Dear editor,

Please find below our responses to the report.

Since our writeup contains a criticism of the analysis strategy advocated by the groups of A. Pich and/or M. Davier, we request that anybody scientifically too closely related to those groups be excluded from reviewing this revised version.

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Report

This contribution aims at showing that duality violations cannot be neglected in the determination of the QCD coupling from hadronic τ decays. While the analysis of duality violations is certainly an important topic that needs to be addressed case by case, I have some doubts about the adopted strategy and I miss some crucial information in the text. Therefore, I would like to ask the authors to consider the points described under "Requested changes", and modify the paper accordingly.

The reviewer's characterization is not entirely accurate, and appears to reflect an insufficiently thorough reading of the manuscript. An important part of the contribution is, in fact, the demonstration that it is not safe to treat the OPE as if it were a convergent expansion at scales s_0 near m_tau^2. While this observation IS potentially relevant to some sum rule analyses of hadronic tau decay data in the literature, its implications are in no way restricted to these.

Requested changes

1. I believe that the authors do not show compelling evidence that the analysis in section 2 of e+e− data (V channel) should imply the failure of OPE-based analyses applied to τ-decay in the V+A channel. As I said, these issues must be addressed in detail case by case. The authors instead do not go beyond general claims, and these general claims are repeated many times in the paper. As the authors themselves observe, the V case is different from the V+A case. I would like the authors to limit their comments to those based on quantifiable and clearly formulated evidence.

As noted above, the reviewer's characterization fails to reflect the actual content of this section of the manuscript. 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. Clearly, the plots shown in Fig. 3 go beyond "general claims repeated many times" and constitute a quantitative test. As to the V vs. V+A comparison, we note that the original version of the manuscript contained the following paragraph,

"Of course, the EM case allows us to consider only the V channel, whereas Ref. [1] considers V + A to be the optimal choice for the truncated OPE strategy. We note, however, that (i) there is not a vast difference between the amplitude of the DV oscillations in the V and V +A channels, relative to the parton model, and (ii), that, in particular for the optimal weights, the results for alpha_s obtained in Ref. [1] on the basis of the truncated OPE strategy are in excellent agreement between fits to the V and V + A channels, while the corresponding agreement for the spectral weights is also very good."

reproduced here since the reviewer's comments indicate that he/she previously missed it.

2. The title is misleading, because it is an analysis of e+e− data, see point 1. Maybe the authors can find a title that better captures the full content of the contribution.

We find the title appropriate as the main conclusions refer to tau data. The fact that we use e+e- data to draw these conclusions is secondary, and it is due to the fact the I=1 component of the EM current and the weak current are the same, while the e+e- data, unlike the tau data, extends to invariant masses higher than m_tau. This is again explained in the paragraph right after Eq. (10). Furthermore, the abstract gives sufficient details to avoid any misunderstanding.

3. I cannot find a clear error analysis of the theoretical predictions, e.g. the uncertainties associated to the curves shown in Figure 3, and how they could actually impact the τ-decay determinations of αs. I would like the authors to address more precisely the theoretical uncertainties in their analysis, also by means of references to the appropriate published works.

There is a rather obvious problem with the reviewer's suggestion. Explicitly, since the theoretical curves are extracted from a fit to the data (at s0=m_tau^2), to plot both the experimental errors and the errors on the theory curves, *induced by those same experimental errors*, would be to double count the experimental errors and serve only to obscure the level of actual incompatibility. These plots in Figure 3 are aimed at displaying the result of tests of the assumption that the OPE can be treated as if it were a convergent series, allowing truncation at orders lower those required, in general, by Cauchy's theorem, is not consistent, and this is precisely what they do. Details about this analysis are deferred to a future publication of which this contribution to the proceedings is a "preview". The referee should not be asking for us to include the entirety of a forthcoming regular journal publication in a conference proceedings writeup.

As to the impact on alpha_s of the assumption of truncating the OPE in tau decay, there is a full analysis dedicated to that in Ref. [9], which is properly quoted in the text.

4. The authors define a truncated-OPE strategy in section 2, where they assume that certain higher dimensional condensates can be neglected. They also seem to imply that this is what is done in other analyses which they criticise. I disagree. My understanding of the latter analyses is that the sensitivity of the result, e.g., αs, to higher dimensional condensates has been estimated through fits to data with varying weights. The same should be done for the example in Figure 3. I would like the authors to better formulate the related text.

We disagree with the referee. As we explain in the text, in the truncated OPE strategy, certain higher-dimension condensates are, indeed, assumed to vanish in order to have fewer fit parameters than observables. The analysis we tested with e+e- data is exactly that of Ref. [2], which the referee is clearly familiar with. The impact of this assumption about truncating the OPE on the value of alpha_s has been analyzed in detail in Ref. [9] and it is not necessary to reproduce it again here.

5. In section 3 the authors discuss a model for duality violations. The parameters of this model are determined in specific ranges of q2 and for specific processes. The authors then observe that the agreement of (20) with the results from the fit involving τ data is rather satisfactory. However, I could not find a complete analysis by the authors showing how their fitted parameterisation (20) behaves outside the fitted regions and for τ-data versus e+e−data. The authors should be more clear about this aspect and provide the relevant information.

The analysis which extracted the value of the DV parameters shown in Eq. (20) from a fit to tau data was published in Ref. [17]. As to the comparison of the corresponding DV oscillations between tau data and e+e- data, we refer the referee to Fig. 5 of Ref. [20]. Again notice that section 3 refers to the VV correlator, which is measured in tau decays.

Furthermore, as the form for the DV oscillations in Eq. (17) is supposed to be valid at high enough s, once the DV parameters are determined at a given s=s_1, they are valid for any other s>s_1. We refer again to Fig. 5 of Ref.[20]

for a comparison of the "behavior outside the fitted region and the comparison τ-data vs. e+e- data" suggested by the referee.

In summary, we think that the questions raised by the referee are already answered in the present text, or in publications properly cited in the bibliography. The only exception could be perhaps point 3 above, but we have already explained why we cannot use these proceedings to give all the details of that analysis that the referee requested, as his/her request corresponds to asking us to include the entirety of a future publication in the current conference proceedings. Such a request is not an appropriate one for a reviewer of such a conference proceedings writeup to insist on.