

Response to the Reviewer 1 comments. Original comments are in bold italics, our response is in regular font.

General comments

This paper presents the evaluation of the performance of NGACv2, the upgraded NEMS GFS Aerosol Component. The evaluations are mostly performed with observed AOT comparison with multi-model product. The description of the model is very simple because it is written in the companion paper (gmd-2017-306). The evaluation methods are simple and conventional RMSE and correlation factor against observations. The model uses a well-established GOCART, which makes it reliable as an operational forecast model but not very innovative. The evaluation methods are conventional but not new approaches. If there are some original characteristics or new concepts in the model, the authors should highlight that part.

We greatly appreciate the Reviewer's positive comments. We believe addressing the comments listed below resulted in a significantly improved presentation. The Reviewer's effort is greatly appreciated.

Specific comments

p.5, line 19: "AERONET AOT at 440 nm and 675 nm were linearly interpolated on a loglog scale to provide 550nm AOT": Isn't the 500 nm wavelength used for the estimation of AOT at 550 nm?

We agree with reviewer comment about 500nm wavelength which is used as a standard to compare against other models and satellite observations. However, we found not all AERONET stations that are used in this study report 500nm wavelength AOT values at regular basis (Level 1.5 data). To make all comparisons consistent, we used linear interpolation between 440 and 675 nm (which are reported in all stations) to derive 550 nm AERONET AOT. Also, MODIS and ICAP-MME that are used in this study provide AOT at 550nm.

Revised manuscript: Page 5, line 18 added this sentence "NGACv2 outputs AOT at 550nm and several AEORNET sites do not report at 500 or 550nm wavelengths".

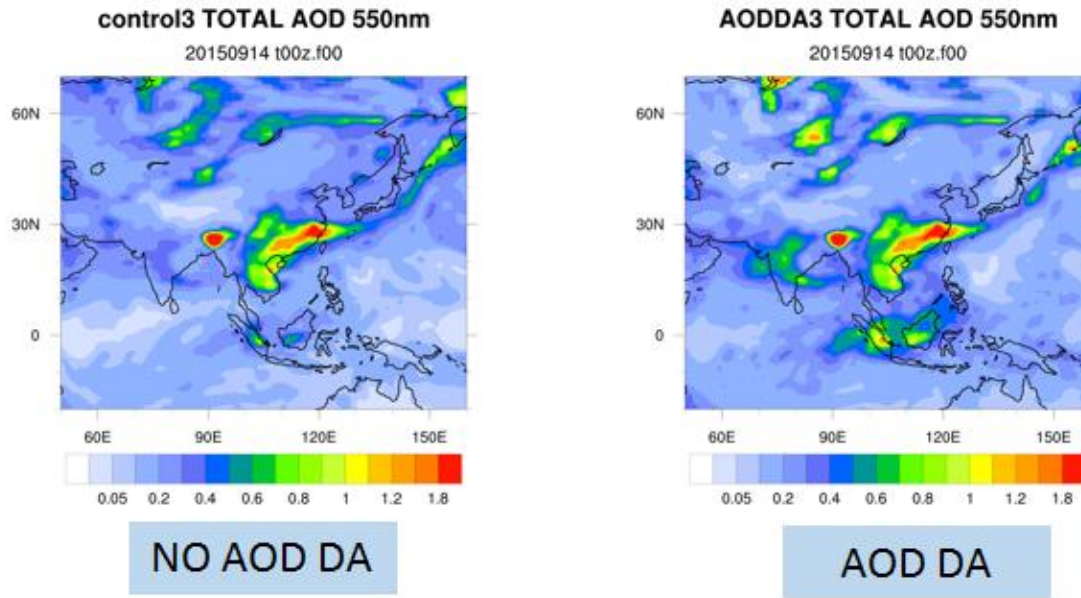
Section 5: What are the standards or criteria for the scores, especially for correlation coefficient? For example, generally $R = 0.28$ seems low but the authors describe it as moderate (p.9, line 34).

We have used number of sample points and location of AERONET center for correlation coefficient criteria. In this case, number of sample points is low (523 from Table 2) for 17 month comparison study. Also, location of this AERONET center is at remote Southern Indian Ocean and no other AERONET centers available near it to validate the observation. Based on these two criteria, we have described the correlation as “moderate” in this case.

Revised manuscript : Page 5, line 27, added this sentence “We have given more weightage on number of sample points and AERONET location (Table 2) for qualitatively describing correlation coefficients at each location as “low”, “moderate” and “high” in this study”.

p.12, line 13: "The model underestimates AOT over the Amazon region in both years and also for the Indonesian fire event in 2015.": It is better to include a presentation of the Indonesian fire event in 2015 in the result or case study section.

We evaluated our model against observations and other models for massive Indonesia fire of 2015. Major cause of NGACv2 underestimation is due to very low emission in GBBEPx detected in this region that has been used in the model. Currently we are conducting experiments



with different emissions and Data assimilation to improve model result over this fire event. Above is one figure that show improvement in AOD forecast over Indonesia (left one without any DA, right hand one is with DA). The one with DA, represent Indonesian fire better than the one without DA.

Technical corrections

p.1, line 15: 3-dimensioanl -> 3-dimensional

Corrected spelling in the manuscript.

Revised manuscript: at page 1, line 15.

p.2, line 10-11: I don't think the paper of Tanaka and Chiba (2005) is about data assimilation (Probably confused with Sekiyama et al. (2010,ACP)?).

We corrected and removed reference from Tanaka and Chiba (2005) from that line and reference list. We have reference of Sekiyama et al 2010 in that line and reference list.

Revised manuscript: page 2, line 10 added reference of Sekiyama et al 2010. Also Page 17, Line 12 added the reference of Sekiyama et al 2010.

p.3 line 6: "... but is is also ...": there is one extra "is".

Corrected.

Revised manuscript: at page 2, line 6.

p.3 line 13: "... observations.": there is strange bar over the period.

Corrected.

Revised manuscript: at page 3, line 13.

p.3, line 34 and p.4 line 9: "Wang et al., 2017" should be "Wang et al. (2017)"

Corrected in both pages.

Revised manuscript: at page 3 lines 15 and 34.

p.4, line 35 and after: References are separated by commas, the others are separated by semicolons.

Corrected.

Revised manuscript: at page 4 line 35.

p.5, line 3: MASSINGAR -> MASINGAR

Corrected.

Revised manuscript: at page 5, line 3.

p.5, line 6: UKMO's model is just written "(UKMO)" while other models are written with names as "(Institution-ModelName)".

Corrected.

Revised manuscript: at page 5, line 6.

p.6, line 31: "the s kind that ...": the "s" may be unnecessary?

Corrected.

Revised manuscript: at Page 6, line 34.

p.11, line 6 and after: Please check whether "Capo Verde" is correct, or typo of "Cape Verde".

We used AERONET station name (which is Capo Verde) in the manuscript, it can be found here at AERONET official site: https://aeronet.gsfc.nasa.gov/aeronet_locations_v3.txt.

p.4, line 28 and p.13 line 10: SNPP or S-NPP?

Corrected to S-NPP in both pages.

Revised manuscript: at page 4 line 28 and page 13 line 25.