

Interactive comment on “A 4-D climatology (1979–2009) of the monthly aerosol optical depth distribution over the Mediterranean region from a comparative evaluation and blending of remote sensing and model products” by P. Nabat et al.

Anonymous Referee #2

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Review of “A 4-D climatology (1979–2009) of the monthly aerosol optical depth distribution over the Mediterranean region from a comparative evaluation and blending of remote sensing and model products” by Nabat et al.

This paper compares several satellite data, model output, and model-data assimilation products of AOD over the Mediterranean region (including Europe and N Africa). By evaluation of each product with AERONET from 2003 to 2009 and evaluation of vertical profiles from the models or assimilated products with CALIOP, a “best” set of satellite

and model results is chosen to construct a 4-D climatology of AOD and its five components (sulfate, black carbon, organic carbon, dust, and sea salt) in this period over the Mediterranean region. For period before 2003, the 4-D climatology is extrapolated based on the sulfate trends from the LMDz-OR-INCA model assuming other aerosol components remains the same as the 2003-2009 average. The purpose of this reconstruction of long-term AOD over the Mediterranean region is for use in regional climate models for aerosol radiative forcing and aerosol-climate studies.

I would like to first commend the authors for their significant amount of efforts pulling 11 satellite products, one global model, one regional model, and two global assimilation (reanalysis) products together in this work. However, there are several fundamental issues in this work that have to be addressed/corrected. The major comments are listed below, followed by specific comments. At this stage, I do not recommend publication in the present form, unless those major comments are adequately addressed.

Major comments:

1. Satellite data: a) It seems the authors did not use the updated or correct version of SeaWiFS. SeaWiFS retrieves AOD over both land and ocean and covers period from late 1997 to the end of 2010 (e.g., Hsu et al., ACP 2012). b) TOMS data covers time period beyond 1992 to the end of 2001, although there are data gaps and platform change (e.g., Torres et al., JAS 2002). Both TOMS and SeaWiFS data are publicly available. You should replace those used in your paper with more complete data.

2. Models: I don't see any point to use Tegen et al. (1997) aerosol. Besides the reason it does not play any roles in the reconstruction, it is a very old, out dated field, although Tegen et al. 1997 is the first study to compare modeled AOD with satellite (AVHRR) data. In that work, five major aerosol components were simulated with different models using emissions in the 1980s, because at that time no single global model was able to do all of them. The models have evolved rapidly since Tegen et al. 1997 and now many global models have the aerosol capability. I would suggest use the recent results

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from the AeroCom study. If multiple models are too much, then at least you can use the AeroCom median to replace Tegen et al. 1997.

3. Components: Several issues regarding the aerosol components: - I don't quite understand how the CALIOP components are used in the vertical but the selected model components are used for column AOD. How consistent are they? Do you just use the vertical shape of CALIOP and distribute the model column values to the shape? - The CALIPSO aerosol types are not equivalent to aerosol chemical composition models simulate. For example, "marine" aerosol is not just sea salt, and the "polluted dust", which is completely ignored in this work, contains multiple components. Even "smoke" and "dust" can include minor non-smoke and non-dust components. These aerosol types cannot be used quantitatively as pure components – they are just masks to indicate dominant components. If you use them literally and quantitatively as aerosol chemical components, errors have to be estimated. - A "best" model is chosen for its best match with the MODIS AOD, but there is no indication of the confidence in model components. For example, MACC matches MODIS AOD the best, largely because the MODIS AOD is assimilated, not necessarily because the model itself gives a reliable estimate of AOD or its components. - None of the models include ammonium nitrate and secondary organic aerosol, which can be quite important over Europe (thus Med Sea). In addition, it seems volcanic aerosols are not included either. At least such missing components should be acknowledged and associated error of omitting them estimated. - There is very little evaluation of aerosol speciation from the model. The only comparison is given for the vertical shape (not the quantity) with CALIOP aerosol type. There are no shortage of systematic surface observations in Europe; why not compare the model concentrations with these data to have some idea of how models do?

4. Long-term trends:

- The assumption of no interannual variability of BC, OC, dust, and sea salt from 1979 to 2002 is not realistic at all. The 4-D data reconstructed in such way is not useful

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and can be very misleading for models. Not only sulfate over Europe has reduced significantly in this period, anthropogenic BC and OC have also reduced. If the LMDz uses you use Lamarque et al. (2010) emission, it must not just use SO₂ but also use BC and OC emissions. Why not include those trends? Also, biomass burning changes from year to year, and Africa has experienced wet or dry periods during those decades, directly effecting dust emissions and transport to the Med Sea. - The large volcanic eruptions occurred in the last 3 decades are important part of the aerosol variations, not just in the stratosphere, but in the troposphere as well, especially in the upper troposphere. But they are completely ignored in this work, and authors even attributed the observation of those large volcanic AOD to “high bias”.

5. Errors and uncertainty range: Given so much assumptions and different ways of data combinations in this reconstruction, I am surprised that there is no error and/ or uncertainty range estimated for the 4-D products. Such estimates are must.

6. At the end, I am not sure how useful this 4-D reconstructed product is – there are so many assumptions involved in reconstructing the component AODs and vertical profiles without a clear picture of possible range of errors, there is no evaluation of SSA and asymmetry factors, there are important aerosol components missing in this reconstruction, and there are lack of realistic aerosol component trends prior to 2003. The usefulness of such products should be objectively assessed.

7. For comparison purpose, the maps for different satellite datasets and models should show the same time period. Otherwise it is difficult to assess the differences, because aerosols do change from year to year. You should use all periods to “reconstruct” the AOD, but for comparison purpose, it is appropriate to use the common time period. This applies to Fig. 2, 3, and 12.

Specific comments:

P 8473, line 1: it is not “either scattered or absorbed”; it should be “scattered and absorbed”.

P 8473, line 7-8: Not all those various authors had derived satellite AOD. Satellite AODs are retrieved by various satellite teams.

P 8473, line 21: What does “a relative agreement” mean? Give some measures (e.g., within x%; within \pm y).

P 8474, line 26: “second time” – when was the first time?

P 8474, line 27: “long period” – be more specific.

P 8476, line 19: Remove “relatively good”. This is a subjective description. Also, the 1 deg x 1 deg MODIS data is a gridded product (Level 3), not the standard product which is at 10-km resolution.

P 8479, line 22: Why do you have to use one of these three sensors? I think you should use all of them (plus SeaWiFS) by weighing the errors.

P 8481, line 15-16: I don’t understand this sentence “. . .the choice of the projection does not have any influence on the aerosol atmospheric content”. Why not? Using different emission projection will surely affect the aerosol atmospheric content, unless all the projections have the same emission.

P 8481, line 17: Do you mean the chronological aerosol from LMDz is fictitious? “Fictitious” has a rather negative meaning as “false”, “untrue”, etc. If you do mean that LMDz generates fictitious results, you should not use it at all!

P 8481, line 23-25, AeroCom: You are already doing model intercomparisons in this paper. You should at least use the AeroCom median, which is available.

P 8482, line 12: How long is “very long”? Years?

P 8483, first paragraph under “3.1 Methodology”: The description of what are shown in the Taylor diagrams and box-whisker figures would be most appropriate to move to the place where you present the figures.

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P 8484, line 21: “a north-south AOD gradient is well established” – only parasol, and perhaps SEVIRI, displayed some N-S gradient. From the figure, one cannot see any gradient from other satellites over the Med Sea.

P 8486, line 5: “inferior” is not used appropriately. There is no “superior” or “inferior” AOD, but “higher” or “lower”.

P 8486, line 6: “lower” is better than “weaker”. I don’t see a lower AOD in RegCM4 from the figure - it is actually higher in eastern Europe than MODIS and MISR.

P 8486, line 8: “slight difference”? The difference over the Atlantic Ocean is quite significant!

P 8486, line 11-12: Why do you attribute the “overestimation” to sulfate?

P 8486, last line: “Mediterranean AOD is indeed controlled by the dust...” – this is a conclusion without the evidence. From which dataset this conclusion is from?

P 8487, line 8: “negative bias” – against what?

P 8487, line 16: Give quantitative numbers of this “very good agreement” (e.g., within x%). In general, such subjective phrases should not be used.

P 8487, line 19: “a positive bias” – compared to what? GEMS is lower than MERIS. If both GEMS and MACC assimilate MODIS, why are they different from MODIS and different from each other?

P 8487, line 23: Not all models show the same variations. The seasonal max appear in different months.

P 8488, line 12: Change “a weaker spread” to “a less spread”.

P 8488, line 17: What kind of error is “important”? “Large errors” is more accurate. P 8488, line 26: Change “weakest” to “smallest”.

P 8489, line 3: Are you talking about scores, or standard deviation, or something shown

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in the figure? If it is score, what kind of scores are you talking about, and how are they calculated?

P 8490, line 11: MACC agrees with MODIS because it assimilates MODIS. Its comparison with MODIS is not an independent evaluation (i.e., use MODIS to generate the product then compare with MODIS).

P 8490, line 16 regarding Figure 8: There are too many lines in each panel, making the figures hard to read and digest. Please separate the regions into different panels.

P 8490, line 26: You should also look the seasonal cycle from the satellite data to convince the readers that RegCM4 also simulate the seasonal cycle correctly.

P 8491, line 28: Change “inferior” to “less than”.

P 8492-8493, first paragraph in “3.4 Vertical dimension”: It is not clear how CALIOP vertical profiles are actually used, and how those “relative distribution” are translated into absolute distribution. Also, which group does the “polluted dust” from CALIOP belong?

P 8493, line 12-16: I don’t understand how you determine the top of the aerosol altitude of 5000m or 6000m. This seems arbitrary and not even correct; for example, the models have dust all the way up.

P 8494, line 14-15: What does “significant aerosol vertical profiles” mean?

P 8495, line 25-27, “high AOD value” in 1992: This is not high bias of AVHRR data - this is the real volcanic aerosol from Pinatubo. If the satellite data do not show such a large volcanic AOD, they would have serious problems! It is the model that does not include any volcanic aerosols. This is the problem of model, not data.

P 8496, line 15-21, LMDz trends: What are the emissions of SO₂, BC, OC used in LMDz-INCA model? Have you examined the trends of aerosols in addition to sulfate? Is sulfate the only species in the model showing a decreasing trend? If it uses Lamarque

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et al. emission, then it should show changes of all anthropogenic aerosols from 1979 to present, not just sulfate.

P 8497, line 2-3, CALIOP for the period of 2003-2009: CALIOP only available since the second half of 2006. What did you use for 2003 to the first half of 2006?

Figures: As I already mentioned earlier, the comparisons in Figure 2, 3, and 12 should use the same time periods. The fonts in all figures are generally too small, particularly in Fig. 4 as they are essentially illegible.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 8469, 2012.

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