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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The aerosol-cyclone indirect effect in observations and high-resolution simulations" *by* Daniel T. McCoy et al.

Anonymous Referee #1

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Review of "The aerosol-cyclone indirect effect in observations and highresolution simulations" by McCoy et al.

This manuscript analyzes idealized simulations and observations of aerosol-cloud interactions mid-latitude cyclones. The paper is timely and of a suitable topic for Atmospheric Chemistry and Physics. I like the subject and think it would be valuable for this research to be published in some form, and think the results are quite interesting.

However, I had difficulty with this manuscript. It seems in the end like 3 half finished papers in one. The first part is 'idealized' (aquaplanet) simulations that are not sufficiently evaluated to be able to understand what can be learned. The second part is a set of observations are drawn from several sources, and probably need a bit more discussion. This is especially true because the 'observations' include essentially diagnostic

aerosol fields from a reanalysis system that does not have aerosol-cloud interactions and are used as a very coarse proxy (without explanation really) for aerosols. Finally there are separate simulations (with a different model I believe) of a recent N. Atlantic volcano, the do not seem to have very robust statistics.

This paper needs at least major revisions. Each of the pieces is quite interesting, but they are not well treated in this work, and I think deserve a more careful analysis. It really feels like these three parts were put together from separate projects, and it makes the whole incoherent. I would urge it be broken into more complete pieces.

For example, the observational analysis is very interesting but needs further development. It challenges previous work, but using different data sets and a different sampling method. Similar cyclone sampling with other datasets or means of these data sets without sampling would be very valuable for sorting out whether the different effect (little change in liquid water path with an aerosol proxy) is due to sampling or data. In addition, the blending of MERRA and observations is interesting but problematic.

Secondly, the model simulations are confusing and not fully evaluated. As noted below, I question 'convective permitting' simulations as being in a really bad part of the 'gray zone'.

Finally, the volcano work needs more sensitivity tests and does not seem robust. Introducing a totally different model to this work is a bit complicated and it's not clear what can be learned from the langrangian dispersion model.

The result is that many of the conclusions are not supported by the analysis, and previous results that contradict these conclusions are only mentioned in the introduction and not analyzed. This is not appropriate.

Specific comments are below, but I strongly suggest this be separated be developed into 3 papers. There is a lot of interesting material on the simulations and observations, but they need to be treated properly.

Page 1, L14: surrogate climate model? What is that? This might need a bit more description in the abstract.

Page 1, L30: it might be useful here to separate out the first and second effects in these studies for clarity.

Page 2, L14: I think you need to reference or show some model validation for the cyclone composites. Maybe it is later but it should be noted here.

Page 2, L25: does MERRA2 assimilate AMSRE or MODIS? If so, then these are co dependent.

Also, the statement here about CDNC from a sulfate regression I think means you are baking in an indirect effect.

Page 3, L20: so these simulations prescribe an indirect effect. How was this tuned?

Page 3, L28: how do these emissions compare to Malavelle et al 2017 and Gettelman et al 2015?

Page 4, L8, but also without variable aerosol sources from land that might co vary with meteorology.

Page 4, L9: This is not resolving, it may be barely even permitting. Most mesoscale meteorologists I know would not run a model in this Gray zone between 3km and 10-15km, the latter with a convection scheme, and usually 3km even with some sort of vertically coherent parameterized turbulence.

I hope there is validation of this model somewhere? How about some cyclone composite maps to compare to observations, not just WCB flux.

Page 4, L20: The ice assumption is simplistic. Is there significant ice in your cyclones? It Depends on the temperature in your aquaplanet run.

Page 4, L30: To what extent might that be a consequence of the model formulation.

Perhaps this figure needs to be in the main text?

Page 5, L2: Again, the frozen water path is not aerosol aware. If the LWP but not the IWP changes, then what does this say about how the IWP is formed in the model? This should be assessed by looking at microphysical process rates.

Page 5, L8: What does it mean that the slopes are very different, and the mean values for the model at low WCB strength are off by a factor of 2.

Page 5, L14: However, you presume that the convective permitting simulations can capture the real physics of convection at 6.5km. I don't think this is true.

Many studies with high resolution limited area models (1km horizontally and finer, with higher vertical resolution) note competing processes such as enhanced cloud depth that may offset some of the impacts. Can you comment on this?

Bottom line: without showing that your aquaplanet simulations are realistic (and they look pretty weak from the one evaluation in Figure 1), you have oversold this conclusion.

Page 5, L16: you need to explain the statement that climate sensitivity is too low. I understand the point: but this implies that people adjust the climate sensitivity to match the forcing. It may imply that sensitivity is higher in reality if the forcing is higher. It needs to be rephrased for a reader who does not understand the direction you are going in.

Page 6, L3: I understand that you have a published reference for this, but taking so4 from a reanalysis and trying to use that as a CCN proxy with observations is problematic. If MERRA2 assimilates AMSRE, then you have a potential co-variance problem.

Page 6, L9: I think you need to show cyclone composites in the main text.

Page 6, L10: Didn't Field and wood use a 2 d cyclone composite? Check.

Page 6, L13: Clarify what is observations and what is reanalysis here. It might be interesting to also look at the CLWP in MERRA without ACI: if it also changes with CCN then there might be an issue with co variance here.

Page 6, L21: These radiation fluxes need uncertainty estimates.

Page 6, L34: except that the analysis says ice is not important but we ignore ice. So if ice was doing something you would not see it. I think it is a bit dangerous to make this assumption. How much IWP Is in the cyclones?

Page 7, L5: Where do all these values come from? Are you just looking at observations + MERRA here or is there something from the model. Also, does CLWP include a rain rain which is part of the WCB metric? What does that mean?

Page 7, L14: I am still confused about this metric. Maybe a better figure would help.

Page 7, L19: what about co-variation?

Page 8, L6: The regression model needs uncertainties on it if you are going to do this. It is not clear whether the results are significant.

Page 8, L9: Can you describe the 'Lagrangian model' NAME in more detail? What met fields drive NAME? Are these MERRA2 cyclones or observed cyclones. I think this needs a more complete treatment. This seems like the beginning of a different paper.

Page 8, L13: I'm not sure that there is a relationship here, or that the anomalies are statistically significant. Shouldn't the line increase with sulfate (i.e. CCN).

Page 8, L18: doesn't this depend on the uncertainty in the regression model (which I do not think you have described)

Page 8, L20: Malavelle et al 2017 also used 10 years of data for a climatological comparison.

Page 8, L20: how are the convective permitting simulations used here? They were aquaplanet?

Page 8, L25: But the sensitivity of convective permitting simulations is higher than observations I think?

Page 8, L29: I think a more thorough sensitivity test is necessary. Also, please check your emissions against earlier work as noted below.

Page 9, L4: but you largely prescribe these effects in idealized models: fixed CCN, no scavenging and infinite sources, fixed relationships. Of course you are going to find this.

Page 9, L6: you have said nothing about radiative forcing over the 20th century. As before: I see your logic but you need to explain it in several sentences with references.

Page 9, L8: But the Holuhraun simulations seem sensitive to emissions, and I'm not sure your statistics are robust. Again, this conclusion does not seem robust

Page 9, L10: I'm not fully clear what is idealized and what is observations in this study,

Page 9, L15: Vague, and of course there is one in the simuLations. The observational part is interesting but needs a more careful treatment.

Page 9, L17: as noted, I think this statement is not defended by the analysis and ignores a lot of previous literature on the complexities of aerosols in convection. It needs a much more thorough analysis of the simulations.

Page 14, Fig2: Are cyclones composited only over the ocean here?

Page 16, Figure 4 needs some discussion of error and/or error bars: are these lines significantly different?

Page 18, Fig6: the presentation is not that effective. It is hard to read the color scale when you use a 2 color gradient. I'm not entirely clear what this is trying to show.

Page 20, Fig8: is this statistically significant? The most 'polluted' storm is not significant. Take out 3 points and there is nothing here. I do not think this is robust.

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