

Reply to the Anonymous Reviewer # 2:

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.

A: We thank the reviewer for the very thorough review of our manuscript and for the insightful comments.

Yes, the primary objective of this study is 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 scientific issue of relationship between dry air associated with subtropical high and intensity changes of TCs over the WNP has not been systematically studied, while the influence of the Saharan air layer (SAL) on the growth of TCs over Atlantic basin has been studied over the past few decades. An recent observational work by Fu et al. (2012) demonstrated that the differences of the characteristics of the subtropical high during the summer season contributed to the different environmental conditions between the WNP and Atlantic basin (their Fig. 1), therefore, the relationship between the WNPSH and intensity changes of TCs is an important and unique issue especially in the most active basin for TC activities in the world.

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 θ_e 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 concerns:

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 θ - 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 θ - 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.

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). According to the composite streamlines (at 500 hPa and 700 hPa) for the weakening and intensifying cases, the solid line in the new Figure 12 indicates the TC inner core-region boundary, and dashed line indicates 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 both different from the shading area indicating a hypothetical region of dry air in Riemer and Montgomery (2011, their Fig. 10). In the revised manuscript, 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.

Moreover, 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. The differences of composites fields such as streamline, VWS (magnitude and direction), vertical

velocity, CAPE between the two groups were all similar to those without these additional geographic restrictions, suggesting that the dry air intrusion and the vertical wind shear (VWS) associated with the WNPSH, indeed affect the intensity changes of TCs over the WNP beyond the difference related solely to variations in geographical locations.

In addition, we examined the shear-induced downward flux of low θ_e 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 positive DFX (denoting downward flux of anomalously low θ_e into the boundary layer of TC) dominating the northwestern quadrant for the former in comparison with negative values over the same region for the latter. In addition, the descending area outside the eyewall coincided with anomalously low θ_e air brought in from the environment for the weakening cases, suggesting an amplified negative impact of dry air intrusion there. 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). We have stated this mechanism and changed some texts to improve link in our revised manuscript.

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 support 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?

A: 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.

We also conducted further analysis in dividing 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 in the same stage of their life cycle. 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 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 statistically significant. Take time t+48 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 lesser positive effect of SST for the weakening cases.

Minor concerns:

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

A: Done.

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.

A: “storm-relative” means storm-centered in such that all events are overlapped at the TC center in the composite.

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?

A: See Major 1). Above.

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.

A: We have rewritten the misleading expression and changed to "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.

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

A: Changed to "within" and "exist".

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

A: Changed.

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

A: Due to relatively low-horizontal ($1^\circ \times 1^\circ$) resolution of the GFS FNL data we used as well as limited availability of inner-core observations being assimilated into GFS, some structures within the eyewall or near-core region cannot 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 revised manuscript.

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

A: Changed.

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 vertical motion rather than lightning flash rate.

A: Thank you for your reminding. We have cited this recent study in the revised manuscript.

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

A: Done.

11) 31830 line 1: should be "convective"

A: Done.

12) 31830 line 9: In what sense does the TC "interact" with the dry air? See Majorconcern 1).

A: See our response to the Major comment (1). Above.

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

A: They did not appear maybe due to the PDF conversion in the uploaded

document. We have added them in the revised manuscript.

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?

A: See our response to the Major comment (1). Above.