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**Interactive
comment**

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

Anonymous Referee #3

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The manuscript by McCoy et al. investigates aerosol-cloud interactions in midlatitude cyclones over the North Atlantic using modelling and the Hohluraun eruption. I think as such the topic is interesting, but the uncertainty has to be discussed much better. My recommendation is to name the motivation and discuss major limitations of the different approaches such that the scientific evaluation of the work is easier. I hope my comments will be useful for improving the manuscript.

General comments

I recommend to provide more information/discussion on uncertainty and the motivation of some specific choices in the methods for this work. I understand that one would want to highlight the positive results that seem to provide a conclusive story, but I recommend to more openly discuss the uncertainty in such work. In my opinion, mete-

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orological variability has a large impact on the perceived aerosol-cloud interaction, no matter whether we look at observations or modelling. My suggestion is to clearly highlight it for supporting an open debate and helping the reader in assessing the results. When we look for instance at the Holuhraun case, we have very few cyclones that have been affected by excessive amounts of sulphate, i.e., 10 cyclones in total according to Fig. 8. Half of these cyclones show an increase in CLWP, but that is within the range of CLWP anomalies that also naturally occur in the absence of SO4 perturbations. The other half of the cyclones with above-threshold perturbations in SO4 show, however, almost no change in CLWP and this includes the cyclone with the largest SO4 perturbation. I would state this explicitly in the text.

In addition to meteorological variability, I wonder how the regionally limited increase in aerosol affects the radiation transfer, thus the temperature gradients and possibly the cyclone/WCB statistics, based on which you construct your argument that aerosol-cloud interaction is the driver of CLWP increases. Have you analysed the changes in the temperature distributions? This would be important for understanding the physical mechanisms behind the model results.

Specific comments

The abstract could be a little longer, e.g., it does not state which model and satellite data has been used, and should more clearly state the uncertainty assessments, e.g., uncertainty in assumptions about the eruption.

p.1, l. 20: "liquid water amount and thus the albedo" The cloud albedo depends on the number and size of droplets. I also wonder whether "constraining predictions of the 21st century warming" is a good word choice as the warming will depend not only on the physics, but also on the socio-economic development. As such we will always have a spread in long-term projections into the future. In any case, citing of references would be useful here.

p.1, l. 22: "thermal contrasts" alone are not enough to form a cyclone. It might be best

to just delete that sentence.

p.3, l.7: I am a little bit surprised that both the configurations with and without explicit convection use the same vertical resolution. Maybe you can explain why you have made that choice. Would you expect the results to differ when you also change the vertical resolution?

p.3, l. 14-15: Do you mean that the exponential decay starts at the surface or above 5km? Both seems to be tricky, unless there is observational evidence for it, since aerosol is typically well mixed in the boundary layer, but only few places have a deep BL of 5 km. Maybe use cm-3 instead of /cc to be consistent with your results section.

p.3, l.18: Please clarify "non-interacting". I guess you mean that no complex aerosol parameterisation is coupled to the atmospheric model, but you prescribe the aerosol concentration as function of vertical velocity and let the aerosol interact with the radiation and clouds in the model (such a setup could also be interpreted as "interacting").

p.3, l.19: Is there a reason why you have chosen to increase the aerosol just in this channel? Such a setup generates a steep (artificial) gradient in aerosol that might change your temperature gradients and thereby the cyclones.

Section: 2.2.2: I think if you could add the uncertainty range of these estimates and maybe even systematically test the effect of such a range on your results, the work could be a much better contribution. Later in the results section you touch on that type of uncertainty. Maybe you could motivate it here already.

p.4, The first paragraph is partly redundant with the method section. Maybe you can merge the text.

p.5, l.1-2: Is this due to the simple parameterisation that you have implemented into the model? In either case I would mention it here again, because the way it is currently written suggests that what your model tells us is a fact and that fact "warm rain process" seems to contradict what one would expect for precipitation formation in midlatitude

cyclones (in reality).

p.6, l.8-9, Fig. 2: It is not clear why you get large CDNC in the cyclone center, typically a decline going outward, but than again an increase in CDNC to the southwest. Could you argue that this is something you would expect? Here I would also want to read more about the comparison of the CDNC of MERRA and MODIS to judge the quality of the re-analysis.

p.7, l.26: "enhancement of cyclone properties" I would speak of changes of cyclone properties.

p.8, l.5-6: MERRA assimilates, however, observations that have experienced a potential effect of the aerosol on the meteorology. So, this might not be as conclusive as one would hope.

p.9, l.18-20: These are big implications, but how could we know that we would get the same answer when we used other models or other volcanic eruptions, given the uncertainties and variability?

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