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Reply to Anonymous Referee #2

Eva-Lou Edwards et al.

Author comment on "Assessment of NAAPS-RA performance in Maritime Southeast Asia during CAMP^{2}Ex" by Eva-Lou Edwards et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-870-AC2, 2022

Referee comment on "Assessment of NAAPS-RA performance in Maritime Southeast Asia during CAMP2Ex" by Eva-Lou Edwards et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-870-RC2, 2022

The manuscript describes an evaluation of the performance of the NAAPS Aerosol Reanalysis over the Philippines region based on airborne measurements conducted in the framework of CAMP2Ex. The introduction is suitable to motivate the importance of information from aerosol reanalysis models in regions such as the Philippines where cloudiness poses a huge obstacle for regular satellite observations. The subsequent presentation of the work leaves much to be desired, though. Overall, the paper is lengthy and rather unfocussed. The authors focus on presenting all they have done in the manuscript and an additional 25 pages of supplement, rather than fitting it into an easy-to- follow story for the readers. This reviewer therefore suggest major revisions before the work could be considered for publication in ACP. Some specific issues are listed below:

The manuscript is written mostly in past tense. Please note that everything that still holds today, i.e. results show, should be in present tense

Response: This has been addressed. Thank you for the suggestion.

The Introduction is very broad and long given that the work deals with a relatively straightforward comparison of AOT and extinction coefficient from modelling and measurements and the effect of relative humidity and hygroscopic growth on the model output. Could the text be sharpened towards what is presented in the paper?

Response: The Introduction has been shortened and we tried to focus more on what this paper actually accomplishes.

Section 2 is too fragmented for my taste. First, it should really be “Data and Methods”, Second, the description of the measurement data, the model and its output, and the comparison methodology should be clearly structures along the lines of, e.g., (i) measurement campaign, (ii) airborne in-situ measurements, (iii) airborne lidar measurements, (iv) NAAPS-RA model description (the relevant part that is needed for this study, and (v) comparison approach and model refinement.

Response: Great suggestion. We now follow this order.

Table 2: is there any source for these values?

Response: Yes, these are now cited in the caption for the table.

Mixed layer heights: Is there any conclusion on which method is used in the final comparison? I read about an abundance of methods, but later in the presentation of the findings, there’s just the parameter MLH.

Response: We agree that the abundance of methods made this an unnecessarily difficult section to understand. We changed our method to only consider MLH from the HSRL-2 product (i.e., MLHs were no longer calculated using dropsonde data).

Section 2.6: The method to replace land-contaminated grid cells with the nearest neighbouring grid cell over open water is problematic as it leads to comparing apples and oranges. Better omit these data points rather than introducing comparisons that complicate the entire procedure. It might be better to instead relax the criterion that lidar profiles need to be in the vicinity of a dropsonde release to increase the number of comparison pairs. The findings later show that the shift to dropsonde RH has little effect on the modelled aerosol-optical properties.

Response: Very good point. We no longer consider land contaminated grids, but we still only consider grids containing dropsonde releases.

Later in the text the authors refer to 1 degree grid. It is not clear of this is a reference to an individual grid cell or a sub-grid within the cell.

Response: We only ever consider 1 x 1 degrees, and sub-grids are not considered. We hope that by specifying "1 deg. x 1 deg. grid" every time we talk about a grid that this will always be clear.

Section 2.8: It doesn't make any sense to me that the authors compare lidar extinction coefficients at altitudes closest to the mid-point of a model pressure layer to the modelled value. Lidar profiles can be very noisy and picking a value at just a single height risks to introduce all this noise of real-world data into the comparison. I'd suggest to work with a mean lidar extinction coefficient averaged over the width of the height layer covered by the corresponding pressure layer.

Response: We agree completely. Thank you for catching this mistake in the method. We now vertically and horizontally average all HSRL-2 data to match the resolution of the model. See Sect. 2.5.3.

Figure 2: please consider a different presentation of the data, e.g. as 2d histograms. It is really hard to extract useful information from the point clouds that are currently presented. The information regarding the research flight is not necessary here, as it is presented also in the next figure. In the context of this figure, I wonder if there is a minimum AOT that NAAPS-RA can represent?

Response: We agree presentation was not as good as it could have been in this figure. We adopted the reviewer's idea of 2D histograms and did not include flight-specific information. The minimum AOT is set by whatever we can verify with AERONET, so in that case it is around 0.01-0.02 (see citations in paper for Dubovik et al. [2000] and Eck et al. [1999] at the end of Sect. 2.4).

Figure 3: please revise into a format that allows to extract the information; or present the findings as a table?

Response: This figure (and flight-specific information such as this) has been eliminated from the paper. We do not feel it is necessary to tell the story anymore.

Figure 4: see comments on Figure 2. All I can see is noise.

Response: We changed presentation format to a 2D histogram as suggested above.

Figure 5: Omit or move to supplement as there is almost no change compared to Figure 2.

Response: We have combined with Fig. 2 and have changed the presentation format to a 2D histogram as suggested above.

Case studies: I understand that it is very interesting to assess the performance of the model under very different aerosol conditions. However, the current presentation of the case studies leaves much to be desired. Rather than shedding light on the model's performance, they are raising more questions than they answer. It is not sufficient to simply leave the deeper investigation of the issues raised by the case studies for later publications. The authors should at least formulate a solid hypothesis as to the nature of the inconsistencies.

Response: We agree that it can be frustrating that we cannot provide more explanation as to why modeled extinction is not in agreement with HSRL-2 retrievals. We adjusted our language to focus only on what we can study, and we are much less speculative on the mass concentration part of the case studies. We hope this reviewer finds our case study analyses more focused on the parts we can evaluate (error due to modeled RH and hygroscopicity).

Table 3 and approach to adjust gamma: Is it possible to revise the table for easier access to the findings; maybe by use of colour coding? Also, does it make sense to just adjust the modelled gamma to the value provided by the in-situ measurements? I reckon that the modelled value is the result of some mixing of the numbers in Table 2. Does this allow for a deeper view into the contribution of the individual aerosol types to gamma by adjusting the individual values that go into the mixing rule rather than exchanging the output? This might also lead to more consistent adjustments? Or are you doing it like this already?

Response: Table 3 has been simplified drastically. We now only focus on mixed layer AOT to evaluate a particular case study.

Sorry if it was not entirely clear before. We did not use in situ gamma values to adjust the gamma value of individual aerosol types (i.e., individual gamma values for smoke, ABF, dust, and sea salt) in the model. We take the in situ gamma value and use that for each species.

Line 406 – 407 states: "Here, we use the same in situ γ in Equation 2 for all four aerosol types."

The modelled aerosol optical properties seems to be dominated by the fine mode. It is surprising to me that a modelled coarse-mode mass that is one order of magnitude larger than the measurements is supposed to have no effect. Or is this effect systematic as the same difference seems to be found for all case studies? I think that this topic should be explored deeper in the paper.

Response: We have eliminated this part of the analysis. We do not focus as much on modeled particle mass concentrations/fractions as we do not have the in situ data to really evaluate this. We eliminated it so that the paper appears more focused on what we can actually do well.

The work ends with a short list of conclusions that don't seem to warrant to amount of material presented in the manuscript and the supplement. I recommend that the authors either streamline the presentation into a short paper or make an effort in exploring all the questions raised by the case studies.

Response: We agree that the paper attempted to discuss more than we could actually support with the data available. We have streamlined the paper to focus as much as possible on relationships between errors in modeled RH and modeled extinction and AOT, as well as errors in modeled hygroscopicity for the case studies. The amount of material in the supplement has been greatly reduced in order to only present the most relevant information. We hope the reviewer finds that the conclusions are well-supported and the paper more streamlined.