

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

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We thank the handing editor (Tim Dunkerton) and the two reviewers for their very thorough review of our manuscript and for the insightful comments.

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 dif-

C13340

ferences 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.

With respect to the general comments on the novelty of our study, 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. It is obvious that one can never be so sure about hypotheses from idealized simulations unless verified by observations.

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.

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 the reviewers' comments are in the supplemental PDF files.

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