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Atmospheric Chemistry and Physics Discussions

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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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.

RC#2

This study presents the results of a thorough statistical analysis of springtime stratosphere-to-troposphere transport to the boundary layer (STT-PBL) over the west-

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ern USA, in the context of the jet structure transition between the winter and summer regimes. The authors use wind fields from the ERA-Interim and JRA-55 reanalyses to identify the dominant Pacific wind patterns at the jet level by the way of EOF analysis and employ previously developed methods to calculate STT-PBL and associated diagnostics. They demonstrate that the intensity of springtime STT-PBL is a function of the timing of the jet transitions, with early transitions leading to more intense STT-PBL driven by deeper and more frequent tropopause folds. Furthermore, they show that the transition timing is correlated with ENSO, and carefully investigate the mechanisms involved, by performing a simultaneous analysis of Rossby wave propagation diagnostics, tropopause folds diagnostics, and PBL depth distributions. The paper is really well written, logically constructed and easy to follow, which is no small feat given the complexity of the subject. It also does a great job referencing the sizable body of literature on the topic. I think the analysis and conclusions are very solid and the paper meets all the ACP criteria for publication. Despite my best efforts (it's my job after all;)) I couldn't find almost any issues with the analysis or presentation. It's a great and important paper and I enthusiastically recommend it for publication almost as is. I do have a few very minor suggestions for edits. The most important one concerns the methods section that, I think, would benefit from adding some more details (even if those details can be found elsewhere) that would make the paper more self-contained.

R: Thank you for the feedback and the attention to our manuscript, we have incorporated your suggestions into our revisions, outlined below.

Minor and technical comments

L82-86. What's the vertical resolution of each reanalysis and why is it sufficient for the present purposes, particularly for driving the trajectory model?

R: For JRA-55 reanalysis we only consider zonal wind on a single pressure level, with a focus on large-scale patterns of variability that do not require high vertical resolution to be determined. The ERA-Interim data used to track folds and STT-PBL is on the 60

original hybrid model levels which extend from the surface to 0.1 hPa, vertical resolution suitable enough to identify tropopause folds (Skerlak et al. 2015). We have added this information to the text (lines 88-91).

LL85-89. Can you comment on how the changing observing system (pre-satellite to satellite to more satellites) in JRA-55 impacts the results?

R: Thank you for raising this point; the JRA-55 documentation shows that temperature values across the varying observational record better match observations compared to JRA-25 in the troposphere and lower stratosphere (Kobayashi et al. 2015), suggesting variations across the changing record are minimal. Also, time series of PC1 shows no discernible transition at the start of the satellite record (figure below), suggesting that the seasonal cycle of the jet captured here is not sensitive to any potential changes in the data associated with changes in the observational record. We have included some of this information in the text (lines 88-89).

L91-93. Can you expand this paragraph a bit? This is one of the main tools used here, and I found it hard to get even a general idea of what's being done there without reading Skerlak et al. 2014. For example, how are the trajectories calculated? Perhaps a simple diagram in the supplementary material?

R: Thank you for raising this point. We have expanded the description of how STT-PBL was determined so that it is (hopefully) easier to follow without having to read earlier papers (lines 105-110).

LL 116-118. Is it possible that this is resolution dependent? Can you comment on that?

R: I believe the question is whether the correspondence between the 2-PVU surface and terminus of the fold is a function of resolution. I do not think, even with perfect observations, that there would be an exact correspondence between the boundary of the fold and the 2-PVU surface. Folding really represents a filamentation of the lower tropopause boundary, involving distortion of lower but still near-tropopause PV values

ranging from 1-4 PVU (Skerlak et al. 2015), which we consider as the main reason that slightly lower PV values better correspond to the terminus of the fold. Studies using different reanalysis at different resolutions (e. g., Breeden and Martin 2018; Albers et al. 2018) reflect similar fold structures, also suggesting that resolution does not affect the correspondence between the fold boundary and PV.

LL207-210. I like the idea of stating the main results in the first concise paragraph. However, at the initial pass, it wasn't clear to me if the second and third sentences are the results or something that we already know. How about something like "The main findings are:. . ." after the first sentence?

R: Thank you for this suggestion, we have modified the text to confirm these results are the main results of this section (line 231).

L220. I'm guessing the bimodality in 5b arises from an oscillation between the two phases during the transition period. It's pretty neat. Can you add a one/two-sentence comment on that?

R: This is an interesting idea, but the bimodal distribution is observed during the positive jet phase, not the transitional phase. Still I think your interpretation is correct – indeed, the leading mode of jet variability during winter looks like the transition (e. g., Athanasiadis et al. 2010), so I think the bimodality is related to wintertime variability, including variability that happens to look like the transition. We have added a comment about this in the text (line 246).

L275. Variability reflected in Fig. 3, right? If so, can you reference Fig. 3 explicitly?

R: That is correct, we have included a reference to Figure 3 (line 301).

LL344-345. I'm struggling to understand this sentence. Please rephrase; it looks like an important point is being made there.

R: Thank you, yes this is an important point in distinguishing mass versus ozone transport when ENSO is involved, as ENSO affects both fold frequency (as we show), as

well as the lower stratospheric ozone reservoir (Albers et al. 2018), thereby affecting how much ozone is transported within each fold (which we do not consider in this study). We have clarified the text (lines 372-373).

Fig. 3 caption. The dashed lines are +/-1 sigma, right?

R: The black dashed lines in Figure 3 show the +/- 0.5 sigma values, since the +0.5 sigma value was used to track the spring transition, and +/- 0.5 sigma range defines the 'spring' jet phase (Fig. 2). We have added this description to the caption.

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2020-604/acp-2020-604-AC2-supplement.pdf

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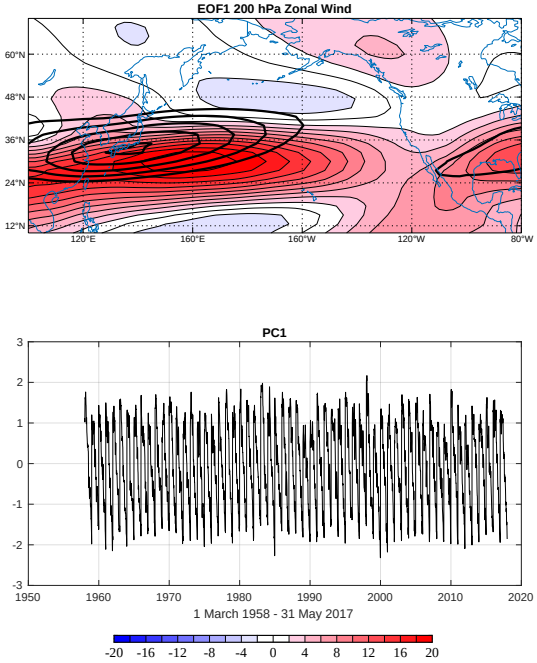

Fig. 1. EOF1 pattern and PC1 using JRA-55 reanalysis, 1958-2017.

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