

Dear Editor,

Please find below our response to the referee's second report. We note that the referee has continued to misread our manuscript in spite of us providing, in our previous response, clarifications to clear up these same misreadings present also in the referee's original report. We comment further on this point in the final paragraph of this communication, following our response, below, to the second report of the same referee.

Response to the referee:

While we may understand why the referee feels prompted to try to defend the work presented in Refs. [1,2], we would like to emphasize that, contrary to what the referee claims, the "main aim" of our contribution to these proceedings is not just "to criticise Refs. [1,2]" but to investigate in detail certain assumptions underlying the methodology used in these articles, which is not quite the same thing. The content of this contribution to these proceedings clearly follows the generally accepted rules of a scientific analysis, is faithful to the presentation in the poster session at the conference, and is partly based on previous research which was published in refereed journals. In our opinion, this is all that one should expect from this contribution.

If the referee disagrees with some of our results, the referee could have challenged them at the conference (assuming he/she was present) or else the referee could now write a response article supporting his/her point of view. In either of these two cases we would be able to respond on an equal basis. But aiming to force the conclusions to be altered or to block the publication of this contribution to the proceedings if they are not so altered seems to us clearly inappropriate. In this sense, the sentence "Why should I then accept for publication a contribution whose main aim is to criticise published work, i.e. Refs. [1,2], and whose criticism appears to be based on a claim rather than a complete and convincing analysis?" is particularly worrisome. Does this mean that, because the referee doesn't like the results, the referee thinks his/her duty is to stop the publication of this contribution to the proceedings?

The following points provide more detailed responses to a number of the referee's objections.

-1) Regarding the referee's incorrect statement, repeated from his/her first report, that the aim of the paper is to provide a proof that it is necessary to include DV contributions in the tau analysis, we point once more to the last part of Section 1, beginning at the bottom of page 5 and continuing to the end of the section, in which it is clearly stated that the tests to be considered in Section 2 are aimed at investigating whether the (strong) assumption that the OPE can be treated as acting as if it were a convergent series, with higher D contributions scaling as $\Lambda_{\text{QCD}}^D/s_0^{D/2}$ up to dimension 16 and at scales $s_0=m_\tau^2$, is a plausible one. As discussed in some detail in the last part of Section 1, this investigation is focused on situations where one expects integrated DV contributions to be suppressed, and hence represents an investigation of the assumptions about treating higher dimension non-perturbative OPE contributions. The fact that the referee does not like the implications of what

the results of this investigation imply, does not justify a misrepresentation of what is actually explicitly stated in the manuscript.

-2) Figure 3 clearly shows a detailed quantitative result, as quantitative as a plot can be, in fact. Furthermore, although the referee claims to have read the text very carefully, the referee is misreading the result shown in this figure. The figure caption clearly states: "Comparisons of differences between general s_0 and $s_0 = m_{\tau}^2$ versions of the OPE and spectral integrals...". Therefore the absolute value on the y axis is meaningless and, in particular, at $s_0 = m_{\tau}^2$ it vanishes by construction. That "Figure 3 shows effects on the EM spectral integrals whose size remains well below 0.0002 for $s_0 \geq 3 \text{ GeV}^2$ " is irrelevant. What is important is the difference between the black OPE curve and the red experimental points, and this shows an obvious disagreement, given the size of the errors.

The referee insists again on the obvious fact that the $e+e-$ and the tau channels are not the same. We also insist that, as we already said in our previous response, all of section 2 is devoted to a detailed explanation of what we have done, so the reader can easily judge how compelling this evidence is and draw the corresponding conclusions. If the referee does not agree with this analysis, we invite the referee to show how SU(3)_F breaking effects in the OPE, acting in an isoscalar contribution already parametrically suppressed by the relative factor of 3 due to the structure of the EM current, can reconcile the results of Fig. 3 with the truncated OPE strategy. We think this is highly unlikely, to say the least.

-3) Refs. [9] and [17] are already a very detailed quantitative account of how DV effects affect the extraction of α_s from tau decay. How can the referee ask us now to "give more space to quantitative evidence that directly applies to the example at hand, rather than criticising other works on too general or indirect grounds"? Refs. [9] and [17] are quoted here for a reason.

-4) Let us make it clear to the referee: It is perfectly OK that the referee may be "not quite agreeing with our conclusions on the effects of DVs." However, stopping the publication of this contribution to the proceedings because after a quick look at Fig. [5] of Ref. [20] the referee feels that DVs may have been overestimated when, as emphasized again above, Refs. [9] and [17] show a detailed analysis of the relevance of DVs, seems to us scientifically inappropriate.

-5) To the best of our knowledge, the other two DV parameters the referee mentions have never been independently determined, and this is why we cannot quote any comparison. Does the referee know of any such independent determination or this is just another generic complaint?

In summary, it is clear to us that, because the referee holds a different view on DVs, the main objective of his/her report is to defend the work done in Refs. [1] and [2], and try to either force us to change our conclusions in such a way that they say things the referee would like them to say but which our results do not, in fact, support, or, failing that, to block this contribution from being published in these proceedings. We find this seriously inappropriate, doubly so given the referee's continued misrepresentation of the goals of the investigation we are reporting, clearly detailed in the last page and a half of Section 1. We thus ask the editor to

either accept this contribution in its present form (which includes additional minor clarifications, following the editor suggestion), or find a truly independent referee.