

Reply to Reviewer

We thank both reviewers for their work and are pleased that they recognize the work we have put into reformulating the manuscript. In the following, we will address the points that arose during the second review.

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.

Report #1

To my view, the authors did an excellent job to revise their previous version and the paper is much better now. Only a few typos should be corrected before printing.

Line 23: Check whether troposphere or rather tropopause is the more appropriate word here.

We rewrote the sentence to

About 20 years ago, Birner et al. (2002) were able to demonstrate with high-resolution radiosonde data that sometimes the upper troposphere and lower stratosphere (UTLS) region encounters a strong temperature inversion known as the tropopause inversion layer (TIL).

L 29: lower atmosphere is not clear enough. I prefer troposphere.

We corrected this.

Check the sentence in LI 30,31. It is unclear.

We omitted that sentence

L 43: irreversibly

We corrected this.

L 162: constituteS, and later serveS

We corrected this.

L 258: replace "live"

We rewrote the sentence to

This means that all profiles are now in the same coordinate system and can be averaged to obtain mean profiles of temperature, humidity and static stability.

L 304: I suggest to write "a key quantity"

We applied the suggestion.

L 396: formation

We corrected this.

L 475: Please try to reformulate. It is a bit ugly that the word "average" appears twice.

We rewrote the sentence to

The averaged RHi for central Europe show only small seasonal differences in the troposphere.

L 507: regionS
We corrected this.

Report #2

1 General

This constitutes the second review of the paper by Köhler et al., submitted to ACP. I see that the manuscript has seen a lot of work; I like in particular the choice of the new metric (new Eq. 8). In spite of the work invested, I suggest a thorough proof reading of the manuscript to remove some smaller grammatical errors (some examples are given below).

As I said in the first review, “I think this is a good helpful paper of interest to the readership of ACP”. And “I think the paper would be more valuable if the message would be much clearer in a revised version”. I think that some further work is needed on the manuscript to make it clearer and more accurate in a revised version.

2 Comments in detail

2.1 Abstract and title

In my first review I mentioned the guidelines for ACP papers, in particular the title, abstract, and concluding section:

https://www.atmospheric-chemistry-and-physics.net/policies/guidelines_for_authors.html

The title has been changed and improved.

However, I am afraid that the abstract is still longer than 250 words (see ACP recommendations). I suggest addressing this issue. I think abstract of the paper could also be a bit clearer about the findings of the paper; would it not be of interest to the reader what the “consistent relationships” and the “differences” are?

I would also suggest replacing “extratropical” with “mid-latitude” in the title, as this paper is not addressing polar issues.

We replaced „extratropical“ with „midlatitude“ in the title and throughout the whole text. In addition, we have shortened the abstract to exact 222 words.

2.2 Comparison of ERA5 and a radiosonde station

In my first review I stated: “As I understand the paper, the basis of the paper is a comparison between the TIL in ERA5 and in the data from a radiosonde station. After a ‘validation’ of the ERA5 data with the radiosonde data, further conclusions for the TIL in the latitude range of the station are drawn.”

The paper has certainly improved as the information on “Idar-Oberstein, Germany” is now much clearer.

However, as stated earlier, the results are relevant for northern hemisphere mid-latitudes (close to 50°N) – is this correct?. I suggest to be clearer about this. The title has changed to “extratropics” (which is not the same thing as mid-latitudes) but this point is not reflected (e.g.) in the abstract. I think the paper should be very clear (in particular in the abstract and in the conclusions) about the range of validity of the analysis.

If the authors argue that the analysis focuses on Northern hemisphere mid-latitudes (e.g., Is. 550/551), my suggestion is to replace “extratropical” with “midlatitude”.

As we explain in the introduction and in Section 3.3, we have chosen the four locations so that they reflect different meteorological characteristics within the midlatitudes. We believe that we have already written this clearly enough.

We agree with you that the term extratropics was not precise here and, following your suggestion, we have used the term midlatitudes throughout the manuscript.

2.3 Timescales

In the first review it was stated: “Here baroclinic waves and radiative (H₂O) processes are relevant, but the time scales involved should not be ignored (and they might be different for the two mechanisms). Does the analysis provided here allow statements about radiative/dynamical time scales?”

I do not think that this question is well answered (manuscript and reply); perhaps, statements about time scales cannot be made based on the analysis, however this point could come across clearer in the paper.

We added text at several locations in the text, indicating that our investigation is based on an Eulerian evaluation, which does not allow to determine timescales of the processes involved for changing the relative humidity. Such approach would require a Lagrangian analysis, which is, however, beyond the scope of this study.

However, a clear distinction between the different processes or the involved timescales is not possible on the basis of RHi values only.

With our Eulerian approach of evaluating RHi at a given location and time, respectively, a further determination of the dominant processes and/or their timescales is not possible. For such investigations, a Lagrangian approach would be necessary.

2.4 Tropopause

As stated in my first review, “the entire concept of the TIL is based on using tropopause relative coordinates”.

The paper now cites a number of reviews (e.g., Reichler et al., 2003; Maddox and Mullendore, 2018, which is good), however, Maddox and Mullendore (2018)

We added this information at the end of section 2.3.1:

In this paper we use the pure WMO criterion to determine the tropopause. For details, the reader is referred to the source code that we have provided (<https://zenodo.org/records/10604349>).

2.5 Radiosondes

If I understand the paper correctly, radiosonde data from one (DWD) station at Idar-Oberstein are considered here. This, to me, seems to be the “anchor”-point of the ERA5 analysis (see also section 2.2).

However, when the radiosonde/ERA5 comparison is described (p. 13, Figs. 4-6), only “the radiosondes” are mentioned. But which sondes, which station, which time period? My guess is that Idar-Oberstein sondes were used here (see also data availability statement), but this point should be clear from the paper. I also suggest adding this information (which radiosondes?) to the captions of Figs. 4, 5, and 6.

We added the information about Idar-Oberstein in the captions of Fig. 4, 5, and 6.

Is it also true that the Idar-Oberstein sondes were assimilated in ERA5? I think the answer is yes. This information could be added to the paper, e.g., in line 328.

We added some text for hopefully making clearer that we just use the sondes from Idar-Oberstein. We added also the information that the used radiosondes were assimilated into the ERA5 data.

*For the first investigation, nearly 10,000 high-resolution radiosonde ascents from **one distinct** weather station in Germany (Idar-Oberstein) are analyzed.*

*This study is partly based on radiosonde data from a **single** measurement site at Idar-Oberstein (Germany, 49.69° N, 7.33° E)*

The radiosonde data as described above (sec. 2.1.1) is also assimilated into the ERA5 data set.

2.6 Findings of Kunkel et al. (2016)

The study aims at corroborating the findings of Kunkel et al. (2016); e.g., l. 81. Kunkel et al. (2016) state for example “Furthermore, updrafts moisten the upper troposphere and as such

increase the radiative effect from water vapor” – this seems to emphasise the radiative effects on the TIL – this emphasis is not obvious from the present manuscript. Perhaps a better link to the paper (and results) by Kunkel et al. (2016) could be made.

Actually, Kunkel et al. suggested that the combination of (reversible) adiabatic processes (as transport) and diabatic processes (as mixing or radiative cooling) lead to a sharpening of the TIL in combination with an enhanced relative humidity. Using our detection method (in terms of enhanced RHi values) we found statistical evidence of the correlation although we cannot distinguish between the different processes (adiabatic vs. Diabatic). We added some text in the result section to make clear the connection to the former study by Kunkel et al.

This result is in agreement with the findings by Kunkel et al. (2016) for a single case analysis; adiabatic processes (transport by atmospheric flows) and diabatic processes (as mixing or radiative cooling) lead to enhanced values of RHi

Minor Points

I. 6: which radiosonde data?

To keep the abstract short, we have omitted the location in the abstract. In our opinion, it would go too far to describe the exact location and position of the radiosondes in the abstract. However, this is described in detail in the data section.

I. 13: extent ---> extend

We corrected this.

I. 14: “relationships” between which variables?

We rewrote the sentence to

These analyses reveal consistent TIL properties in various extratropical regions of the Northern Hemisphere under different meteorological conditions.

I. 15: “Differences” between what?

We rewrote the sentence to

However, differences in the strength of the dependence of TIL properties on relative humidity over ice are evident between the different regions.

I. 134: using an adverb (“evenly”) is not appropriate here.

We corrected this.

I. 202: attempts --> ways

We corrected this.

I. 212: the point is the “vertical gradient” – correct?

No, it is actually a point because it is the height at which the minimum of the vertical temperature profile is reached. See Highwood and Hoskins (1998), which was already given as reference in the revised version.

I. 303: suggest replacing “extratropics” with “midlatitudes”.

We corrected this.

I. 321: exponential ---> exponentially

We corrected this.

I. 378: weakening ---> weaken

We corrected this.

I. 482: I would formulate: “mid-latitude region”

We corrected this.

I. 539: What is causing the variable relative humidity?

We are not sure what is involved in this question. The definition of relative humidity is given in section 2.2 with equations 1 and 2.

I. 540: “sharpening of the TIL”?

We corrected this.

I. 540: drop “the” after “for”

We corrected this.

I. 548: results

We corrected this.

I. 554: drop “we can state that”

We corrected this.

I. 554: “data are”

We corrected this.

I. 554: “kind of investigation”

We corrected this.

I. 555: here it would be helpful to state exactly which findings of Kunkel et al. (2016) are corroborated.

We have rewritten the sentence to:

The connection between relative humidity and TIL features is robust and corroborates the former findings in model studies by Kunkel et al. (2016), who stated that diabatic and adiabatic processes can have similar amplifying effects on the TIL.

Fig. 4: state in the caption which altitude range is considered to be UT and LS.

We included this.

References: journals should not be in upper-case letters (e.g. I. 579).

We corrected this.