

## ***Interactive comment on “Cloud feedbacks in extratropical cyclones: insight from long-term satellite data and high-resolution global simulations” by Daniel T. McCoy et al.***

### **Anonymous Referee #1**

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The paper aims to put forward an alternative view for mechanisms involved in cloud feedbacks over the extratropical oceans. It promotes the role of increasing water vapour fluxes into cyclones as a mechanism to increase the liquid water path and as a result the reflected shortwave radiation. It attempts to contrast this to the mechanism the commonly held view that it is a phase change from ice to water clouds in a warmer climate that provides a negative cloud feedback over the extratropical oceans.

While the idea is well worth pursuing the paper fails to convince me. There are two reasons. First, I am not convinced the methodology is sufficient to make the case the authors wish to make. Second, the paper is written in a very cumbersome way making

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it way too long and a fairly arduous read. The style dilutes many of the arguments the authors wish to put forward with long rambling paragraphs of speculations all throughout the results section.

I will give more details on both points below. I believe the paper can be brought to publication in two ways.

1) Instead of promoting their view of water vapour flux as the alternative to phase changes as the mechanism for feedback, a more measured judgement that both might be at work would be helpful. I believe the methodology used highlights the role of the WCB but cannot exclude the possibility of the phase change hypothesis due to the strong serial correlation of both through temperature. The authors admit this themselves (page 18, line 33). The largest of all correlations in the entire study is that between WCB and SST, making it very hard to argue one way or the other, so why even try. A less strong but equally important conclusion the paper can draw is that it is likely that the WCB effect needs to be considered as a possible, but perhaps not the only, mechanism.

2) The paper needs to be rewritten and significantly shortened. 26 pages of dense text and 28 figures (including the supplement) is simply too much. Many of the supplementary figures are used for major arguments in the text, so they are anything but supplemental.

Major comments

3) The methodology of linear regressions, which is used to make major arguments about processes, is insufficient. Take as an example equation 4. Not only are the two predictors highly correlated, but the physical arguments surrounding them are flawed. Whilst in PBL clouds it is sensible to assume that the cloud temperature is strongly coupled to the SST, I fail to see why this would be true in extratropical cyclones. Furthermore the very old idea the LWP is simply a function of temperature has been discarded for a while now. Not surprisingly then, the method reveals that almost all of

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the relationship resides in the first term, which turns out to be mainly due to water vapour increases directly tied to temperature increases, which themselves prohibit you to exclude phase change effects. This highly circular argument makes it very hard to support that rather strong conclusion that it “appears that once WCB moisture flux is accounted for relatively little room is left for an effect related to phase changes.” (page 19, line 6). We simply don’t know. What we have learned is that phase changes alone might be too simple an explanation. Nothing wrong with that as a conclusion.

4) The averaging over the cyclones is inadequately explained. Page 11, line 10 states that cyclone means are within 2000 km of the cyclone centre. Is this applied to every cyclone? Doesn’t it matter how big the cyclone is? Does within mean that sometimes it’s less? If the cyclone mean is always 2000 km from the Centre, couldn’t this introduce artefacts? If the cyclones are smaller than 2000 km and their size changes, this will change all the averages with little relation to the flux, would it not?

5) The paper needs shortening. This can be achieved in several ways, first and foremost by removing the many long paragraphs of indulgent musings and speculations scattered throughout the results section. They really get in the way of your argument and they should be removed and a short (!) discussion section added after the results instead. Almost every time an interesting result emerges, the reader gets distracted with a paragraph of discussion, sometimes not even strongly related to the results. Sometimes those paragraphs precede new results, making them even more confusing. Here are the most prominent examples for this:

Page 5, Line 5-15: A long paragraph with the figure relegated to the supplementary material. Do we really need this?

Page 14, Line 17-20: What has this to do with what follows? It’s just confusing the reader.

Page 15, Line 8-18: Pure speculation. Not a result, so why is it in the results section?

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Page 15, Line 23-35: Ditto

Page 16-17, Line 18-5: Again, this has nothing to do with results and simply distracts from them

Page 18, Line 7-8: A very strange sentence. What does this refer to? The un-initiated reader has no idea why this needs to be discussed. Please revise.

Page 18, Line 11-16: What is this paragraph trying to say? What is it referring to? The previous paper? A figure (7) in this paper? I found it hard to make sense of. Please explain what you are doing, then what your result is. Leave the discussion for a discussion section.

Page 19, Line 12-22: Another distracting paragraph of discussion.

Page 24, Line 1-14: Ditto

6) Another way to shorten the paper is to move the rather detailed model descriptions in Section 2.3 into an appendix.

7) The paper clearly struggles with the use of figures in the supplement. The choice seems almost random and major conclusions are drawn from figures in the supplementary material (as evidenced by many of them mentioned even in the conclusions section). The authors need to revisit all their figures, select the ones that are absolutely necessary for their arguments and omit all others. Which ones will be required will only become clear after the rewrite of the results section, so it is hard to make more concrete suggestions at this stage.

8) The paper switches from global considerations to the SH only and back to global from section to section. I am not sure we learned anything from looking at both hemispheres, so the authors may wish to consider to look at the SH only throughout. This might also tighten the arguments in the paper. One could always include a result from the NH in the discussion section if needed, but keep the main results to one hemisphere.

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Minor comments:

Page 5, line 14: You did not state how you composite. Presumably by overlaying the cyclone centers?

Page 5 line 24: The “observations” are presumably a reanalysis - this needs mentioning here. Section 2.2.2: I suggest to move this sentence into the composite section. It is needed there and hardly warrants its own subsection anyway. Also, are daily means good enough to do the cyclone detection? Also, the satellite daily means aren't really daily means. Does this matter? Please discuss this.

Page 7, Line 22-24: Propaganda and not needed here.

Page 10, Line 11-13: This is confusing. First you say there is a problem with  $k$ , then you use it anyway. Is there a justification for this?

Page 11, Line 28-29: This is a strange sentence. What has societal importance to do with WCB being a useful constraint? Nothing I think. Please change this.

Page 12, Line 21-22: But isn't it the in-cloud LWP that matters? As the dependence is not linear, we could imagine more rain from lower mean but higher in-cloud LWP through changes in cloud fraction.

Page 12, Line 29: The translation to albedo is also non-linear, and more water does not necessarily mean higher albedo. If we are at high LWP where albedo saturates, further increases in LWP will not change albedo. Please discuss this.

Page 14, Line 2-4: In the figure, there aren't many models that flatten more than the observations. On the contrary, there are some that don't flatten at all. So this discussion is one-sided and needs revising.

Page 14, Line 15: What is the “climatology” here? Is it the climatology for each month so as to remove the seasonal cycle? If so, say so.

Page 17, Line 31-32: How can this sentence be true? Are they higher, or are they in

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good agreement? They cannot be both!

Page 18, Line 9: How? Why? Why is any LWP trend equal to a feedback?

Page 20, Line 5: I don't understand this sentence. What does it mean? Why is it there?

Page 20, Line 11: This should be the second sentence of the previous paragraph!

Page 20, Line 26-27: This is an example for a key result with its figure in the supplementary material

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-785>, 2018.

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