Response to the Comments of Reviewer 3

We are grateful to reviewer 3 for his/her useful comments that will help us to improve our paper. We have already completed a tentative revised version, so we use the present perfect tense in the response. First, overall changes are described. After that, we reply to the specific comments. In this response, each comment is numbered like C1 etc. (Comment 1) and the corresponding answer is like A1 etc. (Answer 1). Pages and lines indicated are usually for those in the original version. For convenience, the original comments from the reviewer are shown by blue color, responses by black color, and text in our revised paper by red color.

Overall changes

1. The title has been changed to "Fast descent routes from within or near the stratosphere to Earth's surface in Fukuoka, Japan" following the comment of Reviewer 2.

2. The Introduction has been considerably changed and extended, citing several new papers, which were not cited in the original version, although all the Introduction cannot be shown here.

3. Supplementary material has been newly constructed, and some parts in subsection 3.2 (Case selection for analysis) including Tables 3 and 4 have been moved to it.

4. Figure 11 has been removed.

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).

First of all, our aim is not general STT, but the deepest STT, i.e., to explore fast descent routes from the stratosphere to Earth's surface. Thus, the mothod of backward trajectory is a powerful tool for this object. From this standpoint, please read and evaluate this paper again.

Our paper shows several new findings, although the destination is only one site in Japan. The main findings are as follows:

1. This paper is the first of the deepest STT in non-mountainous areas.

2. We clarified the descent route from high latitudes to the surface, i.e., the time sequence of descent by tropopause folding, southward transport with slow descent at the rear side of a strong trough, strong descent associated with a sharp drop in the isentropic surface at the southern edge of the trough, and downward transport by appropriate near-surface disturbances.

3. We showed that this high-latitude route is the majority among fast descent routes, and that the majority is necessary.

4. We revealed also the existence of the mid-latitude route.

Moreover, even if a large data-set of ^{7}Be observations (a not unambiguous tracer for STE at surface) is available, the paper results are mostly relying on a 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.

Because our aim is not climatology of STT, as mentioned above, it is a matter of course that climatological results are not offered. It is not the fact that cases studied are a few. The fact is that they are a few tens (strictly, 33 cases). Therefore, we believe that general features of fast descent routes are captured.

Figure 11 has been removed, but 22 figures are still provided. This number is surely many compared with other papers, but we think it is too many. If the reviewer points out that some figure(s) is unnecessary, we would like to remove it (of course, when we agree with the reviewer's opinion). The reason that quantitative information is not provided in some parts is because strict definitions are not given. For instance, in the example provided by the reviewer, the strict definition as to whether or not potential temperature is conserved (e.g., potential temperature is conserved when its decrease is less than 5 K in any 3000 m descents (above 2000 m a.s.l.), and otherwise it is not conserved) is not predetermined. However, this kind of definition is not helpful to understand phenomena in question, but rather confused and troublesome. Thus, we must use the expressions like "many cases" or "a few other cases".

"Air parcel pushing isentropic surface" means that moving direction of air parcels and isentropic surfaces are not parallel, unlike the high-latitude route. This information has been added in 34457, l.23: "descending parcels push isentropic surfaces (moving direction of air parcels and isentropic surfaces are not parallel).".

Specific Comments

C1. 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?

A1. We are not fully familiar with the field of STT. Therefore, we had been worried that the review in the Introduction is insufficient. In this respect, we thank the reviewer for letting us know these two papers and related papers mentioned below.

These papers have been cited. However, they do not treat deep STT, so they do not play a central role in the Introduction. The citation is as follows:

"The intrusion of stratospheric air into the extratropical troposphere involves synoptic-scale and mesoscale processes among which tropopause folds and cutoff lows are the most important (Stohl et al., 2003; Sprenger et al., 2003, Nieto et al., 2008).".

in 34441, l.12-14. Sprenger et al. (2003) has also been referred to in subsection 5.2 (Reason for frequent high-concentration days in spring). See A24.

C2. 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.

A2. These papers as well as other papers have been cited. Deep STT over mountainous areas has been described after 34442, l.14 as follows:

"The surface in mountainous areas has similar height to 700 hPa or so. Therefore, there has been a growing number of publications in which destination sites are in mountainous areas, in paraticular, in the Tibetan Plateau or its vicinity (e.g., Cristofanelli et al., 2010; Bracci et al., 2012; Ma et al., 2014; Ohja et al., 2014). Backward trajectory analysis suggests that the position of the subtropical jet stream could play an important role in deep stratospheric

intrusions (Cristofanelli et al., 2010). Impact of deep STT to atmospheric composition of not only ozone but also other atmospheric tracers has been investigated there (Trickl et al., 2010; Bracci et al., 2012). However, even in these studies, the descent mechanism ...".

C3. 34443-5. I do not agree that ^{7}Be is a "perfect" stratospheric tracer. Tropospheric ^{7}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}Be is produced in the upper troposphere [Koch and Mann, 1996; Koch et al., 1996], and thus it is not possible to consider ^{7}Be as an unambiguous stratospheric tracer. This should be stressed along the paper.

A3. As to the first comment about wet/dry scavenging, we mentioned in the original version that

"However, for the purpose of this study, it is suffice to use ^{7}Be concentrations alone. This is because the objective is not to obtain all STT events and their statistical characteristics; instead, it suffices to consider a few tens of (corrected in the revised version from "several") fast descent events. In other words, missing some fast descents is not problematic. In addition, as high concentrations of ^{7}Be are indicative of an absence of wet scavenging, it is simpler to trace backward trajectories in such cases, which is preferable for the present purpose."

As to the second comment, we have rephrased in 34443, l.6- to:

"^{7}Be is a radioactive isotope, most of which (about 70%) is produced in the stratosphere (the remaining 30% in the upper troposphere) by cosmic-ray spallation (Masarik and Beer, 1999; Nagai et al., 2000; Land and Feichter, 2003; Usoskin and Kovaltsov, 2008; Bezuglov et al., 2012).

We do not consider ^{7}Be as an unambiguous stratospheric tracer. ^{7}Be is just a guide to set the starting time of backward trajectories, as clearly stated in the last part of this paragraph. We therefore set a criterion of $z_1 > 8000$m, which leads to high values more than 2 PVU of PV. We have added the following sentence at the last of this paragraph: "Note that, because high concentrations of ^{7}Be do not automatically guarantee stratospheric (or near-stratospherc) origins, other criteria are set together, as described in section 3.", Furthermore, the title is not "Fast descent routes from the stratosphere" but "Fast descent routes from within or near the stratosphere".

C4. 34443- 20. The discussion about ^{10}Be/^{7}Be is meaningless, considering that no ^{10}Be are available. I suggest to remove.

A4. In this paragraph, we state the reason that the use of ^{10}Be is unnecessary and it is suffice

to use ^{7}Be concentrations alone. The reviewer mentions in the above comment that "^{7}Be is affected strongly by wet/dry scavenging". Therefore, if this part was removed, this kind of question would be raised by the reader as the reviewer. Also, at least the information of ^{10}Be is necessary, because ^{10}Be is often used in the next paragraph. Therefore, we think this part should be retained.

C5. 34444-15. This is not true! Zanis et a. (2003) showed that HIGH ratio were observed for weather types conductive for STE events.

A5. We are sorry we made a mistake. We have changed this sentence to: "Zanis et al. (2003) showed that the ^{10}Be/^{7}Be ratio is generally high during stratospheric intrusion episodes.".

C6. 34444-24. The work by Skerlak et al. 2014 analysed the input of stratospheric air into the PBL.

A6. Skerlak et al. (2014) was already cited. The PBL is not Earth's surface. Therefore, they studied deep STT, but not the deepest STT. This difference is very important, as mentioned in the Introduction and others.

C7. 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 longterm 7Be measurements.

A7. Trickl et al. (2010) did not use ^{7}Be in their analysis. The data length used in Zanis et al (2003) is only one year. Tositti et al., (2014) and Cristofanelli et al. (2006) used long-time data with high-frequency sampling (48 h) at Mt. Cimone. We overlooked these papers, so they have been cited as

"From the very start, it is impossible to present mechanisms from the aforementioned ^{7}Be observational data, except the data at the Mt. Cimone station (Cristofanelli et al. 2006; Tositti et al, 2014)."

C8. Pag 34445 - 20. Please check this statement. At least other two papers (Tositti et al., 2014 and Trickl et al. 2003) analysed daily/bidaily 7Be variability over multi-years period

A8. Tositti et al. (2014) has been cited. Trickl et al. (2003, JGR, 8530, doi:10.1029/2002JD002735; Is this the right paper?) do not seem to mention ^{7}Be measurement. This sentence has been change as follows:

Among previous studies, only Kikuchi et al. (2009) examined the data of ^{7}Be concentration almost daily over several years at a non-mountainous site.

C9. The sentences from row 23 (pag 34445) to row 6 (pag 34446) should be moved to the "Method and data" Section

A9. After row 6 (pag 34446), the results of ^{7}Be concentrations are described. Therefore, we connot move these sentences to subsection 3.1. The meaning of the method of "Method and data" is the "method for obtaining fast descent routes", shown as the title of section 3.

C10. 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 ^{7}Be (see e.g. Tositti et al., 2014, Gerasopulous 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.

A10. Statistical tests are not easy to be made, because ^{7}Be concentration is a time series, and therefore, the degree of freedom is unknown. Although the detail is not explained, the degree of freedom with respect to variances can be roughly and conservatively estimated to 250 in some way. By using this value, SD in spring is significantly different from SD in other seasons at the $P < 0.01$ level. Even with other estimations of the dgree of freedom, the conclusion of statistical significance would not change.

To our knowledge, seasonal averages and SDs of ^{7}Be concentrations are different from site to site (e.g., Feely et al. 1989). As mentioned in the last paragraph in section 2, seasonal variations are not our aim, so we did not mention the difference in SD from other sites.

C11. 34447-10: what do you mean for "measurement time"? Start time? Centroid time?

A11. "Measurement time" means the starting and closing times. We have added this information: "Because the measurement time (starting and closing times of measurement) is around 00:00 UTC,.

C12. 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.

A12. Figure 2 is plotted from April 2011 to September 2014, while the analysis period is from 2009 to 2014.

C13. 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.

A13. We agree with the reviewer. However, these sentences around 34449-9 compare the remaining three seasons (winter, spring, and autumn) but not summer.

C14. 34450. The description of case study selection (line 4-14) can be shortened. In my 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)

A14. We have made supplementary material, and details of case selection for analysis including Tables 3 and 4 are described there, although Table 3 is replaced by several figures.

We do not understand the sentences after "please in the caption ...". In particular, what is "classification"? These all cases are objects for analysis of fast descent routes.

C15. 34451 - 20: "If trajectories vary ... in not valid". This sentence is not clear to me. Please explain.

A15. We have changed this sentence to: If the top-1% trajectories vary widely among each other, the average trajectory is meaningless. Does this make sense?

C16. 34452 - 3: please substitute "systematic trajectories" with "common downward airmass motion".

A16. What "systematic" means is described in the sentences after "That is,". Therefore, "common downward airmass motion" does not fit these meanings. We propose a substitute that "Conversely, trajectories in Fig. 3b show systematic motion in latitude and altitude.". Is this OK?

C17. 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.

A17. The position of "the" (the maximum descent) was wrong in the original version. The right position is that "the maximum latitudinal movement and maximum descent per day". It means the maximum latitudinal movement per day and the maximum descent per day. Is this clear? If this is not still clear, we may rewrite the first part as "Paths showing the maximum latitudinal movement (maximum descent) per day by lines terminated by black (red) symbols. Symbols of + and ∘ indicate the starting and final positions, respectively.". See also A18.

We can never merge Figs. 3 and 4 for the following three reasons. First, 33 trajectories are shown in Fig. 3, while 25 trajectories in Fig. 4. Second, Figure 3a shows horizontal trajectories, which cannot be removed, and thus, this figure mismatches the vertical section.

Third, because Figures 3b and 4 show different parts of trajectories, these two can never be merged.

C18. 34452 - 15/29. In my opinion, this section is really confused and it is hard to follow for the reader. Please try to rephrase.

A18. The confusion and hardness seems to be closely related to C17 (because Figure 4 cannot be understood by the reviewer). Now that Figure 4 can be understood, in addition to minor additions and changes (see below), we expect that this paragraph can also be understood. Please read it once more. If it is still confused and hard to follow, please specify what parts are confused and hard to follow. We will try to paraphrase them.

Several expressions have been added and changed: (1) in the caption of Table 4, "start time of the largest descent per day (t_d)," (2) 34452, l.16-: "Figure 4 shows the maximum latitudinal movement and maximum descent per day for these 25 cases. The maximum latitudinal movement per day means that moving distance per day in the southward direction is the maximum along a trajectory. The maximum descent per day has a similar meaning. Table 5 ...", (3) 34452, l.19-: "In Fig. 4, the maximum descent paths (lines terminated by red symbols) are ...".

C19. 34453-Fig 5. Is this figure referring to all the cases or just to the "special" 25 cases? Please explain!

A19. The caption of Fig. 5 clearly says "all 33 cases". We do not understand where 25 cases come from.

C20. 34453-11. Is this sentence valid only for the area of investigation or can be generalized?

A20. We believe that, even for other sites except mountainous areas, it takes statistically a long time (about 4 days or so) to reach the surface from low altitudes such as 700 hPa. The basis of the belief is that the edge of strong troughs is not located near the surface but in low altitudes. Parcels departing from the edge generally drift in low altitudes.

C21. 34454 - 5/16: please, simplify! Simple say that the case study is 17 March 2009 at 18:00!

A21. We also want to simplify this part. However, it is not easy, because case 1 with the highest z_a was already defined. Therefore, we must mention why we do not take case 1 but case 1d. Otherwise, the reader is never satisfied. Then, we have determined that this part is moved to the supplementary material. The main body of the paper has been shortened.

C22. 34458-22: "transformation of the polar vortex". What do you mean?

A22. "Transformation of the polar vortex" here means the extension of the polar vortex southward to the Altai-Sayan region. Maybe, the latter expression is more specific and preferable. We have rephrased this part to "this movement is associated with the extension of the polar vortex southward to the Altai-Sayan region (around 90°E and 50°N) rather than ...".

C23. 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 midlatitudes! Please, rephrase!

A23. We do not say about STT such as tropopause folds and cutoff lows, but we say that the potential temperature is not conserved along the trajectories to low altitudes, where the potential temperature is very low. We have slightly changed this part to: "Because this descent from high altitudes to low altitudes occurs within the mid-latitudes, the potential temperature is not conserved along the trajectories (Fig. 17b)".

C24. 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 Spernger et al., JGR, 2003. Please comment.

A24. Here is one example (Fig. 1) in which the critical values are 2 PVU and 5000 m, although the abscissa is different from Fig. 20.

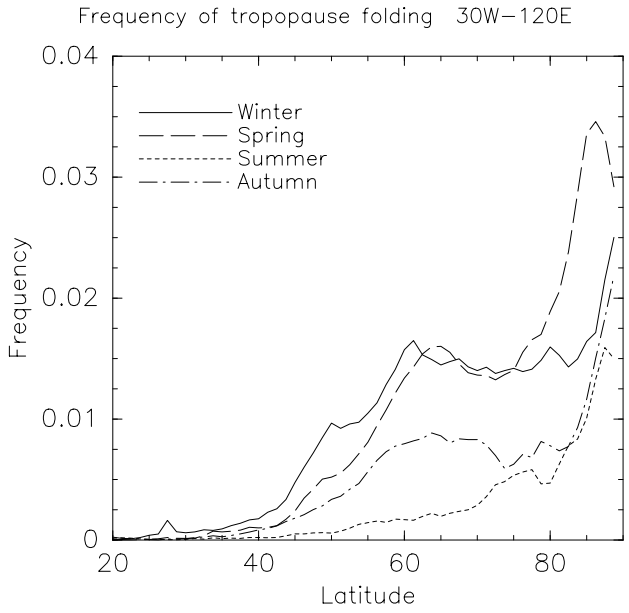

Figure 1: Frequency of tropopause folding for another definition.

"Qualitative similar" means that the statement on Fig. 20 is similar irrespective of the definition. That is, the maximum frequency is seen in spring at high latitudes, and the next is in winter. Also, these two seasons show no significant differences. This usage of "qualitative similar" is conventional. However, this sentence rather raises other questions like the above, so it has been removed in the revised version.

Figure 2 shows schematic figure of two types of tropopause folding. Sprenger et al. (2003) take folding 2 only, while our definition covers both types. This difference seems to produce the difference in the latitudinal distribution of the frequency of tropopause folds. In fact, because folding 2 does not frequently occur near the north pole, it is natural that the frequency in Sprenger et al. (2003) is low in high latitudes. Of course, this is just our speculation, and futher studies are needed.

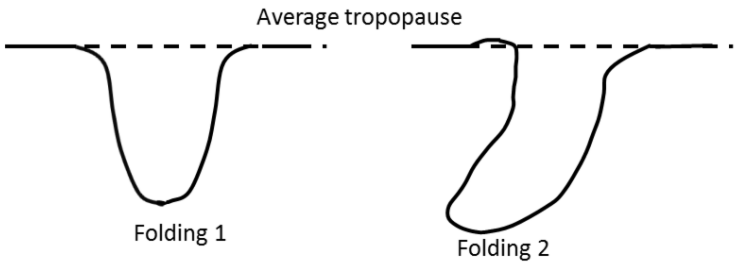

Figure 2: Two types of tropopause folding.

In the revised version, this paragraph has been completely rewritten as follows:

"The climatological tropopause folding was examined by Sprenger et al. (2003). However, their definition seems to cover exclusively folding with bending, but does not cover folding with straightly trailing down. Therefore, we make our own definition, examining tropopause folding. The definition is the state at which the PV surface of 3 PVU trails down to less than 6000 m, although this definition may be a bit exclusive, and it may apply to cases with no precise tropopause folding. Figure 20 shows the seasonal difference in the frequencies of tropopause folding at 30° W–120° E, hereafter referred to as the Eurasian region. This region nearly corresponds to the starting region of the parcels, as shown in Fig. 3a. Figure 20 shows that the tropopause folds are frequent at high latitudes in spring and at 40–50° N in winter. ...".

C25. 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).

A25. We say nothing about STE in general. What we want to clarify in subsection 5.2 is why

high-concentration days in Fukuoka are many in spring. As mentioned almost everywhere in this paper, mid-latitude routes, which may be related to the subtropical jet, are a very few.

C26. 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.

A26. Other STT events are out of scope of this study, and fast descent and other STT are not exclusive. Therefore, we cannot quantify the fraction of events related to fast descent and to other STT mechanism. However, fast descent events are expected to be a very small fraction, because STT events occur almost every time, but fast descent events occur only infrequently. The statement at 34464 - 8 is for fast descent routes to the surface. As mentioned above, we say nothing about STE in general. As to 34464-25, as mentioned in the Introduction, Skerlak et al. (2014) studied "deep STT" but not "deepest STT", which is studied in the present paper.

C27. In the figure maps, please add references for latitudes and longitudes.

A27. Unfortunately, the software used (Dennou Library) cannot give latitude and longitude labels in figures. Therefore, we have added only necessary longitudes by hand, which are cited in text (45°E in Fig. 6a, 45°E in Fig. 7a, 60 and 75°E in Fig. 15, and 105°E in Fig. 18).