Review for

Evidence of small-scale quasi-isentropic mixing in ridges of extra-tropical baroclinic waves

by Kunkel et al.

Summary:

This paper addresses stratosphere-troposphere exchange (STE) that occurs in ridges of extratropical baroclinic waves. The topic is of interest to the readership of ACP, and the study stands out by considering both observations (aircraft measurements, analysis of stratospheric and tropospheric tracers and their correlation) and model results (ECMWF analyses, backward trajectories, idealized simulations). Observations and model results are combined into a coherent story why the location in ridges of baroclinic waves are particularly prone to STE. I definitely think that many interesting and relevant aspects are discussed to elucidate the physical processes at work. In particular, I like the trace gas analysis in figure 5. Still, there is also space for improvement. The main concerns are discussed below, and some minor points are then listed.

Major concerns:

1. Dynamic vs. thermal tropopause: Both definitions of the tropopause are used in the study, whereby I think that the authors are more inclined to the dynamic tropopause – which is OK. Still, I wonder why both definitions are needed for this study because the two definitions agree in a climatological sense, but locally the two tropopause heights can differ substantially. This, for instance, is the case where tropopause folds occur. I would therefore appreciate if the role of the two tropopause definitions is more clearly discussed. More specifically,

- P2,L32: Here it is written that 'these results are independent of the definition of the tropopause'. What exactly is meant by 'these results'?

- Figure 1 and corresponding text: In this figure, the local thermal tropopause is shown; but the dynamic tropopause is missing? Why? Actually, in the text (P7,L32) the measurement are discussed with respect to the height relative to the tropopause, without explicitly mentioning which definition of the tropopause is used (the thermal one; see figure 1) and 'misleading' the reader that the dynamic tropopause is used by mentioning the 'stratospheric PV' values (P7,L28), i.e. the key aspect of the dynamic tropopause.

- Figure 4 contains, in contrast, the dynamic and the thermal tropopause. Why?

- P21,L6-7: "We also find a spatial coincidence in the horizontal plane between the enhancement of N2 above the thermal tropopause and TST across the dynamic tropopause by analyzing passive tracers in our idealized simulations (Figure 10)." Here, both definitions of the tropopause are referred to. This is somewhat 'confusing' to me.

2. Streamlining the introduction: The introduction basically 'offers' everything that is needed for the study. But at some places I felt that a clear storyline was missing. Let me show this with some very specific examples:

- P2,L18 an L26: At both places it is written where STE occurs predominantly, i.e. it looks a little repetitive and the reader must read twice in which sense the two paragraphs differ.
- P3,L3-15: This paragraph discusses the crucial role of Rossby waves for STE, which I fully agree with. What I am missing is the link to the previous paragraph! As a reader I had the impression that this paragraph opens up a new story (Rossby waves), and does not 'naturally' evolve from the previous paragraph. Of course, this always reflects some personal view, but I think the introduction would benefit a lot if the story more clearly from one paragraph to the next.

Hence, many processes (radiation, folds, clouds, convection, gravity waves) are introduced in the introduction, but they remain somewhat 'unrelated' to the main topic (ridges in baroclinic waves). To be sure, I think it is fine to mention all these different aspects, but the processes should be streamlined (or directed) towards the topic of the paper.

3. Role of the enhanced stability above the tropopause: The storyline of the study is built around the enhanced stability near the extratropical tropopause and that STE is encountered in this region. While reading the text, I had the impression that a special role is attributed to this enhanced stability for STE. But there are several other processes at work: gravity waves (as discussed several times), turbulent regions due to lowered Richardson numbers.
In short, I wonder whether the whole story could also be interpreted in a different way, i.e. we encounter STE not because of the enhanced stability but despite of it. Then, the argument could be as follows: (i) a gravity wave evolves near or at the tropopause; (ii) because of this gravity wave vertical wind shears are increased and therefore the Richardson number becomes small; (iii) this reduction in the Richardson number due to the wind shear dominates any impact of the enhanced vertical stability and therefore leads to turbulent mixing and hence STE.
I don't know whether this is a valid interpretation of the current case, but it would see the enhanced vertical stability in a completely different light. I think the authors should discuss these alternative interpretations, or at least make clearer why the enhanced stability is so important for the mixing.

4. Power spectral densities (Figure 6 and corresponding text): The power spectrum is discussed in Figure 6 to show that isotropic turbulence (k=-5/3) prevails for flight leg (FL380), but that geostrophic turbulence (k=-3) prevails at later flight legs. The discussion should be clearer and in particular, I would lile the following aspects to be addressed:

- Why is it possible to identify structures down to 100 m with a sampling frequency of 2-3 Hz? Is this simply given by the aircraft's speed and the sampling period?

- Is there a reference that a slope of k=-3 is typical for gravity waves, as stated in the text (P15,L14-15)? I am certainly not an expert on power spectra, but I would have expected geostrophic turbulence to be typical at larger scales?

- The slope k=-3 (red line) seems to apply for a range between 0.01 and 0.2 Hz, whereas for smaller and larger frequencies it clearly deviates from this behavior (in figure 6b). How has this to be interpreted?

Basically, I think it is nice to have the power spectra in the manuscript, but I would appreciate a more detailed discussion.

5. Idealized simulations: In section 4, the authors refer to an idealized baroclinic life cycle simulation in Kunkel et al. (2016), more specifically to the experiment BRTC LC1. Of course, I understand that not all details of this previous simulation can be given. However, I would appreciate as a reader if could read (and understand!) the current paper without having read Kunkel et al. (2016) -- simply because I could not remember. Hence, I think that the authors should include as much details from Kunkel et al. (2016) in the current study that it becomes understandable without the previous study, i.e. it becomes more or less self contained.

As a specific example, In P20,L17 it is stated that STE starts to occur slightly after the time of the first enhancement of N^2. But where exactly is this N^2 value determined? I might have missed it in the current text, or it might indeed have to be got from Kunkel et al. (2016).

Minor comments:

- P2,L5: “certain trace species” -> You might want to specify already at this place what trace species are meant.

- P2,L6-78: Would it make sense to give, in addition to the height range above and below the dynamical tropopause, also in hPa or m?

- P2,L13: “in the deep branch into the UTLS” -> It is not immediately clear by the term ‘deep branch’, in particular if a reader is not very familiar with STE. It might be helpful to introduce in 1-2 sentences the stratospheric circulation with the distinct branches.

- P2,L16: “two competing transport pathways” -> Why are the two pathways competing? In which sense are they competing?

- P3,L19-20: "Lamarque and Hess (1994) separated between diabatic, i.e., potential temperature changing, and diffusive, i.e., related to friction, processes and showed that diabatic processes play a more vital role for STE than diffusive processes." -> Are there newer studies showing that diabatic processes than diffusive ones? I wonder whether this applies to STT and TST, and I am really not concvinced that turbulent mixing is less important (in particular for STT)?

- P3,L24: "Clouds and related diabatic heating" -> What are the diabatic heating processes related to clouds? Does it refer in particular to condensational heating (phase changes of water and ice)? Or is radiative cooling at cloud top also relevant?

- P3,L26-27: "... can reach the upper troposphere and modify the PV, consequently allowing for exchange between tropospheric and stratospheric air..." -> Note, however, that WCB air masses do not necessarily enter the stratosphere; the diabatic heating during the ascent is associated with mid-tropospheric PV changes, and the WCB is also able to modify the upper-level PV, but further diabatic and/or diffusive processes are needed that the air masses cross the tropopause.

- P4.L31+33: "took place" & "are to examine"; the tense is switching from past to present; please make this consistent (not only at this place).

- P5,L33-34: "had the goal to study the abundance of trace species in the extratropical tropopaus region in relation to the occurrence of enhanced values of static stability in the lower stratosphere and to potential STE." -> Please rephrase in a clearer way; as a suggestion: ".. in the extratropical tropopause region and how they are influenced by the enhanced static stability ... and potential STE. Such conditions were found by Kunkel et al. (2016) to occur in the ridges of extratropical baroclinic waves. Therefore,..."

- P6,L9: Why is a slightly degraded horizontal grid (0.125 deg) for the trajectory calculation compared to the other analysis (0.07 deg)?

- P7,L7: The horizontal resolution of the COSMO output is 0.4 deg; in contrast, it is 0.125 deg for ECMWF, i.e. it is higher for ECMWF than for COSMO. Is this correct? How do the vertical spacing of ECMWF and COSMO compare in the UTLS?

- Section: 3.1: Would it be possible to have one figure (or figure panel) where all flight legs are labeled? While reading this section it was difficult to immediately know where the flight legs are. For instance, it would help to locate the flight legs of figure 3 more easily.

- P10,L23-: Here, a model deficiency is discussed, namely the too high values of the Richardson number near some regions around the tropopause. This discussion of a model deficiency somewhat interrupts the main storyline, and hence I wonder whether it should better be discussed in section 2.2 where ECMWF data are introduced? Furthermore, the term 'in some regions' is rather unspecific, and immediately lets the reader ask where these regions are.

- P10,L34-35: "Thus, the model forecast underestimates the strength of the inversion, most potentially due to defincies in representing the gravity wave in this region." -> First, note the spelling error! Then, how sure are you that this is indeed a gravity wave? Then, the underestimation of the strength of the inversion is attributed to the effect of the gravity waves, i.e. because they are not well enough captured by the model. How do you know that this underestimation is not because of a limited vertical (and horizontal) resolution of the model?

- P13,L2-3: "In general, at the tropopause the CO−N2O correlation starts with larger CO and larger N2O mixing ratios at potential temperatures typical for the extratropical tropopause" -> 'larger' refers to a comparison; but to what is it compared? Of course, I see the point, but I think it is not perfectly clear. Furthermore, I wonder whether it is correct to say that a correlation starts at a larger N2O and CO values. It sounds a little strange to me!

- P16,L12: What does "at some time" mean? Or, stated otherwise: How long are the backward trajectories? Possibly, I simply missed this piece of information, and if not: Please add it!

- P16,L23.24: " Starting from this region the trajectories strongly decelerate in a region of alternating horizontal divergence. During this time the trajectories cross back and forth over the dynamic tropopause." -> How do you see in figure 7 that the trajectories are decelerating? How do you interpret the alternating horizontal divergence? Is the divergence pattern due to the gravity wave?

- P16,L26-27: " The motion back and forth across the chosen PV value for the dynamic tropopause becomes also evident from the PV along the trajectories (Figure 8a)." -> I am not sure whether I see this crossing back and forth over the dynamic tropopause in figure 8. What I see is that both sides (PV smaller and larger than 2 PVU) are 'covered' by the trajectories, but no further details.

- Figure 7: In the text it is written that a transition (although not a smooth one) can be seen in the PV (panel b). This is difficult to see in my print out. It might also be helpful to have the relevant flight legs added to the figure; otherwise, it is a little 'difficult' to relate the trajectories to the measurements.

- Figure 8: Why does the scale go up to 10 PVU in panel a)? In the same line, would it be possibly to adjust the scale in panel d)? Here, Ellrod and Knapp's TI index is shown as an additional turbulence indicator? Earlier in the text, only Richardson number was considered? I think it would be nice to be consistent throughout the manuscript, i.e. either to discuss only one or both indices. Finally, would it make sense to zoom in into a shorter time period around 15 UTC? For example, from -8 h to + 8 h.

- P19,L3: Where does the number 69048 come from? Is this the starting frequency of the trajectories times the duration of the time period?

-P19,L14-23: Here, it is discussed whether the STE (and in particular TST) follows the classical meaning of TST. What is 'the classical meaning of TST'? The term is unclear! Actually, I wonder whether this whole discussion about 'classical' or 'not classical' is necessary? If the authors would like to keep it, a more detailed discussion about the exact meaning of this term has to be included, and it has to be made more clear why it is relevant for the study.

- Figure 9 and the corresponding text: This figure shows the maximum static stability in the idealized baroclinic life cycle experiment BRTC LC1. I have several questions qith respect to this figure: (i) How robust is the maximum static stability? (ii) Are only STT and TST trajectories in the ridges of a baroclinic wave included? I guess that this is not the case, but if so: It distracts the reader from the main story, which is about STE at exactly these locations.

- P21,L3-4: "Thus, there is almost a temporal coincidence between the start of the enhancement of static stability and the first occurrence of STE." -> The statement is fine, but it repeats essentially the first sentence of the paragraph. Hence, it is somewhat repetitive!

- Figure 10: Where are these structures relative to the ridge and trough of the baroclinic wave?

- P22,L4-5: "In contrast to TST and although a temporal coincidence is also evident for N2 enhancement and occurrence of STT, no spatial co-occurrence is evident for STT in regions of enhanced N2 (Figures 10c,d)" -> Rephrase in a clearer way? How do you infer that STT does not co-occur with enhanced N^2? Is this statement based only on the blue area in figure 10? Wouldn't we need a stratospheric tracer to make such a statement?

- P22,L8: "just above a region of ice cloud occurrence" -> What is the relevance of these ice clouds?

- P22,L9: "wave pattern related to a propagating inertia gravity wave" -> The gravity wave seems to be rather important for the mixing across the tropopause? It is not completely clear to me where this gravity wave originates from? Further, it is written that the wave propagates? But in which direction? It seems to me, based on the vertical cross sections, that the wave does not really propagate in the vertical direction. Instead, could it be that the gravity wave actually propagates along the troposphere-stratosphere interface, i.e. along the tropopause? A more refined analysis would be very helpful, given that the wave patterns is mentioned at several places in the manuscript.

- P26,L8-9: "A common feature of the two sets of trajectories is that in both cases the potential temperature values hardly change in the six hours; thus, the TST occurs quasi-isentropically." -> How do you see this in the figure? Or is this statement based on a quantitative analysis of the trajectories? I think a more detailed discussion of the quasi-isentropic transport is necessary, in particular because this is one of the key words in the article's title.