Reply to Sebastian Schemm

The authors appreciate the valuable comments on the manuscript which led to a significant improvement. Referee comments are given in bold, the answers in standard font. Changes to the text are in italics.

Generally, we note that we revised most of the figures based on suggestions from the three referees. We also changed some of the acronyms of our experiments. Moreover, we included three more figures (new Figures 9, 15, and 16 in the revised manuscript) and based on questions and comments from Sebastian Schemm and George Craig we included a new section (Section 5 in the revised manuscript) in which results from LC2 experiments are discussed.

General comments
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?

We agree that turbulence is a diabatic process, since entropy is not conserved, just as with moist or radiative processes. In the initial manuscript we decided to list turbulence separately, since the term diabatic is sometimes used as synonym for moist and/or radiative processes and we wanted to make sure that it is recognized that we also study mixing. However, for the final manuscript we decided to avoid to list turbulence separately when speaking of diabatic processes and changed the text in the revised manuscript accordingly.

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.

Since a similar request/suggestion is given by G. Craig, we decided to include an entire new section on LC2 (new section 5). The appearance of the TIL is slightly different, in particular it is weaker in LC2 than in LC1 experiments. The processes that generally lead to the formation of the TIL are the same. Thus, the overall conclusions do not change significantly. Rather the conclusions from studies of dry adiabatic simulations of baroclinic waves (Erler and Wirth, 2011) are confirmed that the TIL appears to be stronger in LC1 cases than in LC2 cases (compare Figure 3 and new Figure 15 in the revised manuscript).

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.

We generally agree that the simulations conducted for this study do have the potential to discuss more features of baroclinic life cycles and to possibly further relate the TIL to other aspects of the baroclinic waves. The main topic that we wanted to address in this manuscript is the contribution of diabatic processes on the formation of the tropopause inversion layer in baroclinic life cycles. We further link the occurrence of the TIL to specific features of the life cycles. In the dry case we state that the TIL forms after wave breaking (see introduction, page 21498, line 7ff of the discussion paper) as it was also reported by Erler and Wirth (2011). In case with radiation and turbulence this is to a large extent also the case (discussion of Figure 3, page 21507, line 20ff of the discussion paper). Only with moisture we can expect a significant earlier appearance of the TIL, i.e., before wave breaking. We link the TIL evolution to the appearance of the first vertical ascent that reaches up to the tropopause. Along with the discussion around the new Figure 9 this is further related to the warm conveyor belt. Moreover, at the jet exit we also find enhanced values of static stability, which are related to gravity

waves from the jet-front system, at least in our simulations.
The effect of an initially present tropopause inversion layer on the evolution of baroclinic life cycles is topic of another analysis that we are working on. However, the discussion of this topic is sufficient for another manuscript and thus will be addressed in a follow up study in which we will compare the evolution of life cycles with and without an initially present TIL.

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 ~ ∂Θ/∂z), like the TIL, may be understood in terms of the vertical component of the frontogenesis equation. For convenience I write it down below:

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

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 possible 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.

Since from our point of the view the last two comments are related to one topic, we will address them together in one answer. The role of advection has been studied before in Wirth (2003, 2004), Wirth and Szabo (2007), and in Erler and Wirth (2011). We started our analysis based on the results presented in Erler and Wirth (2011) with the goal to extend their analysis by including diabatic processes in the framework of baroclinic life cycle experiments.
In the current study the focus is on the additional effects from diabatic tendencies. Of course, the advective tendencies also affect our results, which we noted in the manuscript (e.g., in the abstract: *"The effect of individual diabatic processes related to humidity, radiation, and turbulence is studied first to estimate the contribution of each of these processes to the TIL formation in addition to dry dynamics."*). Thus, the role of advection is not neglected, we rather want to focus on the additional effects from the diabatic processes.
In section 3 we explicitly discuss the differences from the diabatic tendencies to the results from the adiabatic life cycle, especially in the discussions around Figures 3-6. We admit that we do not extensively quantify the differences between advection and diabatic processes. However, we think that we included sufficient information in our discussion to show how strong diabatic contributions change the appearance and shape of the TIL in the life cycles. Especially, in Figure 6 some quantitative aspects are shown and are also discussed in the text. Obtaining further quantitative estimates of the individual terms is, however, beyond the scope of the present study.

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.

Since minimum sea level pressure or eddy kinetic energy are commonly used in the analysis of baroclinic life cycles, we started our analysis by comparing these quantities to the evolution of the maximum N^2. However, the link between minimum surface pressure or maximum eddy kinetic energy to the maximum TIL or first appearance of the TIL were not so significant. As mentioned in the answer to the third comment we related the appearance of the TIL to features of the life cycles (e.g., before/after wave breaking, WCB occurrence). Furthermore, one of the major results is that the Δz-tracer and N^2max correlate well, temporally and spatially (see Figure 9, 10, and 11 in the revised manuscript). If we had used a normalized time axis, with t0 equal the time of first strong increase in Δz, there would have been almost no difference in the timing of first TIL occurrence between the moist life cycles. In the manuscript we wanted to show how the TIL appearance varies when more and more processes are included in the experiments are for this decided to show the temporal evolution relative to the model start.

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 one 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.

Generally, we think that summarizing the major findings in one figure is a valuable addition to a manuscript. However, since we already have a large number of figures included in the manuscript (13 in the discussion paper and 16 in the revised manuscript), we omit to include another one. We point out that we already highlighted the major findings in the item section in the conclusion. We also added one more sentence in the final paragraph of the conclusion that summarizes the main results of this study:
"While updrafts are important for the first appearance of the TIL when moisture is included in the baroclinic life cycles, the radiative effects as well as the convergence of the vertical wind are more important in maintaining the TIL during later phases of the life cycles."

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.

The TIL with the strongest annual cycle is found in polar regions (see Randel et al., 2010). At midlatitudes several processes contribute to the sharpness of the tropopause, i.e., stratospheric dynamics (Birner, 2010), radiative forcing by water vapor (Randel et al., 2007), and also synoptic-scale dynamics (Erler and Wirth, 2011). We study the additional contribution from the synoptic-scale dynamics that could be attributed to diabatic processes, focusing on tropospheric forcings in contrast to diabatic processes in the stratosphere (see Birner, 2010). Including these diabatic processes leads to a stronger TIL than in dry experiments of baroclinic life cycles. Moreover, we conduct simulations over a time period of 168 hours and thus discuss how the static stability changes rather on a synoptic time scale and not on a seasonal time scale. Also we start from a background state with no TIL. Thus, a fair comparison between the TIL from our experiments and the observed (seasonal) TIL is beyond the scope of this study.

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.

This is correct, the static stability is reduced in this area. This is somehow the opposite from what happens in the region of the warm conveyor belt or any other updraft. Stratospheric air descends into the troposphere and thus the vertical gradient of potential temperature becomes smaller. This then leads to a slightly reduced static stability in this region.

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.

To illustrate the deformation at the tropopause, we included another version of the new Figure 9 in the revised manuscript here. Instead of drawing an isoline of Δz, we included an isoline for the deformation at the thermal tropopause (Figure A). On a brief glance, Figure A and new Figure 9 do not differ significantly. However, we interpret the deformation, just like the change in static stability, rather as a consequence of the tropospheric forcing, i.e., the updrafts, and not as the initial physical process that leads to the increased stability in the lowermost stratosphere.

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

phenomena.

Figure A: Static stability (color-coded) above the thermal tropopause, column integrated cloud ice content (blue lines, in 0.01 kg m^{-2}), deformation (black lines, in $15x10^{-5}$ s^{-1}), and tropopause close column integrated cloud ice content tqi (cyan lines, in 0.001 kg m^{-2}). Tropopause close means the region between the thermal tropopause and 500 m below. The distribution is shown for BMP between 78 h and 138h after simulation start in a six hourly interval.

As we show in Figure 6c in the manuscript, radiation causes a significant increase in potential vorticity just around the thermal tropopause. The PV dipole discussed for instance in Chagnon et al. (2013) is also caused by radiative processes (longwave cooling). Thus, TIL and PV dipole are most probably related to each other. We included the reference of Chagnon et al. (2013) in our manuscript in the discussion of Figure 6:

"In simulations of real extratropical cyclones over the North Atlantic, the evolution of a dipole structure with a positive PV anomaly above the tropopause and a negative anomaly below have been reported by Chagnon et al. (2013). They could also show that these anomalies are largely related the radiation scheme in their model."

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.

We did as suggested.

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.

Figure 8 was included in the manuscript to show that the main features of the baroclinic wave (e.g., evolution of a stratospheric streamer, relative position of the trough after 120 h) are rather similar in all simulations. We revised the discussion partly and have now a combined discussion of Figure 8 and the new Figure 9 in the revised manuscript.

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.

We decided to show absolute numbers and not differences because there are differences in the simulations and their temporal evolution, thus corresponding stages of the life cycle evolution occur not exactly at the same time. One example is that the trough position is slightly different in all simulations. Showing a difference plot would result in a strong dipole pattern at the edges of the trough that might mask smaller scale features.

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.

We included the new Figure 9 which shows N^2 at the tropopause as well as contour lines for column integrated cloud ice water content as well as contour lines of the Δz tracer. Furthermore, we show and discuss the spatial link between updrafts and TIL in Figures 10, 12, and 13 (in the revised manuscript Figures 11, 13, and 14).

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.

We changed the figure and hope that it is now easier to read. We will also be in touch with the production office to ensure that the final version of this figure is large enough.

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).

We hope that Figure 9 gives sufficient proof that there is a strong spatial correlation between the TIL and the first updrafts that reach the tropopause.

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).

This statement was thought to distinguish the last time sector from the other two time sectors. The last time sector includes only two simulations, i.e., the one with only cloud processes (BMP) and the one with cloud processes and turbulence (BMP TURB).

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.

Yes, these are the time sectors which also are mentioned in the two sentences before. For this we do not see the necessity to include more information at this point, also not directly in the figure by additional lines. Nevertheless, we added the following sentences to the Figure caption (Fig.10 in the revised manuscript): *"The time of TIL occurrence is split into three time sectors. Without radiation and convection, the TIL appears after 65 h, with radiation between 50 h-65 h, and with strong convection before 50 h (more information is given in the text)."*

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.

It is correct, we do not explicitly show the presence of conditional instability. However, we show the consequences, i.e., an increase in the Δz tracer just below the tropopause. Moreover, since we added processes individually, we can link effects from specific processes, e.g., from radiation or convection, to our results. Using this information we came to the conclusion that radiation and convection lead to earlier appearance of vertically ascending air masses and that this is related to their impact on the temperature in the troposphere (e.g., radiation) and/or to the smaller scale on which the scheme operates (e.g., convection). However, since we do not explicitly address conditional instability in our analysis, we rephrased the sentence to:
"since they foster an earlier emerging of updrafts in the model."

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).

We do not show it explicitly for the dry case, but the updrafts are stronger in the moist cases. We rephrase this sentence to:
"This finding supports our results from the previous section that moist dynamics including stronger updrafts than in the dry case has a strong impact on the first appearance of the TIL."

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)?

We refer here to the answer given above on the deformation at the tropopause.

Page 2153, L 28: Consider Wernli and Davies (1997), as main reference if you decide to show only one reference.

We also added Wernli and Davies, 1997.

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.

From our point of view the number of grid cells contributing to the average is more straight forward than an area of threshold exceedance. So we directly quantify how many grid boxes contribute to the mean vertical profile which is a valuable information in this case.

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?

As stated in the manuscript, the contribution of the vertical motion is reduced later in the life cycle (see discussion about Figure 11 in the discussion paper, page 21514, lines 1-19). At later stages of the life cycle, radiative processes become more important in maintaining the TIL. Moreover, the contributions from large scale convergence discussed in Wirth and Szabo (2007) and Erler and Wirth (2011) as well as potentially small scale features such as gravity waves become more important.

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?

As stated in the answer to the first comment, we treat turbulence as a diabatic process, however, initially decided to list it separately.

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.

This is an interesting reference. However, it is not supported by our conclusions. We see rather a decrease in potential vorticity as well as a decrease in stability. This might be a topic that is worth looking into it more in detail in the future.

Comments concerning the conclusion

"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?

It might sound simple and one could have anticipated that this result might be relevant but to the authors' knowledge it has not been shown before explicitly.

Conclusion 5. Strictly speaking this is not shown in the current but in the foregoing study. Consider moving this out of the item environment.

This conclusion is an extension of and link to the results given in Kunkel et al., 2014. Moreover, in the revised manuscript the comparison between LC1 and LC2 showed that gravity waves might be of importance to explain some of the difference of the TIL appearance between these two baroclinic wave types. Thus, we decided to keep this item in the list.

Conclusion 6. I am not sure how to understand this sentence. Consider rewriting it. For example start the second sentence with “Because, clouds .”

We rephrased conclusion 6:
“Finally, updrafts enhance the moisture content of the upper troposphere, not only by transporting water vapor to this altitude. Clouds also form within the updrafts and locally alter the thermal structure of the upper troposphere. Especially, at the top of the clouds a strong cooling can occur which further contributes to the formation and maintenance of a strong TIL. In general, radiative impacts become more relevant during later stages of the life cycle.”

Last sentence: “Including the frequency of occurrence of baroclinic 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.

This sentence refers to the sentence before and simply states that baroclinic waves are found frequently at midlatitudes. We rewrote the sentence:
“Taking into account that baroclinic waves occur relatively frequent at midlatitudes, especially from autumn to spring, might further help to explain the quasi-permanent appearance of a layer of enhanced static stability.”

It would be worthwhile to include a final statement which simulation produces a TIL comparable to the one which is observed.

Since we start from a state without a TIL, such a comparison is difficult to realize.

The observed TIL is also affect by other large-scale forcing such as the stratospheric circulation and also by disturbances below the resolved scales of our model. We have shown that by including diabatic processes we at least minimized the discrepancy that has been reported between the TIL from dry baroclinic life cycle experiments and the observed TIL significantly.

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.
In addition to the answer to the first comment, we changed the text throughout the manuscript.

P 21500: Consider adding one sentence to explain why the aspect ration of 1/400 is favourable for studies of the TIL.
From simulations of baroclinic life cycles (Erler and Wirth, 2011) and from GCM studies (Birner et al., 2006) it was inferred that this aspect ratio resulted in the most pronounced 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).
We think that the equation for static stability in height based coordinates is sufficient to show here.

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.

We rephrased the sentence:
"The effect of individual diabatic processes related to humidity, radiation, and turbulence is studied first to estimate the contribution of these processes to the TIL formation in addition to dry dynamics."

Section 2 is written in past tense: "we studied", consider using present tense throughout.
Thanks for pointing this out. We replaced the few occurrences of past tense in section 2 where it was inappropriate.

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