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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Limitations of passive satellite remote sensing to constrain global cloud condensation nuclei" *by* P. Stier

Anonymous Referee #1

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General comments

This paper explores a yearlong simulated data, produced by general circulation model (GCM), to evaluate the ability of passive space-borne instruments to assessing global cloud condensation nuclei (CNN) concentrations, based on aerosol optical properties. The GCM used for this study is the ECHAM6, with the multimodal aerosol model HAM, with a horizontal resolution of 1.8x1.8 degrees. First, the author demonstrates the model's ability to simulate column-integrated aerosol optical properties such as aerosol optical depth (AOD) and aerosol index (AI). Then, he correlates between simulated AOD and AI with simulated CCN concentration for super-saturation level of 0.2% ($CCN_{0.2\%}$), showing reasonable matching to past research that was based on satellite datasets.

The heart of this study includes statistical analysis various simulated aerosol optical parameters, examining the correlation quality between aerosol "retrievals" and CCN concentration. Additionally, a new aerosol optical parameter is presented here, namely the "extinction aerosol index", which better correlates with global CCN concentrations.

Finally, given the simulated correlations between aerosol optical properties and CCN concentrations, the author estimates that only 25% of the CCN variability observed by current space-borne instruments may be explained by variability in retrieved AOD.

Although the objective of this study is in the most important area of interest for the research of aerosol-cloud interaction, which is essential for measuring and understanding the climate system, I believe that such complex theoretical work that evaluates or implemented on observations, should be well validated and clearly presented.

Therefore, I recommend this paper to be considered for publication in *Atmospheric Chemistry and Physics*, only after the author's response to my comments below, in hope that my comments could help the author to improve his paper.

Specific comments

1. Title: *“Limitations of passive satellite remote sensing to constrain global cloud condensation nuclei”*.
   This paper presents a theoretical study whose results suggest certain uncertainty in satellite data interpretation, assuming the numerical simulation well represents the Earth System. For the sake of accuracy, I recommend leaving titles in that spirit to studies based on observed data, technical instrumentation limitations etc.

2. P. 32611 lines 26-29: *"Therefore, use of this model allows to consistently assess the relationship between aerosol radiative properties and CCN as biases in the simulated fields are expected to be consistent"*.
   Please elaborate on the reasons for expected consistent biases in the simulated fields. What perturbations or errors are experienced in such simulation?

3. P. 32612 lines 1-2: *"Nonetheless, it should be noted that the ability of models to mimic the spatial (in particular vertical) and temporal (co-)variability of aerosol and humidity fields introduces some uncertainty"*.
   Please give the reader some quantitative sense of the model uncertainties, in respect to CCN and aerosol optical depth, as required when comparing to other datasets.

4. P. 32615 lines 9-13: *"We further investigate the role of the vertical aerosol distribution using the local (model layer) aerosol extinction coefficient (AEC) as well as the extinction aerosol index (AI_{AEC}), defined here as local aerosol extinction coefficient times the local Ångström parameter"*.
   Please add more details regarding the “extinction aerosol index”, which is presented for the first time. What’s the nature of this metric and in what units (e.g. is it normalized by mass or not)? Besides the better correlation we see later in the paper – what is the physical logic behind the choice of that product?

5. P. 32616 line 5: *"The ECHAM-HAM simulated annual-mean surface CCN concentrations (Fig. 1) show distinct land–sea contrast, with maxima over the main aerosol source areas"*.
   The colour scale of Fig. 1 does not ease the “distinct” observation of land-continent contrast. Please modify the colour scale (i.e by using logarithmic scale to focus on variance in low concentrations), or alternatively add calculated values, indicating that contrast.

6. P. 32617 lines 13-16: *"Note that maps of global correlations for alternative aerosol radiative properties proposed as superior proxies of CCN (Fig. 7), specifically (a) fine mode aerosol optical depth, (b) dry aerosol optical depth and (c) aerosol index do not show significantly improved correlations."*.
   In spite claim (c), it seems the Fig. 7(c) has the best correlation in the panel. Global regional mean correlation values of those maps (over continents) would better make the point.

7. P. 32618 lines 15-20: *"This is likely due to the fact that not only aerosol water uptake but also aerosol removal via scavenging is positively correlated to relative humidity (via clouds and precipitation). This hypothesis is supported by the drop off of this correlation around and below cloud base (green line). However, correlations of column integrated AOD and surface CCN are consistently high for this region as well as for the northern high-latitude oceans."*.
   Having ECHAM6 fully running, it should be simple to add precipitation maps (or values) and easily support this hypothesis. Such comparison would also strengthen the reliability of ECHAM6 model for this study.

8. P. 32619 lines 7-11: *"Note that also correlations between surface layer CCN and AIAEC deteriorate for higher supersaturations (sampling smaller particles of the aerosol size distribution), as expected from Mie theory, as the smaller particles contribute less to total extinction (Fig. 10). This is particularly evident over the*

continents with significant primary fine mode aerosol emissions.".
This statement is inaccurate. In many cases, as expected from Mie theory, particle populations of smaller sizes contribute more to total extinction. Please see Fig. 1 below for example, showing simulated extinction coefficients for black carbon aerosol as a function of the population's mass concentration and mean radius, simulated using SHDOM (Evans, 1998). For this calculation, aerosol size distribution was log-normal (σ=0.7), refractive index of 1.87-0.71i and density of 1.8 g/cm^3, at wavelength of 550nm.

9. P. 32619 line 18: *"This study overcomes this limitation..."*.
I still find it hard to understand how a climate model could "overcome" instrument sampling and retrieval limitations. If the author means it overcomes difficulties in interpreting satellite data, it should be demonstrated and generalized to more than a yearlong simulation, and proven to be robust to variation in all related parameters in the model (e.g. relative humidity, precipitation, sea surface temperature etc.). Otherwise, the boundaries of this statement should be clarified.

10. P. 32620 line 10: *"... and aerosol index do not show significant improvements."*.
As mentioned above in comment 6, to the naked eye it seems that AI shows the best correlation in that panel.

11. P. 32620 lines 16-18: *"...Satellite retrievals based on visible wavelengths are most sensitive to larger particles..."*.
Please see comment 8 above and Fig 1 below. Satellites may be more sensitive to smaller particles in many cases. Especially when aerosol mean radii are below 0.2 micron (which is typical for various combustion by-products).

12. P. 32620 line 27: *"... it should be noted that this approach is free from retrieval errors..."*.
For supporting this claim, I suggest expanding the description of the model's input

emission maps (e.g. AEROCOM), to clarify they are "free from retrieval errors" as well.

13. Figure 3:
There is a notable 'crossed out' region over India and the Indian Ocean. Please mention in the figure caption and as well in the article itself whether this region was neglected in any analysis and why. Also, I suspect that extensive desert dust loads in that area may impact the simulated correlations between CCN and aerosol optical parameters.

Technical corrections

1. P. 32617 line 2: Fig 4d does not exist. Please correct.

References Evans, K. F.: The Spherical Harmonics Discrete Ordinate Method for Three-Dimensional Atmospheric Radiative Transfer, J. Atmos. Sci., 35, 429–446, 1998.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 32607, 2015.

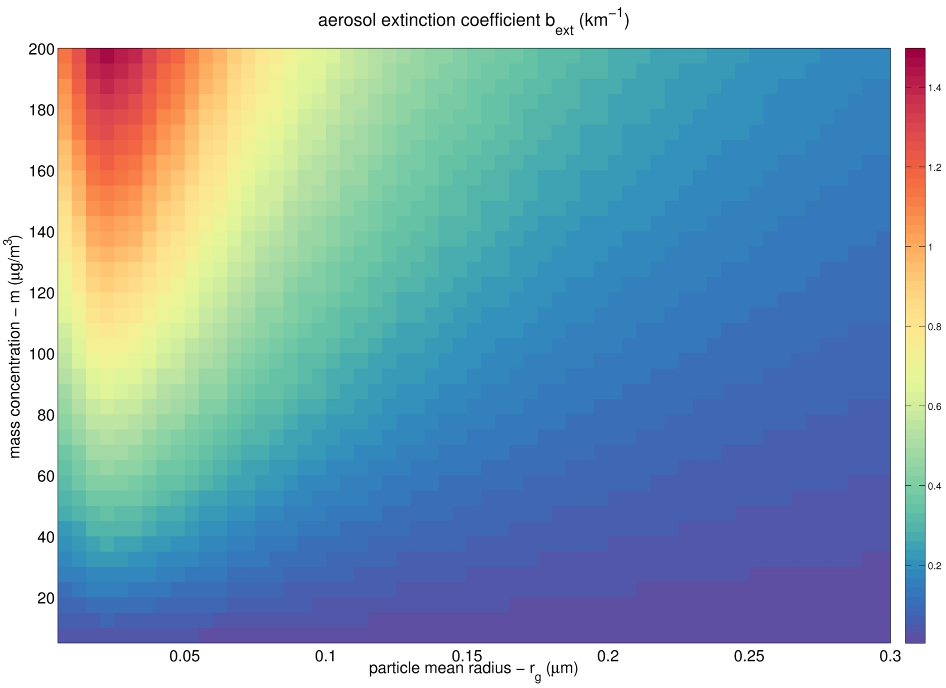

Fig. 1. Simulated extinction coefficient, as a function of particle mean radius, and mass concentration, for log-normal distribution (σ=0.7) of black carbon aerosol (ref. ind. 1.87-0.71*i, density=1.8 g/cmˆ3)