

## General comment

In this study, the SBDART radiative transfer model was implemented over East China, using as input satellite data from MODIS and reanalysis data from MERRA-2, NCEP and ECMWF. Measurements from ground stations were used to validate some of the input data and the output. In terms of methodological approach, the study lacks in innovation, since both the radiative transfer model and the input data have been widely used in the past. However, the authors claim that this is the first time that this approach is implemented over a large area in East China, for the 16-year period 2000-2016. In such a case, the study would benefit if the analysis and discussion regarding the model output, and possible relevant explanations, were expanded. This would lead to a better contribution of this study to the existing literature regarding aerosol loads over China, their effects and changes during the previous years, and I strongly encourage the authors to expand the study in this direction. Overall, I recommend reconsideration of this study after major revisions.

## Specific comments

### Abstract

The authors use three stations to validate their results. How much representative are these stations regarding the entire East China region studied?

### 1 Introduction

Lines 53-55: The statement regarding the different levels of aerosol cooling effects over different areas of China renders the question of representativeness of the three stations used here for validation purposes very important: do the three sites capture the variability in aerosol types and sources (and consequently optical properties) over East China well enough?

Lines 62-63: I understand that the authors want to highlight the advantages of satellite-based aerosol retrievals. The result, however, is misleading and should be complemented with some of the disadvantages. For example, “continuous temporal coverage” is hardly achieved from satellite observations, since it depends e.g. on satellite orbits and the presence of clouds.

Lines 1-2: “... have rarely been addressed...”: Please mention these few studies.

Line 3: This disadvantage of satellite measurements is true globally, not only over China.

Line 90: As with MERRA-2 and MODIS before, please mention here also the data set used for the gridded aerosol vertical profiles.

### 2 Data

Lines 100-104: Please mention that these results regard previous MODIS AOD collections and update with relevant studies using collection 6.

Line 105: “... at a wavelength of 0.55  $\mu\text{m}$ ”. How is SSA treated spectrally?

Lines 120-122: Please be more specific: was the daily MCD43C3 albedo product used? (this is mentioned in Table 2, but it should also be mentioned here). Which band(s)? Which measure is the “confidence index” and which values were selected to ensure accuracy?

Lines 128-144: The aerosol vertical profile plays indeed an important role in the corresponding forcing calculations, but the way that it was estimated and incorporated in the radiative transfer calculations is not clear: what was the default of the radiative transfer model and what changes were implemented? Were the calculations described here performed in this study or in the references provided? Please provide references for the WRF Model and NCEP-FNL algorithm. Please also give more details on the output of these calculations and how it was used in the radiative transfer model.

Lines 141-144: Please mention what kind of interpolation was used for the spatial resolution homogenization. The authors should also provide relevant information on the temporal resolution. As mentioned in Table 2, the AOD and TOA fluxes are instantaneous (although it should also be mentioned that they are available once per day), and other data sets are hourly and daily. What was the temporal resolution of the radiative transfer calculations?

Table 2: To my knowledge, the spatial resolution of the daily surface albedo product MCD43C3 is  $0.05^{\circ} \times 0.05^{\circ}$ , not  $0.2^{\circ} \times 0.2^{\circ}$ .

### 3 Methodology

Please provide more details on the radiative transfer calculations: were they spectral or broadband? Which solar spectrum was used as input? How was the spectral variation of aerosol properties and surface albedo treated?

### 4 Results and discussion

#### 4.1 Retrieval of aerosol properties

Lines 163-164: What do the authors mean by “other sites in East China did not have enough data for analysis”? SSA is a crucial and highly uncertain parameter in the calculation of aerosol radiative effects, and in my opinion, every quality-screened sunphotometer data, even of short ranges or intermittent, would add to the credibility of the SSA reanalysis data used here.

Line 179: Do the authors claim that SSA values are similar throughout the study region? This would be intriguing considering the size of the study region ( $10^{\circ} \times 14^{\circ}$ ) and the high variability of aerosol sources within it. Perhaps an analysis of SSA spatial variability based on MERRA-2 data would clarify this issue.

Last paragraph of Sect. 4.1: The approach used to restrict ASY values described here is interesting and promising. However, it implies that all other parameter values (except ASY) are correct and do not affect the difference between estimated and measured  $F_{u\_toa}$ : the authors practically assume that varying ASY only is enough to match  $F_{u\_toa}$  values, and the ensuing ASY value can then be trusted. This assumption can deviate from reality if differences

between real and retrieved values of other parameters (e.g. SSA, AOD) occur. The authors should include a discussion on this issue and its possible consequences. Additionally, a description of the statistics of ASY values retrieved here would also be helpful and informative.

#### 4.2. Validation of the method

Line 216: "... in the single grid..." Do the authors mean the three grids of corresponding stations? Please rephrase.

Line 221: Please be more specific and give details regarding the performed quality control.

Line 231: I don't understand how the authors reach to this conclusion based on Fig. 5. The fitting lines suggest that the simulated  $F_{d\_sur}$  is overestimated in low values and underestimated in high values. The range of values could easily be explained by e.g. the seasonal variation in solar zenith angle, rather than different pollution levels. Even if pollution levels were the only explanation for this range, low  $F_{d\_sur}$  values should be related to polluted conditions, since more aerosols would block larger parts of the radiation reaching the surface.

Line 232: What do the authors mean with the term "smooth"? Please explain.

Line 235: "... especially in clear conditions". Again, low values of  $F_{d\_sur}$  are somehow associated with clear conditions. Please explain.

Line 236: "... southern and northern sites of East China...". Based on Fig. 1, Fuzhou and Yong'an are in the southern sites of the study region, however Baoshan is more central than northern.

Lines 244-246: How is the presence of clouds inferred from the MODIS true color map?

Lines 270-283: It is not clear what the authors claim here regarding the effect of aerosol origin on ADRF. What is the difference between the northward and southward directions and how does this difference explain the different error sign? If I understand correctly, the authors claim that aerosols from northward directions are mainly anthropogenic and strongly scattering. What about the southward directions? If aerosols originate at sea, aren't they also strongly scattering? Please discuss more and clarify.

#### 4.3 Long-term ADRF retrieval in East China

The authors mention in this subsection many names of places. It would be helpful for the reader to have these places shown on a map.

Lines 297-298: This explanation is interesting. Do the authors mean that AOD values are similar between northern and southern areas, and the large differences in forcing should be attributed to the aerosols in the North being more scattering? Comparing the maps shown in Fig. 6a with corresponding spatial distributions of AOD and SSA could clarify this point.

Line 300: “locates” should read “located”, and “it” before “blocks” should be omitted. Please also rephrase the end of this sentence: are aerosol accumulations higher or lower?

Line 305: “dominated natural aerosols”: please rephrase. “Weaker cooling effect”: is this due to lower concentrations or different optical properties? Please clarify.

Lines 308-311: “In addition... measurements”. There are many grammatical errors in this sentence that need correction. Furthermore, past tense should be used here. More important, however, is the fact that this is a very significant finding of the study, and it should be further investigated here. What kind of changes did the authors find? What were the differences between North and South? The 16-year long data sets used as input are adequate enough to investigate possible reasons for the changes found in the model output, and could provide useful insights. Hence, I do not agree with the statement that this result “needs to be further identified and explored with additional measurements”. This is an important part of the analysis that should be included here.

Lines 312-313. Please provide possible explanations for these patterns. Again, comparisons with input data and relevant studies could give useful insights.

#### 4.4 Sensitivity test and uncertainty analysis

Lines 349-340: I do not understand how the sensitivity test presented here can lead to this conclusion regarding the aerosol profiles. Please clarify.

#### 5 Conclusion

Lines 383-389: Some of the findings presented in previous sections are repeated here. They should rather be summarized.

#### **Technical corrections**

Line 20: please replace “Terra and Aqua” with “Terra and Aqua MODIS”.

Line 32: please omit “with” and “the” in “climate change”.

Line 38: Liao et al. should read “2015”.

Line 43: Is this a global average value?

Line 52: Nyeki et al. should read “2015”.

Line 56: please add “the” before “wider knowledge”.

Line 57: please add “are” after “measurements”.

Line 60: Qiu et al should read “2017”.

Line 65: "Graaf" should read "de Graaf".

Lines 77-78: Please replace "Levet" with "Levelt" and "Tilstra et al." with "Tilstra and Stammes".

Line 78: Please consider replacing "undesirable" with a more appropriate term.

Line 84: Please replace "After SSA determined, ASY, the only unknow inputs" with "After SSA is determined, ASY, the only unknown input".

Line 87: Please replace "propose" with "provide" and "in the clear sky" with "under clear skies".

Lines 88-89: Please consider rephrasing. Furthermore, East China is the study area, rather than the "validation area".

Line 92: Please replace "including" with "includes".

Line 93: Please replace "was" with "is".

Line 94: Please add "is" after "method".

Lines 150-151: Please correct the ECMWF acronym (also in Fig. 2).

Lines 179-180: There is no "Che et al., 2017" study in the references.

Line 182: Buchard et al. should read "2017".

Line 192: "Chang, 2013" is not included in the references.

Line 212: Please add "be" before "input".

Line 215: Please omit "to" before "applied".

Line 217: "was" should be replaced by "were".

Line 287: Please add "the" before "past".

Line 307: Please omit "of".

Line 308: "the positive value of ADRF can occur especially in the bright surface" should be replaced by "positive values of ADRF can occur especially over bright surfaces".

Line 312: "It reflects ADRF shows...". Please rephrase.

Line 313: Please omit "the" before "most".

Line 314: The Alam et al., 2011 citation is not included in the references.

Line 317: Please replace “with combining of” with “combined with”.

Line 318: Do you mean “Wu et al., 2016”?

Line 341: Guan et al. should read “2010”.

Line 370: Please correct the ECMWF acronym.

Line 380: Please replace “additionally” with “additional”.

Line 382: Please include “of” after “validation”.