Review of "The Frosty Frontier: Redefining the Tropopause as a transport barrier using the Relative Humidity over Ice "

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General

This paper reports on a novel way of deducing a tropopause using the relative humidity over ice (RHi). This is good. The tropopause defined in this way is analyzed as a transport barrier between the stratosphere and the troposphere. It is pointed out that RHi is the key variable for ice cloud formation, which incorporates both diabatic and adiabatic processes. The analysis focuses on a ten year record of measurements at a radio sonde station (Meiningen); I like the statistical comparison of the data in Fig. 4.

My reservations regarding the paper are: first, the paper discusses on the tropopause in Northern hemisphere mid-latitudes. Clearly, the tropopause in the tropics is different and the concepts presented here may not carry over (see also below). Second, this analysis focuses in the role of the tropopause as a transport barrier for water vapor. Water vapor is an important compound, but not the only one. My suggestion is that these two points should be made very clear in the abstract and in the title of a revised version of the paper.

Further , the comparison in the manuscript is only with the classic WMO lapse rate tropopause. I agree it is widely used, but it is not the only "tropopause" (see also below). In particular, the lapse rate tropopause and the cold point tropopause are not the same quantity. Also here the paper could be more explicit.

Finally, the manuscript stresses the importance of diabatic processes for determining the tropopause – this is good. But I think the manuscript is unnecessarily vague by simply saying "diabatic". The diabatic processes in question are radiation (but on which time scales) and cloud formation (but phase changes are neglected in the analysis in Sec. 4). These points should be clearer in the manuscript.

Overall, I think there is a lot of good and interesting material here, but I am afraid the manuscript needs work before it can be accepted for ACP.

Detail

Definition of the tropopause

I know that the wording is widely used in the literature, but I do not like the expression of a "definition of the tropopause". As the authors say, "the tropopause acts as transport barrier between the troposphere and stratosphere." – so the ambition has to be finding a way of *locating the barrier* not simply *defining* it. It looks to me that this issue is not taken into account here. I wanted to bring up the issue of a "definition of the tropopause" but this is a choice of the authors.

The tropical and the extra-tropical tropopause

Aren't the tropical and the extra-tropical tropopause two rather different entities? Many papers have discussed this question (e.g, Highwood and Hoskins, 1998; Fueglistaler et al., 2009; Bian et al., 2012; Pan et al., 2018; Krämer et al., 2020; Hoffmann and Spang, 2022). I think this aspect should not be ignored here – to me the discussion has rather a focus on the extra-tropical conditions. Definitely, the high resolution radio sonde data are only available for mid-latitude conditions. If the authors agree, it would be good to state this explicitly throughout the manuscript and even include the information in the title.

The lapse rate and the cold point tropopause

The lapse rate and the cold point tropopause are treated here as two alternative "definitions" of the tropopause. But I would argue that they are not describing the same transport barrier. Pan et al. (2018) and Munchak and Pan (2014) made the point that the lapse rate tropopause and the cold point tropopause are not the same thing. (These studies are for the tropics so perhaps they are not applicable here – but then the question remains how the two tropopauses differ in mid-latitudes.) For example, transport of water vapor should be differently affected by the lapse rate and the cold point tropopause. Further information could be found in König et al. (2019). Convective transport across the lapse rate tropopause is much more common than water vapor transport across the cold point tropopause (Clapp et al., 2023).

In general, for different trace gasses, not necessarily the same tropopause is relevant (Bauchinger et al., 2025). Finally, to play devil's advocate: how different would the tropopause look if simply water vapor was considered instead of relative humidity over ice (see also below)?

Moreover, in the manuscript, the term “thermal tropopause” and “lapse rate tropopause” (where the classic WMO definition uses the term lapse rate) mean the same thing. This could be explicitly explained in the manuscript.

Literature

Overall, the impression of the reviewer is that the coverage of the existing literature on the tropopause is better in the previous paper by some of the authors (Köhler et al., 2024, K24 hereafter), than in the present manuscript. For example the classic study by Hoinka (1997) is discussed in K24 but not here. The same is true for the study by Maddox and Mullendore (2018), who point out that there are potentially two different interpretations of the classic WMO lapse rate definition. Further, using potential vorticity (PV) as an entity to be considered when deducing a tropopause is criticized by the argument that choosing a particular PV value introduces a certain arbitrariness; however, when the maximum *gradient* of PV is considered, this is not the case (Kunz et al., 2011; Turhal et al., 2024).

Transport barrier for certain compounds

The characteristics of the tropopause as a transport barrier might be different for different compounds (e.g., Pan et al., 2018; Bauchinger et al., 2025). From the manuscript it seems that here the focus is on a transport barrier for water vapor (e.g. l. 67). If this is correct, this fact should be obvious in the abstract (and perhaps the title). If this is not correct the manuscript should demonstrate in how far the new tropopause definition is relevant for other trace species (say N_2O or CH_4).

Minor points

- l. 6: “approaches”: only a comparison with the thermal tropopause is presented here
- l. 13: Looks like the entire study is on the extra-tropics

- l. 17: Note that STE (in particularly of H_2O) is often only poorly represented in models (Charlesworth et al., 2023).

- Further studies of the vertical gradient of potential temperature as a measure of the tropopause (Kohma and Sato, 2019).

- l. 36: I do not agree. Already the old WMO lapse rate definition considers two tropopauses. It should be clear here if the classic double tropopause issue is meant here (e.g., Randel et al., 2007; Castanheira and Gimeno, 2011) or something else.

- l. 40: The PV values attributed to the location of the tropopause are to some extent arbitrary, I agree. However this is not the case for more recent studies (e.g. Kunz et al., 2011; Turhal et al., 2024), where the maximum gradient of PV is considered.

- l. 47 ff.: This paper states "In the UTLS we find on average synoptic scale motion, i.e. vertical updrafts in the upper troposphere and subsidence in the stratosphere. These vertical motions induce either adiabatic expansion hence cooling of the air (in case of the tropospheric motion), or even adiabatic compression thus warming of air (in case of stratospheric motion, i.e. the Brewer Dobson circulation…" – I am confused here. Clearly the Brewer Dobson circulation is associated with ascending motion in the tropics. This seems in contrast to what is stated here. – Please clarify.

- l. 49: ice particles are also important for short wave radiation.

- l. 50: it is not clear here why the moist layer should be close to the thermal tropopause (where thermal tropopause is the lapse tropopause if I read the paper correctly)

- l. 59: citations for "former investigations"?

- l. 59: Aren't the time scale relevant here? Radiative processes are surely important, but what are the timescales in question here? The location of the tropopause is valid only for a given time span, isn't it?

- l. 60: one could mention Birner's papers (e.g., Birner et al., 2002, 2006) for the TIL;

- l. 64: There are a lot of diabatic processes of relevance to the atmosphere so it is not sufficient to include diabatic processes in general in the analysis. Aren't the *right* processes important here? And this is again a time scale issue.
- l. 71: vertical gradient
- l. 81: What is the advantage of choosing a regular grid?
- l. 84: "directly measured" – how good is the measurement at low temperature and low water vapor? Can this be quantified? Citations?
- l. 85: citation for the Magnus formula? Is there an assessment of the quality of this formula?
- l. 86. Is there an assessment of the quality of the Murphy and Koop formula?
- Sec. 2.2: Would not the hygropause also be a valid/important comparison?
- l. 94: threshold for water vapor
- l. 99: how is this threshold chosen?
- l. 104: a small gradient in RHi means a small gradient in water vapor – correct? How can this happen if the troposphere is humid an the stratosphere is dry? Is this an indication that the transport barrier is not very pronounced under these circumstances?
- l. 107: report 42 also in percent.
- l. 111: Only 18 "no tropopause cases" vs. 42 (l. 107); report 18 also in percent. Is 18 "better" than 42?
- Figures 1-3: How are these cases selected?
- Fig. 1, caption: state what is temperature (red line I guess) and what is dew point in panel b. The sonde is labeled 12 UTC but the measurements were taken over a certain time period. Report the period.
- l. 121: why "definitions" – you only compare with the thermal tropopause

- l. 121: “caveats”: this is not completely obvious, the figures indicate that the results are different, not which tropopause is better. Also note that the cold point and the lapse rate tropopause are often not co-located as well.
- l. 122: shown – where?
- l. 130: which panel shows the “absolute humidity profile”?
- l. 137: “the θ” does not sound right
- l. 142; “optical”?
- l. 146: The implicit assumption seems here that the upward transport of water vapor (and the freeze out) occurs along the vertical profile. Of course, this is an over-simplification. To understand the water vapor at 10 km in (say) Fig. 2 one needs to consider the Lagrangian history of the air parcel (e.g., Schiller et al., 2009; Khaykin et al., 2022), not only what happens below.
- ls. 147-148: this “confidence” depends very much on the question how the cases in Figs. 1-3 are selected.
- Fig. 4: I like this statistical comparison in this Figure. Nonetheless, the reference tropopause is only the thermal tropopause here as throughout the paper.
- l. 155: “prolonged warming” is unclear; explain and/or provide references.
- l. 169: which Figures are these colors referring to?
- l. 170: results employing RHi for the tropopause determination
- l. 177: Would Krämer et al. (2020) also be a suitable citation here?
- l. 177: What means “close” here? Approximate value in km?
- l. 171: why is the kink nonphysical? In individual ascents (Figs. 1-3) there seem to be also kinks
- l. 187: “signals” in which quantity? Also, does the radiation care about the tropopause? And means relative to a tropopause height?

- l. 195/196: “lower altitude” – should this be visible in Fig. 4? “lower altitude” seems not obvious from Fig. 4. Suggest some discussion.
- l. 223: can you quantify “just above” (in m)?
- l. 230: “definitions”: here only a comparison o the thermal tropopause is presented
- l. 241: I cannot find R_a in eq. 3
- l, 249: mention c_p
- l. 257, eq. (7): The time derivative of RHi (using eq. 3) can be expressed . . . (suggest being a bit more explicit here)
- l. 261: note that the updraft is not necessarily vertical.
- l. 264: Yes, clouds are usually located below the tropopause, but not always. For example short term cooling could be induced by upward propagating gravity waves. Could this aspect be of relevance here?
- l. 265: Is this true also for the situation within a convective cloud, where a tropopause is hard to find (Maddox and Mullendore, 2018).
- l. 267: which “layer”?
- l. 269: I think it would be easier to follow these considerations if it is better explained why the specific latent heat L is relevant here if phase changes are neglected.
- l. 275: Eq 9: I am not sure here: Is this equation true in general or is it formulated neglecting cloud processes?
- l. 276: why is the second term small?
- l. 278: “all” is not applicable here – the comparison in the arguments presented here is comparsion to the thermal tropopause. For example, if an ozone tropopause is considered (Hoinka, 1997), the thermal structure of the tropopause is not relevant.

- l. 283: “diabatic processes” are put forward here; I think the paper could make a stronger point if it states which processes are relevant here. First, I think that friction is neglected. Second, I think that radiative time scales are too slow (do the authors agree?). That leaves phase change (cloud formation) as the central process. If the authors agree, these arguments could be more explicit.
- l. 286: change “the apply”
- l. 287: What are the typical radiative time scales in question here?
- l. 287: How is *w* determined? Citation?
- For the toy model, also the assumption of a temperature profile is necessary, if I understand correctly – how is it determined?
- l. 289 and 291: “showed” is not correct
- Fig. 8, left: the temperature decrease seems to be throughout the troposphere (as far as visible in the figure), more than 1 K per hour. How realistic are such cooling rates?
- l. 306: This analysis assumed the absence of cloud formation .. this should be explicitly stated.
- l. 312: but phase changes are neglected in Section 4 as far as I can see. I think the paper could be more explicit about the relative importance of radiation and phase changes regarding how a tropopause location can be determined.
- l. 330: journal title should not be in capitals
- l. 334: why is “atmosphere” in capitals?
- l. 342: there should be no space in the formula of H_2O

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