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Interactive comment on "Strong downdrafts preceding rapid tropopause ascent and their potential to identify cross-tropopause stratospheric intrusions" *by* Feilong Chen et al.

Anonymous Referee #1

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Review of the manuscript "Strong downdrafts preceding rapid tropopause ascent and their potential to identify cross-tropopause stratospheric intrusions" Author(s): Feilong Chen et al. MS No.: angeo-2018-78

General Comments

The paper addresses an important aspect in the dynamics of the upper troposphere / lower stratosphere region, namely, relating tropopause height variability with stratospheric intrusions into the troposphere. Using VHF radar measurement data, including 3-D wind, the authors are able to diagnose events of considerable tropopause height drops. They additionally find that in a significant number of such cases, strong

downdrafts occur across the tropopause and extend to the mid and lower troposphere. These local downdrafts occur on the hourly time scale, and as such, they are revealed by the high-resolution radar data, but are missed by the coarser reanalysis or sporadic radiosondes.

My general concern is the interpretation of the considered events as 'rapid tropopause ascent', whereas the detailed case study (and many of the other cases presented) actually show a drop in tropopause height (such as occurring with a cutoff low, or an upper trough/PV streamer). The 'rapid ascent' seems to be a recovery from the drop in height, rather than the important phenomenon itself. Additionally, the downdrafts coincide with the lowest tropopause height and are related to the intrusions themselves. The reference to 'rapid tropopause ascent', i.e., higher tropopause height, compared to normal conditions, may give the opposite impression and confuse the readers. This notion appears in the title and throughout the text, and serves to identify the events climatologically using an ascent criterion, as shown in Fig. 13. In my opinion, diagnosing significant tropopause drops (i.e., both rapid descent and ascent) is more meaningful in the context of intrusions. It will be interesting to see how many of those are accompanied by strong downdrafts.

Overall, I found the presentation of the results in the text and the figures to be clear and concise. There are, however, some issues requiring further clarifications, and I therefore recommend publication if the general concern and the specific comments below are addressed.

Specific major comments

1. I do not understand how Figure 7 and the paragraph describing it in lines 283-287 help to relate the observed oscillations to the mountain waves. Please clarify, or delete this part (also from lines 407-409).

2. The trajectory analysis shows that the mid-tropospheric airmasses originate upstream from ∼7000-9000 m in altitude. This is commonly the upper troposphere, rather

than a clear stratospheric origin as stated (e.g., lines 312, 337). Please support the statements on the stratospheric origin by providing evidence of the lower tropopause height at those locations, or alternatively showing high PV values along the trajectories, or refrain from making these statements. It is relevant to note here that in Raveh-Rubin (2017), almost 99% of intrusions were not stratospheric in their origin.

Raveh-Rubin, S., 2017: Dry Intrusions: Lagrangian Climatology and Dynamical Impact on the Planetary Boundary Layer. J. Climate, 30, 6661–6682, https://doi.org/10.1175/JCLI-D-16-0782.1

Specific minor comments

1. Line 102-103: this sentence is unclear.

2. Lines 105-107: Please elaborate on the spatial and temporal relation between the tropopause ascent and the downward intrusions in Hocking et al. 2007.

3. L 148: “the characteristic (partial specular reflection) mentioned above” is unclear. Please clarify the characteristic (also unclear where is it mentioned above).

4. L 153-154: the description of the RT height determination should be written more clearly. Is it determined by searching upwards from 500 hPa for the first maximum of the gradient? It is unclear what “lower edge” or “secondary maximum” refer to.

5. L 247-248: please also refer to the very significant updrafts that follow the downdrafts. They can potentially be important for the recovery of the tropopause height back to normal, as they extend to the increasing height of the upper troposphere.

6. L 271: Is it related to the high winds at Q compared to P?

7. Lines 295-296: How does the figure support the cross-tropopause aspect?

8. Lines 297-304 and Fig. 9c. I suggest adding relative humidity to the profile, which may show clearer asymmetry between the east and western sides of the cutoff.

9. L 304-305: It is not clear if the low-level high PV is indeed stratospheric in origin as mentioned, or whether it is diabatically produced. See the distinction done in Škerlak et al 2015.

Škerlak, B., M. Sprenger, S. Pfahl, E. Tyrlis, and H. Wernli (2015), Tropopause folds in ERAâĂŘInterim: Global climatology and relation to extreme weather events. J. Geophys. Res. Atmos., 120, 4860–4877. doi: 10.1002/2014JD022787.

10. L 354: Where are the high-pressure systems located relative to the events (height and horizontal location)?

11. Out of the 20 cases, it is a bit hard to keep track of their different characteristics. I suggest summarizing these in a table, and including the main features of Figures 12, S2 and the meteorological systems in lines 350-360.

12. L 363: I suggest to replace “predictor” by “diagnostic”, as they occur at the same time. Also, delete “or prediction” from line 365.

13. Figure 1: It is suggested to add panels with sea-level pressure and low/mid tropospheric wind, to understand the environment of the downdrafts at these heights.

14. Figure 8 and in the main text: please add the time range of the satellite passage.

15. Figure 10 and 11 captions: please mention the height of the ending / starting point, respectively.

Technical Corrections

1. Velocities are shown in km/h and m/s throughout the manuscript. I suggest to be consistent and use only m/s.

2. Line 17: delete “possible”, it is repeating after ‘potential’.

3. L 22: delete “(weakened)”, which is unclear in this context.

4. L 48: delete “long-term” from the second time it is mentioned, before ‘seasonal’.

5. L 52: replace “when comes” to “with regards”.

6. L 60: change “air transport” with “air is transported”.

7. L 64: Move “although” to the beginning of the line.

8. L 93: delete “are”.

9. L 97: change “comparing” to “compared to”.

10. L 127: space is missing after the degree sign.

11. L 145: replace “to” with “away from”.

12. L 159: replace “that” with “,”

13. L 182: replace “with” with “interpolated into”.

14. L 199: replace “bottom” to “southern tip”.

15. L 200: add “as shown by the closed geopotential contour” after “site”.

16. L 220: change “a” with “that”.

17. L 221: change “didn’t” with “did not”.

18. L 223: replace “showed” with “shown”.

19. L 224, and throughout the manuscript: change “UT” to “UTC”.

20. L 263: delete “It is indeed reasonable.”.

21. L 266: replace “impinges” with “impinging”.

22. L 280: replace “Someone may be interested to notice” with “Interestingly”.

23. L 298: delete “with”.

24. L 320: Move “dominant” to after “flows”.

25. L 328: “shown placed end-to-end” is unclear.

26. L 328: delete “and”.

27. L 332: “four range gates” is unclear.

28. L 359: Add “(not shown)” after “48h”.

29. L 360-361: delete “and not possible. . . satellite data”, as it is redundant.

30. L 367: replace “have” with “has”.

31. L 377: replace “excess” with “exceeds”.

32. L 413: replace “What counts is” with “Yet”.

33. L 414: add a ‘-‘ between “AIRS-retrieved”

34. L 415: add a ‘-‘ between “radar-derived”

35. Figure 6 legend: replace dotted orange line with a dashed line as in the plot itself.

36. Figure 12b: It is strongly suggested to use the same colour scale in all panels.

37. Figure S2: There are only 12 events presented, not 20. Please change the format of the dates in the panel titles to be the same as in Fig. 12.

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