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Interactive comment

# *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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The paper by Breeden et al. (2020) explores the relationship between the spring transition of the north Pacific jet and stratosphere-to-troposphere mass transport to the planetary boundary layer (STT-PBL) over western North-America. Additionally, the spring transition is linked to the state of the El-Niño Southern Oscillation (ENSO). Analyses are based on the JRA-55 and ERA-interim reanalysis datasets. Interannual variability in the spring peak in STT-PBL found in previous studies is shown to relate to the timing of the spring transition, with larger values of STT-PBL for earlier transitions. Finally, ENSO is found to modulate this timing, with earlier transitions being more prevalent for La-Niña conditions and vice versa. This study adds to previous research by providing underlying mechanisms behind the (variability in the) spring peak in STT-PBL over western North-America that was found before. These results suggest that STT-PBL strength can be predicted based on knowledge of the north Pacific jet and ENSO state in the preceding months. This is relevant for, for instance, air quality prediction at the surface as STT-PBL can function as a natural source of ozone in the PBL. In general, the paper is well-written; the structure and headers of the paper are clear and help to understand the research, the figures diversely visualize the results in both maps and time series, the physical mechanisms discussed in the paper are consistently explained well and create a logic story and the results are frequently discussed in light of previous research. Moreover, I think the paper fits nicely in the scope of this journal. Despite the relatively local study area, the implications of this study are thought to be generally applicable in atmospheric sciences and can therefore be extended to other locations around the world that show similar dynamical patterns. Nevertheless, a significant revision of this paper is required before it can be accepted for publication in my opinion.

Most importantly, in the calculation method of STT-PBL described in line 91-96 (section 2.1) of the paper I miss an assessment of the uncertainty in the calculation that is associated with the sensitivity to parameter choices. Previous studies have shown that this sensitivity might not be insignificant, so that the specific parameter choices could substantially affect the outcome of the results in section 3.2 and 3.3 of this study. Firstly, Holton et al. (1995) show that the 380 K potential temperature surface used in the paper by Breeden et al. (2020) as (one of) the definition(s) of the tropopause





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coincides relatively well with the tropopause in the tropics, but that this value drops for higher latitudes to approximately the 340/350 K potential temperature surface in the region that is considered in the paper (western North-America). Secondly, Skerlak et al. (2014) showed that their STT(-PBL) calculation is highly sensitive to the choice of minimal residence time of the air in the stratosphere/troposphere before/after a crossing of the tropopause. They find that this sensitivity can be very well approximated by a power law with an exponent of -0.5, which means that the STT estimates for a residence time of 24 and 48 hours respectively deviate by as much as 30 percent. The choice is made by Skerlak et al. (2014) to use a constant value of 48 hours for this parameter. This is validated based on the fact that, although the calculated values of STT-PBL are highly sensitive to the parameter value, the geographical distribution of STT-PBL in which the authors of that study are interested is not. However, in the study of Breeden et al. (2020), this validation does not hold anymore as the STT-PBL calculation is applied to the specific region of western North-America instead of it being used to assess the geographical distribution. Thirdly, the STT-PBL calculation also depends on the accuracy of the PBL height forecast, as this influences the number of trajectories that reach the PBL from the stratosphere. To determine the PBL height the critical Richardson number value of 0.25 is used as the criterion for the PBL top (i.e. the transition from turbulent to laminar flow at the top of the PBL). Troen and Mahrt (1986) indicate that this critical value used for the Richardson number does not have a large influence on the PBL height estimation in unstable conditions, but that it does induce variability in PBL height for neutral conditions. Furthermore, according to Seidel et al. (2012) PBL height is especially uncertain over areas with high elevation, which is the case for parts of the study area of the paper by Breeden et al. (2020) due to the presence of the Rocky Mountains. Altogether, I strongly advise including a sensitivity analysis of the results of section 3.2 and 3.3 to the choice of the parameter values used in the calculation of STT-PBL in order to assess the robustness of the current conclusions. I suggest this sensitivity analysis to include (based on the above discussion) the potential temperature surface that is taken to represent the tropopause, the minimum residence time of the trajectories that contribute to STT-PBL and both the forecast uncertainty and the forecast value of the PBL height (as a function of the critical Richardson number chosen to represent the top of the PBL).

Additionally, the rationale of using of Japanese Reanalysis-55 dataset does not become clear to me from the paper. In line 85-86 (section 2.1) the authors mention that this dataset is used because of its relatively long record of ENSO events. Yet, the JRA-55 dataset is only used for assessing the characteristics of the spring transition in section 3.1, as is stated in line 352, without considering any influence of ENSO. According to line 86-87 this is because the transport and tropopause fold diagnostics are derived from the ERA-interim reanalysis instead of the JRA-55 data and, therefore, the former is to be used for the analysis of the relationship between ENSO and the spring transition and STT-PBL in order to be consistent in the data used. Therefore, I would like to ask the authors what the exact benefit of using the JRA-55 dataset is and to incorporate the explanation of this in the description of the data in section 2.1. Additionally, it seems to me that the JRA-55 dataset can in fact be used in the analysis of the spring transition for the different ENSO states in section 3.3 (figure 8) as this analysis does not concern any mass transport or tropopause fold characteristics yet and table 1 shows that data on the ENSO states during the spring transition is available for the JRA-55 dataset. Therefore, I would suggest using the JRA-55 dataset instead of the ERA-interim dataset for this analysis based on the current rationale mentioned in line 85-86. Moreover, this could add a clearer link to the current rationale, but depending on the revisions taken by the authors following the above question to clarify this rationale, this might or might not be preferred (anymore).

Furthermore, the sole use of the ONI index for determining the ENSO states, as described in line 87-89 (section 2.1), might provide a relatively poor representation of ENSO events in the paper, so that the difference in spring transition and STT-PBL presented in section 3.3 might be based on an incomplete definition of the ENSO states. Trenberth and Stepaniak (2001) suggest that at least two indices are required to characterize the variability in ENSO events. They advocate that the ONI index should be accompanied by an (orthogonal) index that represents the zonal gradient in sea-surface temperatures (SST). For this purpose, they have created the Trans-Niño Index (TNI), which represents the difference in normalized SST anomalies between the Niño-1+2 and the Niño-4 regions. However, in the paper by Breeden et al. (2020) all positive, neutral and negative ENSO events are lumped into classes, whereas the study of Trenberth and Stepaniak (2001) is also focused on the variability between different occurrences of positive, neutral and negative ENSO events. Therefore, the cruder representation of ENSO events by Breeden et al. (2020), using only the ONI index, might be justified, so that the results in section 3.3 regarding the effect of ENSO states on the spring transition and STT-PBL would not be significantly affected by this approach. Yet, in order to verify whether this approach is indeed justified, I suggest repeating the analysis for section 3.3, regarding the impact of ENSO on the spring transition and STT-PBL, using more than one index to define the three ENSO groups used in the study (e.g. by including some threshold based on the TNI index presented above). This will provide alternative results for this part of the study than can subsequently be compared to the original for statistically significant differences in timing of the spring transition and monthly mean values of the variables in figure 10 for the three ENSO groups. When significant differences are found in this analysis, it suggests that in fact more indices are required to capture the variability in ENSO events and the effect of that on the spring transition and STT-PBL than just the ONI index, which indicates that this reviewed approach is to be preferred over the original based on the findings of Trenberth and Stepaniak (2001).

Minor comments on the paper:

The role of ozone in this paper is somewhat unclear I find. In my regard it constitutes the context of the study and provides potential for further research, but is not part of the study itself. Yet, it is quite broadly mentioned in the methods and conclusions. I would advise to restrict the role of ozone in this paper to the context in the introduction, further research opportunities in the conclusion and perhaps the background for some of the methods.

The resolution of the zonal and meridional wind on pressure levels (2.5 X 2.5°) is larger than any of the components of the JRA-55 dataset that is used for the calculation of these wind variables (Kobayashi et al., 2015). This seems odd to me. I would advise to explain the reasoning behind the resolution of these variables in the data description in section 2.1.

The use of a fixed amount of mass transport for each trajectory in the calculation of the STT-PBL seems a very simplifying assumption that might potentially cause a lot of variation in STT-PBL to be lost without reading the accompanying reference. I would advise to include a short explanation of the background of this method, especially regarding the fact that the variation in STT-PBL is represented by the number of trajectories rather than the mass of them, after you introduced it in section 2.1.

The significance of the results is currently only assessed visually by means of the 95-percent confidence intervals that result from the significance test described in section 2.3. I would suggest including some form of quantitative assessment of this significance in the paper in the form of, for example, a statistical t-test.

Figure 1a seems random and possibly redundant. It only shows the EOF1 pattern for a PC1 larger than $1\sigma$ and not for the other PC1 states and shows a very similar pattern to what is more extensively shown in figure 2. Therefore, I would suggest removing this figure.

I found figure 7 quite time-consuming to grasp fully. This is mainly the result of the layout of the legend I think. I would suggest mentioning the variables of interest before SST in the legend description instead of ‘STT/variable’ and potentially even to place the description next to the corresponding lines when the available space allows this.

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