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Comment on acp-2021-521

Anonymous Referee #2

Referee comment on "Global maps of aerosol single scattering albedo using combined CERES-MODIS retrieval" by Archana Devi and Sreedharan K. Satheesh, Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-521-RC2>, 2021

This paper combines daily CERES flux retrievals and MODIS aerosol retrievals to estimate aerosol single scattering albedo (SSA) at 550 nm. SSA is, after aerosol optical depth (AOD), the key parameter determining aerosols' radiative effect, but is difficult to retrieve well from most spaceborne measurements. The authors expand the application of a technique called "critical optical depth" they have developed before to a global scale. There is a brief comparison to airborne data, and to similar SSA maps available from OMI.

The study is in scope for the journal, though is also a close fit for AMT. It is fairly clearly presented. My main criticism is that the numerous uncertainties in the technique are glossed over and the reader is instead presented (in the abstract and conclusions) with the claim that the global uncertainty is about 0.03. There is no real analysis to back up this number and it seems to be based on limited airborne measurements over India and surrounding oceans. The manuscript is not very long and I think that the paper would benefit from a much more thorough and honest discussion and quantification of uncertainty sources. Otherwise an inexperienced reader might believe the problem of determining SSA from space is essentially solved.

To that end, I recommend major revisions, and would like to review the revised version. I think the work is valuable but not yet at ACP quality.

General comments:

- The paper is missing references to the existing literature. For example, a lot of similar work has been done framed in terms of “critical reflectance” or “albedo” rather than “optical depth”. The basic idea is the same, i.e. find a value of one parameter (surface/aerosol) where the top of atmosphere signal is invariant to changes in the other. Examples include Seidel and Popp (2012): <https://amt.copernicus.org/articles/5/1653/2012/> and Wells et al (2012): <https://doi.org/10.1029/2011JD016891> The authors should acknowledge and discuss the relative merits of other work using the same basic technique like this.

- The uncertainty discussion really needs to be strengthened. There are claims of 0.03 throughout the paper but they are really not supported. The authors do not really acknowledge that e.g. aerosol vertical location matters a lot as well: you can get quite a different forcing if the aerosols change height due to interactions with Rayleigh scattering (which depends on pressure). This is well established by e.g. the OMI and combined UV-vis work the authors cite during the paper. Other key uncertainty sources are inconsistencies between the aerosol and surface properties assumed by the MODIS and CERES retrievals with each other and with the OPAC-based SBDART calculations. A further is the possibility of variation on scales of the regions used for the linear fitting process; the residuals on the fit (uncertainty on the intercept) would be one easy way to incorporate this effect. There are doubtless others as well. I think the authors need to list the potential uncertainty sources and try to quantify as many as possible – even if approximately – so it becomes clear which are the most important. The comparison against airborne data is good to have but this is only a small part of the picture, and definitely not enough by itself.

- It is not clear to me if the derived SSA data are publicly available; I did not see a link in the paper. They ideally should be somewhere.

Specific comments:

- Abstract: As SSA is a spectrally varying quantity, the wavelength reported should be given here (550 nm). Additionally, the statement about uncertainty is basically unsupported and seems to come from the comparison against a small number of aircraft observations. I recommend that this statement is removed or made a lot weaker, e.g. "limited comparisons against airborne observations over India and surrounding oceans were generally in agreement within ± 0.03 ". The abstract should be an honest summary of what is in the paper, not a place to hype up the work, as unfortunately many people only read abstracts and skim papers.
- Page 6 line 15: what exactly is the significance test done on here? This should be clearer. My guess is that it is on the linear correlation coefficient between the AOD and albedo difference, i.e. the authors are testing whether the probability of observing a correlation coefficient at least that large if there were truly no linear relationship between the two quantities is 0.05 or lower. Is that right?
- Figure 2: here and in the text, it is mentioned that small regression slopes mean that SSA cannot be determined well. Again, the linear model fit uncertainties (see general comments) could be used to do this for every grid cell and associate an uncertainty. If this is not done, though, then the authors should show where and how frequently these conditions occur. It is not clear if, for example, if almost never happens or is common. In the latter case the seasonal maps shown later may have some additional sampling-related uncertainties.
- Section 4: more information about the airborne measurement of SSA, including their

uncertainty, is necessary. I encourage the authors to search for additional airborne data which may supplement their results from elsewhere in the world, which would strengthen the robustness of these comparisons. There have for example been NASA field campaigns through the US, south-eastern Atlantic, and Korea during this time frame, and these NASA data are publicly available (I am sure the investigators who spent considerable time collecting the data would be glad to see them used). Doubtless there are other resources as well.

- Section 4: I disagree with the framing of this section as a “validation” given the small extent of comparison and lack of detail or consideration of uncertainties. I suggest that it be renamed “Comparison with airborne observations” and the use of the word “validation” throughout be changed.

- Section 5: comparing against OMI is one good choice; the authors might also mention the POLDER archive, which is similar or higher quality for SSA, but ended in 2013 before the time period the authors used here. The results could also be compared to global aerosol model simulations or reanalyses. And, although the authors briefly mention AERONET, it would be worthwhile to add a comparison with AERONET for regions where there is a persistent repeatable high aerosol loading. The authors could take AERONET climatologies themselves or go to other analyses, e.g. Giles et al (2012) <https://doi.org/10.1029/2012JD018127> for various types of aerosol or Sayer et al (2014) <https://acp.copernicus.org/articles/14/11493/2014/> for smoke in various regions. POLDER results could also be used in a climatological sense. Finally here the authors should be clearer that the reason for less OMI coverage over oceans is not so much cloudiness but in fact that over ocean the OMI retrieval is only done if the UVAI is high (I believe 0.7 or above). So this introduces a sampling bias towards high-AOD, high absorption cases (as we know baseline sea spray is not very absorbing) which is likely the main reason that OMI SSA is patchier and has lower values over ocean. This could be tested by also subsampling the MODIS-CERES data to only examine those times when OMI also has a retrieval. POLDER and reanalyses do not have this issue. Expanding the comparisons would provide further evidence for where the authors’ technique may be valuable or where there are issues with one or other data set.

In summary, this study has value but I think a much deeper treatment of uncertainty is needed. Otherwise it is not clear to what extent this technique improves our understanding of aerosol SSA, or where the largest challenges remain. We can’t

quantitatively move forward if we don't understand where we stand now.