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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Optimizing Saharan dust CALIPSO retrievals" *by* V. Amiridis et al.

Anonymous Referee #1

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The authors suggest in their paper a methodology for optimizing the retrievals of extinction profiles from CALIPSO that correspond to Saharan dust. The proposed methodology is well documented and the optimized product is compared to independent ground-based and satellite data and the DREAM model. The comparisons suggest that the proposed optimization leads to an improved Climatological data set concerning the dust originating from Sahara. The paper is very well written and structured and it is suitable for publication in ACP. There are certain issues that should be addressed or clarified further before final acceptance of the manuscript, which are outlined below:

Introduction: The selection of the LR in the operational CALIPSO algorithm is discussed in lines 1320 of page 14751 and then again in lines 1-10 of page 14753, mostly referring to Omar et al., 2009. I would suggest merging these two parts to avoid repetitions.

Introduction: Page 14752 lines 1 to 3. The authors suggest here that MODIS is not suitable for a detailed conclusive evaluation of CALIPSO products? If this is the case then why do they use later in their manuscript MODIS for comparisons? Please clarify and rephrase accordingly.

Section 2.1. As it is written is not obvious to the reader, when the authors refer to the official CALIPSO product and when to the level-3 product the produce themselves. Probably the latter is considered in lines 12 to 23 (page 14765) but as it is written it is confusing. Please make it more clear.

Section 2.1. Did the authors perform any consistency check between the 1x1 and 2x5 products? They only mention that they verified the correct use of the level-3 algorithm.

Page 14761, line 7: It is not clear what the authors mean with absolute bias. Is this the mean of the differences between the individual CALIPSO and AERONET measurements or the difference of the means?

Page 14762. To a great part the discussion for the optimum LR is a repetition of what is discussed in the introduction. I would suggest to move most of the discussion included in the introduction here, since it is more relevant in this part of the manuscript.

Page 14764 in section 3.2.1. Since most points refer to maritime areas, is this component somehow considered in the comparisons? Could this component explain part of the observed bias mentioned in page 14765 (line 7)? Please provide a comment.

Page 14765 line 10. The reported correlation coefficient is rather small compared to the CALIPSO-AERONET comparisons. Do the authors have any explanation for that?

Section 3.3.1: This section to my opinion needs a different focus. To my understanding this should be to use version III data set to validate the DREAM model and not the other way as it seems to be the case in the manuscript, i.e. to validate the data set using the model.

Section 3.3.1 Page 14768 Line 15-20. Still the question remains are the 1x1 and 2x5 data sets consistent?

Section 3.3.1. Before using the Tesche et al methodology (based on depolarization ratios measured with ground-based systems), did they authors check if the CALIPSO depolarization ratios are consistent with ground-based ones?

Section 3.3.1 There are few repetitions concerning the description of the various versions of the CALIPSO data sets which should be avoided.

Page 14771 line 4: "We calculated the reported CALIPSO level 2 . . .." .It is confusing as written. Did the authors used the reported values or they calculated the depolarization ratio? (and how?)

I cannot understand the reasoning for using zero extinction values in the averaging scheme. If the authors want to generate a Climatological product that should represent the presence of pure dust, then to my understanding "zero extinction" means absence of dust and then why this should be averaged? The same is valid when averaging the model estimates.

Page 14755, Lines 19 -20: Why these regions are considered as problematic for the model? I guess this is an outcome from the Bassart et al study, but it is not clear what is the problem with the model. Please elaborate here more.

Section 4. I think that a real summary is missing from this section. Line 14-15: The better agreement with a model can be a proof for an improvement of a data set? To my opinion this works the other way. (see also my comment for section 3.3.1)

Section 4 page 14776, line 25. I think that also depolarization ratios are vital for the same purpose.

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