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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 #2

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General comments This paper examines the role of the warm conveyor belt (WCB) moisture flux in determining extratropical cyclone variability. It is argued that WCB determines cyclone liquid water path (LWP) variability more than does phase changes in the clouds. Further, as WCB depends on WVP, which will increase under warming according to the Clausius-Clapeyron relation, a negative feedback is identified and quantified. An additional feedback by the second driver of WCB, wind speed, is also discussed, but can't be quantified or even given a certain sign.

The authors are addressing the previously identified negative cloud feedback in the extra tropics, related to cloud optical depth (via LWP), and suggesting a mechanism in

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complement or in place of phase changes as responsible for this feedback. This is a valuable contribution.

Comparing a range of model resolutions is a useful approach (although more could be squeezed out of this comparison), as is the cyclone compositing framework.

The way the paper is written, it is somewhat difficult to distill out the main points – a multitude of figures and side tracks make the reasoning hard to follow at times. I would advise the authors to tighten up the writing, and consider reducing the number of figures presented, without simply moving them to the supplementary material. Several of the supplementary figures already play more than a supplementary role, the way the analysis is presently presented.

In addition, the following specific comments will need to be addressed by the authors.

Specific comments 1. The study is based on multiple linear regression (introduced as a statement on p 4, line 22). The authors need to explain why, if at all, this is a suitable approach. It is clear that some of the processes investigated have non-linear elements (e.g. Fig 3). It is also clear that in several cases the predictors are not independent (e.g. Eq. 4, Eq. 6). SST determines WVP through Clausius- Clapeyron, and WVP in turn is part of the definition of WCB, and hence SST and WCB, or "thermodynamics" and "meteorology" (p. 18, line 18-19), can't be separated in this way.

The authors occasionally point at these problems, but further explanation and/or justification would be needed (e.g. p. 14 line 21-24, p. 19, line 3-5). For instance, would it be possible to attempt to estimate parameters for a non-linear relation, rather than forcing a linear fit between LWP and WCB? And would it be an option to use only one predictor rather than two, when they are not independent, as is the case for WCB and SST?

2. It also needs to be acknowledged that the degrees of explanation are in general rather low. E.g. p 14, line 31, Fig. 5. P 1 line 33, states that WCB "can explain" trend

in LWP over two decades, which is a pretty strong statement. P 4 line 13 refers to a “clear criterion” between “synoptic state” (WCB) and LWP to test models against. I find this to be a bit optimistic, based on the results presented. On p 13, line 31-33, it also seems as if the large uncertainty in the observationally based estimate would limit the usefulness of the suggested constraint on models.

Figure 6 shows slopes of relations that (according to Fig. S5) have correlations R2 ranging from below 0.3 to above 0.7. Even though the slopes are all significantly greater than zero, the relations are in some cases rather weak, and a chain of weak correlations is simply not enough to support the conclusions drawn. Could a threshold R2 be used to select a subset of slopes to use?

Another example is p 16, where the reasoning seems to be that latitude explains wind speed which explains WCB which explains precipitation. As stated by the authors, the relation between latitude and windspeed is not causal, but can be explained by poleward travelling and intensification of cyclones during their life cycle. The link to a poleward shift in storm track position is not clear. The change in latitude could leave the initial wind speed unaffected, i.e. the intensification of storms seen is not necessarily an effect of their shift in position.

The weak relations also cause problems in the attempts to compare present climate to future (warmer) conditions. On p 20 it remains unexplained why shifts in the LWP-WCB relation occur and why in the NH (Fig. s9) the shift changes sign between low and high WCB, but it is clear that the assumption that the relationship between WCB and LWP is invariant under warming (p 20 line 4) does in fact not hold, other than within a large range of uncertainty.

3. The paper claims to show that precipitation is balanced by WCB, but I would argue that this is not shown, but rather assumed in Section 3.1., and then used to motivate the continued analysis.

Eq. 3 relates WCB to WVP and WS as WCB=k*WVP*WS. Line 10, however, states that the constant of proportionality k is defined based on regression of precipitation rate on WVP and WS, i.e. precipitation=k*WVP*WS. This suggests that an equivalence between WCB and precipitation rate is assumed. This is a logical problem (assuming a relation you set out to test) and a physical problem (as there may be a fraction of the precipitation that is not related to the WCB , see e.g. Pfahl et al. 2013, https://journals.ametsoc.org/doi/10.1175/JCLI-D-13-00223.1)

Further down, a "match" between moisture flux into and precipitation out of a cyclone is said to be examined (page 11, line 14-16), and Fig. 2 suggests that models' estimate of the relation between precipitation and WVP*WS is in general agreement with observations (with large uncertainty). It needs to be sorted out what is assumed and what is investigated, in observations and models, and the recurring assumption that WCB can be replaced with precipitation needs explanation and/or justification.

With the current presentation, the statement on P12 Line 16 is not correct; section 3.1 doesn't show that precipitation is predicted by WCB, it shows that WS*WVP is well correlated with (or "predicts") WCB, and it is assumed that WCB is perfectly balanced by precipitation.

On p 13, line 5 it is contrarily stated that the relation that has so far been assumed between WCP and precipitation can't be evaluated. Adding LWP to the discussion here (p 12-13) does not necessarily help. A time aspect seems to be missing, as it is assumed that more moisture flux in is balanced by more precipitation, but in between an observable build-up of liquid water is expected. This requires some explanation.

One option would be to exclude the section on precipitation, and focus the paper on the discussion of the role of WCB in determining LWP.

4. Some other methodological choices are also not clear or explained, e.g. the separation between NH and SH in some cases, and in others not. Please make clear when and why this separation is useful or meaningful. The main focus seems to be on the SH and the Southern Ocean, and perhaps it could be motivated to make that focus

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even more distinct.

5. P13, line 8-10 The statement has been reversed, according to Fig. S2 the ratio LWP/TLWP decreases with increasing WCB. This is problematic as it contrasts with the following statement that LWP increases with increasing WCB in general. This does not follow from Fig S2. In Fig .3 the relation between LWP and WCB has the expected sign. On p 14, lines 1-2 a more reasonable interpretation of Fig s2 and fig 3 is made: at higher WCB the partitioning of LWP is more biased towards rain, and this contributes to the asymptotic shape of the LWP-curve in fig 3. I would encourage the suggested future study of this aspect.

6. P16, lines 1-5 (Fig. S3) indicates that a shift of 5 degrees improves the correlation between LWP and WCB. P 18 uses a new choice of latitude range, to agree with Manaster et al. (2107) How region-sensitive might the analysis be, or rather what is the motivation for the chosen regions?

7. It is not clear how the regression of albedo on LWP accounts for cloud masking. (Page 22, line 12 and onward, particularly lines 22 -33 of page 22) It seems like the exercise described here is an attempt to quantify the albedo changes due to changes in LWP due to changes in WCB (or SST), i.e. the suggested feedback mechanism, not to correct for overlying ice clouds. The summarizing sentence on p 23, line 6 also indicates that this is what has been done, rather than accounting for cloud masking

Technical comments Fig1: Observations should be MERRA reanalysis

P3, line 10 “increases or decreases”

P3 line 14 “optical depth increase” should rather read “optical depth change” as it was just stated that it is unclear if it is an increase or a decrease

P3, line 22, line 24 “reflected shortwave” should be followed by radiation

In section 2.1 I would suggest to present the Cyclone compositing (2.1.2) before the Regression analysis (2.1.1), as the compositing is referred to in 2.1.1. but not explained until 2.1.2.

Section 2.1.2 Cyclone compositing , p5, could use some clarification. E.g. line 9 “before and after” what? please clarify if p_0' is a function of time or of x,y only. Line 9 “Candidate gridpoints” means what? Line 14 “maximum negative anomaly within 2000 km” of what? Line 17 “of the figure”, comes without previous reference to a figure

Page 5, line 28 Please clarify what “bias-corrected” refers to. The following paragraphs describes various identified problems with the MAC LWP data, but it is not clear which if any of these are corrected for, or if excluding certain years and judging surface contamination as irrelevant is the bias correction

P6, line 26 please spell out FOV. Throughout, there are many abbreviations, some of which may not be necessary to introduce.

P7 line 25 Please explain Easy Aerosol. As the abbreviations mentioned above, jargon makes the paper more difficult to follow.

P7 line 29 How are these “three resolutions” of HadGEM3 referred to?

P8, line 30 “in more detail”

P10, line 8-9 This statement raises more questions than it answers. I would suggest to explain, or remove it.

P10, line 23 The statement “The January SST was reflected north-south” needs explanation

Page 12, line 8: the word models seems to be missing, midlatitude-cyclones can hardly be said to under-estimate precipitation

Page 12 line 10-12 Look over this sentence, it does not make sense

P 13, line 9 fix typo “. . .results in a decreases the. . .”

P14 line 31 missing “is”

P14, line 13 “poleward of 30-80N” should be “between 30 and 80N”

P 14 line 25 one “of” too many

P21, line 15-17 please look over this sentence, perhaps removing “that” is all that is needed

P22 line 15 “shortwave radiation”

P22 line 18 “these data”

P24, line 8 “the magnitude”

P24 line 24-26, please look over this sentence

The paper has a somewhat abrupt ending, please consider a final sentence to wrap up. This would be made easier if the whole paper could be more condensed.

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