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Interactive comment on "Composite analysis of the tropopause inversion layer in extratropical baroclinic waves" *by* Thorsten Kaluza et al.

Anonymous Referee #2

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This paper investigates the tropopause inversion layer (TIL) strength using the maximum Brunt Vaisala frequency within 3km above the tropopause in 2 individual extratropical cyclone lifecycles, and in composites of strong extratropical cyclones in the Northern Hemisphere. This is following on from a number of studies analysing the TIL in idealised model simulations of baroclinic lifecycles. In both the case studies and the composites the authors find that the TIL strength (i.e. the highest values of static stability above the tropopause) can be found in the region of low isentropic PV that is advected cyclonically around the cyclone at upper levels. This is above the location of the ascent in the cyclones, where the clouds are identified. These results seem to be consistent with the previous studies.

I have a few comments, queries and suggestions that the authors should consider.

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Discussion paper

1. I found the introduction rather hard to read, with a lot of jargon that would be impenetrable to anyone new to the topic. I recommend reworking a lot of the language in the introduction section to make it clearer. For example, the 2nd paragraph on page 2 could be reworded. 2. Also regarding the introduction, it could be made clearer exactly where the gaps lie that this paper seeks to address. At the bottom of page 1, "evidence for this relation still missing" implies that this paper will provide evidence, but I am unsure if this is a goal of the study. Some rewording might make the introduction clearer overall. 3. Page 2 line 19: The "residual TIL" is mentioned more than once. Is it possible to define what this looks like? 4. Page 2 line 32: This Kedzierski et al reference is never returned to in the discussion section, although I am sure the results from the present paper confirm those results (the TIL strongest within the ridges). 5. Page 3, line 33: L91 and L137 are not vertical resolutions, but the number of levels. Could you give more information about the actual vertical resolution here? 6. Page 6, line 16: Could you make clearer what is meant by a "lapse rate tropopause based vertical coordinate"? Also page 7, line 7: what is meant by "the absolute height coordinate is recovered by calculating the mean tropopause height at each horizontal location"? 7. Page 7, line 8: What is meant by "horizontal or quasi-horizontal variables"? Does this just mean the horizontal composites of particular variables? 8. Page 8, line 11: Please do not start sentences with numerals. 9. Figure 3: The dotted MSLP contour is hard to distinguish. 10. Page 8, line 30: Is the maximum Nˆ2 above the tropopause the best measure of TIL strength? It does seem to correlate well with the PV pattern, but would an average value give similar but smoother results? Have you tested this? 11. Figure 4: The caption seems to have incomplete sentences. 12. Page 12, lines 9-10: I'm unsure of what this is referring to. Can you point out where the occlusion and the jet are located? 13. Page 12, lines 17-18: This sentence is unclear. Did those authors look at the same events? 14. Pages 15-16: With the discussion of the Richardson number, it would be useful to the reader to have an explanation of what the results really mean. On page 17, lines 7-9, it seems to imply that despite the low Ri, turbulent mixing may occur, whereas in the discussion section (p18, lines 32-33) it seems

to say that because of the low Ri turbulent mixing occurs. Please could you clarify this. In relation to this, are you suggesting that the strong TIL actually enhances the mixing across the tropopause? This would be in opposition to the Heggelin et al 2009 and Gettelman and Wang 2015 references on page 1 of the introduction. 15. Page 18, line 26: “strong tropospheric updrafts”... I wonder here if what you mean is the ascent associated with the warm conveyor belt (e.g. Madonna et al), which is where you would expect to see the diabatic heating. In Figure 3 it looks like the strongest TIL is clearly associated with the WCB at each time in the lifecycle. After the maximum intensity, the WCB anticyclonic outflow region (to the northeast of the cyclone) is the location of the strongest TIL. I think you need to include reference the WCB and how the observed features relate to it and the associated outflow. Ref: Madonna, E., H. Wernli, H. Joos, and O. Martius (2014a), Warm conveyor belts in the ERA-Interim dataset (1979–2010). Part I: Climatology and potential vorticity evolution, J. Clim., 27, 3–26, doi:10.1175/JCLI-D-12-00720.1.

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