

Reply to Reviewer

We would like to thank both reviewers once again for taking the time to review the revised version of the manuscript. We are grateful for their suggestions, which have led to a significantly improved manuscript!

In the following, we answer to the comments point by point. Questions and remarks of the reviewers are marked in orange, reply of the authors are marked in black and changes to the manuscript are marked in blue.

Reviewer #1

General

As stated in the first review, this paper reports a novel way of deducing a tropopause using the relative humidity over ice (RHi), which is good. The paper discusses the tropopause in Northern hemisphere mid-latitudes, which is now reflected in the title (and throughout the paper). Overall, the paper has been revised thoroughly. I also note that focus of the paper is less on the tropopause as a transport barrier (e.g. for N₂O or CH₄) and that the relation of the new tropopause and the thermal (lapse rate) tropopause have been clarified.

I think the authors have adequately replied to the questions and suggestions of reviewer 1 (and also reviewer 2). I suggest that the paper is accepted now (any very small remaining changes can be done in a revised submission).

Minor points

- comment on l. 40: I think the word “artificial”, which is introduced here is too strong, perhaps “picking a particular value of PV” or similar

We have changed it as suggested.

- comment on l. 59 (former investigations); avoid “z.B.” in the text of the manuscript

“z.B.” was only in the document to the reviewers, it was already corrected in the manuscript.

- comment on l. 59 (radiation): I am not arguing against radiation here, but some of these arguments could be helpful in the paper as well.

We added following text based on our reply:

Radiative heating rate estimates (Fusina and Spichtinger, 2010) suggest characteristic time scales of several hours $O(10\text{ h})$, which are relevant compared to the lifetimes of upper-tropospheric moist layers (e.g. Irvine et al., 2014; Spichtinger et al., 2005). Recent results (Emig et al., 2025) also indicate that long-lived cirrus clouds can modify the tropopause structure, confirming the key role of radiation on these time scales.

Reviewer #2

In revising this paper the authors have resolved many of the concerns I expressed in my first review (and my impression is that they will similarly have resolved many of the concerns of the other referee). I repeat my previously expressed view that this new definition of the tropopause is an interesting and potentially useful idea and given the revisions I recommend that this paper is published without undue delay.

I do have some comments on this version of the paper that I recommend are considered by the authors before the paper proceeds to publication.

My main critical comment is that whilst in their reply to my original comment on the use of the term 'transport barrier' the authors state:

'Following the reviewer's detailed comment, as well as feedback from the other reviewer and discussions with colleagues, we agree that calling the tropopause a transport barrier was too strong. ... We have therefore removed statements referring to the tropopause as a transport barrier and focus on documenting the occurrence and characteristics of the RHi-GT tropopause. ...'

But in fact there are some sentences where the term 'transport barrier' remains and is used in pretty much the same way as before. For the reasons given in my previous reviews I think that the paper would be much better if these sentences were removed.

We thank the reviewer for pointing this out. A few instances of „transport barrier“ were inadvertently retained. We will review the manuscript and remove these occurrences to ensure full consistency with the revised discussion.

More generally, and while the paper has improved significantly in revision, I still think that the paper would benefit from the 'less is more' principle -- if something does not have to be said, then it is better not to say it. Many of my detailed comments are along these lines.

DETAILED COMMENTS:

L50: 'oxid' to 'oxide'

We corrected this.

L53-55: Ordering might be better as: 'including some, such as the cold-point tropopause, that are not applied in the present study because they are primarily relevant for tropical regions'

We have changed it as suggested.

L63: 'close to the thermal (i.e. lapse-rate) tropopause' -- I am wondering why this statement is not simply 'close to the tropopause'? We have changed it as suggested.

L64-69: There seems to be some redundancy between these two sentences -- before and after the paragraph break -- and a paragraph break at this point does not seem necessary.

Together with the comments from Reviewer 1, we expanded the previous paragraph slightly with regard to radiation and then added the statement about “friction” at the end, so that the short paragraph is now omitted.

On the other hand, diabatic processes play a role. Water vapor (and also solid water particles, i.e. ice crystals) is absorbing and re-emitting infrared radiation as an almost ideal black body, leading to a local cooling on top of moist layers situated close to the tropopause. At this level, strong vertical gradients in temperature and humidity make moist layers particularly effective in modifying the local radiative balance (Fusina and Spichtinger, 2010). In addition, ice particles play an important role for the interaction with shortwave radiation, further contributing to the radiative impact of tropopause-near moist layers. Radiative heating rate estimates (Fusina and Spichtinger, 2010) suggest characteristic time scales of several hours $O(10\text{ h})$, which are relevant compared to the lifetimes of upper-tropospheric moist layers (e.g. Irvine et al., 2014; Spichtinger et al., 2005). Recent results (Emig et al., 2025) also indicate that long-lived cirrus clouds can modify the tropopause structure, confirming the key role of radiation on these time scales. Finally, friction and irreversible mixing, as e.g. driven by turbulence, contribute to the change in variables as diabatic processes.

L71: I don't really see why the thermal lapse-rate tropopause definition 'relies exclusively on adiabatic processes'.

We have changed it to “mostly”.

L64-85: Looking at these paragraphs, there seems to be a certain amount of repetition -- e.g. radiative effects mentioned in L65 and then again in L77-79. My general feeling is that the authors are trying to claim too much for the justification of their new tropopause definition -- from my point of view the new definition is an effective way of identifying the tropopause as an air mass boundary -- considering how this helps understand the processes that maintain and determine the structure of the tropopause comes later.

We have summarized and shortened this section

Yet diabatic processes - including radiative cooling, latent heating, and small-scale mixing - also shape the tropopause structures, as exemplified by the tropopause inversion layer (TIL) (e.g., Birner et al., 2002; Randel et al., 2007; Fusina and Spichtinger, 2010; Spichtinger, 2014; Köhler et al., 2024; Kunkel et al., 2016). Relative humidity over ice (RH_i) captures both adiabatic and diabatic contributions to TIL formation, motivating a tropopause definition based on RH_i gradients.

L85-86: 'emphasising its role as a transport barrier for water vapour' -- but, as noted above, you have previously agreed in your response that the use of 'transport barrier' is too strong and have removed statements that the tropopause is a transport barrier'.

We omitted this statement.

L80: ... Kohler et al (2024) demonstrated ...?

This part has been rewritten.

L129: trivial point, but this derivative must of course be negative

You are right, we corrected this.

The derivative of RH_i with height ($\partial \text{RH}_i / \partial z$) must be smaller than $-0.15\% \text{ m}^{-1}$.

L135: List the break criterion as an extra numbered criterion?

We added the break criterion as the fourth point and omitted the following paragraph.

4. Break Criterion: To avoid unrealistically low tropopause heights, any detected height below 3000 m is discarded and no value is assigned for that profile.

~~*Note, the RH_i-GT algorithm has problems with profiles that show only very small gradients in RH_i. It can happen that the criterion for the gradient only switches on very far down in the profile and delivers unrealistic values. Therefore, a break criterion has been introduced to avoid unrealistically deep tropopause heights lower than 3000 m. In such a case, no value is specified for the tropopause.*~~

L138: 'Further studies will have to show the influence of the threshold in other regions such as tropical or polar regions.' But I thought that we (two reviewers + authors) were all agreed that the tropical tropopause is very different to the mid-latitude tropopause, so finding a useful criterion for the tropical tropopause is not simply going to be a case of 'changing the threshold'.

You are absolutely right and we therefore omitted this sentence.

L174: 'In summary, this indicates a strong transport barrier ...' -- contradicts your claim to have remove mention of transport barriers.

We rewrote this to:

*In summary, this indicates a strong **confinement** of water vapor at the RHi-GT level, whereas the conventional thermal tropopause definition fails completely to detect this feature.*

Figures 2 and 3: in looking at these cases I am again wondering about the synoptic situations that give rise to these situations where the RHi-GT is well above or well below the thermal tropopause. But you have said that this will be the subject of future investigations.

Yes, the examples are intended to illustrate different ways in which RHi-GT and TTP behave.

L181: *** 'transport barrier' ***!

We rewrote this to:

*These cases, together with a substantially larger number of additional profiles that were analyzed, make us confident to use RHi-GT as a new definition of the extra-tropical tropopause in terms of **water vapor distribution**, preferred over the conventional thermal tropopause.*

Figure 5: Probably not very surprising -- but nonetheless reassuring -- that an RHi-based tropopause signal gives a clearer signal in RHi.

Yes.

L255: *** 'transport barrier' ***!

We deleted this.

L256-266: This paragraph summarises the differences in the N^2 profile as seen according the two tropopause definitions and then concludes that a 'more distinct separation ... has been achieved by simply using the gradient of relative humidity ...' Is that really the case? It is not what

I see when I simply look at the two panels of Figure 7. This seems to be an example of the authors feeling that they have to claim that the RHi-GT definition is 'better'. I think that the differences between the two distributions shown in the two panels of Figure 7 prompt all sorts of interesting questions about the nature of the TIL and the structure of the TIL that appears in profiles constructed from the thermal tropopause definition, but on the basis of the information pre-

sented I cannot see clear evidence that a more distinct separation of tropospheric and stratospheric N^2 values has been achieved using the RHi-GT definition.

You are right, we reformulated this part to be more neutral:

The original definition of the TIL using the thermal tropopause shows a structurally more complex pattern directly below the thermal tropopause. While the tropospheric values of the Brunt-Väisälä frequency are similar for both tropopause definitions and thus normalized profiles, a noticeable kink is visible at the local minimum of $N^2 \approx 2 \cdot 10^{-4} \text{s}^{-2}$. This kink can be seen in many studies regarding the TIL (Birner et al., 2002; Gettelman et al., 2011; Köhler et al., 2024). Starting from this kink, N^2 rises sharply and then reaches its maximum 50 m above the thermal tropopause with $N^2 \approx 7 \cdot 10^{-4} \text{s}^{-2}$. In contrast, for the RHi-GT definition, the N^2 maximum coincides with the tropopause level itself. Above this level, N^2 decreases more gradually for the thermal tropopause definition than for RHi-GT, leading to similar values only about 3000 m higher.

These differences suggest that the RHi-GT and thermal tropopause definitions emphasize different structural aspects of the TIL. While the RHi-GT-based profiles appear smoother and more vertically confined, the thermal tropopause definition highlights a more extended transition region.

Section 4: I think that the information presented here is a useful addition to the paper. Of course what one might hope would be that there would be a range of values of the RHi threshold and the vertical gradient value for which the corresponding tropopause level would be relatively insensitive. Figure 9 seems to imply quite a bit of difference for the three choices of gradient shown -- perhaps not surprising for the 0.01% criterion but the difference between 0.15% and 0.25% more surprising. Figure 10 right panel is more encouraging -- for the choice of 0.15% the altitude seems very insensitive to the min (RHi) criterion. (This is just a comment -- no response needed.)

We thank the reviewer for the careful inspection of the sensitivity analysis. We agree that the differences between the 0.15 % and 0.25 % gradient thresholds appear somewhat pronounced. This is mainly due to the steepness of the RHi gradients in the tropopause region, where small changes in the threshold can lead to noticeable shifts in the detected level. Nevertheless, as also noted by the reviewer, the overall sensitivity remains limited for reasonable choices of the gradient threshold (around 0.15 %), and the definition is quite robust against variations in the minimum RHi criterion.

L297: 'threshold for this particular dataset' -- of course what one hopes is that the a threshold can be selected that works for many extratropical locations -- the new criterion would be much less useful if it had to be 'tuned' for different locations.

We fully agree with the reviewer that a broadly applicable threshold is essential for the usefulness of the RHi-GT definition. In this study, we indeed optimized the threshold based on the available dataset to illustrate the concept, but the goal is to identify a criterion that remains robust across different extratropical regions in the future.

L306: As already commented, it is not very clear to me that the RHi-GT criterion shows a 'much sharper and clearer transition between troposphere and stratosphere' with regard to N^2 .

We reformulated this part to:

When analyzing thermodynamic variables over 10 years relative to the RHi-GT tropopause height, RHi and static stability (N^2) show a more coherent transition between tropospheric and stratospheric conditions than when referenced to the thermal tropopause.