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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Saharan dust long-range transport across the Atlantic studied by an airborne Doppler lidar and the MACC model" *by* F. Chouza et al.

Anonymous Referee #2

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General:

The paper is based on airborne Doppler lidar observations collected during a unique and complex field campaign in the Caribbean. The paper is appropriate for the Special issue of SALTRACE. However, the paper needs major revisions.

I have problems with all the aerosol-related results and comparisons. Spaceborne lidar observations, coherent lidar observations and MACC aerosol transport model computations are compared. I do not fully trust the CALIOP observations (my reasons are given below), I do not fully trust the Doppler lidar estimates of aerosol extinction, so that all the comparisons and conclusions of the paper do not really convince me. So, at the end, I do not know whether the MACC aerosol products or the lidar products

show the truth.

Especially, I am missing the integration of the ground-based SALTRACE aerosol Raman lidar observations into the study. They deliver the most trustworthy aerosol profiles. To convince the reader, in the first place, the Doppler lidar data of particle backscattering and extinction estimates should be compared with Raman lidar observations for some Barbados cases. After demonstrating the usefulness of the Doppler lidar for quantitative aerosol profiling in the Barbados area. . ., one may continue with comparisons with CALIOP products. So, my main point is that I am missing SALTRACE ground-based with airborne lidar comparison of aerosol profiles.

Please keep in mind, your paper is a contribution to a SALTRACE Special Issue. So the reader expects complex papers with complex integration of lidar and model results.

Details:

Page 2, line 7: I strongly recommend to check the special issues of SAMUM 1 and 2 for potential references. For example, why did you (the SAMUM-SALTRACE science group) make so many observations at Cabo Verde on SAL (winter campaign as well the SAMUM-2 lidar campaign in summer 2008) and finally there is no SAMUM reference at all. This is not professional, I mean other groups usually put all the own papers in the foreground whereas you seem to ignore them simply. I have to assume that you do not know the SAMUM special issues.

Page 2: You need to explain all abbreviations when they appear the first time. ALL! SAMUM, AMMA, NAMMA, BERTHA, POLIS, CALIPSO etc.

Page 3: There are references for SALTRACE station at CIMH (Gross 2015, 2016, Haarig ILRC 27 New York, Toledano ?), should be given. . ..

Page 5, before the CALIOP section: I think we need a small section on the retrieval of the 532 nm backscatter coefficient from the Doppler lidar observations. This not trivial. Please provide something like a summary (step by step of the entire retrieval scheme)

and provide uncertainties.

Page 5, line 18: CALIOP provides profiles of the particle backscatter coefficient, and then with the help of lidar ratios the extinction and AOD is estimated. The forward Klett algorithm has to be applied. As a consequence, the solution profiles are rather uncertain, and they are rather sensitive to uncertainties in the assumed lidar ratio profile (input profile). That should be made very clear. This approach is rather different from the SALTRACE Raman lidar approach. Did you check the lidar ratios used by CALIOP and the ones obtained by the SALTRACE lidars. To my knowledge, CALIOP uses 40 sr for dust extinction conversion. However, dust lidar ratios are typically close to 60sr. . ..So, there may be an error (systematic bias of 30%), and because of the fact that the solutions get more and more unstable from the tropopause towards the ground, the full profile gets increasingly corrupted with range.... towards the surface. Please check and comment on that.

Page 5, line 20, dust vertical distribution of what??? The vertical distribution is not a parameter!

Page5, lines 22-28: Are their CALIOP overflights over Barbados! . . . so that one could check the quality of the CALIOP profiles directly with Raman lidar profiles.

Page 5, line 32: Explain abbreviation MACC

Page 8, line 16: Extinction coefficient and AOD are reported. . ..., so no separation of dust and non-dust particle extinction? Just the total particle extinction! What lidar ratios are then used. . ..., for the dust/marine mixture from about 1000-1500 m (SAL base) down to 400 m (below 400 m one may assume pure marine. . .)

Section 3.3. It is a bit confusing, when just total particle extinction coefficients are compared without an approach to separate dust from marine. . ..

Page 10, line 2: Do you believe that ext coef. obtained from CALIOP is just 50 Mm-1 plus minus 20Mm-1 in the ABL, and all this is just marine aerosol. . .?

Page 10, line 10: Who is right? MACC may be wrong, such layers reaching to 20 km height are not 'realistic'. And CALIOP cannot resolve such tiny aerosol traces.

Page 11, line18-20: I have many questions regarding the comparisons, without having a good answer, how to handle the quality of the comparisons. I would like to see a very careful discussion. One should again mention all the potential errors sources, and clearly state that the comparisons are combined with high uncertainties. As you know, CALIOP delivers only particle backscatter profiles. The Klett forward integration method is used. The solution can thus be very erroneous, especially at the end of the profile (lowest part of the atmosphere, i.e., in your cases, in the lowest part of the SAL and the layer below the SAL down to the ground). The uncertainty can easily be 30-50%, even larger. On the other hand, your DWL does not allow to retrieve rather accurate backscatter coefficients, too. The conversion to 532nm backscatter is combined with high uncertainties, and the further conversion to 532 nm extinction, as well. This introduces a systematic bias to the entire profile within a given layer (SAL, transition layer, marine MBL), and this bias is different for the different layers. How can you then state, for example, that MACC underestimates the extinction of the marine boundary layer and overestimates the extinction of the SAL? So I would like to see a very sensitive discussion in view of all the uncertainties on both sides, observations and modelling. . ..

Page 12, lines 23-25, so if CALIOP cannot measure the 'artifact' (as produced by the MACC model) why do you not at least check the SALTRACE ground-lidar data, whether there was an aerosol layer in the upper free troposphere or not. Such layers at such great heights are clearly a large scale phenomenon. . ... and should have been seen by the Barbados lidars, if existing.

Page 13, section 4.3. This could be the central subsection of the entire paper. Here one could start with the comparison of DWL backscatter (and extinction profiles) with ones from the ground lidars. Afterwards, one could step forward with CALIOP and model output discussions.

The DWL extinction profiles... what lidar ratios did you use? For the dust layer, for the mixed dust/marin layer, for the marine layer....? Is that in agreement with ground lidar data ? The ground-based lidars measure lidar ratios during darkness, and these lidar ratios are certainly valid hours before or later..., and thus applicable to the DWL observations.

At the end I must say: It is quite strange to see an aerosol-related comparison paper on the basis of airborne Doppler lidar measurements, a lidar which does not measure backscatter coefficients...., furthermore based on CALIOP which does not measure extinction profiles, and the only lidars, delivering extinction profiles are not included in the paper

I found a SAMUM 2 Raman lidar vs CALIOP intercomparison paper, Teshe et al. (JGR, 2013). You may not know that paper, but it should be referenced.... More general, one should check and know all the SAMUM papers from 2009 and 2011 and provide proper referencing to all the SAMUM efforts done.

Some comments to the figures:

Do we need Figure 2 in this paper on CALIOP and MACC? Is that not already presented in wind-related SALTRACE papers?

Figure 3: Same question...

Figure 5: Why do you show this figure? You show two-month mean values, right? From all the CALIOP observations in June and July 2013? For proper comparison, the respective MACC results were averaged for the same CALIOP observational times within the two months?

Figures 5 a and b: Who is right? CALIOP or MACC? Who knows, I do not know? Because MACC is based on MODIS AOD, I would trust MACC. Because CALIOP needs lidar ratios they do not measure, and thus do not know..... these AOD values are less trustworthy. What lidar ratio did they use for dust 40sr or 55sr? CALIOP suffers from

multiple scattering effects in dust. Is that taken into account. MS leads to underestimation of AOD.

Figures 5 g and h.... Again, because the forward integration Klett method has to be used (very sensitive to uncertainties in the lidar ratio input profile), I do not trust the CALIOP values in the MBL and the mixing zone above MBL.... Again, who is right? There is no answer! Should be critically discussed! The only answer could be given by the SALTRACE ground lidars....

So, at the end, Figure 5 shows a comparison of MACC vs CALIOP! No DWL observation at all.

Figure 8: Again! Who is right (in figs 8 a and b). DWL cannot measure extinction. This is a wrong statement. The lidar can even not measure backscatter! It needs help by 'real'aerosol lidars. So I am always puzzled, what the basic and essential goal of this paper is...? Yes, the wind data comparison is very attracting, very convincing! This is the strongest part of the paper. MACC obviously does a good job.

Figure 9: Again....., the only reliable information (in a and c) is the observation of dust layering...

Figure 10: Is this figure needed. Ok, convincing! MACC does a good job! If that is an important finding, leave it in. If not, remove the figure.

Figure 11: At least for 11 July, I expected to see a broader view on the aerosol situation (conditions). Here, I would like to see the other 'real' SALTRACE aerosol lidar observations... in comparison with the DWL observations.

I am always confused by the fact, that this will be a contribution to a SALTRACE Special Issue, but the special issue aspect, integration of all available measurements to design a complete aerosol picture, is only poorly given. It seems to me that authors need publications and do not really take care and the time to look at all available data.