<u>Reply to the Anonymous Reviewer # 1:</u>

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 intensityevolution with a threshold of +/- 15 kts. Differences between these compositesare noted in the underlying sea surface temperature (SST), and in the magnitude anddirection of the environmental vertical wind shear (VWS). The authors tend to emphasizethe differences in the direction of the VWS. Building on the recent examination ofthe quasi-steady flow topology in an idealized numerical experiment of a TC in VWSby Riemer and Montgomery (2011), Shu et al. argue that the VWS direction in theweakening composite promotes interaction with dry environmental air while the VWSdirection in the intensifying composite constrains such interaction.

The manuscript is well written and demonstrates (in this composite framework) a distinctevolution of environmental air masses relative to the TC in line with Riemer andMontgomery's hypothesis. This is a valuable and interesting result. The manuscriptcontains several other aspects, however, that do not appear to be novel (e.g. dependenceof intensity on VWS magnitude and SST). The authors should emphasize andfocus on the novel aspects of their work and prune the remaining material. I'll leaveit to the editor to decide if the manuscript contains enough novel material to warrantpublication. Furthermore, there are conceptual issues with the authors' discussion ofintrusion of dry air into the TC circulation and their attribution of differences in the intensityevolution to the direction of VWS. It is important to remedy these issues beforefinal publication in ACP. In addition, several minor revisions should be considered, asoutlined below.

<u>A:</u> **We thank the reviewer for the very thorough review of our manuscript and for the insightful comments. With respect to the general comments on the novelty of our study in light of Riemer and Montgomery's recent studies, we believe this study is quite complementary and unique in that this is the first observational study based on best track estimate, global reanalysis and satellite retrievals of moisture that confirms some of the hypotheses of Riemer and Montgomery (RM, 2010, 2013) on the impact of directional shear with respect to environmental moisture distribution, while the RM studies are based on idealized TC simulations under limited variations in the large-scale environment. Through compositing analyses of a large number of observed events, this is also the first systematic study to document the close relationship between dry air associated with subtropical high and intensity changes of TCs over the WNP. The influence of the Saharan air layer (SAL) on the growth of TCs over Atlantic basin has been studied over the past few decades.**

Nevertheless, according to per comments from both reviewers, we have done some additional analyses which include(1) dividing the intensifying/decaying events based on their relative locations to the WNPSH, (2) examining the shear-induced downdrafts flux low θe air into the inflow layer of TC, and (3)

comparing the maximum potential intensity between the two groups (intensifying versus weakening events). Also, we made all the minor fixes and improvements as the two anonymous referees suggested in our revised manuscript. Our point-to-point responses to your comments are as follows:

Major, conceptual issues:

1) Disentangling individual contributionsa) Several processes may lead to TC intensity change. The authors clearly recognizethe well-known role of the SST and VWS magnitude. To me, it did not become sufficientlyclear why the authors believe that they can provide evidence that the VWSdirection plays a significant role also. Rephrasing: It did not become clear to me howthe authors can (conceptually & methodically) disentangle the contributions from individualprocesses.

A: **Thanks for your comments. We agree that several processes may lead to intensity change of a TC, that is why we examined several candidate environmental factors including VWS (magnitude and direction), moisture field associated with the dry intrusion, CAPE, vertical velocity, and SST (as well as MPI and DFX (Riemer et al. 2010, 2013) in revised version) in our study. We assumed that a distinction could be found between the intensifying and weakening cases if the WNPSH played a role in intensity change of TCs. In the meantime, we acknowledge that as a common to most observational studies, it is very difficult (if not possible) to isolate individual contributions completely. For this reason, the 48-hour intensity change in the maximum sustained wind speed is categorized into weakening, neutral (no or small change) and intensifying and we select the intensifying and weakening cases to discuss the impact of the WNPSH on TC intensity. Note that doing so does not mean that the WNPSH has little influence on the neutral cases, but the cases with significant intensity change give a clearer signal on the influence of the WNPSH (and the associated dry air) than those neutral cases that may be more affected by the combination of different factors that are harder to separate.**

We also performed the significance test that shows the composite differences in VWS between the two groups were statistically significant at all times, especially for the VWS direction, which were totally opposite for the two groups. Although it may be hard to separate the individual factor contributions, the distribution of moisture, vertical velocity and CAPE were all in accordance with the VWS fields in the west and southwest sides of TC circulation, where the dry air is closer to the TC center under the westerly shear for the weakening events. In summary, our observational study complements and verifies the idealized studies of Riemer and Montgomery(2010, 2013) as one can never be so sure about hypotheses from idealized simulations unless verified by observations.

b) Besides VWS magnitude and SST value, I can see two morefactors that appear to be associated with the observed intensity changes. First, SSTsin the weakening composite are not only lower than in the intensifying composite, theyalso _decrease_ with time (by 1.6 K between 0h and 48h, as compared to 0.3 K in theintensifying composite). Basic axisymmetric, steady-state theory (e.g. by Emanuel)predicts weaker TCs for decreased SST. Second, the intensifying composite appearsto 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 intensificationthan their counterparts in the weakening composite. The storms in that composite tend to start recurvature already. One way to account for this point might be to takeinto consideration the difference between current intensity and Emanuel's potential intensity.

<u>A:</u> Yes, we agree with the reviewer that besides VWS, the difference in SST between the two groups may also contribute to the difference in intensity changes since the SSTs in the weakening composite are not only lower than in the intensifying composite, they also decrease with time. However, the mean SST of the weakening cases is above 27 C was still high enough to support strengthening by past observed and idealized studies which by itself shall not be the direct cause of weakening. In other words, the impact of the SST might be regarded as a less positive factor for the weakening events from a perspective of direct influences of SSTs.

The reviewer also made an intriguing and valuable comment in that "*the intensifying composite appears to comprise TCs that are early in their life cycle, weaker and have a higher potential for intensification than their counterparts in the weakening composite*". We conducted further analysis accordingly. Firstly, we divided the intensifying/decaying events based on their relative locations to the west of the WNPSH, where the dry air associated with the WNPSH was evident from the satellite data (Fig. 2). In this way, the two event groups were in more or less the same stage of their lifecycle. All the events with the subselection were located to the west of the WNPSH while the averaged intensity for the weakening cases (33.2 m s^{-1}) were approximately similar to the intensifying cases (32.8 m s^{-1}) at t+48h. All the composites fields including VWS (magnitude and direction), CAPE, moisture and streamline for the two groups were similar to those without these additional geographic restrictions.

Moreover, we examined the shear-induced downward flux of low θe air into the inflow layer of TC (DFX) following Riemer et al. (2010, 2013) and found that the distribution of DFX for the weakening cases was distinguished from that for the intensifying cases, with the positive DFX (denoting downward flux of anomalously low θe into the boundary layer of TC) dominating the northwestern quadrant for the former in comparison with the negative values over the same region for the latter. In addition, the descending area outside the eyewall coincides with the anomalously low θe air brought in from the environment for the weakening cases, suggesting a magnifying negatively impact of dry air intrusion in this quadrant. Although it remains unclear how precisely the drier air entered the eyewall updrafts and finally inhibited intensity of TC due to the coarse resolution of the GFS FNL data we used, the apparent difference of DFX

between the two groups demonstrates clear links between the environmental moisture content and TC intensity, which complements recent idealized studies of Riemer et al. (2010, 2013).

Also in response to your comments, we added the POT (the difference between the maximum potential intensity (MPI) and the current intensity of a TC) in Table 3 in the revised manuscript. We compared the maximum potential intensity and found that the differences of POTs between the two groups were also statistically significant. Take t=+48h for example, for the weakening and intensifying cases, the POT were 35.5 m s^{-1} and 46.8 m s^{-1}, respectively. However, the positive values of POTs for both cases suggested that the thermodynamic environment all had the potential to support the TC to intensify, which further supports our hypothesis on a less positive effect of SST for the weakening cases.

2) Underlying intensity change theory It remains unclear to me on what underlyingtheory/ 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 ofdry air into the TC circulation (see also below) and reference to several different ideasis given in the introduction. The discussion of different processes in this manuscriptwould greatly benefit if the authors explicitly stated their underlying conceptual ideas inthe introduction.

A: Thanks for your comments. As noted above that there are several processes that may lead to intensity change of a TC. We first introduced several likely relevant hypotheses and theories from recent studies on the negative versus positive effects of different processes on TC intensity changes. Our underlying conceptual theory is closely related to the recent idealized studies of Riemer et al. (2010, their Figs. 1, 2) in that the drier and cooler air (low θe) could serve as "anti-fuel" for the TC power when they enter the core region of a TC intensity, especially at mid-levels where mixing of low θe air into the eyewall is thought to be particularly effective. We have explicitly stated it and changed some texts to improve link in the revised introduction.

3) Intrusion of environmental dry air into TC circulation Much of the discussion in thismanuscript focuses on the intrusion of dry environmental air into the TC circulation.Throughout the manuscript, however, it remains unclear what exactly the authors meanwith the terms “intrusion”, “inner core”, and “TC circulation”. It seems to me that theapproach of dry air to within 500 km and 300 km qualifies for the authors as intrusioninto the inner core. With “TC circulation” the authors seem to refer to the primarycirculation, i.e. the swirling winds. Therefore, the authors seem to invoke a “guilt-byproximity”argument, which is a questionable argument at best (see critical discussionof 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 ideathat VWS acts as a constraint on the TC’s heat engine (Riehl and Simpson (1958), Tangand Emanuel (2010)). A thorough clarification of the

authors' concept of "intrusion intothe TC (inner core) circulation" is required in the revised version of the manuscript.

<u>A:</u> This is a fair comment about the limitations of the current observational composite study with a global analysis which we have acknowledged explicitly in the revision. Due to the relatively low-horizontal (1°x1°) resolution of the GFS FNL data and the limited availability of the inner-core observations (if at all) being assimilated into GFS, some structures within the eyewall or near-core region can not be well depicted. We hereby loosely define the area with a radius of 400 km from the TC center as the inner-core region of a TC, and the broader area of cyclonic circulation as the "TC circulation". We have clarified these concepts before using them in the revised manuscript.

Even with the aforementioned limitations, we believe our current study goes well beyond the "guilt-by-proximity" scenario shown by Braun (2010). Braun argued that "in many cases, attribution of storm weakening to the SAL is based upon the proximity of SAL air near the time of storm weakening rather than a clear demonstration of the direct impacts of the SAL". He gave two examples to explain his "guilt-by-proximity". One is in Dunion and Velden (2004), who subjectively determined the proximity of dry air as the dry SAL air without examining the origination of dry air long before storm weakening. Another is in Jones et al. (2007) in which they examined the evolution of Hurricane Erin (2001). Very dry air aloft (500-300-hPa layer) was incorrectly attributed to the SAL, but is clearly at heights typically above the SAL. In Braun's work (2010), separate composites for strongly strengthening and weakening storms showed few substantial differences in the SAL characteristics between these two groups. So he suggested that the SAL is not a determinant of whether a storm will intensify or weaken in the days after formation. In the current study, we examined a period of 60h (from t-12h to +48h) for the time evolution of the dry air. It is evident from the composite that the dry air originates from the anti-cyclonic subtropical high (from wind vectors at time t_{-12h}). Also, the differences of moisture fields between the two groups were statistically significant, unlike the similar RH distributions for the two groups in Braun (2010, their Fig. 14). Above all, we believed that we have demonstrated quite systematically that the dry air associated with the WNPSH has impact on TC intensity despite limitations the coarse observational dataset used.

Minor issues:
(the two last digits of the page number are given)
pg 16, line 12: "bring" where to?

<u>A:</u> Changed to "....bring warm moist air from the south and southeast to its southeast quadrant within 500 km".

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

<u>A:</u> Changed.

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

<u>A:</u> Yes, the reviewer is correct that there are two prevailing mechanisms for the adverse effect of vertical wind shear on storm intensity: (1) ventilation of the TC core with dry environmental air at mid-levels and (2) the dilution of the upper-level warm core. The low-level pathway proposed by Riemer et al. (2010, 2013) is now included in the revised manuscript.

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

<u>A:</u> The sentences on pg 18 have been rearranged or rewritten in order to improve the link between the individual aspects.

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

<u>A:</u> We have added another reference and some new sentences to explicitly clarify why the influence of the WNPSH were not be considered when developing MPI in Zeng et al. (2007).

As Fu et al. (2012) noted, the background climatology in the WNP is different from that in the North Atlantic (NATL). Unlike the NATL, where easterly trades and the subtropical high occupy the majority of tropical basins, the easterlies and southwesterlies associated with the WNPSH (and also the western Pacific monsoon) are important components of summer circulation in the WNP (their Fig. 1). Because the answer of whether and how the WNPSH influences TC intensity has not been obtained, some negative or positive environmental parameters associated with the WNPSH were not taken into account in developing MPI in Zeng et al. (2007). This can partly explain why most TCs could not reach their MPIs even in the favorable environment in their study.

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

<u>A:</u> A radius of 800 km is chosen somewhat subjectively in this work, however, at least in the sense that a distance within 800 km indicates a more possible interaction between the dry air associated with the WNPSH and TC considering the scales of them.

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

<u>A:</u> The sentence has been changed to: After a careful analysis of the synoptic weather maps in GFS analyses, as well as the AIRS-AMSU satellite data, 37 TCs

consisting of 1472 sample times which constitute about 56.4 percent of all 60 TCs selected from2000 to 2011were examined in this paper.

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.

A: We have kept the penels at 500 hPa and deleted those at 400 hPa in Figs. 4, 6, 7 since there is little difference between 500 and 400 hPa according to your suggestions. Associated texts have also been changed in the revised manuscript.

pg 24, line 26: -12h

A: Done.

pg 25, top of page: The discussion here can only be appreciated by the reader inassociation with Fig. 11. I suggest re-iterating the hypothesis by Riemer and Montgomery(2011) on the role of VWS direction earlier in the manuscript.

A: Thank you for your valuable suggestion. We have rewritten the paragraph by reiterating the hypothesis by Riemer and Montgomery (2011) and improving the link between former figures and schematic diagram Fig. 11.

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 morecomprehensive explanation of the flow topology, i.e. the streamline pattern.

A: We did not directly or intentionally copy the right panels in Fig. 11 from Fig. 10 in Riemer and Montgomery (2011), although we admit there are some similarities and we benefit from the illustrative schematics in their study. Our Fig. 11 is based primarily on our composite analysis of the two groups while we indeed referred to the display methodology used by Willoughby et al. (1984, their Fig. 18) and Riemer and Montgomery (2011, their Fig. 10). Also, in order to make our results more robust, a new section 4.3 (the original section 4.3 Underlying SST has been changed to 4.4) has been added in the revised manuscript, in which the composites are further subdivided into weakening/intensifying cases to the west of the WNPSH in order to reveal factors that affect storm intensity locally with geographic restrictions. According to the composite streamlines for the weakening and intensifying cases, we plotted Fig. 11 (which has been changed to Fig. 12 because we have added a new figure in the revised manuscript) with solid line indicating the TC inner core-region boundary, and dashed line indicating the storm relative environmental flow. The regions of dry and moist air in this figure also represented the actual areas of dry or moist air in earlier composite results, which are totally different from the shading area indicating a hypothetical region of dry air in Riemer and Montgomery (2011,

their Fig. 10). According to your suggestion, we have added some description of the schematic diagram in the caption. Also, we have cited the previous studies of Willoughby et al. (1984) and Riemer and Montgomery (2011) in the discussion of the main text.

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

A: **The time evolution of mid-tropospheric moisture for the strengthening cases is significant. In comparison with 24 hours before (t0h), we stated that "the cyclonic TC circulation may have begun to bring some of the dry air associated the WNPSH closer to the TC to the northwest at t+24h".**

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

A: **The sentence has been changed to "..... the mid-tropospheric dry air is present in nearly three quadrants (from east to north to west) of the TC circulation for the weakening events but only about two quadrants (north and west) for the strengthening events".**

pg 27, line 20: “bring” where to?

A: **Changed to ".....bring abundant dry air in the northwest/west side of the TC (subsiding dry intrusion) to its southwestern quadrant".**

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

A: **We revised the confusing to read as "Although there is not remarkable difference in the magnitude of the average environmental VWS between the two groups, the spatial distributions of the VWS for the two groups are very different" in the revised manuscript. Also, the wind vectors in Fig. 9 have been thinned out to make the direction of VWS clear in revised manuscript.**

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

A: **The outflow asymmetry at 200 hPa could be seen by the wind vectors in Fig. 10. Since we used the deep-layer vertical wind shear between 200 hPa and 850 hPa, the comprehensive asymmetries between the two groups should be shown by the shear in Fig. 9.**

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

A: Although the GFS data we used did not have high resolution, the asymmetry in vertical velocity (shown by shading) in inner core region (the inner circle with a radius of 300 km from the storm center) was still obvious, with the strongest updraft (represented by the purple area) occurring in the downshear-left side in the inner core for both the weakening (Fig. 8a) and the strengthening cases (Fig. 8b).

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

A: The last sentence has been changed to "The detailed physical processes such as how cooler and drier air enters the core region and inhibits the development of a TC, as well as which environmental factor dominants the intensity change of a TC will be discussed by using a numerical model in the near future".

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

A: We have checked the whole manuscript to use SI units as much as possible according to your suggestions, except for Table 1 and Table 3.

References:

Simpson, R. H. and Riehl, H.: Mid-tropospheric ventilation as a constraint on hurricanedevelopment and maintenance, Proc. Tech. Conf. on Hurricanes, D4.1âˇAˇ TD4.10,Amer. Meteorol. Soc., Miami, FL, 1958.

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

A: We have added this two and some other references in our revised manuscript.