Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-767-RC1, 2019

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Site representativity of AERONET and GAW remotely sensed AOT and AAOT observations" *by* Nick A. J. Schutgens

Andrew Sayer (Referee)

andrew.sayer@nasa.gov

Received and published: 1 October 2019

I am writing this review under my own name (Andrew Sayer) as I have a current collaboration with the author. I believe I am able to provide an unbiased review of the paper. I made a few suggestions at the Quick Report stage – the author has addressed these in this version of the study, except for adding a discussion of the paper Li et al (JGR 2013, https://doi.org/10.1002/2016JD025469), which he is still digesting, and a paper of mine still in review (Sayer and Knobelspiesse ACPD 2019, https://www.atmos-chem-phys-discuss.net/acp-2019-372/), which will hopefully be accepted in the not-too-distant future.

This paper uses the high-resolution GEOS-5 Nature Run (G5NR) to assess the rep-

resentivity of AERONET and GAW sites for total and absorption aerosol optical thickness (AOT/AAOT) on coarser spatial scales, on various temporal scales and collocation strategies. The sampling of these data sets is applied to G5NR fields, and then compared with averages from the full G5NR. This builds on the author's previous work on representation and sampling-related uncertainties in creating/comparing aerosol data sets, and is a worthwhile extension of sufficient novelty to warrant separate publication.

The analysis is in scope for ACP. The quality of language is pretty good overall. There are a lot of figures but I don't know that it can be condensed much, and the paper is not that too long so I think it's ok. I have a number of comments, below; I'm not really sure whether they fall into minor or major revisions, but I would be interested in reviewing the revised paper.

Page 4 line 7: "sphotometers" - should be sun photometers?

Section 3: This has only one subsection. Could that subheader (3.1) be deleted? Or else another one be added (e.g. for the text summarising the difference between S17 and here)?

Page 7 line 11: Holben (ACP, 2018 https://www.atmos-chem-phys.net/18/655/2018/) is a good reference for the DRAGON campaigns, which could be cited here.

Section 4: the evaluation of G5NR is presented mostly in terms of correlation coefficient and regression slope of AERONET vs. G5NR mean and standard deviation of AOT/AAOT. In a sense each site is collapsed down to provide a single data point for the analysis. So this is somewhat different from typical validation analyses where one looks at individual AOT pairings (and in those cases regression is not so appropriate; it is probably fine here, see next paragraph). The reason for this is that G5NR is a nature run so corresponds not to the real (historical observed) world but a realistic world driven by the model. I have used G5NR data before so am familiar with this subtlety, and the author does state it, but I wonder if a less-familiar reader might be confused. I wonder if this point can be hammered-home a bit more with tweaks to working. For example

page 7 line 3 says “simulated AOT shows good agreement with the observations” - this might be changed to read “simulated site-mean AOD” to reinforce the point that we are comparing site averages, not individual points, here. Unless I have misunderstood what is being done. That is one example, but the same applied throughout the section.

More generally the use of correlation and slope can be a bit problematic for AOT analyses, because of the distributions of the data and their error characteristics. It is probably fine here because we are looking at summary statistics for individual sites, rather than individual points themselves, which is a different application from normal. However, because AOD distributions are skewed (and often close to lognormal on timescales like the year evaluated here – see the Sayer and Knobelspiesse reference mentioned above), I wonder if this analysis and Table 5 might be better presented in terms of geometric mean and geometric standard deviation (i.e. in log space). Perhaps the author could do this (doesn’t necessarily mean both sets of analysis need to be shown in the paper); if the results are basically the same, great, but if not, it reveals something about limitations of the model simulation.

Page 7 line 20: it might be worth being clearer here that the AERONET AOT requirement for level 2 is 0.40 at 440 nm. For an Ångström exponent (AE) of 2 you get to about 0.25 at 550 nm from this. But for dust-dominated columns with an AE around 0.5 you are around 0.35. So the threshold translates to 550 nm differently dependent on aerosol type. As this threshold is mentioned again on page 9, I think it’s worth devoting another line or two to the point here. I realise that the author is using 0.25 as a threshold on the simulation here (i.e. not using the actual thresholds AERONET applies in each case), but that will affect the conclusions systematically at e.g. dust-dominated sites (true AERONET sampling will be poorer than the OSSE suggests because the true AERONET threshold for dust will be more like 0.35 than 0.25).

Page 7 line 21: I think this should be “fewer”, not “less” (in both cases), because the observations and sites are countable.

Figure 1: it’s not clear what the distinction between solid and dashed lines in the lower panel is here. I know it is pairs of correlation and slope for mean and standard deviation of AOT/AAOT. But I did not see which is which given in the caption or text.

Figure 2: I know there were reviewer and editor comments about number of figures. I think this is one which could potentially be cut (or moved to a supplement) and summarised in the text instead, since the main point (if I understand correctly) is that the statistics for the level 2.0 inversion data are not that different from the less-restrictive level 1.5.

Page 8 line 5-6: I would check in with a member of the AERONET team about this. I don’t know what the main uncertainty source leading to AERONET AAOT uncertainties (which are driven by SSA uncertainties is). If it is calibration then that would have an air mass factor dependence so could manifest in apparent daily variation (and violate the author’s assumption). If it is something like surface albedo then that may be more of a constant uncertainty which might (consistent with the author’s assumption) not affect daily max vs. min AAOT so much. However in Tom Eck’s 2014 paper (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/jgrd.50500, Figure 4), looking at the variation of SSA at Mongu with day of year, he found different slopes in different years, and attributed this to calibration uncertainties (as the sensor is calibrated before and after each individual deployment, calibration uncertainty is systematic within a year, but random year-to-year). This implies that calibration may be one of the largest contributors, in which case it’s possible that the daily variation of SSA (and hence AAOT) is affected (although that paper did not look at SSA diurnal variations). It would probably depend on both the daily variations of SSA and AOT – if AOT varies a lot that may win out over any false signal from SSA. I am not sure whether anyone has looked in great detail but the AERONET team might.

Page 8 lines 12-13: another factor is instrument maintenance issues (e.g. cleaning, replacement when it is sent back for cleaning). Even if this is only 1 week per year then that’s still up to 2% coverage (or about 1% when accounting for daylight), which is simi-

lar to the difference observed at many sites. So I'd say "meteorological differences and site maintenance issues" or something. This is addressed in the following paragraph but relevant for the direct-Sun data discussed here too.

Page 8 line 17: "several times per day" - I believe it is at specific optical air mass factors but I did a quick look and can't find what those are. I want to say it is a maximum of 6 per day. In the newer data they have hybrid scans nearer solar noon which can extend this, but for the year 2006 simulated by G5NR these were not available. So in that sense the newer AERONET data will fill in some of the gap that is in the observation but not predicted by the model.

Page 8 lines 16-21: One issue is that the inversions require a high degree of azimuthal symmetry (see their QA document at https://aeronet.gsfc.nasa.gov/new_web/Documents/AERONETcriteria_final1.pdf). So for example if an aerosol plume is thicker to one side of the site than the other, then the scene may be rejected. I don't have a good idea how often this happens; the AERONET team might. I wonder if that is one of the larger factors accounting for the overestimation of AERONET inversion coverage. There are a few other things too, e.g. the AOD threshold for AERONET is stricter for dust-dominated scenes than that applied in this study (see earlier comment) which would affect some of the sites in the tropics.

Page 8 line 25: I believe style guidelines for the journal require sequential appearance of Figures; here Figure 12 is mentioned for the first time, in between 4 and 5. From context it is clear that Figure 12 should not be shifted back here, but the Copernicus style guide disagrees. Perhaps this sentence could be shifted later in the paper instead (and so call back to this section).

Section 5: I realise that this is framed as relative errors throughout. But many applications require absolute uncertainty, so absolute values are also important. So perhaps some text and/or a table could be introduced, with a summary of what fraction

of sites the representation error is smaller than some threshold (perhaps the nominal AERONET AOT uncertainty of 0.01, or the GCOS goal of max[0.03,10%]), for each grid size and time stamp? A large relative sampling uncertainty might be unimportant for a pristine location, for example. Alternatively Figures framed that way could be placed into a Supplement.

Section 5.1, title: I suggest "Representation errors in yearly AOT" to make it clearer up front this is about comparing yearly aggregates colocated in different ways. It will help make the contrast with section 5.2 (monthly) clearer up-front.

Figure 6 caption: "Yeraly" should be "Yearly"

Figures 6, 7 (and dots in 21): can these be regenerated with a different colour bar? The rainbow doesn't print well, emphasises certain parts of the data range but suppresses others, and can't be understood in greyscale or by many colour blind readers. The "viridis" palette is a good alternative, and other options can be found online. Here's a link to an IDL implementation from the CRU: https://crudata.uea.ac.uk/∼timo/idl/mkviridis.pro Also, panels are presented as left/right but captions indicate top/bottom, and it would be good to add latitude/longitude labels and/or national borders to this for ease of reference if the reader wants to look up the value for a specific site.

Page 9, lines 4-5: yes, it is clear from this Figure that the bias is negative much more often than it is positive. This implies that higher-AOT times are not sampled by AERONET as often as they should be. One explanation is coincidence (plumes systematically avoid them) but I find that unlikely. So, what is the other mechanism? Could this be the clear-sky bias, i.e. AOT is higher near clouds but near-cloud cases are not sampled? I wonder if there is some way to quickly examine this (e.g. rerun part of the analysis with a cloud fraction threshold of 0.9 instead of 0.01, see if the bias in the representation error shrinks)? Ok, reading ahead to page 10, from Figure 13 it looks like it might be the clear-sky bias. Perhaps that figure and text could be moved up a page. This part –

quantification of clear-sky bias – is to me quite an important result.

Page 9 line 10: this is an important point, I'm glad the author highlighted it again in the Conclusions.

Page 9 line 14: I would say "limitation of" rather than "issue with", to help emphasise this is due to the measurement type rather than being something which was done wrongly.

Page 9 line 22: is -410 m really correct? Which site is 410 m below sea level?

Page 9 lines 29-31: the symbol r was previously used for correlation (e.g. prior paragraph), now is being used for Kinne's rank score. Also, this second use of r does not appear to be stated explicitly in the text. I suggest finding another symbol for the rank score and defining it explicitly in the text. Perhaps capital regular R rather than lower-case italic r.

Page 10 line 1: I would say "typically cannot retrieve aerosol when there are clouds". CALIOP, for example, can retrieve under some clouds. Other retrievals could be extended to do so (see e.g. Lee JGR 2013 https://doi.org/10.1002/jgrd.50806 for an attempt I was be involved with – I don't know that this paper needs to be cited or discussed, just providing it here for an example). I suggest the rephrasing because in part this is a sensor issue but in part it is an algorithm issue.

Page 10 lines 10-11: I am not sure that I follow this. I agree that it will be true if there is correlation from year to year as well. Which there almost certainly is in many parts of the world. But I think that's a bit different from the month-to-month correlations here. I think this should be clarified/spelled out a little more clearly.

Page 10 line 13: I think the words "radiation records" are missing from the end of the Schwarz paper cited here.

Figure 16: what are the dashed lines here?

Page 10 line 21: "criterium" should be "criterion".

Page 10 lines 21-22 and Figures: The impact of the AOT threshold imposed on AAOT representivity is clear. However I am confused because I thought from Table 2, the AOT threshold was taken as 0.03 for level 1.5 data, and not 0.25 (which was for level 2 data). The text (and Figures) here refer to level 1.5 data, but to the 0.25 threshold. Is there a typo here or have I misunderstood? If the threshold was 0.03, why is the bias so positive? If it was 0.25, why are we discussing level 1.5 data and not level 2 data?

Page 11 line 6: there is a missing Figure reference in this line (appears at ??). From context I think that this should be Figure 18, which seems to fit and is not mentioned elsewhere in the paper.

Page 12, lines 11-12: Thank you for making this list available. I downloaded the file from the DOI linked to the citation and it was clear.

Page 13 lines 20-31: I'd personally split this out as a bulleted list (and perhaps the point about the Wang analysis too), to better drawn attention to these conclusions and recommendations.

Figures 8, 9, 10, 12, 13, 14, 15, 17, 18, 19, 20: I think a note should be added here to state that the colours (and, except for Figure 15, numbering legend) follow Figure 5.

As a general question: Is one take away that AERONET and satellites should if possible provide additional hourly products, for intercomparison purposes? Since hourly collocation minimises the representation error for longer-term aggregates, making these more readily available might spur users to use them (rather than the current approach which is more or less monthly collocation).

Language comment: I think in some places the term "uncertainty" should be used instead of "error". The calculation of representation error via difference between the differently-sampled G5NR simulation is an error. But I think when talking in a larger sense, we are using this representation error (from the OSSE) to estimate the actual

representation uncertainty (which we don't know for sure). Also when talking about AERONET inversions, we should be typically talking about the uncertainty in the retrieval (as the error is not known). I suggest checking individual uses of these terms in the papers.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-767, 2019.