Review of

"The extratropical tropopause inversion layer and its correlation with relative humidity "

by Daniel Köhler et al.

General

This is a good paper. It addresses the formation mechanism of the extratropical tropopause inversion layer (TIL) and the different forcing mechanisms of the TIL discussed in the literature (Randel et al., 2007; Wirth and Szabo, 2007). Here baroclinic waves and radiative (H_2O) 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?

The paper is based on high-resolution radiosonde data from Idar-Oberstein (but give location (lat/lon) at first mention, and the relevant time period) with ERA5 data. This is good. However, I would emphasise differences as well a similarities between ERA5 and the sondes (see below).

The paper then moves on to investigate the influence of relative humidity with respect to ice (i.e. H_2O) on the extratropical TIL. Further, based on ERA5, longitudinal and seasonal variability of the TIL is discussed. But the paper should be quantitative and more accurate (and less vague) than saying something like "...reveal consistent relationships in various extratropical regions of the Northern Hemisphere under different meteorological conditions".

I am not sure if the authors agree with the assessment in this review – they do not need to do so. But a much clearer message of the paper would be very helpful. This is not clear from the present draft – in particular not in the title and the abstract. I think the paper would be more valuable if the message would be much clearer in a revised version.

Finally, while I am mentioning papers here that might be potentially of interest (and some already cited), I am certainly not suggesting the citation of particular papers.

Overall, I think this is a good helpful paper of interest to the readership of ACP. I suggest a major restructuring to make the key points of the paper clearer and more accurate in a revised version.

Comments in detail

Abstract and title

There are guidelines for ACP papers, in particular the title, abstract, and concluding section:

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

Titles should be concise and consistent with the content and purpose of the article. For research articles, ACP prefers titles that highlight the scientific results/findings or implications of the study. Abstracts should have fewer than 250 words – I think the paper can be improved in this respect.

The paper should be very clear what the main findings are and what the advance of knowledge of the study is.

Comparison of ERA5 and a radiosonde station

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.

This is good, but the paper is not very clear about this. The stations is called "Idar-Oberstein", sometimes only "Germany" is mentioned, the period of radiosonde data is often not mentioned, on other occasions the latitude/longitude of the station is mentioned – all the information is in the paper, but the reader should not be forced to search the entire paper to find the necessary information.

Most importantly, as I read the paper the results are relevant for northern hemisphere mid-latitudes (close to 50°N) – is this correct?. If the authors agree, this fact should be evident in the paper, in particular abstract and title.

Tropopause

The entire concept of the TIL is based on using tropopause relative coordinates. Yes, this is reported on page 7 of the manuscript, but I suggest making this concept clear upfront. Further, determination of the tropopause is not straightforward (e.g., in ERA5), there is a an extensive discussion in a recent publication (Hoffmann and Spang, 2022); this publication also addresses the issue of a fixed pressure grid and different interpolations, which might be helpful here.

Moreover, on page 6 of the manuscript, the classic WMO definition of the lapse rate tropopause is cited, however this definition leaves room for interpretation (Maddox and Mullendore, 2018). Exactly which definition of the lapse rate tropopause has been used? As stated in the paper the classic WMO definition is old and does not necessarily take into account the use of more recent gridded and high resolution data (see e.g., Reichler et al., 2003).

The authors mention the review by Gettelman et al. (2011), which is good. However, there are also other reviews of the determination of the tropopause (Hoinka, 1997) and there is also a tropopause definition based on isentropic potential vorticity gradients (Kunz et al., 2011).

Figs. 1, 2 and 3

First, I find these figures very helpful, they explain the concepts used here before more general statements are made.

However, I suggest that the scheme in Fig. 3 is closer to reality (Fig. 2); the static stability is not constant wit altitude (above 11.25 km) and the kink at 11.25 km in Fig. 3 is not seen in the real data (Fig. 2).

Further, I like Fig. 2 a lot, but I cannot see why an interpolation to a fixed altitude grid is necessary to produce the figure – doesn't the interpolation introduce an unnecessary smoothing? Most importantly, likely, the difference between sonde and ERA5 that I see in Fig. 2, is an important result. If I were an author, I would flag this result more strongly and more quantitatively in the paper (e.g. abstract, conclusions).

Equation 8

In Equation 8, a measure is defined for the deviation between ERA5 and the sondes. However, this definition is not unique. It is a choice, isn't it? The problem I see is that deviations between E and R could cancel out when integrated over a certain altitude range. That is locally there could be a substantial deviation between E and R, but $\bar{D}$ could be rather small, depending on how the range $z' - z_0$ was chosen. Why are no absolute values considered of the deviation between E and R?

Minor issues

- l. 10: I would not use the term "strong agreement" when two temperature profiles (say) are very similar.
- l. 10: "geographical": what is meant here is the longitudinal variation. Correct?
- l. 18: "distinct and intriguing feature known as the tropopause inversion layer": here it would be helpful to report particular features of the TIL, rather than saying "intriguing". What are the most relevant properties of the TIL? Such information comes later in the introduction, but it would be good to have this up front.
- l. 25: why "inert substances" ? Isn't the TIL a barrier for vertical transport even if substances are not chemically inert?
- "hypothesis " should be plural
- l. 51: give latitude and longitude of Idar-Oberstein
- l. 52: radiosonde data (not "sondes")
- l. 69: focuses
- l. 72: these thresholds look somewhat arbitrary. Are there citations? Are there any indications in the household data? Would a temperature of (say) 450 K be okay?
- l. 74: units should not be in italics

- l. 80 latest $\longrightarrow$ most recent
- l. 83: state explicitly how many levels were used. State the top altitude used. Also the approximate vertical resolution in ERA5 here would be useful to report.
- l. 85: “closest grid point” – this is always the same point in the ERA5 grid – correct? This point could explicitly be mentioned.
- l. 90: here and elsewhere “the data sets” is used, but is must be made clear that ERA5 and radiosonde is meant. It is likely better to err on the safe side and explicitly state what is meant.
- l. 91: the “improved statistical analysis” is not obvious from the paper.
- l. 92: “height” be more precise here, geometric altitude, pressure altitude, geopotential altitude etc., is not the same thing and not available in each data set.
- l. 94: citation for the buoyancy speed?
- l. 96: what is the argument for cubic spline?
- l. 99: “inconsistent with respect to time” is unclear.
- l. 99: Which “data set”?
- l 110: citation for this statement?
- l. 117: quantify “slightly”
- Eq. (2): I am not familiar wit this approximation; is there a citation? Or an explanation of Eq. (2)? How accurate is Eq. (2)?
- Eq. (4) is stated here that as an approximation for dry air: this aspect should be made clear here. Make clear what the issue is of wet vs. non wet conditions.
- Eq. (5): I suggest to make clear (here and elsewhere) what z is – is it geopotential height in Eq. (5)?

- l. 155: see also other work (e.g., Reichler et al., 2003; Maddox and Mullendore, 2018) on using the classic WMO tropopause definition for modern, gridded data.
- l. 157: be clear about which data sets, “both” is a bit vague
- l. 169: which period of the radiosonde data?
- Fig. 2: This figure is good. I suggest adding some discussion in how-far the sondes and ERA5 do *not* agree.
- l. 172: “Another”? this paper is on the TIL.
- l. 175: Suggest stating the importance of the TIL earlier in the paper (introduction).
- l. 177: How is the value of 3 km chosen?
- l. 182: say which features.
- Eq. 7: $p500_z$ is an altitude (not a pressure) – the symbol is confusing.
- sec. 3.1.1: be clear what is compared with what.
- l. 211: stating (e.g.) “upper troposphere” is not enough here, the exact range $z' - z_0$ that was used should be reported.
- l. 220: are $\longrightarrow$ is
- l. 223: quantify “thin”
- l. 226: do you really want to give three significant figures here?
- l. 241: is this statement consistent what is shown in Fig. 2?
- l. 244: “averaged” over which region?
- l. 269: I agree, but Fig. 2 also shows the limitations.
- l. 272: which “data”?
- l. 275: Unclear sentence, two times wRHi?
- l. 275: give the Fig. where the PDF can be seen

- l. 286: report the latitude range for which the comparison is valid.
- l. 289: resolution or vertical resolution?
- l. 291: again, point to the figure in question here, merge wit the next sentence (we show)...
- l. 293: "the TIL depth dTIL is shifted to lower values" – this is clear from Fig.9. But then (l. 294) "the depth of the TIL is always in the range" – so does DTIL change with humidity or not? Where would I see the "second mode"? In 9b? More help for the reader? I find this discussion somewhat confusing here.
- l. 296: it is not clear why there is an "artefact" here and what this implies.
- Fig. 9; mention a and b in the caption.
- l. 302: "sharpening the TIL ... depth" – do you have an argument why?
- l. 306: state here immediately which three regions. I think the main point is longitudinal variation here.
- l. 328: 328: "to act" $\longrightarrow$ "acts"
- l. 337: give section/figures for "previous findings"
- l. 340: quantify the "differences" found here
- l. 348: is this true? I do not see the strong increase in RHi in Fig. 2
- l. 354: "could be due" sounds rather speculative.
- l. 366: do both "effects" have similar time scales? Would this not be important?
- l. 373: compared
- l. 375: confused about "polar" and "summer" in this sentence
- l. 378: maximum in what?
- l. 381: what are the time scales in question here? Should not be forgotten.
- l. 384: amplitude yes, but what about time scales?

- l. 386: really “upper troposphere”? That means below the tropopause?
- l. 387: “different” sounds vague here.
- l. 391: give latitude and longitude of Idar-Oberstein, give length of the time period
- l. 393: same location is not clear; ERA5 does not have a grid point at Idar-Oberstein.
- l. 398: this is not important, but meteorologists typically talk about high temperatures, not warm temperatures.
- l. 401: at which altitude?
- l. 401: “too stable”: compared to what?
- l. 418: provide the code on a web-page, not only “upon request ”.
- l. 420: It would be good to report not only the raw data, but to also create a location where the processed data of this study are available (say TIL strength).
- l. 427: give correct “spelling” of ECMWF
- l. 479: correct authors list?
- l. 490: give page rage for the citation.

References

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