

# ***Interactive comment on “The implementation of NEMS GFS Aerosol Component (NGAC) Version 2.0 for global multispecies forecasting at NOAA/NCEP: Part II Evaluation of Aerosol Optical Thickness” by Partha S. Bhattacharjee et al.***

## **Anonymous Referee #2**

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This is a companion paper to Part I, which presents a new version of NGAC including a fuller range of aerosol species compared to the previous dust-only version. In this paper, a thorough evaluation of the new model is presented, demonstrating good performance in many cases but also identifying a number of errors and biases that might be addressed in a subsequent version. I would recommend publication in GMD subject to the following major comment and further minor comments below

**Major comment.** I would draw particular attention to the Section 5 discussing Figure

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6, where a number of aspects of the seasonal cycle are stated in the text, while the figure doesn't appear to present any seasonal information. I would strongly recommend to ensure that the necessary information to visualise these seasonal effects is included in the figures.

- p.1, line 23:** data assimilation does not *improve* the actual model bias, it merely compensates for it in the overall forecasting system (which would still be expected to perform better if the model were unbiased).
- p.2, line 22:** the MACC project has now transitioned into the operational Copernicus Atmosphere Monitoring Service (CAMS).
- p.3, line 32:** I'm not sure "represents an efficient way of transitioning research..." is particularly relevant for a model evaluation paper.
- p.4, line 8:** I would suggest "as well as" rather than "apart from" – the latter suggests that mixing ratios are the exception that is *not* available.
- p.4, line 18:** as mentioned later in the paper, using gridded Level-3 data may result in sampling errors compared to matching the model output with Level-2 data; given the known limitations it would be good to explain the reasons for and implications of this choice.
- p.5, lines 1–2:** as above, MACC is now CAMS.
- p.5, lines 10–12:** if "no horizontal interpolation was needed", this is only because it has already been performed upstream – not because the data is being used at its native resolution.
- p.5, lines 23–24:** please explain the rationale for the choice of 2.5 as a threshold, and quantify the proportion of data excluded.

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- p.6, line 1:** It is suggested that Figure 1 shows seasonal variation to be in qualitative agreement, but this figure only appears to show one season so what is the evidence for this statement?
- p.6, line 5:** The text says South Africa (usually referring to the specific country), but from the plot it looks more like southern Africa in general is probably meant? Please check and clarify if necessary.
- p.6, lines 8–10:** Given these sampling errors, why not work why not collocate the model output with level-2 data?
- p.6, line 13:** why are only dust and OC shown in Figure 2? Is this because other species can be considered negligible here?
- p.6, line 31:** “of the s kind” – please check!
- p.7, lines 10–11:** agrees in general but the peak is overly broad.
- p.7, line 20:** also very low correlation in MAM16 as well as high RMSE.
- p.8, lines 9–13:** it’s unclear why increased cloud thickness would lead to reduced regions of high humidity and thus less hygroscopic growth and lower AOT.
- p.8, lines 22–23:** where is it shown that the model overestimates AOT during November–March? I can’t see this in the figure referred to here (Fig. 6).
- p.9, lines 9–10:** again, where is it shown that the model underestimates AOT in September–November?
- p.9, lines 15–16:** again, where is it shown that the model closely reproduces this higher AOT?
- p.9, line 24:** Should this be “The remaining 13 sites” rather than “the rest of the 13 sites” (which would suggest there are only 13 in total)?

**p.9, lines 25–26:** again, where is it shown that modelled AOT is higher than AERONET in May–October?

**p.9, lines 35–36:** where is this underestimation during the summer months shown?

**p.10, line 14:** Figure 8 is not properly introduced until the following paragraph. Also, it might be worth including the ICAP MME in this one.

**p.10, line 23:** “some... are higher than the model” – this is over-optimistic; from the figure it appears that almost all are, and some significantly.

**p.11, line 12:** Quantify “correlates well” with an  $r$  value.

**p.11, lines 15–16:** the peaks coincide, but the matching of intensity is quite variable.

**p.11, line 24:** it is not obvious why combining multiple observation sources increase uncertainty; much retrieval and data assimilation theory is about using these to *reduce* the resulting uncertainty.

**p.11, line 29:** ICAP-MME is not observations, but an ensemble of models.

**p.12, line 14:** Should be “a” large underestimation, not “at”.

**p.13, line 4:** a link or reference should be provided for NCL.

**Table 1:** This table is quite hard to digest; consider presenting visually e.g. with a Taylor diagram.

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Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2017-307>, 2017.

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