Weather Clim. Dynam. Discuss., referee comment RC1
https://doi.org/10.5194/wcd-2021-8-RC1, 2021

Review of wcd-2021-8

Anonymous Referee #1

Referee comment on "On the occurrence of strong vertical wind shear in the tropopause region: a 10-year ERA5 northern hemispheric study" by Thorsten Kaluza et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2021-8-RC1, 2021

This paper investigates the occurrence of large vertical wind shear in the ECMWF ERA 5 reanalysis, taking advantage of its increased vertical resolution compared to earlier reanalysis products. The authors find that wind shear is enhanced in a region just above the lapse rate tropopause, named here the tropopause shear layer. The climatology and structure of shear around the tropopause is analyzed and the physical processes responsible for its generation are discussed, as well as implications for turbulence and stratosphere-troposphere exchange.

The paper is within the scope of WCD. Overall, I found it interesting and well-written and the referencing appropriate. I recommend publication subject to a few minor revisions (additions) detailed below.

Main comments:

1) The role of model vertical resolution is acknowledged in several instances but could be discussed more thoroughly. The authors mention in the introduction (p 3 l 67) that the previous generation of ECMWF reanalysis (ERA interim) was unable to describe the shear layers. How much improvement has ERA 5 brought ? The statement that it has ‘a sufficient resolution to realistically resolve central features in the UTLS’ (p 6 l 167-168) could be justified, although a comparison to radiosonde or other observations might be beyond the scope of this paper. The authors could refer to Figure 1 of Hoffman et al. (2019) which shows the respective resolution of ERA interim and ERA 5.

Similarly, on p 6-7 l 197-199, the issue of vertical resolution variations with altitude is raised but its impact is not estimated quantitatively. The authors may want to mention that the change in vertical resolution is slight in the region of interest, a few percent (how many ?) compared to the changes in shear occurrence frequency which varies by 2 orders of magnitude in their Fig. 3.

Finally, the authors could briefly comment on the performance of ERA5 compared to the ECMWF operational analysis which they analyzed in Kaluza et al. (2019).

2) Reading the paper made me wonder how much of the shear structure might be diagnosed/explained using the thermal wind relation (for instance the pattern in Fig. 4 and 5 a, the relationship with ridges in Sect. 4.2 or the seasonal variation of the EAJS in Sect. 4.3.1). The authors emphasize (p 24 l 547) that part of the co-location with the tropopause is related to thermal wind balance, as suggested for example in the cited study by Endlich and McLean (1965). They could test this hypothesis quantitatively. Sure, it does not directly explain how the temperature gradient are generated but this is documented elsewhere and a simple diagnosis could here help disentangle "balanced dynamics" from gravity wave effects.

3) If the resolution of ERA5 is good enough to distinguish the LRT from the cold point tropopause (CPT), it would be interesting that the authors determine which of the LRT or the CPT is closest to the enhanced shear layer in the tropics. This would be particularly relevant to the question of the stratosphere-troposphere boundary in the tropics (e.g. Pan et al., 2018) . I note that, in Fig. 5, the shear layer in the tropics is shifted upward by ~ 1 pixel (500 m) with respect to the LRT.

Other points:

p 2 l 30: 'thermodynamic structure' : do you mean because of mixing and heat exchange? If yes, this should be explained. Otherwise, 'dynamic structure' or just 'structure' would fit better (wind shear is strictly speaking not a thermodynamic feature).

P 3 l 61: Please convert feet to meters, following WCD guidelines

p 3 l 91: The authors may want to cite the recent paper by Trier et al. (2020). In this paper the occurrence of CAT around a mid-latitude cyclone is investigated with a special emphasis on its relation with gravity waves. This paper would also be relevant in the discussion, with the caveat that the small-scale waves are likely not resolved in ERA 5.

p 6 line 191 : There is a typo in the definition of Q, with an extra $\times$ which should be removed (the dot is the conventional notation for the scalar product). Also, is it the 'full definition' which is used here, with $\mathbf{\Omega}$ the vector of angular rotation? Although I imagine the differences will be small, I believe it should be replaced by $f \mathbf{k}$ where $\mathbf{k}$ for consistency with the primitive equations solved in the ECMWF model.

p6 line 196-197: I guess altitude is retrieved from the geopotential. Maybe state it explicitly.

P7 l 211: This paper is submitted within a special issue (WISE) and I guess the field campaign motivated the choice of the date. This could be mentioned here.

P10 line 251, figure 5a) and related discussion: Could you also show the equivalent of Fig. 4 a) on top of Fig 4c) which is shown here? This would emphasize the relevance of using tropopause relative coordinates and help understand lines 256-258.

p 13 line 305-306 and Fig. 6: Do you know how exactly this surface is defined in the ECMWF ? In particular, I am surprised that the PV=2 PVU surface crosses the equator in Fig. 6. If there is some adjustment at low latitudes in the ECMWF field it would be useful to mention it here.

P 18 l 384: low ☐ lower . 40 m/s is not a particularly low wind speed even compared to the subtropical or eddy-driven jet.

P 18 l 391: I am not sure how the geographic distribution here can be compared with the radiosondes from 2 stations in Sunilkumar et al. (2015). Agreed, the stations are influenced by the TEJ but from two points it seems complicated to validate a geographic pattern.

A slight difference is that S2015 see this increase above the monthly mean CPT (their fig. 4), which might be 600 m (2-3 ERA5 levels) above the lapse rate (Sunilkumar et al., 2013; Munchak and Pan, 2014). Given the depth of the layer (1 km) used by the authors to investigate shear in tropopause relative coordinates, 600 m is significant. See also main comment 3.

p 19 l 409 : Could you provide the correlation coefficient ?

P 20 l 410: 'neutral and La Nina conditions': do you mean ' neutral and El Nino conditions'?

P 20 l 440: you might consider showing a scatter plot of N2 and S2 to demonstrate this

p 21 l 450: you might note that ERA 5 has been shown to represent realistically part of the gravity wave activity (e.g., Krisch et al., 2020; Podglajen et al., 2020), which justifies that Gws might indeed be responsible for the enhanced shear in the reanalysis.

p 21 l 459 and fig. 13: for comparison, you could depict the distribution of Ri for all values of shear in the same region as well as the distribution over a deeper layer, to determine whether or not the TIL is a region of low Ri number

Typos and suggested reformulations:

p 2 l 30: "an substantial" ☐ a substantial

p 2 l 37: 'linear wave theory' ☐ 'linear theory'

p 6 l 185 : I think it is the pressure velocity ω rather than w which is provided by ECMWF.

P10 l 255 : 'compare e.g.' ☐ 'compare with'

p 13 l 298 : 'barclinic'

p 24 l 547: 'fulfils' ☐ 'fulfills'

p 18 legend of Fig. 11: "destails"

p 22 l 467: “e.g.” should be before the reference

p 24 l 544: I would remove ‘exceptionally’ since your analysis shows that this feature is not an exception

p 24 l 552 : operational analysis □ ERA 5

References:

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