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Atmospheric Chemistry and Physics Discussions

Interactive comment on "How does the UKESM1 climate model produce its cloud-aerosol forcing in the North Atlantic?" *by* Daniel P. Grosvenor and Kenneth S. Carslaw

Anonymous Referee #1

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In this study, the authors discuss three related analyses: (1) quantification of biases in cloud properties and radiative fluxes simulated by the UKESM1 climate model in the North Atlantic against diverse observations, with good performance overall but regional-dependent biases; (2) decomposition of aerosol effective radiative forcing into microphysical response in cloud droplet number and rapid adjustments in liquid water path and cloud fraction, finding that adjustments contribute strongly albeit again in regional way; (3) identification of cloud types and cloud regime transition that contribute to forcing, finding that forcing is mostly exerted in regions of low clouds with no regime transition.

This is a good paper, which covers a lot of material. The analysis of biases is very careful and informative. I also commend the authors for clearly discussing the impact of biases on aerosol forcing in the conclusion section 4.1 – this is often not done but is important. The authors make an interesting use of offline radiative transfer calculations to quantify contributions of adjustments and give an interesting analysis of cloud regime transitions.

The paper is long, but there are no clear candidates for shortening. The conclusion repeats many points made in the body of the paper but is well structured and many readers will only read that anyway. The many figures illustrate the discussion well. The appendices provide useful information.

I have three major comments, regarding the method used to isolate the impact of aerosols on autoconversion; the impact of temporal variability on the conclusions; and improving the discussion of links between model biases and aerosol forcing. Because answering those comments may involve additional analyses, I recommend major revisions. I also recommend clarifying the discussion in places.

1 Main comments

- According to Appendix D, the impact of aerosols on autoconversion rates is isolated by ignoring modelled cloud droplet numbers and using fixed values instead. Doing so measures the impact of switching to a different set of cloud droplet numbers but does not isolate the impact of aerosols on autoconversion. Indeed, the reason why the prescribed numbers differ over land and ocean is to crudely represent the more polluted conditions over land. The easy option would be to take the prescribed values from the land/ocean averages of the PI simulation. A much more accurate configuration would be to use the distribution of cloud droplet number simulated in the PI simulation.

- Clouds are a very variable component of the atmosphere, as evidenced by the noisy aspect of many figures, so I was surprised that the authors base their analysis on a single, 1-year simulation. There is year-to-year variability even in stratocumulus regions, and as acknowledged by the authors nudging will not suppress that variability, which is a good thing if one wants to isolate adjustments. So readers need to be told which results can be safely generalised. This is especially true of the decomposition of aerosol forcing and the analysis of aerosol-driven cloud regime transitions. I acknowledge that extending the simulated period represents a lot of work, so a discussion of relevant literature in the conclusion section may be a good alternative, although I could not identify specific papers. Perhaps some AeroCom papers looked at interannual variability in aci?

- As I said above I very much like that the authors try to evaluate (or at least speculate) the potential impact of model biases on simulated aerosol forcing. However, that discussion could be more complete. Could biases in f_c affect adjustments in that variable – for example allowing f_c adjustments in sky that should be overcast, or vice-versa? Could that be significant? Also, ari is easily masked by even moderately thick clouds, so biases in f_c or LWP could translate in the wrong masking of ari. Is that important? A similar comment could be made about high-cloud biases, as those biases would affect the amount of aerosol forcing that is masked by clouds above. Finally, the authors dutifully restrict their analysis to ocean regions, but land-based biases (which seem much larger than over ocean according to section 3.1.1 figures) likely matter for aerosol transport to ocean regions and their biases.

2 Other comments

- Line 5: Caution is of course also needed when interpreting high-resolution, process-resolving models!

- Line 14: "further large increases in f_c" implies that aerosols can only increase, and not decrease, cloud fraction, at least on average over the regions studied. This is true in the model used in the present study, but not in the real world, so I would suggest rephrasing here.

- Lines 28-29: Why the sudden focus on the north of Scandinavia?

- Line 75: HadGEM2-ES was a CMIP5 model, wasn't it?

- Line 84: Could give the resolution here, as "coarse resolution" for a given model may be medium or even high resolution for another.

- Line 113, lines 148-150 and lines 164-165: The paper should say early (and in the abstract) that it only considers aci with a subset of liquid (not ice) clouds, but determining what that subset is seems complicated by the distinction between convective and large-scale clouds. Does the model carry two sets of cloud variables (especially water content and cloud fraction)? Are those two sets considered separately for cloud fraction and radiative purposes? Or is f_c based on both types of clouds? Would it then follow that aerosols only affect an unknown fraction of the cloud field? What would that mean for linking cloud biases to forcing?

- Lines 201-205: Should say here that the analysis of biases is limited to ocean surfaces.

- Line 220: To evaluate whether aerosol forcing contributes to biases, one could probably identify regions where aerosol impacts on f_c go in the same direction of the bias. Or you could repeat your bias analysis with the PI simulations. If it looks better against observations than PI, then aerosols must be to blame.

- Lines 238-240: This is an important observation if one wants to link the present paper to Booth et al. (2012). Because of the definition of ERF, one needs to assume that forcing decomposition and cloud regime transitions are not affected by the coupling with the ocean.
- Line 269: Clear regions do not really contribute to LWP. I suggest rephrasing.
- Line 409: "usually larger" – should be "smaller" I think.
- Lines 424-429: It would be useful to clarify here that the hypothesis is reasonable because in the model aerosols only affect autoconversion rates. Alternative mechanisms that could potentially decrease liquid water content and/or cloud fraction, for example easier evaporation of smaller droplets or changes in above-cloud air entrainment, are not represented in the model.

3 Technical comments

- Line 332: "exclude" rather than "prevent"?
- Figures 9 and 10: It would help to apply the same colour scale for panels (c) of both figures.
- Caption of Figure 17 could note that category 1 is clear sky.

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