

Interactive comment on “Improvements in AOD retrieval from geostationary measurements over Asia with aerosol optical properties derived from the DRAGON-Asia campaign” by M. Kim et al.

Anonymous Referee #2

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Review of Kim et al.: Improvements in AOD retrieval from geostationary measurements over Asia with aerosol optical properties derived from the DRAGON-Asia campaign. AMTD 15, 10773, 2015

Summary The authors use an extended sun photometer data set to improve their aerosol model for South-East Asia using a longer time series and using the denser network of sun photometers in two areas around Seoul (Korea) and Osaka (Japan) during the DRAGON-Asia campaign in 2012 (March-May). The improvement resulting from using each data set is monitored and the results are presented in the Tables. Subsequently the improved aerosol model is used in a single channel aerosol retrieval

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algorithm using data from a meteorological imager (MI) on the geostationary satellite COMS. The results (AOD) are evaluated by comparison with AERONET data. Notation in this review: (pagenr, line nr)

General comments The title is misleading. Although Seoul and Osaka are situated in Asia, the aerosol models applying over these regions are unlike to apply over all Asia and hence cannot contribute to retrieval improvement over Asia, only over part of SE Asia. As explained in my comments below (detailed comments, 10788), the improvement in the AOD retrieval is questionable.

The goal of the paper is to improve the single channel retrieval algorithm applied to MI to retrieve AOD, as mentioned the title and the discussion. The study builds on earlier work by Kim et al. published in RSE in 2014. Since the retrieval method is key to the work described here, the authors should provide a brief summary how a single channel algorithm can be used for aerosol retrieval, what the assumptions are, what the status was before this study, and what improvements are made in the current paper with respect to the previous work. In particular, with only a single channel available, only one parameter can be retrieved of the many which determine the radiance at the top of the atmosphere observed by the instrument. Since the parameter the authors are after is the AOD, all other parameters have to be assumed or estimated. How well is this done and what are the implications due to uncertainties in the assumptions?

The authors put a large emphasis on the improvement of the aerosol model used in the retrieval, in particular they use data from the DRAGON-Asia campaign in 2012. Since this campaign had a limited duration (3 months), how do these data apply to the rest of the year and what has been done to extend for the full year? How are seasonal variations taken into account (Tables 2 and 3, bottom)? How can the results in these tables change for other seasons than those for which DRAGON was deployed? Seasonal variation appears to be an input parameter to the retrieval! Especially because the authors mention that the largest improvement was due to the use of the DRAGON campaign, while the use of an extended time series of other sun photometers with

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2 more years made hardly any difference (10783, 15-21). This would imply that the aerosol properties over Asia had hardly changed over these years. How does the single channel algorithm select the aerosol model? How are spatial variations taken into account? In Tables 4 and 5 I see validation statistics for 39 sites but it is hard to believe that the aerosol models are invariant over the wide area considered in the study (cf. Figure 1: 15x10 degrees!) For example, Figure 3 shows a very strong variation in the average AOD and AE over the study area which strongly indicates that the aerosol models vary spatially. I'd also like to see how the measured (AERONET & DRAGON) and derived (model) compare, in particular how well do the derived properties describe spatial and temporal variations during the DRAGON campaign and over the long time series (cf. Table 1). How representative was the DRAGON period for the whole time series?

An aerosol model is described by a size distribution, preferably multi-modal to take into account at a minimum the fine and coarse mode fractions, plus the imaginary refractive index for each mode. However, the authors only report the real and imaginary parts of the refractive index which makes it impossible for others to use their results. I see no metrics indicating a measure of the quality of the model parameters as obtained from the analysis, i.e. what is the accuracy with which the real and imaginary parts of the refractive index are obtained? In 10792, 27 is mentioned that the use of the DRAGON data resulted in an increase of SSA of 1.1% while a few lines below (10792, 4) they mention that the assumed SSA error is 3% resulting in an AOD error of -20% to +23% for the situation given in that para. Furthermore, since the authors don't provide the particle size distribution, it is impossible to estimate the effect on the SSA of a change of about 0.001 in the imaginary refractive index, combined with a change of about 0.01 in the real part. Another issue is the wavelength dependence. Where AERONET provides data in discrete wavebands, the MI measures in a single VIS waveband (0.55-0.80 micrometer). How is the variation of the aerosol optical properties over this quite broad band accounted for? How is AOD at 550, as reported e.g., in Fig. 8, obtained?

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The authors write several times that a goal of their work is to improve their understanding on the aerosol optical properties over the study region (e.g. 10791, 5 and 21). However, they don't report what in their understanding has improved, what have they learned from this study? They only report changes in the numbers they use to describe the aerosol properties, and correlations.

For these and other reasons summarized below, the manuscript is not ready for publication in ACP. Major revision and a second review are required. I further suggest thorough proofreading: I noticed several sentences which should be corrected (e.g. 10790, 13-16, see also minor comments), and some references are missing (e.g. Frey et al. 2008; Kim et al., 2015; Choi et al. 2015; there may be more, I checked only a few).

Minor comments. 10775, 10-15: I miss here the more recent retrieval improvements, in particular for LEO there are no references after 2010 for, e.g., MODIS (C6), MISR, SeaWiFS or European efforts (AATSR, MERIS), which all have significantly improved over the last 5 years and provide similar validation metrics. 10775, 16-20: surface reflectance can be accounted for using dual (AATSR) or multiple (MISR, POLDER) view algorithms, or for single view algorithms using certain assumptions (MODIS) or modelling approaches. 10776, 5, 6: references are needed for each statement 10776, 22: which satellite algorithm? 10776, 24: I think this study would better be done for a multi-channel algorithm? Hence some justification is needed for using a single channel. The sensitivity of a single-channel algorithm to the assumptions in AOPs (10776, 28) is not explicitly tested. Only improved statistics from using an improved aerosol model are presented, but I see no explanation where this improvement comes from. (see also general comments) 10777, 5: typo 10777, 7: when an aerosol model is representative, how can it be improved? 10777, 14-end of para and Section 3.1: it seems awkward to use a minimum reflection method over a heavily polluted region like SE Asia, cf. the AOD values in Figure 2. The BAOD most likely has a substantial contribution to the total TOA radiance. How can this ever result in a realistic surface reflectance

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value? Most likely uncertainty in surface reflectance has a much larger influence on the retrieved AOD than the choice of the aerosol model, or tweaking the aerosol model as done in this MS. This method should be evaluated for the study area before it can be applied in the retrieval study. Section 3.1 discusses the BAOD which ranges from 0 to 0.56, as determined from 7 years of MODIS AOD data. But how do these and other numbers connect with the numbers in Figure 4 which shows a maximum BAOD of 0.3 as the 'absolute minimum AOD' (caption) rather than an average value (10782, 6). I wonder about the representativeness of the surface reflectance data determined using the BAOD for atmospheric correction. The result is at best an average for these 7 years, but does not take into account year-to-year, seasonal or other temporal variations. Hence large surface reflectance errors are likely to result and hence large errors in the retrieved AOD. 10778, 20-end of page: needs some proofreading: 'was decreased', 'values of Japan', 'AE represents the change of particle size', 'where' 10778, 25: I see AE changes from blue (1.1) to red (1.4): these are certainly not 'not significant' changes in particle size distribution. 10779, 10: likely the authors mean that they kept part of the data apart from the model analysis and used them only for validation. Please reformulate this sentence. 10780, 11: one-to-one relationship between two variables: what do you mean: which variables? 10780, 12-25: see my general comment: authors need to provide more detailed info on the aerosol model parameters used 10780, 26 to end of para: see comment above on surface reflectance 10781, 10-end of para: how well does the cloud screening work? Could this be evaluated form comparison with collocated AERONET measurements? 10782: section 3.2: what is the methodology used? 10782, 19: what is 'radiative absorptivity'? 10782, 23: 'regionally integrated aerosol model': what is meant with that in view of large spatial variations as shown in Figure 2? Are spatial variations not accounted for? 10783, 3: was the original data set re-analysed, or are the results from Kim et al. (2014) used for comparison? 10783, 6: 40 new sites? Table 1 lists only 35, including the original sites . . . When the measurement period was extended by 2 years, was this for the additional sites? Which two years? How was temporal variation accounted for, i.e. were time series inspected

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on temporal variation over the extended period (previous period plus two new years)? Was this in some way accounted for in the application in the MI retrieval? 10783, 12: AOP for 675 nm are shown, how about the wavelength dependence? 10783, 22 and 24: which AOD bins? 10783, 23: increase of SSA above 0.005? I assume that here the authors mean an increase with 0.005, which is still very small considering that a few lines above the SSA was reported with only 2 significant digits and hence this is an increase with less than 1 %! What is the uncertainty in this analysis and how significant is an increase with 0.005? Especially since only 3 months in the spring are considered here versus a long term data set? Is this a seasonal variation? An anomaly for spring 2012? Does such a minor change indeed warrant a change of aerosol model in the retrieval which already carries other important uncertainties? 10783, 24-end of para: the numbers shown here for other months than MAM, and thus using other sites than from the DRAGON campaign, are larger than the SSA increase of 0.005, and thus the extension of the number of sites and the longer period seems to have more influence than the higher density of sites, which contradicts the conclusion on 10784, 1-5. Furthermore, when the authors say 'is believed to have caused this change (10783, 29), why don't they just checked it rather than believe? 10784, 6- end of para: I don't see how this discussion contributes to 'better understanding'. Furthermore it contradicts earlier statements that particle size distributions don't change. Also, the parameters describing the volume size distributions in Fig. 5 should be provided, they are not listed in table 2 as suggested in the caption. 10784, 23: a new para should be started with 'Using ..' 10785, 3: what are the assumptions in the retrieval? Is there some iteration to obtain the optimum solution? Any converging criteria used for this? 10785, 10 and further: 4% variation in SSA corresponds to about 0.04 in absolute value, i.e. more than discussed above and more than the effect of a new aerosol model as compared to the old one. 10786, 5: negative SSA? Probably a negative error in the SSA is meant? 10786, 14: 'were ranged' or 'ranged'? 10786, 21: surface reflectance at which this occurs is (insert 'at which this occurs') 10787, Fig 7 and discussion in first para: the Figure annotations are too small to see the meaning of the colour bar and also

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the figures are too small. After enlarging I determined that red means an AOD of 2, and there seems to be much more red in the MI than in the MODIS images in which the AOD is about a factor of 2 lower. Hence I don't understand what is meant with 'spatially well matched', and that values are 'slightly higher'. Also, which MODIS images match up with which MI images? 10788: section 4.2 and Figure 8: The authors claim the success of their updated aerosol model by the slope in Figure 8b which now is one, at the cost of more scattered data and lower r . However, if I compare Figures 8c and d, where, if I understand it correctly, independent data are used for the comparison, i.e. data which have not been used in the model improvement, the comparison becomes worse with the improved model: slope becomes smaller than one, and all other metrics are also decrease slightly. So in contrast to the conclusion of the authors, my conclusion would be that independent validation shows that the new model does not lead to improvement of the retrieval and hence the improvement in Figures 8a and b is due to using the same data sets for model improvement and testing. Also the Taylor diagram in Figure 9 and the data in Tables 4 and 5 do not convince me of a clear improvement: changes are visible, mostly small and are sometimes a bit better, sometime a bit worse. Were the numbers used here with the independent data or with the full data set? 10789, 5-14: see my comments above on the accuracy in SSA and related AOD uncertainty. 10793, 1: how does the algorithm select an optimized aerosol type at each measured pixel? I don't think I have seen that in this MS. The flow chart in Figure 3 doesn't explain it, nor the text in Section 3. Since this is the key subject of the paper, this should be explained. Section 5: several comments which have been given in the above.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 10773, 2015.

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