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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 #2

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This manuscript examines the impact of aerosols on liquid water path in extratropical cyclones using a combination of model simulations and observations. They classify the cyclones based on the strength of the warm conveyor belt, estimated as the product between surface winds and total precipitable water (i.e. moisture flux) averaged in a 2000 km radius region centered on the minimum in sea level pressure that indicates the location of the cyclone. Then they use both MERRA-2 and MODIS information to characterize the aerosol concentration (using the cloud droplet number concentration) in the southwest quadrant of the cyclones and AMSR-E/AMSR-2 liquid water path to evaluate the correlation between cloud droplet number concentration and liquid water path in the storms. They do the same exercise with two aqua planet type simulations of differing resolution and convection treatment (one resolved, the other parameterized).

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Discussion paper

They propose a model to evaluate the expected cloud liquid water path given the moisture flux and cloud droplet number concentration and use it to evaluate the impact of the Holuhraun eruption on clouds in north Atlantic storms.

There are a number of issues in the manuscript, that would need to be addressed before it can be accepted for publication:

1. There are not enough details and a lot is left for the reader to find in other publications. 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. I am not sure what the length requirements are with ACP, but it seems to me that all figures could be easily integrated in the main manuscript, and the text should be enriched with more explanations. 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. 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 effect might have. 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 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 interactions in the midlatitude using extratropical cyclones to constrain the large scale environment.

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.

6. Page 2, line 1: here refer to Igel et al., 2013 before Malavelle et al. 7. Page 2, line 20: "the algorithm of Field and Wood (2007)", please provide some details of what it is. 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? 9. Page 2, last paragraph: how accurate is this rain water path estimate?

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

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). References 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).

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. 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? 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.

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

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? 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.

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 regression 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.

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

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

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