

## ***Interactive comment on “Fast descent routes from within or near the stratosphere to Earth’s surface” by H. Itoh and Y. Narazaki***

**Anonymous Referee #3**

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### General Comments

This paper by Itoh and Narazaki provide mainly analysis case studies about stratospheric intrusion in Japan, providing a description of the transport processes (mostly by back-trajectory analysis) from stratosphere to surface. In general, no new information about the occurrence of STE (and limitation on STE detection by modeling tool like air-mass trajectories) are provide. I cannot see a large advance of the STE understating in respect to the large number of researches already done (some of which reported in the reference list). For instance the need of “sloping” isentropic surface for STE in the mid-latitudes, stressed in several part of the paper, is well provided by Holton et al. (1995). Moreover, even if a large data-set of  $^7\text{Be}$  observations (a not unambiguous tracer for STE at surface) is available, the paper results are mostly relying on a

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few case studies, without providing clear or quantitative climatological (or systematic) assessment. In general, some part of the paper are rather confused (e.g. Section 4.1 pag 34452, see also specific comments) and too many figures are provided (which not favor the paper readability). In some parts, the text is not accurate/quantitative: less generic/qualitative info should be provided (e.g. 34452 – 10: “. . .in a few other cases.”: how many? Again, along the text, several indication of “air parcel pushing isentropic surface” are provided: please explain the dynamical meaning of this words!) Thus, I think that publication to ACP is possible only after major revisions.

### Specific Comments

Introduction: at least two papers, describing processes which favour STE can be profitably cited (Nieto et al., Identification and Climatology of Cut-off low near the Tropopause, 2008 and Sprenger, M., M. Croci Maspoli, and H. Wernli (2003), Tropopause folds and cross-tropopause exchange: A global investigation based upon ECMWF analyses for the time period March 2000 to February 2001, J. Geophys. Res., 108, 8518, doi:10.1029/2002JD002587, D12) The aim of the current study should be better stressed. Especially which is the advance in respect to previous studies on STE?

34442 – 14: Impact of deep STE to atmospheric composition has been investigated at mountain stations (considering not only ozone but other atmospheric tracers). See e.g. Trickl et al., ACP, 2010 or Bracci et al., 2012.

34443-5. I do not agree that  $^7\text{Be}$  is a “perfect” stratospheric tracer. Tropospheric  $^7\text{Be}$  concentrations are largely determined by the fate of its carrier aerosol, which is affected strongly by wet/dry scavenging [Gerasopoulos et al., 2001; Zanis et al., 1999]. Most important, 1/3 of  $^7\text{Be}$  is produced in the upper troposphere [Koch and Mann, 1996; Koch et al., 1996], and thus it is not possible to consider  $^7\text{Be}$  as an unambiguous stratospheric tracer. This should be stressed along the paper.

34443- 20. The discussion about  $^{10}\text{Be}/^7\text{Be}$  is meaningless, considering that no  $^{10}\text{Be}$  are available. I suggest to remove.

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34444-15. This is not true! Zanis et al. (2003) showed that HIGH ratio were observed for weather types conducive for STE events.

34444-24. The work by Skerlak et al. 2014 analysed the input of stratospheric air into the PBL.

34444-26. Please check this statement. As an instance the papers by Trickl et al. (2010), Tositti et al., (2014), Cristofanelli et al. (2006), Zanis et al. (2003), analysed STE event development also as a function of "high-frequency" daily/bi-daily or long-term <sup>7</sup>Be measurements.

Pag 34445 – 20. Please check this statement. At least other two papers (Tositti et al., 2014 and Trickl et al. 2003) analysed daily/bidaily <sup>7</sup>Be variability over multi-years period

The sentences from row 23 (pag 34445) to row 6 (pag 34446) should be moved to the "Method and data" Section

34446-24. Is the Spring SD significantly different from other (e.g. autumn)? Did you perform a statistical test? Other locations in Europe showed summer maximum for <sup>7</sup>Be (see e.g. Tositti et al., 2014, Gerasopoulos et al., 2001) due to enhanced vertical mixing (and downward transport) within troposphere during summer months. This can be an interesting difference with your data-set that deserve comment.

34447-10: what do you mean for "measurement time"? Start time? Centroid time?

34448–28: total number of events were 43. Looking to Figure 2, it seems that less events were present. Maybe is just a plotting issue but please check.

34449-9: you explained the summer minimum only in terms of "fast descent routes" occurrences. I suppose that also a role due to wet deposition (higher in summer) can be considered for this region.

34450. The description of case study selection (line 4-14) can be shortened. In my

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opinion, in the current form, it is rather difficult to follow. I suggest to use Supplementary material for describing more details (and show example, e.g. Table 3) about the selection criteria. Also Table 4 can be moved to Supplementary Material: please in the caption substitute "object" by "case studies". For each case you should add information about classification ("fast descent routes" or others)

34451 – 20: "If trajectories vary. . .in not valid". This sentence is not clear to me. Please explain.

34452 – 3: please substitute "systematic trajectories" with "common downward air-mass motion".

34452 – Fig 4. It is not clear to me what do you mean by "maximum latitudinal movement and maximum descent per day". In any case, please try to merge Fig 3 and Fig.4.

34452 – 15/29. In my opinion, this section is really confused and it is hard to follow for the reader. Please try to rephrase.

34453-Fig 5. Is this figure referring to all the cases or just to the "special" 25 cases? Please explain!

34453-11. Is this sentence valid only for the area of investigation or can be generalized?

34454 – 5/16: please, simplify! Simple say that the case study is 17 March 2009 at 18:00!

34458-22: "transformation of the polar vortex". What do you mean?

34458-24: "Because. . .is not conserved". I do not agree that the potential temperature is not conserved because the downward motion occurs in the mid-latitudes. Tropopause foldings and cut-off lows (which occur on isentropic surface) occur at mid-latitudes! Please, rephrase!

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34461 – 12: the definition of tropopause fold is rather rough. See for instance the paper by Sprenger et al, JGR, 2003. You mentioned that the results did not change when different tropopause fold identification methodologies are used: please provide more details. The frequency of tropopause folds presented in Fig. 20 as a function of latitudinal gradient is very different from the results provided by Fig. 8 in Sprenger et al., JGR, 2003. Please comment.

Section 5.2: along this discussion you neglect the role of the Sub-tropical Jet Stream which is a driver for STE in the Eurasian (see e.g. Cristofanelli et al., ACP, 2010).

Section 6: please quantify the fraction of events related to “fast descent” and to other STE mechanism. Add this info also to the abstract. 34464 – 8: it should be clear that this statement is valid only for events characterized by low mixing with tropospheric air and not for the occurrence of STE in general. 34464-25: please clarify which is the add-on in respect to the work by Skerlak et al., ACP, 2014.

In the figure maps, please add references for latitudes and longitudes.

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