Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2019-339-RC1, 2019

Atmospheric Measurement Techniques Discussions
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Interactive comment on "Retrieval of aerosol optical properties over Beijing: a comparison between SKYNET and AERONET" *by* Xianyi Yang et al.

Anonymous Referee #1

Received and published: 9 October 2019

Title: Retrieval of aerosol optical properties over Beijing: a comparison between SKYNET and AERONET

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Review: Atmospheric Measurement Techniques

General Comments:

This paper overall contains some useful information on the SKYNET measurements

and retrievals. However, I feel the material does not meet the standards of an AMT publication since these is relatively little new information that is not already in the scientific literature and therefore does not present significant or substantial scientific progress in remote sensing of aerosols. I urge the authors to submit this paper to a different journal since there are some analyses that may provide some insight into aerosol properties in Beijing. Additionally, the paper is poorly written with numerous issues such as lack of clarity on dates utilized for analysis, poor statistical representativeness (only one pollution event and one dust event), incomplete or even erroneous descriptions of AERONET measurements and/or algorithms, and in some cases poor choices of comparison metrics. I provide numerous examples of these issues and other issues below in Specific Comments so that these may be improved in a revised submission.

Specific Comments:

Line 14, Abstract: Correlation coefficient is not the best way to compare AOD from these two networks. Better comparison metrics for AOD would be RMS differences and bias.

Line 16, Abstract: The SVA cannot be changed as it is a characteristic of the hardware of the instrument. I assume you might mean calibration adjustment therefore this should be reworded.

Line 17-19, Abstract: For size distributions it is more important to compare the modal or peak sizes of both the fine and coarse modes for the 2 retrievals rather than comparing the volume.

Line 17-19, Abstract: Be clear here on how many cases were utilized to compute these comparisons. It seems to me that the sample size of one pollution event and one dust event compared to one clear event is not nearly large enough to be statistically robust.

Line 45, Introduction: Please mention here that AERONET is global, as compared to the regional SKYNET.

Line 60, Introduction: This line about the Pandithurai et al. (2009) reference should be removed, it does not fit or make sense here.

Line 66-67, Introduction: Very poorly written, were all AOPs in the best agreement at 675 nm, including the AOD?

Line 68-69, Introduction: Why do you not provide a summary of the Estelles et al. (2012) results when you did for the other comparison papers that you referenced?

Line 78-79, Introduction: This is the wrong reference (Mok et al., 2016) for this analysis package. Note that in the first sentence of section 2.2 you give a web address for this package. This is an example of errors and lack of attention to detail in this manuscript.

Line 84, Section 2.1: Please be specific on the AERONET data used, did you analyze the Beijing-CAMS site data in Sep 2016 and the Beijing site data in March 2007? Also be clear here: are there only 2 months of data used in the comparisons between SKYNET and AERONET retrievals?

Lines 89-90, Section 2.1: This is a very poor description of the Cimel spectral measurements of most of the AERONET network. The AOD measurements at most sites also include 340, 380, 500 and 1640 nm. Also, the three channel 870 nm-channel polarization measurements were made at only a few sites in AERONET and retrievals did not use these channels. Additionally, you need to include uncertainty values of the AERONET data, such at the 0.01-0.02 for AOD (Eck et al. 1999) and the 0.03 for 440 nm SSA when AOD>0.5 (Dubovik et al., 2001).

Lines 99-100, Section 2.2: Again, an incomplete description here. The AERONET algorithm uses a mixture of both spherical and spheroidal particles, with the percentage spherical determined by the best fit to the measured sky radiances.

Line 104, Section 2.2: You should note that this results in an uncertainty in AOD ranging from 0.01 to 0.025 for overhead sun (optical airmass=1).

Lines 106-108, Section 2.2: It seems that you are retrieving SKYNET AOD from two

different methods. More detail on the differences in these methods needs to be provided.

Lines 125-129, Section 2.2: However, this does not take into account the fact that AOD versus wavelength is not linear in logarithmic coordinates (see Eck et al. 1999). This is particularly true in Beijing since the fine mode particle size is often relatively large when AOD is high, and needs to be discussed.

Line 138, Section 2.3: Explain why you chose 72 hours for back trajectories given that aerosol lifetime is typically 1 week (7 days).

Line 156, Section 3.1: AOD is not retrieved by AERONET, no inversion algorithm is used to determine AOD. It is measured from direct sun observations. Also, you should cite the reference of Giles et al. (2019) when discussing the V3 AOD data.

Line 162, Section 3.1: Please give the biases in units of AOD also (i.e. $\sim$0.02 etc.).

Lines 162-164, Section 3.1: The way this sentence is written it is impossible to tell which of the 2 different SKYNET AOD values you are discussing here.

Line 168, Section 3.1: Please give some specifics here about what is done to select data 'carefully' here.

Line 170, Section 3.1: Please state in the text what is correlated in Figure 3, AERONET versus SKYNET from the SR-CEReS retrievals?

Line 174, Section 3.1: It would be useful to know if the correlation increases for the data subset where AOD(440)>0.4, since the Angstrom Exponent is highly uncertain at low AOD.

Lines 179-180, Section 3.1: You should interpolate the SKYNET 400 nm and 500 nm SSA to 440 nm by linear interpolation in linear coordinates, before making comparisons to AERONET.

Line 182, Section 3.1: Please also provide the bias in units of SSA (i.e. $\sim$0.03 differences).

Line 183, Section 3.1: Please note here that the SKYNET SSA retrievals hit the saturation value of 1.0 in a significant number of cases while AERONET SSA does not.

Line 188, Section 3.1: You need to define SA here and explain briefly how it relates to influencing the magnitude of sky radiances.

Lines 198-199, Section 3.1: Please make it clearer that these constant values of surface reflectance for all sites is a very crude assumption. Also state in the text that AERONET uses geographically and seasonally varying surface reflectance values that are much more robust.

Lines 215-217, Section 3.1: This is obvious, and you should mention that the imaginary refractive index is the retrieved parameter and that SSA is derived from this information along with the size distribution and the real refractive index.

Lines 225-227, Section 3.1: It would be useful if you provided some comparisons of volume size distributions of fine mode dominated (AE(440-870)>1.2) and also coarse mode dominated cases (AE<0.6).

Line 238, Section 3.1: These are typically called volume concentrations not volume spectra.

Line 247-251, Section 3.1: I think your earlier analysis of the effects of surface albedo and solid view angle are the more likely reasons for differences in SSA and IM between the AERONET and SKYNET retrievals. I suggest removing this sentence.

Line 251-252, Section 3.1: Some of this fine mode peak volume concentration variance is due to changes in fine mode radius from low AOD to high AOD conditions and also from dry to humid conditions. Showing one average size distribution is a very poor way to compare these two products. A scatterplot of SKYNET versus AERONET would be much more informative.

Line 254-255, Section 3.1: This is by definition since these are the limits of the size distribution for AERONET and there is a strong constraint in the AERONET retrieval that results in near zero values at the limits.

Line 260-265, Section 3.2: Please make it clear in the figure captions that these frequency distributions are for the SKYNET retrievals only.

Line 266-271, Section 3.2: Are these frequency distributions from multi-year data? Or just one year for each season? The AERONET data base would provide much more robust statistics for fall versus winter than just these 2 seasons of data from SKYNET. I suggest remaking the frequency distribution plots from multi-year AERONET data in order to have a more robust seasonal comparison, including for all four seasons.

Line 280-281, Section 3.2: Similar comment as above: Since the study period of the SKYNET data is so limited it seems that you cannot make any general statements about fall versus winter aerosol properties unless you redo this climatological analysis with AERONET data and expand it to all 4 seasons.

Line 284-286, Section 3.2: It is well known that the major dust season in this region is spring and you ignore this since the SKYNET data set is so limited.

Line 288-289, Section 3.2: Hygroscopic particle growth leads to larger fine mode particles, not a shift from fine to coarse mode, as this sentence suggests.

Line 292, Section 3.3: Figure 9 x-axis labels are wrong, it is not hours but days, from Dec 27 through Jan 9, 2017.

Line 296, Section 3.3: Define the ratio here in the text as PM2.5/PM10.

Line 302, Section 3.3: You should also discuss the large amount of fog present from Jan 1 - Jan 5, 2017 in the region around Beijing (MODIS images show this), and the high cloud cover amount on Jan 4, 2017. Li et al. (2014) in Atmospheric Environment analyze the high AOD and PM associated with fog in Beijing in January of 2013. Fog may be associated with large secondary production of aerosols in the region. Also, Eck et al., 2018 show the association of high cloud amount in Beijing with high AOD levels, likely due to humidification growth of fine particles.

Line 319-320, Section 3.3: You show a PM2.5 value in Figure 10 for Jan 4 but in Figure 9 you only show the PM10 value for that day. This inconsistency needs to be explained or else corrected.

Line 371, Section 3.3: The AOD levels are too low on the clean day to have reasonably accurate SSA retrievals. You should not even discuss such SSA values that would have very large uncertainties ($\sim$0.10).

Line 391-392, Section 3.3: Please explain the trajectory # 1 and #2 as shown in the Figure 12.

Line 405, Section 3.3: All 3 days show 2 clusters of trajectories although the differences in these are not explained. Are they from different altitudes?

Line 420, Section 4: Add 'of absorption ' here, after 'weakening'.

Line 425-426, Section 4: This may be true for past data sets, but the newest AERONET instruments now take hybrid scans hourly that provide more frequent retrievals throughout the entire day.

Line 427-428, Section 4: This sentence does not make any sense in the context of this paragraph and therefore should be removed.

Line 435-438, Section 5: In addition to the correlation coefficient is it import to also provide the rms differences and the average bias for the AOD and AE plus refractive indices (both real and imaginary) comparisons.

Line 454-455, Section 5: It was never explained why only autumn and winter were analyzed, and the other 2 seasons ignored in this paper.

Line 462, Section 5: Again, poor writing, since Dec 27 is a clear day, Jan 2 is a dust event and Jan 4 is a haze event, yet you identify the whole two-week interval as a haze

event.

Line 468-475, Section 5: The way this is written it implies some statistics of many days of data in each category of clear, dusty and hazy. In actuality this is only one day of each type and therefore these numbers of very limited value.

Line 478-479, Section 5: These sentences should be removed since they are vague and it was not clearly shown in the paper why the SKYNET retrievals will have better performance for dust events than for haze events.

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