criteria has no significant influence on the undulations in the background in the signal region.

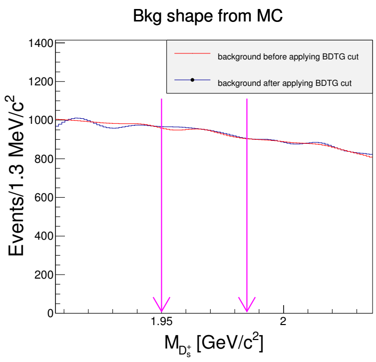

Figure 1: The comparison of the background shape before (the red line) and after (the blue line) applying BDTG cut. The pink arrows denote the signal region.

- 8. P15 line 5: $L = l_{\max}$ Eq. 8: should there not be a factor of $Y_j(cos\theta)$ in the integrand to utilize the orthogonality condition. Also, confused by the numerical integration statement. This confusion continues through to the key expressions. Eq. 9 i.e. $\langle Y_1^0 \rangle$ will also depend on S and/or P, why is that information not used? I assume there is a reference from where you took this so the reader could follow up or the discussion expanded.

  See Sec. 4.3 on page 15 and 16. The reference has been added on page 16 line 13. L should be $2l_{max}$ not l_{max} and you can see the reference on page 16 line 13. $Y_k^0(cos\theta)$ is added in Eq. 8. Exactly, $\langle Y_1^0 \rangle$ is the term related to the relative phase of S wave and P wave. We do not want to measure the relative phase of S wave and P wave in the low end but extract the S wave shape, so we ignore it.

- 9. P23 line 8 and Table 5: it would be more natural to deal with $2\Delta lnL$ than just the likelihood difference.

  Table 7 and Table 8 on page 26 have been updated.

- 10. Section 5.6: the discussion is too perfunctory. There should some discussion of the fit bias results i.e. f0(1370) magnitude is biased as is S(980) FF width. Also, how the results are converted into a systematic. Experimental effects needs references and/or quantitative values of the uncertainties assumed. I have already mentioned the concern about the S-wave assumptions.

  We have subtracted the shape of $D_s^+ \to f_0(1370)\pi^+$ and take the influence as a resource of systematic uncertainty in MIPWA analysis on page 18. The systematic uncertainty related to experimental effects in amplitude analysis (Sec. 5.6) use the same PID and tracking efficiency as that in Sec. 6.4. The reference has been added.

- 11. P30 line 17: how much is the change in the efficiency with model assumption? Is there a systematic associated with this?

  We have replace the propagator of $\bar{K}_0^*(1430)$ with LASS model and take the shift as a resource of systematic uncertainty. Also, The systematic uncertainty of amplitude analysis related to the systematic and statical uncertainties of $S(980)$ has been taken in account.

- 12. P34 line 6: if this bias is relative and the average is measured to around 0.5% therefore there is no no need to assign a systematic. If it is absolute why is 0.1% assigned?

  The bias here is (5.462% - 5.47%)/5.47%=0.1% and we neglect it.