Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-850-RC1, 2019

Atmospheric Chemistry and Physics
Discussions

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Interactive comment on "Detection and attribution of aerosol-cloud interactions in large-domain large-eddy simulations with ICON" *by* Montserrat Costa-Surós et al.

Anonymous Referee #1

Received and published: 21 October 2019

General comments: The authors present analysis of new very high resolution simulations over the EU domain for one day near peak emissions in 1985 and one day in the present decade with relatively low emissions. The authors carefully analyze the high-resolution simulations using satellite and ground-based data. They find that AOD differences, and Nd differences between 1985 and 2013 are reproduced. Changes in cloud macrophysics are too small relative to natural variability to observe. The authors derive an ERFaci for the global mean using scaling with traditional GCMs.

The paper is a very nice analysis of cutting-edge new simulations and provides an interesting new evaluation of ERFaci. I have the following major issues with the paper:

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Discussion paper

A lot of the paper is given over to ground-based remote sensing. This is fine, but it is not a field that I am very familiar with and I recommend that a reviewer who is an expert be nominated to comment on this. However, I am concerned by the characterization of standard deviation as uncertainty in comparing observations and models (as discussed in specific comments) and I think this needs to be explained more clearly.

I am not sure that the authors have made a meaningful comment about the adjustment strength, besides the fact that adjustments are small compared to meteorological variability and are hard to see in one day of data- which doesn't preclude them being important to ERFaci.

Critically, I think the scaling to the global ERFaci could be done better (see specific comments below) by expanding the number of GCMs and by showing that the relationship is linear.

Specific comments: Pg1 Ln11: I kind of follow what the authors are trying to say here, but it is a little easy to lose track. I would suggest not using reference and perturbed to refer to 2013 and 1985 in the abstract. It will be easier to follow which conditions are consistent and inconsistent. Effectively it sounds like the model needs the appropriate year of aerosol data to get the right output, which could be said more succinctly.

Pg2 Ln6: What is large LWP? Is it in or area-mean LWP? I am not sure what I should really be taking away from this result. Is it really a key result that needs to be shown in the abstract?

Pg2 Ln16: The results in Rosenfeld 2019 are no longer accurate. There is an errata that revokes most of the findings of the original paper.

Pg3 Ln9 “to what extent”

Pg3 Ln17: Split this into two sentences. CCN is changed and INP is not. Direct effects are not considered. This is really confusing. How is AOD being evaluated if the direct effect isn't considered?

Pg7 Ln17: Please discuss Song et al 2018 (https://www.geosci-model-dev.net/11/3147/2018/gmd-11-3147-2018.html) in the context of using COSP for ICON at this resolution. What is used to drive COSP in this case? Is there subgrid variability assumed?

Pg9 Ln9: Typo- sentence needs to be reordered. Maybe "AOD is only available over the North Sea region for xx% of retrievals." To reduce ambiguity.

Pg9 Ln17: The authors show a systematic difference in the mean CCN profiles from observations and the CCN used to drive the model. I think the authors are somewhat misusing the uncertainty range. Don't you want uncertainty in the mean, not just the variability, which is what this shows? Shouldn't these be standard error in the mean? Ultimately it seems like there is a 10-30% overestimate in CCN relative to the observations (I assume the standard error in the mean is small). Can the authors convert that to an overestimate in Nd using the nucleation scheme, which is the more relevant quantity in this study?

Pg 12 Ln8: I am not sure that 10% change in LWP is small (am I reading table 3 right?). It's certainly true that variability in LWP due to meteorological variability is large, but this doesn't really tell us anything about the radiative forcing induced by adjustments.

Pg12 Ln11 Is the cutoff for large LWP? Does this just mean not thin clouds?

Pg12 Ln18: How many more days of simulation would you need to beat down the noise and be able to see the LWP perturbation clearly?

Pg 20 Ln29 I think the authors are just calculating the ERF over Europe versus the global mean and coming up with a scaling factor. I think a better approach would be to plot ERF_EU_1985-ERF_EU_2013 versus ERF_global_mean_PD for each CMIP5 model. The way that the authors are doing this assumes linearity in this relationship, which is not necessarily true since the EU in 1985 is so polluted. Based on Carslaw et al. (2013), I am not sure that this calculation should really reduce uncertainty much, but Carslaw et al. (2013) paper implies strong non-linearity in the relationship between local ERF and global-mean ERF. If the authors could increase the number of GCMs beyond 4 and show that the relationship is linear this would be a more robust calculation. How do the authors deal with the direct effect not being calculated in the simulations for this comparison since it will be in the GCMs (Pg3 Ln17)?

Carslaw, K. S., Lee, L. A., Reddington, C. L., Pringle, K. J., Rap, A., Forster, P. M., . . . Pierce, J. R. (2013). Large contribution of natural aerosols to uncertainty in indirect forcing. Nature, 503(7474), 67-71. doi:10.1038/nature12674

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