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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Environmental influences on the intensity changes of tropical cyclones over the Western North Pacific" *by* Shoujuan Shu et al.

Anonymous Referee #2

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The authors contrast composites of the environment of weakening and strengthening WNP TCs using primarily global model analyses supplemented with other data sources. The authors' stated objectives are to investigate how the environmental conditions associated with the subtropical high impact intensity change and to provide additional guidance for intensity forecasts in this region. The manuscript was generally written well, although I have a couple major concerns regarding the connection between the analysis and the schematic in Fig. 11 (which illustrates a bit more than the authors have actually shown) and other potential explanations for the weakening versus strengthening cases. I therefore recommend major revisions to the manuscript.

Major concerns:

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Discussion Paper

1) Regarding the authors' first objective, the analysis falls short in my opinion. Based upon the summary schematic in Fig. 11, it appears that the authors wish to invoke the study of flow topology by Riemer and Montgomery (2011). One objective of the Riemer and Montgomery paper, it seems, was to encourage a view of dry air impacts on TC intensity change that goes beyond "proximity of dry air to the TC" arguments that are often invoked in real time to "explain" why a given TC might or might not intensify. However, this type of "proximity" argument permeates the present manuscript. Although mid-level ventilation of the eyewall is possible in some cases, Riemer and Montgomery also tied the flow topology to the mechanism described in their 2010 paper. That is, if low theta-e air could approach near enough to the TC core to be transported into the boundary layer (outside of the eyewall region) by the mechanism described in their 2010 paper, then intensity impacts could occur. They furthermore noted that when the mesoscale region of descending air outside the eyewall coincides with anomalously low theta-e air brought in from the environment, that the intensity impacts would be magnified. So in light of this, I would ask the authors what dry air at 500 km radius from the TC center, or even 300 km from the TC center has to do with intensity change? They need to explain the conceptual model more clearly before they engage in the analysis.

To answer the above question, the authors may find that to clearly motivate their summary schematic in Fig. 11, they will need to explicitly show the flow topology in a TC-relative frame (that is, moving with the TC) with appropriate diagnostics. In my opinion, this type of analysis would be more insightful than simply observing the closer proximity of dry air to the TC in the weakening cases. I do not believe that the future numerical study proposed at the end of the manuscript should excuse the authors from considering this analysis here.

2) The authors anticipate that readers will ask whether SST differences might be a plausible explanation for the intensifying versus weakening stratifications. Indeed, the intensifying TCs do experience higher SSTs than the weakening TCs. The authors note, however, that the mean SST of the weakening cases is still high enough to sup-

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port strengthening. What if the weakening cases are simply closer to their MPI than the strengthening cases? The authors do not explicitly note the average intensities of the two stratifications, but the wind fields seem to suggest that the intensifying storms are at an earlier stage in their lifecycle. The weakening storms seem to be more mature. Could differences in where the storms are in their lifecycle play a role in future intensity change?

Minor concerns:

1) 31819 line 8: remove “by the way” Too colloquial.

2) Does “storm-relative” just mean storm centered, or does it also mean relative to the translating storm? If the storm translation has been removed from the wind field in each case before the compositing, then the authors should explicitly state that.

3) 31824 line 22: “wrap into the TC circulation” This is one of many examples where the analysis would benefit from activities listed in Major concern 1). How do I know that the dry air is actually wrapping to a location where it might impact intensity? The authors show dry air streaming all the way into the TC symbol in Fig. 11b. Does it?

4) 31827 line 24: Are the authors using “significant” in terms of magnitude, or do they mean in a statistical sense. If the former, to avoid confusion I would avoid using “significant” here since the authors have previously stated that the differences are statistically significant.

5) 31827 line 27: “with” or “within”?; line 29: “exit” or “exist”?

6) 31828 line 12: should be “characteristic”; line 22 “be served” should be “serve”

7) How are “TC circulation” and “inner core region” defined?

8) 31829 line 7: “In the meanwhile” should be “Meanwhile”

9) 31829 line 11: The authors are fine to refer to Corbosiero and Molinari here, but I believe Reasor, Rogers and Lorsolo (2013, MWR) actually show observed composite

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vertical motion rather than lightning flash rate.

10) Where “warm moisture” is used, replace that with “warm, moist air”.

11) 31830 line 1: should be “convective”

12) 31830 line 9: In what sense does the TC “interact” with the dry air? See Major concern 1).

13) Fig. 2: (a) etc. not labeled in my copy

14) Fig. 11: How do the authors arrive at the schematics here? What level of the flow is it supposed to represent? Are the authors certain that the schematic faithfully (or at least broadly) represents the observed flow topology in the weakening and strengthening cases?

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