

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2022-583

Anonymous Referee #2

Referee comment on "Change from aerosol-driven to cloud-feedback-driven trend in short-wave radiative flux over the North Atlantic" by Daniel P. Grosvenor and Kenneth S. Carslaw, Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-583-RC2>, 2022

The authors present a very thorough investigation into the behaviour of shortwave radiation fluxes above the North Atlantic, over the historical era, in two versions of the UK climate model (UKESM, HadGEM3). They find two regimes with markedly different behaviour; before and after 1970; and attribute them (primarily) to an increase in aerosol concentrations and cloud responses to surface temperature change, respectively.

Overall, this is an impressively detailed study, with well described reasoning and broad ranging but established methods. It reads almost like a textbook at times, taking the reader through all main factors thought to be able to influence F_{SW} and disentangling their various influences. The analysis and the manuscript are clearly very well worked through, and I therefore have very little to offer in terms of deeper feedback. This paper could well be published as-is, and should certainly not require more than a minimal revision.

Some minor questions and comments:

* If I have one concern with the paper, it is that it is very long and at times quite wordy. There is a risk that the nice and highly instructive results get lost because the community doesn't have time to read through it all. Therefore: Would it be an idea to include a process level schematic of the factors contributing before and after 1970? I.e. an annotated version of Figure 1a, with some arrows and icons, to show what is changing and why?

* Unless I'm missing it, I don't think you discuss the temperature feedback on sea salt aerosols as a potential contributor to F_{SW} trends? This effect should be there for the North Atlantic, at least for the post-1970 period, somewhat counteracting the reduction in anthropogenic CCN. (You have the DAMIP natural forcer experiments, and show that it can be ignored in this context, but that simulation will not have the natural aerosol feedbacks.)

* In section 2.3, I can't quite see that you've quantified the impact of excluding grid

boxes with sea ice formation. Presumably the effect is small, but could it introduce some biases? (Domination of southern grid boxes, or spurious seasonality?)

- * Figure 1, and others: Would it be worth also showing NA AOD? Simply because there are so many other studies that use AOD, and therefore it becomes easier to compare your results to theirs?

- * Figures 4, 7, 8, ...: In many of these, one dot is the net of others. Could this be highlighted more clearly, with colors, symbols or similar? (You do this for aerosols in places, but I still struggled a bit to understand how all the factors summed up - or not - in the various figures.)

- * Your convention is that F_{SW} is upwelling; I got this after reading a bit, but I don't think you explicitly define it? Maybe have it already in the abstract, line 5? ("positive upwelling F_{SW} trend"?)

- * The last reference (Zhou et al. 2016) comes twice in the ref.list.

Thanks for a very interesting paper.