Atmos. Chem. Phys. Discuss., 15, C13610–C13617, 2016
www.atmos-chem-phys-discuss.net/15/C13610/2016/

Atmospheric Chemistry and Physics Discussions

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

P. Stier

philip.stier@physics.ox.ac.uk

Received and published: 2 May 2016

I would like to thank the reviewer for the helpful comments that substantially improved the manuscript. I very much appreciated the detailed remarks and hope to have addressed all raised issues.

General comments

(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".

There exist of course a number of (global) aerosol models (c.f. Myhre et al., 2013). A smaller subset of these models deals explicitly with aerosol microphysics (c.f. Mann et al., 2014). Of these models, only few explicitly diagnose CCN at various supersaturations from the prognostic size-distribution and composition and only few calculate aerosol radiative properties via Mie theory from the same prognostic size-distribution and composition. So while it is nowhere claimed that this model is unique, there exist only few models with comparable self-consistent diagnostics. To overcome this unsatisfactory situation I have proposed explicit CCN as diagnostic for future experiments of the AeroCom intercomparison project.

The proposed distinction of between "fully self-consistent" and "self-consistent" seems fairly arbitrary. The fact that some processes cannot be explicitly resolved in global models means that global models (as models in general) are not perfect but that does not change the definition of self-consistency introduced in the paper as quoted above.

In any case, I have removed the "fully" in the usage of "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.

Figure 1: the main purpose of Fig. 1 and Fig. 2 is to highlight the difference in the geographical spread of CCN at different supersaturations vs. the spread of aerosol radiative properties. The colourbar has been designed to be constant across the different

properties. None of the (many) colourbars I tried is perfect but the one in the revised manuscript is probably a bit better than the original one.

Figure 4,5,6: I agree that the figures appear too small in the ACPD layout (which is a common problem with ACPD) and will liaise with the production team to ensure good reproduction in the final ACP format.

(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.

As indicated by the title, the focus of this study is to highlight limitations in commonly used proxies for cloud condensation nuclei, which is the underlying logic of this order.

I have slightly extended on the discussion of the correlations with aerosol extinction coefficients and extinction aerosol index in the revised manuscript.

Specific comments

(a) Page 32609, line 16: "and humidity" is redundant and should be removed, because the discussion is "at fixed supersaturation"

The idea is that if humidity as well as size, shape and composition are constant also the water uptake is constrained, so that aerosol extinction (at this humidity) is linear in CCN concentration.

(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".

The full statement reads "Assuming identical size, shape, composition and humidity,

CCN concentrations at fixed supersaturation are linearly related to aerosol light extinction, so that AOD, the column integrated aerosol extinction, could be expected to provide a first order proxy for CCN. "

For aerosols with identical size, shape and composition the CCN concentration is well defined for each supersaturation. The additional constraint of constant humidity determines the water uptake per particle, constraining the composition and the wet radius of the particles. The aerosol extinction is simply the sum of the extinction of each aerosol particle at ambient radius and composition and therefore linear in the number of CCN.

(c) Page 32610, line 14: "Not clear what "also" refers to.

"Also" here refers to AI vs. the prior discussion of AOD and extinction.

(d) Page 32610, lines 24-28: Past tense should be used when referring to the Liu and Li (2014) study.

Good point. I have cleaned up the use of tenses in the introduction.

(e) Page 32611, lines 27-28: To say that the biases are "consistent" sounds strange. How about replacing "to be consistent" by "affect the two of them similarly" or something like that?

Replaced by "are expected to affect both parameters similarly".

(f) Page 32613, line 22: Insert "by" before "Kazil"

Changed to "... by Kazil et al. (2010)"

(g) Page 32614, line 2: "empirical estimation" is rather cryptic. Can you provide some insight into the physics involved?

Activation schemes are generally based on approximations of the supersaturation balance equation in which the updrafts provide the source term for supersaturation and the condensation on the growing droplet spectrum the sink term. No analytical solutions exist for this equation so the widely used Abdul-Razzak & Ghan (2000) scheme is based on a fit of parcel model simulations. I would refer the interested reader to the literature cited in the manuscript.

(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?

Aerosol activation schemes estimate the maximum supersaturation for a given updraft velocity and particle distribution. As our results focus on CCN at fixed supersaturations the results are not directly dependent on the choice of the aerosol activation scheme. However, it should be noted that adsorption activation on insoluble aerosol would increase CCN for the insoluble HAM modes, which currently do not contribute to CCN in HAM (as outlined in the model description).

(i) Page 32614, line 14: "wet- able" should be 'wettable'

Corrected.

(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.

Good point. I have added "such as the Saharan dust outflow" to this sentence.

(k) Page 32617, lines 15-16: How do you define "significantly improved"? Clearly, Figure 7a shows some improvement.

Good point. See response to reviewer 1. I have now included global mean correlation coefficients in the title of each figure and discuss its variation quantitatively.

(l) Page 32618, line 12: Something wrong with "particularly than over". Please rephrase.

Rephrased to "particularly higher than over South America"

(m) Page 32619, line 5: "significantly improved", compared to what?

This directly links to the previous sentence "These results suggest that vertically integrated aerosol radiative properties, as retrieved from satellite imagers, are of limited suitability as proxy for global surface or cloud base CCN"

(n) Page 32619, line 6: "surface extinction aerosol index" needs to be defined.

Agreed. This is now properly defined in the introduction.

"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: AI$_{AEC}$=AEC$\times\alpha_{AEC}$, where $\alpha_{AEC} = -\frac{\ln(AEC_{550nm}/AEC_{865nm})}{\ln(\lambda_{550nm}/\lambda_{865nm})}$ is evaluated from the local aerosol extinction coefficients, instead of from the column integrated aerosol optical depths used in AI. "

(o) Page 32619, line 9: "as the smaller particles contribute less to total extinction" is not a full explanation. Something is missing.

I am not entirely sure what is meant by this comment but have clarified this statement as follows:

"as expected from Mie theory, as the smaller particles selected by higher supersaturations contribute less to total extinction"

(p) Page 32619, line 22: How large is "large" and how long is "long"?

Clarified to " large (continental) spatial scales and long (monthly) averaging periods"

(q) Page 32620, line 2: A verb is missing. I suggest resolving that by replacing "an analysis" by "according to our analysis".

Corrected to "according to this analysis, the temporal correlation..."

(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.

Which would not be strictly local either... Clarified to "local (global-model grid) scale".

(s) Page 32620, lines 6-7: "This suggests particularly limited constraints" is cryptic. Please rephrase.

Changed to "This suggests that constraints from passive satellite remote sensing are particularly limited in areas key for radiative forcing due to aerosol–cloud interactions."

(t) Page 32621, lines 12- 15: The parentheses should be removed, because this is highly relevant information.

I do not believe that the parentheses make this statement less relevant.

(u) Page 32628, Figure 1: The caption must explicitly state that the figures are from simulations with ECHAM-HAM.

Good point. I have updated all captions accordingly.

Bibliography

Mann, G. W., K. S. Carslaw, C. L. Reddington, K. J. Pringle, M. Schulz, A. Asmi, D. V. Spracklen, D. A. Ridley, M. T. Woodhouse, L. A. Lee, K. Zhang, S. J. Ghan, R. C. Easter, X. Liu, P. Stier, Y. H. Lee, P. J. Adams, H. Tost, J. Lelieveld, S. E. Bauer, K. Tsigaridis, T. P. C. van Noije, A. Strunk, E. Vignati, N. Bellouin, M. Dalvi, C. E. Johnson, T. Bergman, H. Kokkola, K. von Salzen, F. Yu, G. Luo, A. Petzold, J. Heintzenberg, A. Clarke, A. Ogren, J. Gras, U. Baltensperger, U. Kaminski, S. G. Jennings, C. D. O'Dowd, R. M. Harrison, D. C. S. Beddows, M. Kulmala, Y. Viisanen, V. Ulevicius, N. Mihalopoulos, V. Zdimal, M. Fiebig, H. C. Hansson, E. Swietlicki, and J. S. Henzing (2014), Intercomparison and evaluation of global aerosol microphysical properties among AeroCom models of a range of complexity, Atmos. Chem. Phys., 14(9), 4679-4713 doi: 10.5194/acp-14-4679-2014.

Myhre, G., B. H. Samset, M. Schulz, Y. Balkanski, S. Bauer, T. K. Berntsen, H. Bian,

N. Bellouin, M. Chin, T. Diehl, R. C. Easter, J. Feichter, S. J. Ghan, D. Hauglustaine, T. Iversen, S. Kinne, A. Kirkevag, J. F. Lamarque, G. Lin, X. Liu, M. T. Lund, G. Luo, X. Ma, T. van Noije, J. E. Penner, P. J. Rasch, A. Ruiz, O. Seland, R. B. Skeie, P. Stier, T. Takemura, K. Tsigaridis, P. Wang, Z. Wang, L. Xu, H. Yu, F. Yu, J. H. Yoon, K. Zhang, H. Zhang, and C. Zhou (2013), Radiative forcing of the direct aerosol effect from AeroCom Phase II simulations, Atmos. Chem. Phys., 13(4), 1853-1877 doi: Doi 10.5194/Acp-13-1853-2013.

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