

Atmos. Chem. Phys. Discuss., referee comment RC1  
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## Comment on acp-2022-496

Anonymous Referee #1

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Referee comment on "On the differences in the vertical distribution of modeled aerosol optical depth over the southeastern Atlantic" by Ian Chang et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-496-RC1>, 2022

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### General comments

This is the first review of the article by Chang et al. "On the differences in the vertical distribution of modeled aerosol optical depth over the southeast Atlantic". A comparative study of aerosol optical thickness calculated by seven models and measured by various in-situ (airborne) instrumentation is documented in the manuscript.

The manuscript is well written and lets you read it well. So apart from a few stylistic details or minor technical corrections, the exposition is adequate for publication.

Scientifically, the chosen statistical tools are also adequate, although I personally would prefer to read predictive statements about the significance of the differences found between models and measurements, even in the presence of a relatively limited statistical sample, given the time window analyzed. I also think that much of the statistical information in the figures should be reproduced in a separate table for easier reference.

Second, another remark is the role MODIS ACAOD plays in the storytelling: although a separate section is devoted to it in the text, too little consideration is given to this dataset. This is from the abstract, in which MODIS is not even mentioned, throughout the conclusions. The MODIS dataset, which is a nice addition to the suite, should be discussed more because it carries valuable information.

Third (and most important remark so far).

The manuscript appears to be more of an account of a technical project than a scientific article. As it seems to me, this paper goes so far as it does in squeezing out information from the results presented so far. But a paper comes alive not by documenting a particular accomplishment, but by making connections with previous research. If this is not done, then also the interpretation of the results will be less informative.

While the differences between the models and measurements are clearly reported, I found no convincing attempt to go nor to the source of these differences neither to scientifically pinpoint the outcome of these differences. All the more so when the authors themselves in the text indicate possible causes of discrepancy (i.e. PBL calculations, emission inventories and others) and also the possible consequences of these discrepancies (direct, semi-direct, indirect aerosol effects). As such the analysis feels shallow and much more could be inferred from the datasets at hands.

I believe is a reasonable task for such a comprehensive list of co-authors with their respective expertises.

Although I believe it is a choice of the authors with what content to populate a scientific article, I would appreciate more effort regarding two issues: (i) going as far as possible - and reasonably - to the source of AOD differences from models (e.g., looking at emission inventories and the treatment of air mass evolution with aerosols); (ii) identification - through measurements or other sources - of those instances where entrainment occurs, that is how much of the differences can be explained by suboptimal representation of aerosols AND clouds together? Do the lidar profiles provide additional information on the aerosol and cloud phases (along the vertical) that might be of interest to the ESMS?

I want to be even clearer on this point: I do not expect the authors to dissect the source code of ESMS and analyze the respective parameterizations. Just as I do not expect them to quantify changes in radiative forcing depending on whether aerosols are detached, in contact, or inside clouds. This would be a topic for a separate study. What I would expect is an analysis - at least a qualitative one, based on both the abundant existing literatures and original reasoning - of the two aspects mentioned above.

And this should then be reported in a section called "Discussion", right before "Summary and conclusions" (which reads just as a repetition of the abstract).

I could request "minor revisions" and the paper might even be fine as it is, although scientifically thin. But I am requesting "major revisions" just to make sure that my comments are considered to augment the interpretation and to warrant the full scientific exploitation of the results obtained so far.

Specific comments

P2 L71: the authors may want to be more precise with the reference about cloud lifetime and add the standard Albrecht's study  
(<https://www.science.org/doi/10.1126/science.245.4923.1227>)

P8 L267-269: is it really true that varying the grid cell size affects the standard deviation only? How do you know this is a fact? I would expect that also the mean AOD will be affected. More precisely, finer cells have higher means than coarser cells. As a consequence, this will impact the comparison and the vertical partition of aerosol loading.

P12 L-378-380 (and P7 L225-226): How can MODIS ACAOD report higher AODs while the reported mean biases are negative?

P13 L 417-418: "A deeper analysis of biases in model processes than is possible through the AOD comparisons presented here is essential in order to understand the cause of model biases."

First, check the wording (than). Second, I understand that the results presented here are a first-order assessment of model performances against in-situ and spaceborne observations. As such, the authors suggest the examination of those assumptions and parameterizations leading to the found biases. While the statistics and the presentation of the found biases are sufficiently clear and exhaustive, I miss then the takeaway. Isn't an author's task to reasonably pinpoint the error sources instead of leaving the question open?

P14 L 444-458: The short discussion about the nature of aerosol radiative forcing, while correct, feels premature or feels like a natural conclusion of the study for others to be answered.

Except for the wording that could be stylistically improved ("In conditions where ... play roles". Seemingly redundant), I am left with the question of where and how the authors ever touched in their main text upon the presence of layered clouds, the entrainment of aerosols, and ensuing change of thermodynamic phase and change in extinction profile.