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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 #1

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This manuscript by Shu et al. considers intensity changes of tropical cyclones (TCs) in association with the "North West Pacific subtropical high" in a composite framework. They compile composites of weakening and intensifying storms based on the 48 h intensity evolution with a threshold of +/- 15 kts. Differences between these composites are noted in the underlying sea surface temperature (SST), and in the magnitude and direction of the environmental vertical wind shear (VWS). The authors tend to emphasize the differences in the direction of the VWS. Building on the recent examination of the quasi-steady flow topology in an idealized numerical experiment of a TC in VWS by Riemer and Montgomery (2011), Shu et al. argue that the VWS direction in the weakening composite promotes interaction with dry environmental air while the VWS direction in the intensifying composite constrains such interaction.

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Interactive Discussion

Discussion Paper

The manuscript is well written and demonstrates (in this composite framework) a distinct evolution of environmental air masses relative to the TC in line with Riemer and Montgomery's hypothesis. This is a valuable and interesting result. The manuscript contains several other aspects, however, that do not appear to be novel (e.g. dependence of intensity on VWS magnitude and SST). The authors should emphasize and focus on the novel aspects of their work and prune the remaining material. I'll leave it to the editor to decide if the manuscript contains enough novel material to warrant publication. Furthermore, there are conceptual issues with the authors' discussion of intrusion of dry air into the TC circulation and their attribution of differences in the intensity evolution to the direction of VWS. It is important to remedy these issues before final publication in ACP. In addition, several minor revisions should be considered, as outlined below.

Major, conceptual issues:

1) Disentangling individual contributions

a) Several processes may lead to TC intensity change. The authors clearly recognize the well-known role of the SST and VWS magnitude. To me, it did not become sufficiently clear why the authors believe that they can provide evidence that the VWS direction plays a significant role also. Rephrasing: It did not become clear to me how the authors can (conceptually & methodically) disentangle the contributions from individual processes. b) Besides VWS magnitude and SST value, I can see two more factors that appear to be associated with the observed intensity changes. First, SSTs in the weakening composite are not only lower than in the intensifying composite, they also _decrease_ with time (by 1.6 K between 0h and 48h, as compared to 0.3 K in the intensifying composite). Basic axisymmetric, steady-state theory (e.g. by Emanuel) predicts weaker TCs for decreased SST. Second, the intensifying composite appears to comprise TCs that are early in their life cycle, i.e. rather soon after their formation. Thus I'd suspect that these TCs are weaker and have a higher potential for intensification than their counterparts in the weakening composite. The storms in that composite

ACPD
13, C11535–C11539, 2014

tend to start recurvature already. One way to account for this point might be to take into consideration the difference between current intensity and Emanuel's potential intensity.

2) Underlying intensity change theory It remains unclear to me on what underlying theory/ conceptual model of TC intensity change the authors base their discussions. The authors refer rather vaguely to the detrimental impact of VWS or to intrusion of dry air into the TC circulation (see also below) and reference to several different ideas is given in the introduction. The discussion of different processes in this manuscript would greatly benefit if the authors explicitly stated their underlying conceptual ideas in the introduction.

3) Intrusion of environmental dry air into TC circulation Much of the discussion in this manuscript focuses on the intrusion of dry environmental air into the TC circulation. Throughout the manuscript, however, it remains unclear what exactly the authors mean with the terms "intrusion", "inner core", and "TC circulation". It seems to me that the approach of dry air to within 500 km and 300 km qualifies for the authors as intrusion into the inner core. With "TC circulation" the authors seem to refer to the primary circulation, i.e. the swirling winds. Therefore, the authors seem to invoke a "guilt-by-proximity" argument, which is a questionable argument at best (see critical discussion of the "guilt-by-proximity" concept in Braun 2010). It is intrusion into the _secondary_ circulation of the TC that is needed to impact TC intensity, at least based on the idea that VWS acts as a constraint on the TC's heat engine (Riehl and Simpson (1958), Tang and Emanuel (2010)). A thorough clarification of the authors' concept of "intrusion into the TC (inner core) circulation" is required in the revised version of the manuscript.

Minor issues:

(the two last digits of the page number are given)

pg 16, line 12: "bring" where to?

ACPD

13, C11535–C11539, 2014

p18, line 2: Simpson and Riehl (1958) seems to be the more appropriate reference for the ventilation idea.

p18, line 2-7: As the authors mention the mid-level and upper-level pathways, reference to the low-level pathway (Riemer et al. 2010, 2013) should be included also.

pg 18: The link between the individual aspects should be improved.

pg 20: Clarification would be helpful why the effect of the WNPSH should not be included in the environmental parameters considered in Zeng et al. (2007).

pg 21, line 21: What is the motivation to consider a radius of 800 km?

pg 22, top of page: It would be interesting to note what percentage of all storms the selected sample constitutes.

It seems redundant to show all of the individual panels in Figs. 3,4,6,7. There is very little difference between 500 hPa and 400 hPa and slow temporal evolution.

pg 24, line 26: -12h

pg 25, top of page: The discussion here can only be appreciated by the reader in association with Fig. 11. I suggest re-iterating the hypothesis by Riemer and Montgomery (2011) on the role of VWS direction earlier in the manuscript. Talking about Fig. 11: Panels (b) and (d) are close copies of Fig. 10 in Riemer and Montgomery (2011). It seems fair to note in the caption that the schematic is based on their figure. Furthermore, the description/ discussion of the schematic requires a much more comprehensive explanation of the flow topology, i.e. the streamline pattern.

pg 25, line 18, “closer to the TC”: I cannot see this in the figure.

pg 26, line 2: I think it would be fairer to say that the dry air is in two, not one, quadrants in the intensifying composite.

pg 27, line 20: “bring” where to?

ACPD

13, C11535–C11539, 2014

pg 27, line 24, “no significant difference”: This statement is in contrast with the discussion above. Furthermore, I have trouble identifying the discussed features in Fig. 9. The wind vectors are rather small.

End of pg 27, top of pg 28: How much do differences in the outflow asymmetries contribute to the VWS metric? Such differences could compromise the characterisation of the environmental VWS.

pg 29, second paragraph: I am not sure that I can see the asymmetry in vertical motion in Fig. 8. Is the data actually suitable to consider inner core asymmetries?

pg 33, last sentence: To strengthen the statement, it should be noted explicitly what these “detailed physical processes” are.

I strongly encourage the authors to use SI units throughout the manuscript. One exception may be the use of the intensity change magnitude from the operational forecast centres (e.g. table 1).

References:

Simpson, R. H. and Riehl, H.: Mid-tropospheric ventilation as a constraint on hurricane development and maintenance, Proc. Tech. Conf. on Hurricanes, D4.1âĂŤD4.10, Amer. Meteorol. Soc., Miami, FL, 1958.

Riemer, M., M. T. Montgomery, and M. A. Nicholls, 2013: Further examination of the thermodynamic modification of the inflow layer of tropical cyclones by vertical wind shear, Atmos. Chem. Phys., 13, 327-346, www.atmos-chem-phys.net/13/327/

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 31815, 2013.