

Atmos. Meas. Tech. Discuss., referee comment RC1  
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## Comment on amt-2022-319

Anonymous Referee #1

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Referee comment on "Evaluating the effects of columnar NO<sub>2</sub> on the accuracy of aerosol optical properties retrievals" by Theano Drosoglou et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2022-319-RC1>, 2023

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### General comments:

Overall, this is a well-written paper evaluating an interesting aspect like the effect of the columnar NO<sub>2</sub> correction in the accuracy of the Aerosol Optical Depth (AOD), the Angström Exponent (AE) or the Single Scattering Albedo (SSA) using multiannual records (5 years). This paper addresses some important aspects for the scientific community, such as the investigation of the effects of using different NO<sub>2</sub> data (no correction, direct retrievals or climatological values), which can impact the aerosol products retrieved with different global aerosol networks (NASA-AERONET, GAW-PFR or SKYNET-Prede). Moreover, NO<sub>2</sub> satellite data (TROPOMI) and spectral ground-based data (from Pandonia Global Network, PNG) were used to investigate the possible improvement in aerosol properties retrieved from these three largest ground-based aerosol networks. Trend analysis has been included to understand the impact of the NO<sub>2</sub> correction on the derived aerosol products, although the authors make it clear throughout the article that the number of data in the database is insufficient to carry out this type of study.

I consider that this manuscript fits perfectly into the scope of AMT and that the results presented here are relevant. There are only a few minor remarks.

### Minor comments:

Abstract: Is uncertainty estimation a goal of this paper? I consider that this work deals with the impact of the columnar NO<sub>2</sub> effect rather than evaluating/investigating the uncertainty introduced by this term. Please state.

Page 2, lines 34-37: The authors are introducing the direct and indirect effects of aerosols. Don't the authors believe that there are more adequate references to introduce these effects? At least one more recent version of the IPCC exists than the one included in this article.

Page 4, line 101: Is "specifically" right in this sentence?

Page 4, line 112: The authors write "Cimel" in this sentence but "CIMEL" later in the text. Please homogenize.

Page 4, line 118: Please note that Version 3 Level 1.5 includes data with near-real-time automatic cloud screening and automatic instrument anomaly quality controls while Level 2.0 additionally applies pre-field and post-field calibrations. This means that the 1.5 level does not in any way apply the final calibration, so the lack of certainty in the verb "may" does not seem correct.

Page 4, Fig. 1: The authors present here a time evolution of the AOD and AE observations at APL-SAP and CNR-ISAC. I don't see the point of including such a figure, because these data are not exploited here nor are they mentioned throughout the text.

Page 4, last paragraph: The authors have used level 1.5 SSA information from AERONET. However, as stated by Sinyuk et al. (2020), quality-controlled SSA data (level 2.0) should be retrieved for AOD larger than 0.4 and SZA larger than 50°. How the authors have ensured the quality of the SSA information included in this paper? Why the authors have not included AERONET Level 2.0 data in this study?

Page 6, lines 183-185: The authors introduce here a past comparison between Pandora and Brewer without giving any result of this comparison. This sentence seems dispensable if it does not provide more information about the validity of Pandora NO<sub>2</sub> data.

Page 6, last paragraph: This paragraph explains the NO<sub>2</sub> deviation Pandora versus OMI (AERONET) as is displayed in Fig. 4. It is written that, according to Fig. 4 (lower panel), biases (Pandora-OMI, I guess) of 89% and 87% are found. I'm not able to see these results in the lower panel. Later, the authors give another result: Pandora-OMI average differences of 61.5% at both stations. Could you please explain more in detail these different results and where do they come from?

Page 9, line 281: As mentioned before, the authors acknowledge throughout the text that this database is too short to perform statistically meaningful trend analysis. The question is obvious: why then carry out this analysis?

Page 11, lines 311-314: A reference to previous studies in Rome including some climatological data and aerosol types would be useful in this context.

Page 11, line 332: The values of 1.1% and 1.9% included in this line (as well as in the following lines) don't correspond to the values in the table. Are the authors reducing the floating points in the text? The use of these values can cause confusion in the reader.

Page 12, lines 350-352: The authors stated that, according to Table 2, SKYNET retrievals are quite similar irrespective of the TROPOMI data used. However, similar results (low difference with the PNG product) were retrieved also in Table 1 for Pandora. Furthermore, mean deviations of AERONET products also displayed very low values...

Page 12, Figs. 8 and 9: Why not merge these two figures into one?

Page 12, line 363: Please define what "modified AOD" is.

Page 12, lines 365-370: I find relevant the lack of information (numbers) to quantify these results.

Page 14, section 3.6: The authors stated in section 2.2.1 that level 1.5 SSA AERONET data were used in this paper. However, in this section, it is not clear to me what SSA product was used. If I understand well, a mimic of the AERONET product retrieved by GRASP was used as a reference, instead of the AERONET SSA standard product. If so:

- Please correct the information provided in section 2.2.1 including a suitable explanation of GRASP algorithm and products used in this paper.
- Why not use the real product instead a "mimic" product? At least these two SSA should be compared...

Could you please clarify it?

Page 15, Fig. 2: There is no information about the lower panel plot. Is the SSA difference?

Page 15, Fig. 2: Y-axis of the upper plot should be SSA and not NO<sub>2</sub>.

Page 15, Fig. 2: Information about correlation is written in the text in terms of r-squared while in this figure is expressed as correlation coefficient "R" (in capital letters). Please homogenize.

Page 15, line 454: Again, the numbers provided in the text do not correspond to the ones provided in the plot. It is a matter of rounding correctly to the appropriate significant digit. For example: with RMSE values of 0.035 and 0.031 I don't consider it appropriate to conclude that RMSE is < 0.035. The same for R squared.

Page 15, line 452: Why the threshold of 0.9 DU?

Page 15, line 453-454: The authors stated that a positive bias of 0.02 was found in conditions of high NO<sub>2</sub> concentrations. Are they talking about SSA or NO<sub>2</sub>? From what figure (upper or lower panel) this result comes from? I see in the lower panel an average difference of 0.01 for NO<sub>2</sub> > 0.9 (high NO<sub>2</sub> conditions) but 0.02 for all conditions. From where did the authors find this result? I feel lost with this section.

Page 15, lines 455-458: This sentence seems confusing to the reader. Please rephrase. It has also some typos, like the comma after the word "studies".

Page 16, line 477: The general result stated here (AOD differences below 0.01 because of this NO<sub>2</sub> correction) seems really relevant. In fact, this is the main result a reader is expecting. However, is this general result written somewhere in the text?