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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The tropopause inversion layer in baroclinic life cycles experiments: the role of diabatic and mixing processes" *by* D. Kunkel et al.

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The presented study by Kunkel et al. aims to investigate the mechanisms underlying the formation of the tropopause inversion layer (TIL) by means of idealized baroclinic life cycle simulations with varying complexity. The study addresses an open and debated issue, which is carefully examined and the study is performed thoroughly. Therefore I recommend this study for publication in ACP if my concerns and comments are sufficiently addressed. Most of my comments are of minor character, however some of them might consume a considerable amount of time and therefore the editor may choose to accept the study with major revision.

General comments on the study

- I am confused by the title which suggests that mixing is a process distinct from diabatic processes? If you refer to turbulence I would argue that turbulence is a diabatic process because it is not conserving enthalpy. Do you refer to moist diabatic processes to distinguish it from friction and turbulent mixing?
- The study addresses only life cycles of type LC1. I do recognize that the authors already present numerous simulations and additional simulations might exceed the reasonable amount of presentable material. Nevertheless I argue that any comparisons of the presented findings with measured inversion layers or inversion layers obtain from global climate simulations are hampered, because in reality various types of life cycles may occur (LC2, LC3). In particular the relative importance of dry dynamics and turbulence may vary significantly in a cyclonic life cycle that develops in a cyclonic sheared environment. I therefore highly recommend that the authors include a discussion on this issue and indicate if they think that their findings might significantly differ in a LC2. Such a discussion may build upon the findings presented in Wirth (2003) where different flow regimes (cyclonic/anticyclonic) are discussed. I leave it open for the authors, to perform for example one LC2 of their choice that shows that the findings to not differ significantly and present this as an appendix.
- The authors clearly state the overall aim is to rank and identify the processes underlying the formation of the TIL. Given the large number of simulations at hand, I suggest that the author could use this opportunity to broaden the scope of the study. Although I don't want to force my view too strongly on this, I would appreciate to see a discussion relating the presented findings to more general concepts of baroclinic life cycles. The authors could show at which stage of the Shapiro Keyser life cycle the TIL forms, e.g., during frontal fracture? During the

T-bone stage? Before or after the wave breaking? This might also include an outlook discussion of potential feedbacks of a strong TIL on the life cycle. I suppose that enhanced stability above the surface low can alter its circulation (if PV is conserved, vorticity decreases around the TIL because stability gradients increase?). Such a discussion has the potential to broaden the scope of the study and making it of interest to a wider part of the community. At the moment it appears very specific. Such a section can also have a speculative character in which the authors relate their findings more clearly to synoptic-scale or mesoscale characteristics of a cyclone: for example, the conveyor belts, wave breaking or the jet stream. For example, one major finding is the strong relation between the TIL and vertical motion. Is the TIL forming preferentially in the right entrance of the jet streak, which is forming at the tip of the stratospheric trough, or is it the warm conveyor belt outflow or both (From Fig. 8 I think both are true)? Because both are promoting vertical motion. I would appreciate such a discussion at the end of the study, which classifies the findings a little bit more into a conceptual broader framework of cyclonic life cycles.

- The study carefully examines a number of individual processes and their role in the initial formation of the TIL. However I am missing a discussion on the role of advection in all of this. I assume that after the initial formation of the TIL, the air that constituted initially the TIL is advected downstream and weakly stratified air is advected into the area where the TIL initially formed. Advection may explain the observed weakening of the TIL during the life cycle which in the presented version of the manuscript is only partly explained. Differential advection during the life cycle may for example affect the temporal evolution of the vertical profiles of N (Fig.11). In the current version of the manuscript the discussion suggests that the observed differences in the vertical profiles can be attributed to the added process in the numerical simulation. A discussion of the role of differential advection might be important, because the study has not a Lagrangian focus. Advection

may play a minor role (see equation in my next comment) in the initial formation of the TIL but may add an important contribution to its temporal evolution and the variations between the model runs because it cannot be assumed to be equally strong in all of the runs.

- Following on my comment above regarding the discussion on the role of advection, I would like to propose the authors an additional way of quantifying the role of different physical mechanisms leading to the formation of the TIL. The local tendency of forming a zone of enhanced/reduced static stability (N $\sim \frac{\partial\theta}{\partial z}$), like the TIL, may be understood in terms of the vertical component of the frontogenesis equation. For convenience I write it down below:

$$\frac{\partial}{\partial t}\frac{\partial\theta}{\partial z} = -\mathbf{u}\cdot\nabla\frac{\partial\theta}{\partial z} + \frac{\partial}{\partial z}\dot{\theta} - \left(\frac{\partial u}{\partial z}\frac{\partial\theta}{\partial x} + \frac{\partial v}{\partial z}\frac{\partial\theta}{\partial y} + \frac{\partial w}{\partial z}\frac{\partial\theta}{\partial z}\right). \quad (1)$$

The notation is mostly standard. The first term on the right hand side is the advection, the second term the vertical gradient of diabatic heating and the last term in brackets a collection of some deformation terms and one tilting term (typically the last term is referred to as tilting). The calculation of all three terms in the TIL region might be straightforward and if you can obtain the diabatic tendency from the different physical parameterization of the model you can even estimate the importance of them individually. If this is not possbile you can still estimate a total diabatic tendency to obtain an estimate for the second term on the r.h.s of the equation. I leave it open to the authors to use this equation, but it appears to me an excellent way to quantify the underlying physical mechanisms forming the TIL.

- I am not sure if the timing of the TIL formation is at least partly different between the model runs because the time when the TIL is forming is measured with respect to the start of the model simulation. Since the stage of the life cycles will likely differ between your runs (i.e., frontal fracture occurs after 24 hours of integration in one simulation but after 28 in another simulation), the TIL might form earlier in one case simply because the life cycle is accelerated as a whole. I recommend introducing a relative measure instead of the start of the simulation. For example you additionally show the minimum SLP of your cyclones in Fig3. You could compare the time when the TIL forms relative to the time when the minimum SLP occurs in your runs or when eddy kinetic energy has a maximum in the channel or relative to the strongest deepening rate within a reasonable time window or the occurrence of the first stronger vertical motion. I would argue that we gain a more general view on the formation of the TIL within a baroclinic life cycle if its formation can be related to such a relative measure. It will also allow researchers to compare the formation of the TIL in real case studies to your findings and thereby add an important information to the existing literature.

Comments which the authors may choose to address

- I recommend summarizing your findings in a diagram, which may help readers to further appreciate your work. For example a diagram with two y axes and on x axis, where x shows the type of simulation (or process), y1 shows the onset of the TIL and y2 shows the strength of the TIL. Such a multi-axes figure may also help to simplify some of the current figures.
- Previous studies suggest differences in the character of the TIL during winter and summer seasons. To allow for a simpler comparison with earlier findings the authors may introduce a short discussion whether their TIL relates more to a summer of winter time TIL.
- If the TIL forms in an outflow area of the warm conveyor belt, is the TIL destroyed in the area of the dry air intrusion behind the warm sector? Consider speculating

on this based on the experience you gained during the analyses. If so this might open new research question left open for an outlook section.

General comments on the formation of the TIL

- I am wondering whether the formation of the TIL can be understood in terms of two fluids with different stratifications lying above each other (troposphere and stratosphere). If vertical motion occurs in the weakly stratified fluid, vertical motion of an individual fluid parcel is damped at the boundary to the strongly stratified fluid and momentum is transferred across the boundary and deformation occurs. Typically we would expect the growth of a deformation zone characterized by enhanced stratification comparable to a collision zone (inelastic collision; the isentropes in the stratosphere are squeezed/pushed together by vertical motion from below; like in a car crash). Is this explaining the correlation between vertical motion and the formation of the TIL (at least for the dry case)? If so I would appreciate if the authors can include such a simplified explanation for the formation of a TIL and have a quick look at deformation.
- How is the TIL and the presented findings related to the PV dipole that has been described in earlier studies as a consequence of longwave radiation. If the author can relate their findings to this literature they will broaden their study significantly and link it better with the existing literature on diabatic modification of baroclinic life cycles

(1) Jeffrey M. Chagnon and Suzanne L. Gray, 2015: A Diabatically Generated Potential Vorticity Structure near the Extratropical Tropopause in Three Simulated Extratropical Cyclones. Mon. Wea. Rev., 143, 2337–2347. doi:http://dx.doi.org/10.1175/MWR-D-14-00092.1
(2) Chagnon, J. M., Gray, S. L. and Methven, J. (2013), Diabatic processes modifying potential vorticity in a North Atlantic cyclone. Q.J.R. Meteorol. Soc.,

139: 1270–1282. doi: 10.1002/qj.2037

Is the TIL distinct from the PV dipoles described in these studies or is it the same phenomena.

Further comments

- Consider increasing the size of figure labels in Fig.8, I even recommend to split it into two different panels. You may also use only one color table and put it vertically to the left/right of the figures, this might help to increase the size of the individual figures.
- The discussion of Fig.8 is rather limited. There is almost no comment on Fig.8a-e. Consider including a minor discussion on the main differences.
- Would it be more insightful to show the differences between the individual runs compared to the reference run, instead of absolute fields? For example some interesting differences between the runs in Fig.8 might be masked. Similar for Fig.2.
- Consider inserting one or two (green) contours of vertical motion in Fig.2 and Fig8. These contours would allow the reader to compare the areas where a TIL forms to the area of strongest vertical motion., which is one of your key findings.
- Consider increasing the size of Fig.11, I had a hard time to visually inspect the figures. For example by splitting it into two figures instead of one panel with a and b. Maybe you can even show all axis labels only in one figure and not in all figures of the panel, this would allow you to move the single figures closer together and to increase the panel as a whole. Consider summarizing the key finding in one sentence in the figure caption.

- Page 21512, L 11: In the discussion on Fig.9a I recommend to include a statement on the spatial differences. How different is the position of the first occurrence of the TIL and relate it to the difference in the timing of its formation. This might be helpful at this stage (a comment related to my more general comment above).
- Page 21512, L 14: "The latest appearance is..when considering cloud processes and turbulence only", from Fig.9 I would argue that the latest appearance is in BMP (dark blue) and not BMW TURB (light blue).
- Page 21512, L18: "This division into three time sectors...". How are they defined? t<35h, 35h<t<65h,t>65h? Consider inserting a thick vertical line at these time steps in Fig.9 to highlight the three time periods.
- Page 21512, L27: "since they foster an earlier emerging of conditional instability". How do you know that conditional instability is emerging earlier in your life cycle? I am not sure if this statement has been sufficiently shown by the presented analyses. Conditional instability may occur in any of the presented simulations but only with parameterization of moist convection the model is able to release the instability. Without it, the instability needs to grow until resolvable by the large-scale motion.
- Page 21512, L 27: "This finding supports our results from the previous section that moist dynamics including strong updrafts has a strong impact on the first appearance of the TIL". Please clarify this statement. Because dry dynamics can also include strong updrafts, I suggest to say: "moist dynamics has a strong impact. . ..because of stronger /increased updrafts compared to a dry run" (or comparable).
- Page 21513, L15: "Indications of increased static stability are found in all cases above the updrafts which reach the tropopause." Because we are looking at

dry static stability, is this also supporting my deformation-collision argument from above? Would it be possible, and I think this might be novel in the discussion of the TIL, to include a contour of deformation in the discussed figure (Fig.10)?

- Page 2153, L 28: Consider Wernli and Davies (1997), as main reference if you decide to show only one reference.
- Page 21514, L 8: "...to the domain mean TIL which becomes stronger but also to the fact that the number of model grid cells in..." Why don't you show a more straightforward number such as "(area > 2.5km)/total channel area" ? I find the analysis between the two types of N a bit odd. Comparable to the area you discuss on page 21508.
- First paragraph on page 21514: Although I tend to agree with your discussion, I am wondering to what extend the first contribution of vertical motion to the formation of the TIL is later during the life cycle superseded by advection? Because your are showing vertical profiles which are averaged over the domain, the role of advection of air with high N values in the stratosphere is not clear. Is the region of initial TIL formation unaffected by advection of the strongly stratified air downstream away from its source region and weakly stratified air into the region above the convective cloud?
- Page 21515, last paragraph: I am wondering why the author do not treat turbulence as a diabatic process. The formulation suggests that it is a distinct process. I think it is a diabatic process because it is not conserving entropy (see below); maybe you refer to moist diabatic process if you refer to condensation/evaporation/ice formation instead?
- Following on the discussion on turbulence, I am wondering how turbulence is altering the stratification. Is the interpretation of turbulence as heat flux by Shaprio (1976) a possible explanation? If so, the author may choose to include a short

discussion on this into the paragraph.
(1) M. A. Shapiro, 1976: The Role of Turbulent Heat Flux in the Generation of Potential Vorticity in the Vicinity of Upper-Level Jet Stream Systems. Mon. Wea. Rev., 104, 892–906. http://dx.doi.org/10.1175/1520-0493(1976)104<0892:TROTHF>2.0.CO;2

Comments concerning the conclusions

- "showed that there is a correlation between the first appearance of the TIL and of updrafts reaching the tropopause". Is this correlation surprising of given my simplified view on the TIL (presented above) to a little extend an expected result?
- Conclusion 5. Strictly speaking this is not shown in the current but in the foregoing study. Consider moving this out of the item environment.
- Conclusion 6. I am not sure how to understand this sentence. Consider rewriting it. For example start the second sentence with "Because, clouds ."
- Last sentence: "Including the frequency of occurrence of baroclinc waves might further help to . . ." I don not understand what is meant with frequency of occurrence here? Where should it be included? Please clarify this statement.
- It would be worthwhile to include a final statement which simulation produces a TIL comparable to the one which is observed.

Comments related to language/notation

- At various places the formulations used in this study suggest that diabatic effects are a mechanism distinct from turbulence. I would argue that turbulence is a diabatic mechanism (process which is not conserving entropy). Consider rewriting throughout.

- P 21500: Consider adding one sentence to explain why the aspect ration of 1/400 is favourable for studies of the TIL.
- P 21497, L 4: Consider writing N in pressure coordinates, because you speak about the measurement in the following sentence (these are likely taken in pressure coordinates).
- P 21496, L 9: "The effect of individual diabatic, i.e. related to humidity and radiation, and turbulent processes is studied first to estimate the additional contribution of these processes to dry dynamics". Consider rewriting for clarity. For example: Firstly, the effect of individual diabatic processes, e.g., radiation, condensation and turbulence, are examined to assess their individual contributions to the formation of the TIL in addition to dry dynamics.
- Section 2 is written in past tense: "we studied", consider using present tense throughout.