Response to reviewers

We thank the reviewers for their careful reading of our manuscript. Several of the comments by reviewers helped us significantly improve the paper so that is great. In response to their comments we have made several significant changes to our paper.

1. We have replaced the AMSR data set with the MACLWP data set. This reduces uncertainty related to diurnally averaging the dataset. Full details of the improvement in MACLWP data relative to AMSR data are given in Elsaesser et al. (2017).
2. We have expanded the methods section to more completely describe what has been done in the paper.
3. Based on comments from the reviewers we have found that use of the albedo from CERES from all solar zenith angles (SZAs) was introducing a substantial bias due to SZA effects. This effect is discussed in detail in Bender, Engström, Wood, and Charlson (2017) and we have taken steps to show sensitivity to this effect in our analysis.
4. Based on further analysis of the Holuhraun eruption we have decided to remove this case study from our paper for more in-depth evaluation at a future time. Our previous results were based on AMSRE/2 data, which changed overpass time in the transition from AMSRE to AMSR2, spuriously creating differences in cyclone properties within the data record. Use of MACLWP data shows that our previous analysis is less robust once the diurnal cycle is more carefully accounted for.

We will answer each reviewers comments in detail below- reviewer comments are in italics.

Reviewer 1:

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.

We did not explain this aspect of the paper sufficiently. In fact, in the paper we emphasized the reanalysis aerosol giving the impression that our results are completely dependent on this. We have rewritten the methods and paper to show that our results are not qualitatively sensitive to whether we use MODIS or MERRA2 to look at CDNC. We have also tried to provide a more thorough evaluation of how MERRA2, MODIS and the OMI SO2 product covary in McCoy et al. (2017) to support these observations further. Of course the paper should stand on its own and our results using MODIS and MERRA2 are presented in parallel now. We have also expanded our discussion of the data sets and methodology.

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'.

Thank you- good point. We have inserted a discussion regarding pros and cons of the simulation resolution. Overall, we find the same results in a qualitative sense at both the convection permitting resolution and the GCM resolution so our results are not hugely sensitive to our choice of resolution and the contrast between the GCM and 6.8km resolution demonstrates how sensitive a single model might be to these choices in terms of its cyclone behavior.. We have added a reference to a paper showing that within a resolution range of 1-16km the mean field statistics of parameters such as broadband fluxes and water path do not change much (Field et al., 2017).

Finally, the volcano work needs more sensitivity tests and does not seem robust. Intro- ducing 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.

See comment above, we have removed this work as we believe our results are robust without it and the analysis appears to be sensitive to how much ocean coverage is required in the cyclone composites and how far we assume the lagrangian dispersion model is accurate for. We will return to this analysis in a future study.

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

We feel that the main focus of our paper should not be examining the contrast between the GCM-resolution and convection permitting simulation because this contrast may not be highly representative of the population of GCMs and this has been deleted from the abstract. The simulation discussion in the methodology has been expanded. The surrogate climate model is just a coarsened (140km) version of the high resolution model with parametrized convection switched on. The use of parametrized convection at coarse resolution makes it more similar to a current climate GCM than the 6.8km with explicit convection. The coarse model with parametrized convection provides a convenient comparison to suggest what climate model might do, but without too many changes that make it difficult to disentangle the cause of the difference.

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

We noted that the first group of papers address the first indirect effect and the second group address the lifetime effect. We have rewritten the introduction to try and make it clear which papers refer to each effect.

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.

We have added comparisons of the cyclone composites across WCB regimes between the observations, simulations, and MERRA2. There are big differences, although the convection permitting simulations look generally ok. Since the WCB moisture flux- rain rate relation appears to be a feature of GCMs and observations (Field, Bodas-Salcedo, & Brooks, 2011) and we hypothesize that the inhibition of rain via aerosol-cloud interactions leads to divergence between low and high CCN simulations the mean-state of the simulated cyclones does not have to perfectly match the observations. The comparisons in the paper discuss the differences and similarities between the observations and simulations now.

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

This is a very good point- we have re-examined the MERRA2 documentation. In (McCarty et al., 2016) a list of assimilated radiances and data is made. SSM/I rain rates are retrieved through 1987-2009. SSM/I radiances are also used for this period and are cloud-cleared following (Derber & Wu, 1998). AOD from MODIS is assimilated in the aerosol analysis, but cloud properties are not (Randles et al., 2016). We have noted this in the text, but we also note that all else being equal higher LWP should imply a higher rain-rate and lower aerosol mass so the co-dependence should lead to anti-correlation between LWP and SO4. We have added analysis of the MERRA2 total precipitable liquid water path and indeed see that this is the case. That is to say, higher CDNC (from the MERRA2 SO4) corresponds to lower CLWP cyclones and vice versa. Thank you for suggesting this.

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

As noted above we use MODIS and MERRA2 SO4 as proxies for CDNC and both direct observations and the MERRA2 proxy produce similar results.

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

This indirect effect was not really tuned. In a sense the simulations are more of a 'fixed CDNC' set of simulations than a 'fixed CCN' simulations (as in Lu and Deng (2015)). Another Twomey-type activation scheme would have produced results that show the same qualitative result. We have added some verbage to the text to explain this more clearly. We have added a note that while the CDNC is sensitive to both vertical motions and CCN, the sensitivity to CCN contributes the vast majority of variability in CDNC. Thanks.

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

Although this material has been removed we note that these emissions are somewhat more in keeping with observations than the fixed 40kT scenario in Malavelle 2017. The time varying flux is somewhat more realistic based on observed fluxes (Schmidt et al., 2015).

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

The aerosol in the aquaplanet was constant so it can't vary.

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 com- posite maps to compare to observations, not just WCB flux.

Please see comments above regarding model resolution and additional plots of composites.

Page 4, L20: The ice assumption is simplistic. Is there significant ice in your cyclones? It

Depends on the temperature in your aquaplanet run.

There is significant ice, but it does not seem to be a strong function of WCB moisture flux or aerosol (see Fig S2 of the original SM). The ice number is linked to 'Cooper temperature dependence'. We do not change the ice number representation when we change the CCN. We have added a note to this effect in the text in section 3.1.

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?

This is a common function of GCMs and observations (Field et al., 2011; Field et al., 2008) and not a function of this particular model formulation and appears to be a consequence of mass conservation within the midlatitude cyclone systems.

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.

Please note that the aerosol exponentially decreases with height. IWP is aerosol aware, but the assumption here is that aerosol is not uniform with height throughout the atmosphere.

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.

Do you mean the slopes in the observations or the simulations? It seems like the model is more sensitive to WCB at high aerosol, which seems reasonable if precipitation is being inhibited. The model mean is much lower than the observations we now note this in the paper and this is likely just because we have not used a cloud scheme, which would add additional complexity and make it harder to understand the differences between the low- and high-resolution simulation. We should note that the goal of this study was not to create a simulation with perfectly realistic cyclones, but to examine how cyclones respond to aerosol in a qualitative sense and use that insight to analyze observations in a sensible way.

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.

We don't say that the convection permitting simulation is perfectly representing convection in this section. We note that the convection parameterization, which is not aerosol-aware, is not acting on any of the clouds.

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?

We argue that our simple model formulation appears to be consistent with the observations. We have added a note to this effect to the discussion. Thank you.

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 conclu- sion.

As suggested we have added more evaluation of the aquaplanet. The goal of the aquaplanet is to offer some sort of framework to analyze the models in. The main framework is the strong dependence of rain rate and by extension CLWP on WCB moisture flux. This appears to be a feature of many GCMs (Field et al., 2008). In our idealized simulations we have been able to run high and low aerosol perturbation simulations. When observations are analyzed in the same way high and low CDNC cyclones behave in a qualitatively similar sense to both our high resolution and low-resolution simulations. Based on Reviewer 1's careful analysis we have tried to prune back the conclusions and shore up the methodology.

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.

This comment has been removed. Given the spread in aerosol-cloud forcing it is likely many models also have too strong an aerosol cloud indirect effect. Admittedly, if all climate models without aerosol-aware convection miss this effect this does make it a systematic error, but going through explaining this is convoluted.

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 problem- atic. If MERRA2 assimilates AMSRE, then you have a potential co-variance problem.

See comment in regards to this above.

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

See comment related to P2 L14 above- we have added figures for models and observations to the text.

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

They did, as does this paper. I don't understand how it relates to the material here. High and low aerosol cyclones are for the cyclone as a whole, not for a specific part of the cyclone.

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.

Thank you for your suggestion. We did this and MERRA2 implies the opposite of the observations.

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

Great point- one of the major things that we have changed in the paper is the radiation calculation. We realized that the dependence of albedo on solar zenith angle was yielding unrealistic results (high albedo cyclones having the lowest water path because they had very high SZA). We have added additional analysis exploring this to the paper. We have also added uncertainty to our analysis.

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?

I am not clear what you mean by this, but we do show IWP for the simulations in the SM.

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?

This is from the observations. As noted in the methods section the CLWP is precipitating and non-precipitating liquid. We have chosen to look at this because it is what the microwave radiometer is sensitive to.

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

Thank you- we have remade this figure and tried to explain it better in the text. The idea was to point out that in the context of the regression model not all cyclones are equally sensitive to aerosol forcing. Not a hugely surprising result and really just restating Carslaw et al. (2013), but we thought it was important. We have added notation to show where different ocean regions fall on this figure.

Page 7, L19: what about co-variation?

This is just the weighted mean of the figure so it takes covariation into account. We have noted in the text that this analysis is hopefully illustrative.

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.

Removed- see above.

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.

Removed- see above.

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).

See above- removed.

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

See above, removed

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

Sorry- we phrased that poorly. Removed.

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

This material has been removed.

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

This material has been removed.

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

Agreed- we found sensitivity to our assumptions in this analysis. This does not affect the conclusions in the remainder of the paper.

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.

This is a good point. Our model by default tends toward a positive lifetime effect on LWP, although this is not a given because of interactions with other clouds and the environment – see Fig 6 and 7 of Miltenberger et al. (2017). We have added to text to make this clearer.

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.

The argument here is just that the forcing is sufficiently large to be non-negligible. We have expanded our discussion to make it clearer that this is all we are saying.

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

We have tried to expand our methods and discussion to make the analysis clearer.

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

See above. We have tried to more clearly articulate the analysis and acknowledge that the model set up we have will enhance LWP with enhanced CCN, all else being equal.

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.

We have clarified this statement to reflect the fact that we have shown this via observations, for the first time.

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

That is correct, we have expanded our methodology to more clearly articulate this.

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

We have updated our analysis of the albedo effect.

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.

See above. We have tried to more clearly explain the figure.

Reviewer 2:

1. There are not enough details and a lot is left for the reader to find in other publi- cations. The accuracy of the various derived observations would be very helpful. The paper is rather succinct, and some figures were moved to a supplement document, as if it were intended as a letter or short publication.

This is correct- we have tried to more clearly articulate our research in the revised submission.

2. I am not convinced by the work done with CERES on the impact of the aerosols on the storm albedo (Figure 4 and associated discussion), possibly because there are not enough explanations on how the results are obtained. First it is not clear whether the WCB is constrained in the figure, then there is very succinct discussion on what actually might impact the albedo: with the warm frontal and warm conveyor belt regions of the cyclone dominating the signal and their large amount of high level, mostly ice clouds, there is little signal to be expected from changes in *aerosols or low level clouds.*

This is a good point- we have significantly reworked our evaluation of the albedo effect, primarily due to difficulties resulting from SZA bias (see previous reviewer comments). The reviewer also makes a good point regarding ice cloud, however, assuming that the ice cloud effect on albedo is more or less randomly distributed and is unaffected by CCN then it should just add variability to the populations of low and high CCN cyclones. We have compared albedo from the high and low CCN cyclones in the SM of the original submission and we show that most of the effect in the observations and simulations is in the post cold frontal clouds. This is consistent with the proposed effect (eg CCN affecting liquid cloud cover). We have expanded this analysis and moved it to the main text.

In addition, if all cyclones are included, then the CDNC classification can be highly correlated with the cyclone properties and this would mask any impact aerosols direct and indirect ef- fect might have.

This is a good point regarding the direct effect. To try and better understand the possibility that we are somehow aliasing this effect in we looked at cyclone composites of CF and CLWP differences between high and low CDNC cyclones. We see that there is a general agreement between the regions where CF and CLWP enhance and where the all-sky albedo increases. Of course the partitioning of this radiative effect into components owing to changes in CF, LWP, CDNC is difficult, but it does seem like changes in cloud macrophysical properties and changes in albedo are happening in the same area so that supports the idea that these changes are driving the changes in albedo.

3. More details are needed on the work of section 3.2, especially the method, the whole section is confusing and so the importance of the results somewhat degraded

This has been expanded- thanks.

4. In the title, and in the conclusions, the "aerosol-cyclone indirect effect" is mentioned. This is misleading, as this would entail an observational evidence of an impact of aerosols on the cyclone dynamics. This study is about aerosol-cloud in- teractions in the midlatitude using extratropical cyclones to constrain the large scale environment.

Admittedly it's an aerosol-cloud effect, but we are using the cyclone as a constraint to order the meteorology so it seems reasonable to call it this since we are referring to the cyclone as the clouds that compose it. Please note that we have altered the main body of the text to reflect that it is the clouds within the cyclone changing to avoid any confusion, but we feel that completely spelling this out in the title would make it clunky.

Detailed comments: 5. Page 1, line 21-22: Here you introduce the role of extratropical cyclones: why not include their role for precipitation in the midlatitude which would be appropriate with the rest of the paper? Reference to the work of Hawcroft et al (GRL 2012), and Catto et al (GRL 2012) would make sense here.

Thank you. These are very good references to add. One thing we did find is that the precipitation is controlled by the WCB, not aerosol so the effect we show should not alter the total rainfall.

6. Page 2, line 1: here refer to Igel et al., 2013 before Malavelle et al. 7.

Done.

7. Page 2, line 20: "the algorithm of Field and Wood (2007)", please provide some details of what it is.

This section has been expanded- thanks.

8. Page 2, line 21 onward: when you introduce the CDNC product of MODIS, some details of what it is, its strengths and limitations should be included. The same is true of the other observations/products introduced in this section. There are many observations of the same parameter that are available, so it would be good to justify a bit more why these particular ones are used. For example, cloud fraction is from CERES, why not from MODIS (which the CERES product is in fact retrieved from if I am not mistaken)? How good is the MERRA-2 reanalysis for the sulfate mass concentration product?

This would be good to do. As you point out the CERES product is partially from MODIS- what was meant is that we used the data included as part of the CERES product. This has been altered to specify that the cloud fraction is originally from MODIS and geostationary satellites and the SYN1DEG product has been referenced. We have tried to provide some additional background and more complete citations on the other data products.

9. Page 2, last paragraph: how accurate is this rain water path estimate?

We have redone this part of the paper to use the MAC-LWP data set, which comes with the total (precipitating+non-precipitating) liquid water path already calculated. In the original paper we were just inverting the algorithm used by RSS to calculate the rain rate because the quantity that the microwave actually measures is the total liquid water path, not the liquid water path and rain rate. The rain-cloud partitioning from RSS was based on SST alone, which added ambiguity to the results since it covaries with WVP.

10. Page 3, section 2.2.2: more details on the model would be helpful. What does "NAME" stand for?

This has been removed.

11. Page 4, line 28: you write that the cyclone-centered mean is used to obtain the cyclone moisture flux. You should justify this a bit more, as the link to warm conveyor belt is not obvious: this is not the definition used typically. One argument is that cloud and precipitation occur predominantly in the warm conveyor belt and the warm frontal region, so the signal averaged in the entire cyclone region would be dominated by these two areas. Another is that cyclone cloud and precipitation depend strongly on the strength of the cyclone (here characterized by the surface wind) and the amount of moisture ingested in the cyclone (here characterized by the total *water path). Refer- ences are many, but as an example, one could be given to the Field and Wood paper (2007), and/or Bauer and Del Genio (JCLI 2006) and/or Rudeva and Gulev (MWR 2011).*

We referenced the Field and Wood 2007 paper in this paragraph, but we will add the other references in addition to the paper used to justify the simple model used in the original FW07 paper.

12. Page 5, line 1-2: "indicating that this aerosol-cyclone indirect effect acts through

the warm rain process." This is quite a leap, how do we know this is not model-specific? Also, in Igel et al. (2013), even though the total ice mass in a warm front shows very small changes with an increase in aerosol concentration, the microphysical processes differed such that the aerosol had a compensating impact on vapor depostion and riming efficiency in the mixed phase region. So could it be the case here as well? in which case you might want to change this statement as the indirect effect here would not just act through the warm rain process. And this is not an aerosol-cyclone relation, but an aerosol-cloud relation.

This was meant to refer to the model alone, and indeed this might be different in a different model. We have updated this to reflect this.

13. Page 5, line 14-16. In figure S3, how do we know that the change in ToA SW flux is not caused by the direct aerosol effect instead of the effect on liquid water path?

See discussion above.

14. Page 5, line 22: "allowing for accurate observations", how accurate? There are issues in heavy rain situations with microwave radiometer retrievals of water path and wind speed, which could impact the estimate of the moisture flux and the classification used in the paper. This should be discussed, preferably as early as section 2.

We have deleted 'accurate' as this is not a quantifiable statement. We have extended the discussion of biases in WVP and wind speed from microwave observations.

15. Page 6, line 4: "highly consistent" is vague, could you be more quantitative? How is CDNC obtained when clouds are present? How often do you have retrievals in the southwest quadrant? Do you have a threshold on this number below which you do not consider the cyclone in question?

We did not fully explain- CDNC is retrieved from cloud top effective radius and optical depth. It is only retrieved for overcast 1°x1° regions. We have added additional text to clarify how the retrieval is performed. Since the SW quadrant is highly cloudy it is not too difficult to perform the retrieval. There is no lower-bound on the number of retrievals required. We also perform the same analysis with MERRA2 SO_4, which never has missing data since it is reanalysis and get the same results so this does not appear to be an issue. The inter-cyclone correlation between the

$CDNC_{SW}$ calculated using MERRA2 and MODIS is shown in the supplementary figures along with the effects of resampling MERRA2 SO4 so it is only sampled when MODIS can perform a retrieval. We have added text discussing differences in cyclone composited CDNC.

16. Page 6, line 5: because the cold front is moving with respect to the center of the cyclones, sometimes it is in the southwest quadrant, other times in the southeast quadrant, and so the aerosols could be ingested in either quadrant. Have you tried to use the southeast quadrant instead to see if the results change?

We have expanded the discussion related to this uncertainty to also include a recalculation of our results (characterized as the low-high CDNC plot as a function of WCB) using the cyclone mean CDNC and using the SE, and South part of the composite. The difference between high and low CDNC cyclones narrows when the cyclone-mean CDNC is used. This seems reasonable as it is adding a lot of noise to the calculation. However, if the SE part of the composite is used the separation between high and low CDNC cyclones narrows considerably. If just the south part of the composite is used it narrows much less. This seems like a case that would be improved by front identification, but for the purposes of this article we will stay with our simple cyclone compositing algorithm and note that there is some sensitivity to the sector of the cyclone composite used to calculate CDNC in the cyclone. We have also added these figures to the SM and additional discussion to the main text.

17. Page 6, line 9: Figure S4: the two composites look rather different, the two color bars should match to make the comparison easier, and a 1-1 line should be added to the (c) scatter plot. Also, why not add these three plots to Figure 2 and make it a 4-panel figure?

Thank you- we have moved all of these figures into a 1 panel figure and have added discussion regarding why they look somewhat different in structure. Overall the inter cyclone variability agrees well and once differences in sampling are accounted for MERRA2 and MODIS agree decently well in the region where there is abundant low, liquid cloud that MODIS can retrieve CDNC from.

18. Page 6, line 16-17: why not discuss the very obvious differences in the southeast quadrant between observations and model?

We have expanded the discussion and comparison between model simulations and observations. Thank you.

19. Page 6, lines 18-26: so here the albedo is estimated with the CERES data, correct? and so is the cloud fraction? how can you have 100% cloud fraction in your cyclone area? you did not explain how this is obtained. Also, if you really have 100%CF, how do you have C4

MODIS CDNC? Finally the differences between MERRA-2 and MODIS are not that different in magnitude from the differences between high/low CDNC, how significant is this effect on albedo?

We found that the SZA significantly impacted these results and we have redone this calculation. The MODIS CDNC retrieval is at cloud top, not in clear sky.

20. Page 6, lines 27-33: This is not very convincing, as there is no mention of the moisture flux being constrained, which means that the albedo effect could come from cyclone with low vs. high CDNC having different mean moisture flux and thus different cloud cover caused by this instead.

We have now constrained our estimate by WCB. See above.

21. Page 7, the regression model work: I am not sure I se the link between the albedo discussion and this work. Why not introduce this before the albedo work? This re- gression model is obtained how, based on Figure 5? Finally, I am not sure what the implication of these results is? In particular the very last sentence is unclear, please elaborate. Line 15, and line 17, large and small cyclones do not really mean anything, you do not know anything about their spatial extent. You could use strong/weak maybe, but you would need to specify that this is in term of moisture flux strength, not winds alone.

We train the regression model using the observational record. Figure 5 just shows a summary of this showing the average of all the observations in predictor space to show that the lines curve a little more sharply at low CDNCSW. We have specified that by large and small we mean large and small moisture flux. We have also remade the plot and tried to make it clearer that it is just a plot summarizing the regression models fit to the data.

22. Page 8, line 10: "both simulations", not clear what the two simulations are, only one is mentioned above.

Removed- see above.

23. Page 8, lines 22-26: This feels out of place, why mention the convection permitting simulations in this context? It just repeats what has been said a few times already about the merits of the high resolution vs GPM-resolution simulations.

This has been removed.

24. Page 9, line 15: "an aerosol indirect effect on midlatitude storms" is not accurate, maybe add "clouds" after "storm"

That is a good point. Done.

Reviewer 3:

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.*

Thanks- we have tried to more fully explore the uncertainty in our analysis and appreciate the reviewers help in improving our paper.

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, meteorological variability has a large impact on the perceived aerosol-cloud interaction, no matter whether we look at observations or modelling.

We agree that meteorology controls the majority of cyclone behavior, but it does seem like once we remove variability associated with meteorological variability there is still a signal associated with aerosols. This result appears to be present in both highly idealized simulations and observations. Overall we estimate an impact of aerosols from a standardized perturbation in CDNC that is less than 30% of the response to meteorology- so meteorology still dominates cyclone behavior.

My suggestion is to clearly high- light 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 pertur- bation. I would state this explicitly in the text.

See comments above, we have removed this.

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.

Removed. See above.

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.*

Very true- we have expanded the abstract- thanks.

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.

Optical depth is a function of both liquid content and CDNC, not counting whether the lifetime effect changes cloud fraction. All else being equal we should expect that increasing liquid water amount should increase albedo. Since the range of possible climate sensitivity is strongly affected by the assumed strength of aerosol indirect effects if we had a tighter constraint on indirect effects we should have a tighter constraint on climate sensitivity. This statement implies that it is for a given emissions scenario, which should give a more constrained prediction based on a better constrained climate sensitivity. We have tried to expand this section to better explain what we meant.

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

Removed.

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?

We are using the model configuration based on the operational model used for CMIP and trying to make as few changes as possible.

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.

The exponential decay starts at 5km. Given that it is constant and non-interacting it is not intended to be highly physical. We also tried a constant vertical profile, but this generated a great deal of ice due to the highly simplistic ice nucleation used in the simulations at high altitudes and the results were clearly unrealistic.

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 radia- tion and clouds in the model (such a setup could also be interpreted as "interacting").*

Thank you- yes- what we meant was that precipitation does not deplete the aerosol and no new aerosol is generated. 'Fixed' would have been better. We have changed this to clarify what was meant.

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.

This setup was chosen because it was simple to implement and didn't require any assumptions regarding the aerosol gradient. The SST is fixed in the simulations so the aerosol shouldn't change the overall temperature profile beyond changing the atmospheric temperature. We do not think that the step function of CCN should affect our results since their purpose is to give us insight into how to analyze the observations- however, this is something that we plan to look at more in the future with more simulations so we will be looking at step function and gradiated channels for the purpose of understanding model behavior more clearly.

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.

Removed- see above. We will consider this in a future paper more fully examining these results.

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

We have substantially re written both the methods section and this section and hopefully it flows better now.

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).

That was poorly worded, we have altered the text to make it clearer we just meant the model.

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.

We have expanded this discussion, but the CDNC retrieval outside of the regions of low lying cloud (such as the cold front) are not reliable and the increase toward the center of the cyclone is not likely to be robust.

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

Altered- thanks.

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

Very good point- we were concerned about this and have undertaken a separate and more extensive analysis of the MERRA2-MODIS CDNC products (McCoy et al., 2017). It does seem like there is a reasonable agreement between these products and long term changes due to pollution and volcanic degassing that cannot be explained by meteorology. We also added evaluation of the CLWP-WCB relationship partitioned into high and low CDNCSW populations (inferred from MERRA2) to our analysis and see that MERRA2 CLWP actually has the opposite behavior to the observations suggesting that the MERRA2 analysis is not baking in the increase in CLWP with increasing CDNC.

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?

You are correct that assigning a systematic bias to GCMs was not fair. Some GCMs have extremely strong lifetime effects that are sure to exceed the observations shown in our study. This sentence has been removed.

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