

Interactive comment on “The Spring Transition of the North Pacific Jet and its Relation to Deep Stratosphere-to-Troposphere Mass Transport over Western North America” by Melissa Leah Breeden et al.

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Received and published: 10 December 2020

Interactive comment on “The Spring Transition of the North Pacific Jet and its Relation to Deep Stratosphere-to-Troposphere Mass Transport over Western North America” by Melissa Leah Breeden et al. Author response to RC#1

This is one of the most polished papers I have reviewed in several years! The investigation is comprehensive, well motivated, and the text and figures are of high quality. It is an excellent contribution to the field and was a pleasure to read. Many thanks to

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the authors for the careful and considerable work put into this manuscript! I have only a handful of comments/suggestions outlined below.

R: We appreciate your attention to our manuscript and are very pleased you found the results of interest!

Lines 41-44: It seems that an annual cycle of tropopause altitude would also be relevant here. Perhaps it frequently reaches a minimum in late winter/early spring.

R: We had not thought of that, thank you for raising this point. The tropopause is certainly lowest in the extratropics during winter and into early spring (Seidel and Randel 2006), while the maximum in STT-PBL is confined to spring, so it is difficult to ascertain how much the lower tropopause itself aids in STT-PBL. We suspect forcing for regionalized areas of descent are the dominant process responsible for the STT-PBL differences between winter and spring, as stated in Skerlak et al. 2014.

Line 82: Why is 2.5° resolution data used instead of finer data that is available? It seems reasonable to me since JRA-55 is used mainly for context, but a simple justification along those lines would be nice to include.

R: Thank you for raising this point, since we were interested in large-scale zonal wind variability, using 2.5X2.5 degree resolution seemed sufficient, further supported by the fact that the ERA-Interim results at higher resolution yielded nearly identical results, considering PC1 of 200-hPa zonal wind. We included this point in the text (lines 84-85).

Lines 116-127: The mention of "dynamic tropopause" here is sudden. I would recommend simply specifying the range of PV thresholds used in these studies. Otherwise, the discussion could become confusing and is a bit nuanced as is.

R: Thank you for raising this point, we have modified the text to clearly link the 2-PVU boundary to the dynamic tropopause definition (lines 133-134).

Line 180: I recommend citing Rossby wave breaking studies supporting this statement.

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R: Thank you for this suggestion, we agree and have included references (lines 205-206).

Line 307: I would recommend also pointing out that the ENSO-jet relationship is consistent with Rossby wave breaking activity given the occurrence of the jet breaks during La Niña.

R: We have added a sentence highlighting this connection (lines 332-333).

Line 339-347: Excellent points here! :)

R: Thank you, we felt it very important to distinguish mass and ozone changes associated with ENSO, given the added complexity when ozone transport is considered.

Please also note the supplement to this comment:

<https://acp.copernicus.org/preprints/acp-2020-604/acp-2020-604-AC1-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-604>, 2020.