Comments by Nikhef and VU University on LHCb-PAPER-2012-019:

Measurement of the ratio of branching fractions $B(B^0 \to K^{*0}\gamma)/B(B_s^0 \to \phi\gamma)$ and direct CP asymmetry in $B^0 \to K^{*0}\gamma$

Dear authors of this paper,

We discussed the draft with Nikhef and VU University group members on Friday 29th of June.

General comments:

1) The comparison between experiment and theory needs to be done with care, since the experimental branching ratio is time-integrated, while the theoretical branching ratio is defined from the amplitude level at $t = 0$. The translation between the two values is given in Eq. (12) in [arXiv:1204.1735].

We suggest to add this sentence after line 17: "When comparing the experimental and theoretical branching ratios, special care has to be taken due to the lifetime difference of the B_s^0 system. The correction can be as large as 9 %, depending on the polarization state and the value of $A_{\Delta\Gamma}$ as described in Ref.[arXiv:1204.1735]."

2) Fig. 2: The amount of partially reconstructed $B \to hh\gamma$ is very different between the two final states. Do we understand that? This directly affects the combinatorial background, which in turn affects the signal yield and thus the measured A_{CP}.

3) Narrow windows in line 87. In this paper you provide no evidence that you look at resonant KK and $K\pi$: all you measure are $K\pi\gamma$ with $K\pi$ around the K^* mass, etc. It would be nice to show in the paper the mass distributions of the ϕ and K^* (B background subtracted, e.g. with splots) and compare those with simulation. We could not find these plots in the analysis note, where we think they are mandatory in any case.

In the J/psi ϕ analysis we have seen a significant (≈ 1 MeV) shift in the ϕ mass peak between data and MC. This would affect your evaluation of the selection efficiency. Have you looked at this?

4)We are puzzled by your treatment of the CP asymmetry in the background (Figure 2 and beyond).
We think that there are two valid scenarios: - You fix the asymmetry in the background using some external source. Then you assign a systematic for fixing it. - You float it (eventual with some constraint) in which case the uncertainty is accounted for in your statistical error.

Judging from the description, you seem to do both: You float the background to get the result in line 225. Judging from the figure, you see relatively large asymmetries in the background, but judging from your statement in line 222 these are actually consistent with zero.

Then, you do more fits fixing the background asymmetry to different values, to see how this affects the result. You assign the average as a correction and the spread as an additional error.

Its an interesting cross-check, but it isn't clear what systematic you are probing. The ranges that you float the asymmetries with may be totally incompatible with your data. Finally, this leads to a double counting of errors: if you fix the background asymmetry, your statistical error on the asymmetry will reduce. So, there is overlap between the 0.7 % in line 231 and the 1.7 % in line 225.

Note that he bias is probably just a consequence of the fact that you fit a non-zero asymmetry in the background while your background variations are centered around 0: Since you claim no significant asymmetry in the background, the bias must be irrelevant!

We suggest the following: Strengthen your statement that the background asymmetries are compatible with zero. You could for example do this by performing a single fit in which they are all fixed to zero and look at the change in likelihood and the change in the value of A_{raw}. If you are comfortable with the result, you write in the paper: "Note that the backgrounds in figure 2 exhibit a non-zero asymmetry. These asymmetries are compatible with zero. As a cross-check we also extract the result of A_{raw} with a fit in which they are fixed to zero and find no significant change in A_{raw}.")

Then, entirely remove the discussion in lines 226 to 235 and the corresponding systematic.

5) We are a bit confused about $\Delta\mathcal{A}_{\mathcal{M}}$ in line 262. At first we thought that $\Delta\mathcal{A}_{\mathcal{M}}$ is the difference between the field-up and field-down results. However, then its error should have been roughly a factor two larger than that of A_{raw} (so more like 3.4 % rather than 0.2 %).

I had to read the paper more than once to understand that $\Delta\mathcal{A}_{\mathcal{M}}$ is actually the difference in A_{raw} using two different ways to combine field-up and field-down data, namely that it is simply the difference between the results in line 225 and line 260. In fact, it is unclear what this number means: assuming that the number of signal decays per fb isn't time dependent, the result in line 225 should already be lumi-weighted. As far as we understand, there is no reason that the difference between the results in line 260 and 225 has anything to do with magnet polarity. (It also seems odd to use the difference between two methods as a correction. Why not just use the second method then.)

We strongly suggest that the procedure to evaluate this systematic be changed. First, we find it mandatory to report the results for field-up and field-down data separately in this paper. Then, to combine those two results you can follow either of the two following approaches, but not both:

a. You assume that there is an acceptance affect but that this is exactly opposite in field-up and field-down data. In this case you use a straight average (not a lumi-weighted average) to cancel the systematic. Your uncertainty will come from the fact that the recipee (exact cancellation) is possibly incomplete.

To estimate how wrong you could be you could use the up-down difference, and multiply this with a scale factor that somehow encodes your belief in the cancellation. (See also the D_s production asymmetry analysis.)

b. You assume that there is no effect and just combine the results with their statistical errors. (This will give you the result in line 225.)

Below follow our comments per line.

- Abstract
  - Remove the world average number; at first glance it looks as our measurement. The full information is given in the Results.
  - Add the final states that you look at (so $K^{*0} \to K\pi$ and $\phi \to KK$).
  - Some of us find it more natural to present the inverse ratio; in our case the statistical error on the ratio is limited by the $B_s \to \phi\gamma$ yield. Therefore, the statistical error on the inverse ratio is more Gaussian.
  - Remove "which is the most precise measurement to date". It is OK in a Conclusion but doesn't work out in an Abstract. It also suggests that your A_{CP} measurement

is not the most precise and, therefore, of less interest. It would make sense to say that these measurements are in agreement with expectations.

- Line 3.
  Change "through $b \to s\gamma$ one-loop electromagnetic penguin transitions" to " through the electromagnetic penguin transitions, $b \to s\gamma$, at one-loop level".

- Line 21.
  Change "current measurement" to "current values" and remove "so far". Current measurement can be mistaken for this measurement, and the experiments in [5] are already finished.

- Line 23.
  Change "enhanced up to -15%" to something else, because -15 % is already excluded and up to -15 % would mean < -15 %. Use a proper minus sign.

- Line 25, footnote 2.
  The footnote index reads like the power of 2. Also in line 114. Connect footnote references to words, instead of to symbols.
  "Theoretical" branching ratio is undefined if you do not explicitly quote arXiv:1204.1735 (hep-ph). Should it still remain a footnote then?
  Remove "However,".

- Line 27.
  Replace by "A measurement of the direct CP asymmetry of the decay $B_0 \to K^*\gamma$ is also presented."

- Line 33.
  Replace "bending power" by "field integral".

- Line 52.
  Change "rate to a point ... be recorded" to "rate such that the HLT2 can perform full event reconstruction to further reduce the data rate".
  Or remove "and reduces ... recorded".

- Line 55.
  Move early definition of "V" to line 80, where it is used, or to the Introduction.

- Line 58.
  "IP χ^2" is undefined. It will not be obvious without specifying how the reference point is obtained.
  Change "for a photon with" to "when the photon has".

- Line 79.
  Rephrase "to reject kaon mis-identification". In fact, why not skip the full sentence? The fact that charged tracks are identified is already mentioned in line 77.

- Line 80.
  Add the definition of V here.
  Preference for "(pions)" over "(Pions)".

- Line 87.
  Your ϕ and K^* mass windows are quite narrow, which means that you miss a considerable part of the lineshape. (For the ϕ, this is probably close to 20 %.) We need

to state what models are used for the lineshapes in the MC, because this is relevant when we compute the efficiency ratio.

- Line 90.
  An alternative for this sentence may be: "The invariant mass resolution of the selected B candidates amounts to ≈ 100 MeV/c^2."

- Line 107.
  Change "on both side(s)" to "on either side".

- Line 110, 111.
  The alternative "The reconstructed mass distribution of the combinatorial background has been determined from the sidebands as an exponential with different attenuation constants for the two decay channels" has the following advantages. It avoids "mass shape of the ... background", "decay constant", and "each channel".

- Line 115.
  Change "mostly located in the signal region" to "located in the region that contributes to the signal of the invariant mass peak of $B_s^0 \to K^*\gamma$".

- Line 118.
  Change "by estimating from the signal yield ... (...)" to "by directly estimating the signal yield from the observed signal ($\Lambda_b \to \Lambda^*\gamma$)."

- Line 124.
  An alternative for "a wide contribution ... region" is "a wide distribution at lower masses with a high mass tail that contributes to the signal region".

- Line 125.
  Why "radiative decays" when also charge particles are produced in these decays?

- Line 133.
  "$K^{*0}(\phi)\pi^0 X$" is not clear here. We can alternatively use V in "$V\pi^0 X$". Also in Table 1.

- Line 129.
  Replace "The neutral partner decays of those charged B decays" to "The partially reconstructed neutral B meson decays".

- Line 137.
  Replace "the ECAL is not yet calibrated" by "the ECAL calibration is different from that in the off-line analysis".

- Line 139.
  Replace "The acceptance is modelled" by "The inefficiency at the edges of the mass window is modeled".

- Line 152.
  Do we understand correctly that the only reason that we do a simultaneous fit is that we want to fix the $m_{B_d} - m_{B_s}$ mass difference? If so, does this really gain us anything? It is not wrong, but given the big signals it seems a bit overkill.
  Did you take into account the correlation in the errors in the yields when you computed the ratio?

- Line 169, Figure 1.
  - Replace "in log scale to enhance ... contributions" by "including different background contributions". It is not needed to mention the "logarithmic" scale, nor "to enhance" the background.
  - Can we replace the last line by "The bottom histograms display the residuals to the fitted function" without loss of relevant information?
  - Can we have plots with larger size labels ?
- Line 193.
  - Change "samples have been reweighted to reproduce the signal distributions seen in data" to "samples were weighted for each signal and background contribution to reproduce the reconstructed mass distribution of the data".
  - You don't need to do the splot technique to do this and without further explanation it doesn't help that you mention splot here. We propose to to remove the sentence about the splot.
- Line 202.
  The statement that the TCK's have been dealt with properly is superfluous. Add that the trigger efficiency is obtained from simulations.
- Line 212.
  - Remove the second term in Eq. (4).
  - The variables $\mathcal{N}$ are not defined.
  - Replace B^0 by "$K^+\pi^-\gamma$" etc., because you ignore eventual opposite sign corrections. (Somebody will draw you a diagram for $B^0 \to K^-\pi^+\gamma$.)
- Line 214.
  In Eq. (5) the terms with $\mathcal{A}_{bkg}$ and $\mathcal{A}_{\mathcal{M}}$ are missing.
- Line 233.
  Replace "misidentification $(K^-\pi^+) \to (K^+\pi^-)$" by "misidentification of $(K^-\pi^+)$ by (π^-K^+) in the final state".
- Line 240-241.
  - Remove space in "K π".
  - Replace two sentences "It was found ... detection asymmetry" by one "It was found that for $K\pi$ pairs in the kinematic range relevant for our analysis the detection asymmetry is ...%".
- Line 246.
  Reference [24] is published; arXiv:1202.6251.
- Line 251.
  The explanation of how we extract $\epsilon(t)$ is incomplete: Just the splot would not give the efficiency. We suggest that you replace this sentence by:
  "The time acceptance $\epsilon(t)$ has been extracted from data by using to the decay time distribution of background-subtracted signal events and the known B_d lifetime" (or something equivalent).
- Line 253.
  Replace "by the magnetic field that spreads out ... detector" by "by the magnetic field that deflects oppositely charged tracks to different regions in the detector".

- Line 262.
  We are a bit confused here. At first we thought that "$\Delta\mathcal{A}_{\mathcal{M}}$" is the difference between the field-up and field-down results. See general remarks.

- Line 267.
  Consider the alternative for "In 1.0 fb-1 of pp collissions collected with" of
  "With the data sample corresponding to an integrated luminosity of 1.0 fb^{-1} collected with".

- Line 309.
  Ref. [8] is not published in a paper. Is this the best reference for New Physics A_{CP} ?

- Line 339.
  Replace "sPlots" by "Plot" in Ref. [23].

Tjeerd Ketel for the Nikhef and VU group, 2 June 2012.