Atmos. Chem. Phys. Discuss., 15, C11096–C11098, 2016
www.atmos-chem-phys-discuss.net/15/C11096/2016/

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 #2

Received and published: 4 January 2016

General comments: This paper deals with aerosol-cloud interactions, which is a challenging research area, not least because of the lack of reliable observational constraints. The study presented is a valuable contribution to this research field, because it challenges the common use of AOD as a proxy for CCN concentrations and instead proposes to use vertically resolved aerosol information from LIDARs as a proxy. The study uses a global aerosol-climate model ECHAM-HAM, which the author describes as "fully self-consistent", meaning that "the calculations of the aerosol radiative properties (based on Mie theory) and CCN (based on Köhler theory) are fully consistent in terms of the size-distribution, composition and mixing state".

It is an interesting and potentially quite important study. However, the following items need to be addressed before the paper can be recommended for publication in ACP:

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(1) A key issue for the credibility of the results is the methodology. The author relies on a model (ECHAM-HAM) that is referred to as "self-consistent". It needs to be clarified to what extent this model is unique, i.e. it would seem that there are several other models out there with the same capability. Secondly, given the model's coarse spatial resolution (1.8 degrees horizontally), meaning that relative humidity distributions and vertical velocities are not resolved, the term "fully self-consistent" sounds rather excessive. I suggest "self-consistent" instead of "fully self-consistent". (2) The figures need to be improved, in particular: a) In Figure 1, the color scale must be changed to better highlight the signals. As it is now, both panels look almost universally blue, with little information to the reader. b) The panels in Figure 6 are too small, so it's almost impossible for the reader to extract any information out of them. c) The panels in Figures 4 and 5 are too small. It is imperative that the reader can easily read the labels (e.g. "South America r=0.50", etc.), but currently this is very difficult. (3) The logical thread of the paper could be improved. As it is now, the reader quite early on becomes convinced that AOD is an inadequate proxy for CCN at the surface. Yet, one has to wait until top of page 32619 and Figures 9-10 before a good alternative is proposed. And, that part of the paper – i.e. lines 1-11 on page 32619 – is very brief compared to the more lengthy discussion of the less successful attempts described on pages 32617-32618.

In addition, there are several minor issues listed below which the author needs to pay attention to.

Specific comments: (a) Page 32609, line 16: "and humidity" is redundant and should be removed, because the discussion is "at fixed supersaturation" (b) Page 32609, lines 16-17: An equation needs to be provided for the claim that "CCN concentrations at fixed supersaturation are linearly related to aerosol light extinction". (c) Page 32610, line 14: "Not clear what "also" refers to. (d) Page 32610, lines 24-28: Past tense should be used when referring to the Liu and Li (2014) study. (e) Page 32611, lines 27-28: To say that the biases are "consistent" sounds strange. How about replacing

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"to be consistent" by "affect the two of them similarly" or something like that? (f) Page 32613, line 22: Insert "by" before "Kazil" (g) Page 32614, line 2: "empirical estimation" is rather cryptic. Can you provide some insight into the physics involved? (h) Page 32614, lines 3-4: Recently, a significant sensitivity to the activation scheme has been found in several studies, e.g. Gantt et al. (2014, ACP). How might the results of this study be affected by the choice of activation scheme? (i) Page 32614, line 14: "wetable" should be 'wettable' (j) Page 32616, lines 11-12: It sounds strange that Saharan dust isn't explicitly mentioned here (as an example of aerosols downstream of source regions), because it is the most striking feature in the figure. (k) Page 32617, lines 15-16: How do you define "significantly improved"? Clearly, Figure 7a shows some improvement. (l) Page 32618, line 12: Something wrong with "particularly than over". Please rephrase. (m) Page 32619, line 5: "significantly improved", compared to what? (n) Page 32619, line 6: "surface extinction aerosol index" needs to be defined. (o) Page 32619, line 9: "as the smaller particles contribute less to total extinction" is not a full explanation. Something is missing. (p) Page 32619, line 22: How large is "large" and how long is "long"? (q) Page 32620, line 2: A verb is missing. I suggest resolving that by replacing "an analysis" by "according to our analysis". (r) Page 32620, line 2: "local (grid)": Need to remind the reader here what the model resolution actually is, i.e. we are not dealing with a cloud-resolving model. (s) Page 32620, lines 6-7: "This suggests particularly limited constraints" is cryptic. Please rephrase. (t) Page 32621, lines 12-15: The parentheses should be removed, because this is highly relevant information. (u) Page 32628, Figure 1: The caption must explicitly state that the figures are from simulations with ECHAM-HAM.

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