Anonymous Referee #1

"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 13-20 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".

On Page 14751, we briefly refer to the LR selection methodology adopted by the operational CALIPSO algorithm for the six aerosol sub-types defined by the corresponding classification scheme. On Page 14753, the LR discussion is developed so as to focus exclusively on the dust sub-type. In particular, the paragraph expands on the methodological adaptations of the operational CALIPSO algorithm specifically applied to dust due to its non-sphericity, as well as the need to apply specific dust retrievals in the case of isolated layers. We consider the two paragraphs to be independent - with the first paragraph providing an introduction on the generic CALIPSO approach for LR selection, and the second situated in context where the dust case is thoroughly analyzed.

"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".

MODIS data in our study are used for cloud-free MODIS cells only so that the comparison presented is trustworthy. The reviewer is right in that the phrasing is misleading. Thus we have rephrased the text as follows:

"However, MODIS AOD accuracy decreases with cloud cover (e.g., Loeb and Smith, 2005; Zhang and Reid, 2006), thus, trustworthy comparisons between CALIPSO and MODIS should contain only cloud-free MODIS retrievals that constrain the correlative datasets to a small but more reliable number of coincidences. Recently, Schuster et al. (2012) compared CALIPSO AODs with ground-based retrievals using AERONET…"

Instead of:

"However, MODIS AOD accuracy decreases with cloud cover (e.g., Loeb and Smith, 2005; Zhang and Reid, 2006), and a detailed evaluation of CALIPSO products using other data sources is needed for conclusive comparisons."

"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 they 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".

These parts have been revised to make this distinction clearer. We thank the reviewer for his/her suggestion.

"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".

Our intention in this paper is to verify our algorithm for the generation of Level 3 CALIPSO product. Thus, we compared our retrievals with the 2x5 CALIPSO product by applying our algorithm to the same cell resolution. Then, we produced the CALIPSO L3 product at 1x1 degree resolution and compare with our retrievals also at a spatial resolution of 1x1 degree. It is not our intention to discuss the representativeness of the 1x1 product compared to the 2x5 product in our work. Downscaling to 1x1 resolution was done for ease of comparison with BSC-DREAM8b simulations.

"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?"

By absolute bias we refer to the absolute difference of the two means (i.e. the absolute value of the averaged CALIPSO value minus the averaged AERONET value). We rephrased the relevant line in the text as follows:

"A significant absolute bias (absolute difference of the means: i.e. averaged CALIPSO AOD minus the averaged AERONET AOD)"

Instead of:

"A significant absolute bias (average CALIPSO AOD minus the average AERONET AOD)"

"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".

These parts have now been revisited to include the reviewer's suggestions, thank you.

"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".

The comparison between CALIPSO and MODIS is done for collocations where only dust sub-type exists in the overpass of the CALIPSO curtain in the 1°x1° cell of MODIS. More specifically, we use only those cases where all of the profiles of the CALIPSO in the 1°x1° cell consist of pure dust aerosols, in order to have homogeneous dust data. While a proportion of the cells are indeed over maritime areas, the predominance of pure dust aerosol means that we don't expect a bias due to maritime component.

"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?"

In the revised version of our manuscript, after applying more conservative quality screening, the correlation coefficients for the 2 datasets are now more similar (0.9 vs. 0.8). We believe that MODIS retrievals are affected by other factors (characteristic of this sensor) as well, which however are minimized for maritime areas.

"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".

We fully agree with the reviewer that, in general, models should be evaluated against observations and not the other way around. However, the main focus of our paper is to compare the performance of different versions of CALIPSO products against a standard reference. BSC-DREAM8b is used here as a well-validated dust model with known spatial under/over estimations. A detailed evaluation of BSC-DREAM8b against AERONET is available in the literature and we have used these results as a reference point for studying possible improvements to the Version III CALIPSO product.

"Section 3.3.1 Page 14768 Line 15-20. Still the question remains are the 1x1 and 2x5data sets consistent?"

Our intension is to verify our algorithm for the generation of Level 3 CALIPSO product, thus we compared our retrievals against the CALIPSO product for the 2x5 resolution, where CALIPSO delivers the L3. Then, we produced CALIPSO L3 product in 1x1 degree resolution and we compare with Version III retrievals in the same spatial resolution of 1x1 degree. It is not our intension to discuss the representativeness of the 1x1 product compared to 2x5 in our work. We wanted to use 1x1 resolution to compare with BSC-DREAM simulations and we keep that resolution uniformly in our manuscript.

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

The particle depolarization CALIPSO product is considered to be of high quality in our study when using the standard depolarization formula (Equation 4) instead of the depolarization product reported by CALIPSO. This finding has been reported by Tesche et al. (2013), as we mentioned in our manuscript. The authors performed a detailed validation of CALIPSO depolarization retrievals, reporting satisfactory agreement against ground-based collocated measurements for this product when Equation (4) is used. A paragraph has been added in the new version of our manuscript to address the reviewer's comment:

"This finding has been already reported by Tesche et al. (2013), who performed a detailed validation of the CALIPSO depolarization retrievals and found satisfactory agreement with ground-based collocated measurements for this product when Equation (4) is used instead of the depolarization product itself.”

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

The text has been revised to fulfill the reviewer’s suggestions.

“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?)”

We used the calculated depolarization in our work, produced by the application of Equation (4). In this section we describe the distributions presented in Figure 4. Both depolarization versions are presented: the black line refers to the CALIPSO reported values while the red line to the calculated depolarization ratio. The latter calculations are performed using Equation (4).

To avoid confusion we revised the text as follows:

“In order to examine the true particle depolarization of the layers included in our study instead of relying on the Level 1 approximated value used for the classification, we vertically average the reported CALIPSO Level 2 particle depolarization ratio for each layer and present their distribution in Figure 4 (black line). In the same Figure, a second distribution is also presented (red line), representing the layer-averaged particle depolarization ratios retrieved for the same dataset using the standard equation:”

“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”.

This is discussed also by Reviewer#2. We revised the manuscript to demonstrate that we produce a spatial average that is more representative for each grid cell. A new Figure (7) has been inserted to demonstrate the reasoning for doing so and the resulting differences with CALIPSO L3 product.

“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”.

The evaluation of the BSC-DREAM8b model against satellites and ground-based AERONET observations, shown in Basart et al. (Tellus B, 2012a), highlighted significant limitations of the model in Northern Africa. The model strongly underestimates the aerosol optical depth (AOD) in the Sahel and the Eastern Tropical North Atlantic regions in winter and spring. This result suggests that there is a severe underestimation in the emission from South Saharan sources, a problem in the low-level dust transport over the region, or a combination thereof. Under the influence of the Harmattan, dust storms in the Sahara are quite frequent - for example, on the alluvial plain of Bilma (Niger) and Faya Largeau (Chad). Dust is transported from these regions to the Gulf of Guinea (e.g. Middleton and Goudie, 2001). With respect to the latter, a detailed analysis of the simulated dust transport reveals very efficient dry deposition along the southwestward dust transport (e.g. Middleton and Goudie, 2001).

The model also overestimates the AOD in Northern Algeria as well as the dust transport towards the Western and Central Mediterranean, mainly in spring. The overestimated dust emissions in Algeria are always related to the lee of the Atlas Mountains. Several causes could induce the overestimation of dust emission in this area. For example, the misrepresentation of a small-scale atmospheric convection processes in this complex region by the meteorological driver could overestimate the surface winds and consequently the dust emissions. In addition, threshold friction velocity is essentially a property of the soil surface, rather than that of a soil particle. In the BSC-DREAM8b model, the soil humidity is climatologically described, and this could introduce errors in the calculation of the threshold friction velocity.

Moreover, the comparison of the model with satellite-derived data, highlights that the versions based on the original emission scheme misrepresent dust sources over Eastern Nigeria and at the Mali-Mauritania border and overestimate the AOD in Northern Algeria.

As a result, the comparison of the model against AERONET observations on an annual basis (see Fig. 10) show moderate differences in the Iberian Peninsula, Atlantic and Mediterranean regions (absolute bias < 0.1), underestimations in the Sahel (bias = +0.27) and overestimations in NW. Africa (bias = -0.14).

The reasoning behind the origin of model biases is available in the literature provided in the manuscript and we would like to not elaborate on model issues in order to keep our concept straightforward and focused on CALIPSO optimization methods.

"Section 4. I think that a real summary is missing from this section".

The reviewer is right. We have removed the summary from the title of section 4 so as to focus on the conclusions of our study.

"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)"

In the present analysis, the model is used just as a reference to compare the different CALIPSO climatological dust products. This is due to the lack of observations in Northern Africa. Regional models are useful to understand the dynamics and transport of desert dust. Moreover, dust regional modeling can be regarded as a useful analysis tool as shown in the literature (i.e. Amiridis et al., Ann. Geophys , 2009; Klein et al., ACP, 2010; Papanastasiou et al., 2010; Basart et al., ACP, 2012b). The model has been evaluated in detail for a complete annual cycle (2004) in Basart et al. (Tellus B, 2012a) using satellites and ground-based AERONET observations. In particular, Basart et al. (Tellus B, 2012a) highlighted weaknesses and strengths of the model and provided detailed behaviour of the model in the region. As a result, the model was chosen for its trustworthy point of reference for the CALIPSO climatological dust products comparison performed here.

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

We agree and would like to thank the reviewer for this addition. The text has been revised to include the need for depolarization measurements as follows:

“Ground-based measurements of the dust LR and particle depolarization ratio for these regions will be vital for the success of implementing similar improvements.”