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Report of the Referee -- DF11615/Lees  
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The submitted paper entitled "Search for mixing-induced CP violation using partial reconstruction of  $B^0 \rightarrow D^{*+} X l^- \nu$  and kaon tagging" is a detailed description of an analysis already published in

J.P. Lees et al. (BABAR Collaboration), Phys. Rev. Lett. 111, 101802 (2013).

It doesn't not contain any new result that is not already published. All figures and tables of the PRL article are re-used "as is" in the PRD paper, which is about twice as long as the PRL paper. The structure of the article is the same, just expanding some paragraphs and explanations. Obviously, more information is given about the analysis in the PRD paper.

I understand that the editorial policy of PRL and PRD <http://journals.aps.org/prd/authors/editorial-policies-practices> allows a published PRL paper to be the basis for a more detailed article in PRD. However, "such an article should present considerably more information and lead to a substantially improved understanding of the subject. Reproduction of figures, tables, and text material that have been published previously should be kept to a minimum and must be properly referenced."

In Table 2, not included in the PRL paper, we show the fit results for the most significant parameters both on data and in the simulation. We then compare them, when possible, with the values in the simulation at the generation stage. In the new version of the manuscript, we include additional information, both in the text and in figures 4 and 5.

I read also the published PRL paper. The PRL paper was understandable and convincing already.

Thanks a lot.

The extra information given in the PRD paper is of rather technical nature and is still somewhat abbreviated. I am not sure whether the PRD article really leads to "a substantially improved understanding of the subject".

In the new version we explain in a more comprehensive way the analysis method, following your very useful suggestions.

Furthermore the PRL paper is now two years old (received on May 10, 2013 and published on September 6, 2013). As far as I can tell, the analysis has not evolved in these two years, and I am wondering why the authors have taken so long to submit the PRD version. May be there should be a time limit ...

As you may understand, I am really not a fan of such practice to publish a PRD paper on the same topic as a PRL. I think that, in general,

authors should be most strongly encouraged to publish their results only once, providing from the start all the necessary information. In any case, I am happy to let to the Editor take the final decision on how to apply the PRL/PRD editorial policy in this case.

OK, thanks.

The work I now do to review the PRD paper, knowing that the PRL paper has been accepted, does not make full sense. I trust the review of the PRL paper, and therefore cannot possibly question part of the analysis (nor in an extreme case propose to reject the PRD paper), otherwise we get a serious inconsistency. Hence, for this review work, I feel that my role is now limited to making comments on the presentation.

Having complained about policies, I should now say that this analysis is very nice, and reports about an important result in B physics. The paper presents a measurement of the parameter  $\Delta_{CP} = 1 - |q/p|$  in the  $B^0$ - $B^0_{\text{bar}}$  system. A value different from zero would mean CP violation in  $B^0$ - $B^0_{\text{bar}}$  mixing, which is predicted to be very small and has not been observed yet. The result obtained by BABAR is consistent with zero. It is so far the most precise measurement of this parameter at the B factories, but a recent measurement from LHCb, published in Phys. Rev. Lett. 114, 041601 (2015), is slightly more precise. The topic is important, the analysis is solid and well described, and clearly deserves/deserved to be published.

Thanks again for your appreciation.

I append below specific comments, questions and suggestions on the paper. Most of them are minor. The main points are the following:

- There is some confusion in the notation (e.g. the G functions vs the F functions).
- I feel that the determination of the statistical error on the result is not well justified (although I trust that this error is correct).
- A by-product of the analysis is a measurement of  $\Delta m$  (the mass difference between the mass eigenstates of the  $B^0$ - $B^0_{\text{bar}}$  system) which has a statistical uncertainty much smaller than the total uncertainty of the current world average; I encourage the authors to assign a systematic uncertainty such as to turn this result into a real measurement (and the paper would then contain a new result as compared to the PRL paper).

We rephrased some sentences and we changed a bit the notation to clarify better the analysis strategy.

About the inclusion of the  $\Delta m$  result, we see your point.

There are several measurements of CP violation parameters, both at the beauty factories and at hadronic colliders, where  $\Delta m$  has been fitted as an additional quantity, without giving the full list of the systematic uncertainties.

The effort of including a result for  $\Delta m$  with systematic uncertainties in our paper would be large. It would require reprocessing the full dataset with the very last release of the various tools. In our opinion this effort would be justified only in case some relevant improvements in the analysis method would be developed. This is something that can be in principle done in the future.

I suggest that the authors be asked to consider my comments below and submit a revised version.

----- Detailed comments:  
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Title: - I think that the expression "mixing-induced CP violation" should not be used in this case. Instead one should write "CP violation in  $B^0$ - $B^0$ bar mixing", as in the original PRL paper. Mixing-induced CP violation usually refers to CP violation in the interference between a decay to a CP eigenstate directly or after oscillation, like for example the measurement of  $\sin(2\beta)$  with  $B^0 \rightarrow J/\psi K$ short.

We modified the title in: "Search for CP violation in  $B^0$ - $B^0$ bar mixing using partial reconstruction of  $B^0$ bar  $\rightarrow D^{*+} X l^-$  nu<sub>bar</sub>" and kaon tagging".

Abstract: - I don't think that "recently published" is OK for a paper published two years ago; "previously published" would be appropriate here. - Replace "induced by" with "In" - "B" should appear in math mode in "one B meson" and "the other B"

Thank you. We have implemented your comments.

Section I: - "B" should appear in math mode - Replace " $0.510 \pm 0.004$  [1] ps<sup>-1</sup>" with " $0.510 \pm 0.004$  ps<sup>-1</sup> [1] " - Replace " $B^0 B^0$ bar oscillations" with " $B^0$ - $B^0$ bar oscillations", i.e. add hyphen - Eqs. 6 and 7 are a bit inconsistent in the use of the "approximately equal" sign. Indeed one of them indicates " $\Delta CP \sim 1/2 * A_{CP}$ ", while the other one indicates " $\Delta CP = 1/2 * A_{CP}$ "; please make this consistent, including in the last (unnumbered) equation of the paper. - Replace " $B^0 B^0$ bar mixing" with " $B^0$ - $B^0$ bar mixing", i.e. add hyphen.

Thank you. We have implemented your comments.

Section II: - In the sentence about hadron collider, replace "B mesons" with "b hadrons". - "B" should appear in math mode. - Replace "the  $B^0$ bar  $\rightarrow f$ " with "the decay  $B^0$ bar  $\rightarrow f$ ". - Note that CLEO has also published a dilepton measurement: D. E. Jaffe et al. (CLEO collaboration), Phys. Rev. Lett. 86, 5000 (2001). - Ref. [8] from  $D^0$  has been superseded with a more recent publication: V. M. Abazov et al. ( $D^0$  collaboration), Phys. Rev. D89, 012002 (2014); their final claim about their deviation with respect to the SM is 3.6 sigma. Please update your reference ! - Replace " $B_s$ " with " $B^{\pm 0}_s$ " and  $B$ sbar" with " $B^0$ sbar"

Thank you. We have implemented your comments.

Section III: - Replace "chamber" with "chambers".

Thank you. We have implemented your comments.

Section IV: - It is a bit misleading to call the quantity of Eq. 12 the "invariant mass squared of the neutrino", since there may be additional

decay products of the B besides the D\*, lepton and neutrino. - Replace "amount of D\*\* events" with "number of D\*\* events".

We replaced "invariant mass squared of the neutrino" with "squared missing mass" in all the paper.

Section V: - "B" should appear in math mode.

Thank you. We have implemented your comments.

Section VI: - Remove parentheses around "1.641 +- 0.008". - Replace "According ro" with "According to". - In Eq. 18, the two superscripts "Data" are written in different fonts; I assume that the Roman font should be used everywhere. - In Eq. 18, the two superscripts "MC" are written in different fonts; I assume that the Roman font should be used everywhere. - Replace "amount of events" with "number of events".

Thank you. We have implemented your comments.

- The section starts by introducing "eight tagged samples", but later you refer to "four categories"; this is a bit confusing. I guess that muons and electrons have been merged at some point in the description.

Yes, sorry. The categories are actually eight (four different tagged sample for each lepton flavor).

-Replace "K" with "\$K\$".

Thank you. We have implemented your comments.

- In Eqs. 19 and 24, what are the functions " $\mathcal{G}_{\text{BB}}$ " ? Do you mean " $\mathcal{F}_{\text{BB}}$ " instead, i.e. the functions of Eqs. 13-16 ?

We rephrased this sentence.

- In Eq. 19, I do not understand the factor " $(1-\omega'_{+-})$ " in front of " $G_{\text{K}_R}$ " ? Can you please explain it ? It seems to be me that you cannot consider just correctly tagged  $\text{K}_R$  events, you should also have a contribution from incorrectly tagged  $\text{K}_R$  events ... Note that in the  $\text{K}_T$  case, you have terms for both correctly and incorrectly tagged events.

Thanks a lot, you pointed out a cut and paste mistake, not affecting the already published PRL paper, where we reported only the contributions for the mixed ( $\text{K}^+\text{l}^-$  and  $\text{K}^-\text{l}^+$ ) samples. We write now the correct expressions in Eq. 19. In case of absence of mistag ( $\omega'=0$ ), the  $\text{K}_R$  samples would contribute only to the mixed event samples (first two rows in Eq. 19), since the K in this case has the same charge as the lepton.

- What is " $p_k$ " in " $f^{\text{pm}}_{\text{K}_R}(p_k)$ " ? Do you mean " $p_K$ " instead of " $p_k$ " ? Please make the change everywhere. - Replace "used for B-events" with "used for B events".

Thank you. We have implemented your comments.

- In Eq. 25, the different functions " $\{\text{\cal G}\}_{\dots}$ " are not defined. Some of them should be called " $\{\text{\cal F}\}$ ", I suppose, for example " $\{\text{\cal F}\}_{B^+}$ " defined at the bottom of page 8, and " $\{\text{\cal F}\}_{CP}$ " defined in Eq. 17; there is also a notation problem, as the subscript " $B^0$ " seems to stand for what was written as " $B^0 B^0$ ", " $B^0 \bar{B}^0$ ", " $\bar{B}^0 B^0$ ", " $\bar{B}^0 \bar{B}^0$ " in previous equations. The notation should be improved, or shortcuts mentioned.

In the new version we define the different functions, we changed equation 25, and we fixed the notation problem.

- In the unnumbered equation after Eq. 26, there is too much space before "exp" in the last occurrence of " $P_m^{\text{exp}}$ "

Thanks, we fixed it.

- I do not understand the sentence "The corresponding value of  $P_m^{\text{exp}}$  for the sample of  $B^0$  combinatorial events ... appears in the fit to  $\Delta m_{\text{comb}}$ , ...". What does it mean that a value appears in the fit to certain parameters ? Do you mean that it is a free parameter of the fit ? Or expressed as a function of other parameters ?

Thanks for pointing out a misleading sentence. We rephrased it: "The corresponding value of  $P_m^{\text{exp}}$  for the sample of  $B^0$  combinatorial events tagged by a kaon from the  $B_T$  meson decay depends on  $\Delta m_{\text{comb}}$ ,  $\tau_{B^0 \text{comb}}$ , and the wrong flavor... subsamples."

- Is it correct that  $N_m = N_{mK^+} + N_{mK^-}$  and  $N_u = N_{uK^+} + N_{uK^-}$  ? This should be stated.

Yes, we included this information, thank you.

- For a paper with such detailed description of the fit, I would expect to see the explicit expression of the likelihood used in the fit. Also the reader may want to check that the "0.5" mentioned at the end of Section VII is indeed correct.

We included the explicit expression of the likelihood. We rephrased the sentence at the end of section VII including a reference.

Caption of Fig. 3: - Replace "Cos" with "cos"

Done, Thank you.

Section VII: - The last paragraph is a bit puzzling. Does this mean that your negative log-likelihood function is far from parabolic near the minimum ? Usually, the parabola is only an approximation, and the correct value of the statistical error is obtained from the detailed

shape of the log-likelihood. Here you seem to say that the parabolic approximation gives a better result. Do you use the parabolic approximation "by chance", just because it gives the error suggested by your pull distribution ? Or is there some better justified rationale, e.g. from first principles ?

We rephrased the last paragraph. We fit data using the MIGRAD minimizer of the MINUIT physics analysis tool for function minimization. The negative log-likelihood function has a nice parabolic shape near the minimum. We checked it by performing a scan in MINUIT. We found that the statistical error estimated from the difference between the abscissa of the points in which the parabola assumes a value equal to the minimum plus 0.5 is in good agreement with the RMS of the distribution of the pseudo experiments result for Delta\_CP. Therefore we assumed this value as the statistical error. In this way we circumvent the issue with MIGRAD which gives a statistical error 40% lower.

I would much prefer that you determine the statistical error from the distribution of the results of the pseudo-experiments, rather than from the parabolic approximation. So I don't request a change in the numerical result, but rather in the way you obtain it.

We rephrased the sentence: "This result is in good agreement with the RMS width of the distribution of the pseudo-experiments results, which we take as the statistical uncertainty of the measurement."

Section VIII: - Put parentheses around "50 +-25". - Replace "section VII" with "Sec. VII". - Replace "CP-eigenstates" with "CP eigenstates".

Thank you. We have implemented your comments.

- Why is there a need to take the S and C parameters of the CP eigenstates from simulation ? You should take them from measurements (in any case, the simulation contains nothing else than what we know from measurements, I suppose).

We take the parameters describing the CP eigenstates distributions from the simulation since this sample has several components, and it is strongly biased by our selection cuts, therefore it is not possible just to take numbers from the literature. We added a sentence below equation 17.

Table I: - Replace "affecting the result" with "on  $\Delta_{CP}$ ". - The capitalization seems a bit random, e.g "Composition" is capitalized, but not "fraction". - The words "Peaking Total" are unclear; why is this not simply "Total" ?

Thank you. We have implemented your comments.

Section IX: - The result for delta m is 0.5085 +- 0.0009 ps<sup>-1</sup>, with a statistical uncertainty which is 3 or 4 times smaller than the total uncertainty of the latest HFAG average. I know that delta m is not the subject of this paper, but would you consider making the effort to assess the systematic uncertainty on this result, such as to convert it

into a real measurement of  $\Delta m$  ? With this world's best measurement of  $\Delta m$ , the paper would become really worth publishing on its own, not just as a more detailed version of the PRL !

See our response before the list of your detailed questions.

Section X: - Replace " $B^0$   $B^0$ bar oscillations" with " $B^0$ - $B^0$ bar oscillations". - I think that the consistency with non-B factory measurements must be mentioned, in particular with the LHCb result (which is the most precise one).

We added "LHCb and  $D^0$ " to the sentence. See ref. [9] and [13] cited in section 2.

Figs. 4 and 5: - Can you please make plots showing all (at least more) components of the fit ? This would actually provide more graphical information than in the PRL, which goes in the direction of the PRL/PRD policy ...

We modified figures 4 and 5. We split the sample in four categories: Tag side peaking, tag side BKG, reco side peaking and reco side BKG.

Fig. 5: - Missing full stop at the end of the caption.

Thank you. We have implemented your comments.

Table II: - The capitalization in the table header seems a bit random, e.g "Fit" is capitalized, but not "information".

Thank you. We have implemented your comments.

Thank you very much for your very careful reading of our manuscript.