Response to the Comments of Reviewer 1

We are grateful to reviewer 1 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). In accordance with the above expressions, we have changed, for example, "see (4)" in the comments as "see C4". 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 Remarks

The paper describes a thorough and thoughtful study of pathways of stratospheric air reaching the low-lying surface site Fukuoka in Japan that deserves publication. The application of 7Be as a tracer of UTLS air is a reasonable approach that could gain from a combination with low water vapour. The combination with trajectory analyses looks convincing, although the station is located at 30 m a.s.l. where mixing could play a role. However, the paper lacks from important deficiencies that must be overcome before publication in ACP can be recommended. In particular, the literature included is sparse and does not indicate the enormous amount of work done in this field. There seems to be a strong bias towards theoretical studies (that are adequately selected). There is also no explanation how the 7Be-loaded air parcels enter the boundary layer. This is a difficult topic, and there have been hints that there is low penetration

at least during daytime. Also the validity of trajectory calculations in the boundary layer must be discussed.

We, both the authors, are not fully familiar with the field of STT. Therefore, we had been very worried that the review in the Introduction is insufficient. In this respect, we are especially grateful to be given many comments. Based on the papers mentioned and the comments by reviewer 1 as well as the other reviewers' comments, the Introduction has been largely rewritten and improved.

Details

C1. Abstract, Lines 20-23: This statement about mixing is courageous. Is there observational evidence that ^{7}Be is higher at higher altitudes? I would expect mixing to be most pronounced in the boundary layer.

A1. From the facts that the potential temperature decreases along the trajectories, and the structure of the trajectories and isentropic surfaces is favorable to mixing, we concluded that ^{7}Be also decreases due to the mixing. We found at least one paper (Kownacka 2002, Nukleonila, **47**, 79-82) showing that ^{7}Be is higher at higher altitudes based on observations. Also, at least one paper (Land and Feichter 2003, JGR, **108**, D12. No.8523) shows the same feature, though this is not based on observations only. (These two papers have been cited in subsection 5.1.) Since the source of ^{7}Be is located in the stratosphere and ^{7}Be is transported to lower layers, it is logically reasonable that ^{7}Be concentrations are higher at higher altitudes in the troposphere. We agree with the reviewer about mixing to be most pronounced in the boundary layer, but this is a different story.

C2. P. 34441, Line 3: Tropopause folding has been reported to have a low seasonal dependence elsewhere (Beekmann et al., J. Atmos. Chem. 28 (1997), 29-44) and deep stratospheric intrusions maximize in winter (Stohl et al., Atmos. Environ. 34 (2000), 1323-1354; Trickl et al., ACP 10 (2010), 499-524). Is this different for East Asia?

A2. These results are obtained in mid-latitudes. All papers state that the occurrence is minima in summer, and Stohl et al. (2000) and Trickl et al. (2010) point out maxima in wither. On the other hand, in our analysis, seasonal variations in high latitudes are mentioned. Therefore, the difference comes from the difference in the latitudinal belts. In fact, tropopause folds occur most frequently in winter and least frequently in summer in mid-latitudes in Fig. 20. Also, the difference between spring and autumn seems to be small in mid-latitudes, compared with that in high latitudes. Thus, ours and these three papers do not contradict each other.

Since the climatology of tropopause folding was reported by Sprenger et al. (JGR 2003), we have completely rewritten a paragraph in subsection 5.2 (34461, l.10-20). Therefore, the difference in the seasonal dependence has not been mentioned.

C3. P. 34441, Lines 8-10: Investigations of STT start in the 1950s. Please, explain why you start with Kida (1997), or include some of the pioneering work.

A3. Kida (1977) made a great advance in STT, but it is wrong that this paper is a pioneering work, as pointed out by the reviewer. We have changed this sentence to the following one.

Therefore, numerous studies have focused on STT since its investigations started in the 1950s (e.g., Reed and Sander, 1953; Machta, 1957).

C4-1. P. 34441, Line 17: "Only recently" is not true. Deep STT has been studied for several decades. In recent years strong interest in the potential impact of STT on near-surface has started, in particular in the U.S. Recent papers are (e.g.) Ambrose et al. (2011), Langford et al. (2012), Lin et al. (2012), Lefohn et al., (2012), Yates et al., (2013), Dempsey, (2014), Langford et al. (2014), Lin et al. (2015).

A4-1. Although we read the above papers, most of them refer to papers in and after the 2000s. Papers before this period seem to us to just infer that elevated surface ozone may be related to stratospheric intrusions without showing routes from the stratosphere. Therefore, we have rephrased this sentence to the following one:

Only recently, full-fledged studies on deep intrusions to the troposphere (deep STT) have been conducted, although, before the 2000s, there were several papers showing that stratospheric intrusions influence surface ozone (e.g., Oltmans and Levy, 1992).

Also, the papers mentioned above, in which trajectory analysis techniques are used, have been newly cited as

"Ambrose et al. (2011), Lefohn et al. (2012), Langford et al. (2012), Yates et al. (2013), Langford et al. (2015), and Lin et al. (2015) also investigated influences of STT to elevated ozone concentrations at several stations in the USA, by using numerical models and aircraft and lidar observations.".

C4-2. A penetration of stratospheric air into the PBL is not trivial. Intrusions tend to slide along the top of the PBL without much vertical exchange. A recent study in the eastern U.S. (not yet published) addresses the issue of daytime in the entrainment of STT air in the PBL. Eisele et al., (1999) show a case of deep STT reaching low altitudes in the early morning followed by downward mixing in the developing PBL. They also discuss previous literature on this subject.

A4-2. It seems to us that Eisele et al., (1999) did not mention the mixing associated with the change of the PBL. However, we think that the indication of the reviewer is likely. So we would like to cite the recent study if possible. Can we cite it in the form of personal communication?

C5. P. 34442, Line 6: There is also a growing number of publications on measurements in or in the vicinity of Tibet, e.g., Zhang et al., Adv. Atmos. Sci. 27 (2010), 1344-1360; Chen et al., ACP 11 (2011), 5113-5122; Cristofanelli et al., ACP 10 (2010), 6537-6549; Bracci et al., J. Appl. Meterol. Clim. 51 (2012), 1489-1507; Sarangi et al., JGR 119 (2014), 1592-1611; Ma et al., ACP 14 (2014), 5311-5325; Ohja et al., Atmos. Environ. 88 (2014), 201-211.

A5. Citing some of the above papers (Cristofanelli et al., Bracci et al., Ma et al., and Ojha et al.), we have added the following sentences after 34442, l.14: "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 ..."

The reason that we have not cited all the papers is because our aim is that the destination is on flat surface but not mountainous regions.

C6. P. 34442, lines 8-9: The first part of this sentence is trivial! You should rephrase the following part like "which is our direct environment". However, I would rewrite the first two sentences in this paragraph entirely. You could also refer to the recent interest in the impact of STT to air quality and cite the papers listed under C4.

A6. We have changed this part to the following one. Because the deepest area of the troposphere is above Earth's surface, a study of descending air from the stratosphere to the surface is very intriguing as "the deepest STT".

C7. P. 34442, Line 19: (Langford, 2014)

A7. Langford et al. was published in 2015. The year in References was wrong, and has been corrected.

C8. P. 34443, Line 7: "mainly" : Please, more precisely! (about 2/3 according to Lal and Peters: Cosmic ray produced radioactivity on the earth, Handb. Phys. 46 (1967), 551-612)

A8. This part has been changed as follows: ^{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)

C9. P. 34443, Line 17: Add citations of such STT studies! You could add those on the following page and some more. From the past 20 years I remember a number of climatological studies, e.g., Elbern et al., Atmos. Environ. 31 (1997), 3207-3226; Stohl et al., 2000, see (2); Zanis et al., ACP 3 (2003), 763-777; Cristofanelli et al., JGR 111 (2006), D03306; Trickl et al., 2010, see (2)). In addition, I found a recent one on low altitude surface observations: Tan et al., J. Radioanal. Nucl. Chem. 298 (2013), 883-891. This one could be cited in connection with your low-altitude study.

A9. Zanis et al. (2003) was already cited. Remaining all the papers have been cited except Tan et al. (2013). However, detailed descriptions of all the papers considerably lengthen the paper, so that descripions have been simplified as a whole. Tan et al. do not treat STT, so we have not cited it.

C10. P. 34443, Lines 25-26: "several" is not enough. One possibility could be "a limited number" . The sentence "In other words, ..." is unnecessary and should be deleted. You do not explain where the problems are!

A10. "several" has been replaced by "a few tens of ...". "In other words, ..." has been deleted.

We are sorry that we do not understand "You do not explain where the problems are!". Does the reviewer mean "problematic"? If so, the problem is explained in the first part of this paragraph as "when 7Be is used alone, rapid descent might be missed". Anyway, this sentence has been rephrased as: "Missing some fast descents is not a weak point for the present purpose.".

C11. P. 34443, Lines 1-3: The references listed under (8) are also related to mountain sites. Ozone is frequently elevated in the presence of elevated 7Be. If you want to present Tsutsumi et al. as a special example, you could introduce that sentence by "In East Asia, Tsutsumi et al. ...." .

A11. The references under (9) (corrected) were already mentioned in A9. As to Tsutsumi et al., we have changed to "In East Asia, Tsutsumi et al. ....", as suggested by the reviewer.

C12. P. 34444, Line 14: Add (Stohl et al., 2003)

A12. Added.

C13. P. 34445, Line 5: I am missing a summary of the existing knowledge on STT air reaching low altitudes (see C4), as a transition to the current study.

A13. The existing knowledge on STT air reaching low altitudes is already described in 34441 and 34442. The effectiveness of the use of ^{7}Be is also already mentioned in the last part in 34444. Therefore, we think that a transition to the current study is not necessary here.

C14. P. 34446, Line 5: Why do you mention two companies (Ortec, Canberra) for one detection system?

A14. Ortec was replaced by Canberra, so we mentioned the two instruments. However, the replacement was done before 2009. Therefore, we have changed to Canberra only in the revised manuscript.

C15. P. 34447, Lines 7-11: "concentration" is not fully precise. However, "specific activity" is related to mass (Bq kg^{-1}) and is, thus, not a better solution. Maybe you should use quotation marks around concentration in Line 7 (first case). What is the accumulation time?

A15. We understand that "concentration" is not fully precise. However, we checked that similar papers use concentration without quotation marks, so we think this usage is permissible.

We do not use "the accumulation time" anywhere.

C16. P. 34448: There is no information on the calculation of PV!

A16. PV is given at isentropic surfaces in the JRA-55 data. We can obtain PV at arbitrary points by the interpolation of two PVs at two adjacent isentropic surfaces. We have added the following sentence: Since PV is given at isentropic surfaces in the JRA-55 data, PV at arbitrary points is obtained by the interpolation.

C17. P. 34452, Line 3: What is the "eastern hemisphere"?

A17. "Eastern hemisphere" means the hemisphere from 0°E eastward to 180°E. This word is found in many dictionaries, so we think we can use it without annotation.

C18. P. 34452, Line 7: "higher latitudes"? "high" could be around the North Pole! 45° is normally named "mid-latitude". Please, carefully examine the paper elsewhere!

A18. In this paper, "high latitudes" generally means north of 60°N. In this particular part, 25 cases out of the 33 cases attain their highest altitudes north of 60°N, so that we express that the main descent routes are high-latitude ones. We have added some words in this sentence to avoid the misunderstainding: ..., and 25 cases attain them north of 60°N, i.e., in high latitudes.

C19. P. 34457, Line 1: "that the potential temperature is not conserved along the trajectories"?

A19. We have changed this sentence as suggested by the reviewer.

C20. P. 34457, Line 12: Why do values mix? Air parcel can mix. Is this a numerical effect? Free tropospheric mixing is a rather slow process (see Trickl et al., ACP 14 (2014), 9941-9961, for some discussion).

A20. The potential temperature is not conserved along the trajectories. This is evidence of mixing. Generally speaking, free tropospheric mixing is a rather slow process, as mentioned by the reviewer. However, when the deformation is large, mixing is also large even in the free atmosphere.

C21. P. 34457, Line 18: "high altitudes" or "high latitudes"? Please, make sure. If "altitudes": Please, specify!

A21. This is definitely "high altitudes" as in the text. "Altitudes" are specified as more than 8000 m in Fig. 15. To avoid the misunderstanding, we have changed this sentence to "The area near air parcels at high altitudes (the area near 40–45°N and 60–75°E)...".

C22. P. 34457, Line 19: "diffluent area" looks strange. Do you mean "area with predominantly diffluent air masses"?

A22. Yes, we do. Maybe, we should change to "a diffluent-wind area". Does this make sence?

C23. P. 34457, Lines 21-23: "this southward flow": there is also a northward flow! Please, describe clearer. The sentence "That is, descending parcels .... but parts of them." is also difficult to understand.

A23. We have rephrased this part as follows: The parcels on the trajectories ride on the southward flow of these two flows. That is, descending parcels are not most of the parcels coming from the west but parts of them, because the other parts ride on the northward flow.

C24. P. 34458, Lines 15-17: First you write "rapid descent", here "considerably long (longer?) time". I suggest writing "somewhat longer time" . "low altitudes" is confusing. The intrusions start at high altitudes!

A24. We meant "the rapid descent from high altitudes to low altitudes (but not Earth's surface)". This sentence has been deleted, because the information given by this sentence is not important, and might be confusing.

C25. P. 34459, Line 18: "it is rare" : Do not generalize! Better "First, we conclude from our analysis ..." .

A25. We have changed this sentence as suggested by the reviewer: First, we conclude from our analysis that it is rare ... .

C26. P. 34460, Line 12: "reference altitudes of 8000 and 10000 m."?

A26. We have changed this part as suggested by the reviewer: ... of the two reference altitudes of 8000 and 10 000 m.

C27. P. 34461, Lines 1-3: According to Beekmann et al. (1997) (see (2)) the frequency of tropopause folding is rather constant over the year. Please, explain or rephrase! Do you refer to deep intrusions which would make sense?

A27. See A2.

C28. P. 34461, Lines 5-6: I think you do not exclude the difference! The impact is what matters.

A28. We do not understand the reviewer's question. We have shown that the inclination of isentropic surfaces in the troposphere is almost same irrespective of seasons. Therefore, the seasonal difference in the frequency of high-concentration days does not depend on the inclination.

C29. P. 34461, Line 13: Is 3 PVU correct or a misprint? (2 PVU?)

A29. 3 PVU is correct. Therefore, we have written as "This definition may be a bit exclusive".

C30. P. 34461, Line 24: The variances of what? Please, more precisely!

A30. We have meant the variance of the 2–8-day period component of the meridional wind ("day" was dropped in the original). However, the repetition of this long expression is awkward. It seems to us that "its" does not fit other than "of the 2–8-day period component of the meridional wind". If the reviewer has a more reasonable expression, please give us it. We are willing to replace "its" by it.

C31. P. 34463, Line 2: "folds are frequent" : See C27!

A31. See A2.

C32. P. 34464, Lines 4-5: "mixing" : See C20. See also Line 9.

A32. See A20.

C33. P. 34465, Line 9: What means "high" ? Lower-tropospheric ozone peaks rarely exceed 80 ppb. Maybe "elevated" is more adequate.

A33. We have changed this part to "... predict elevated ozone concentrations".

Tables and Figures:

C34. Table 1, Line 1: What is the accumulation time?

A34. We do not use "the accumulation time".

C35. Fig. 3: Specify units (altitude, potential temperature), also in other similar captions

A35. We have specified units in Figs. 3, 4, 6, 12, 13, and 17

C36. Fig. 5: "the last two days of travel" ; "Horizontal component of the trajectories" (please, examine also elsewhere, e.g., Fig. 6)

A36. We have changed the expressions including other figures (maybe, Figs. 3 and 6 only) as suggested by the reviewer.

C37. Fig,. 16, Line 2: "on 16 December" (see also following captions)

A37. Maybe, this expression (expression without "on") is permissible. There have been many other papers in which this expression is used.

Style

We have changed all the items as suggested by the reviewer, except the following ones. The order has been changed.

(2) P. 34442, Line 3: "33 years of" ?

We have changed this part to "by using 33-year ECMWF reanalysis data". Does this make sense?

(5) P. 34443, lines 1-2: "backward destination" ?

In this case, "forward destination" is right, and we have changed so.

(7) P. 34443, Line 24: "is sufficient"

This part has been deleted.

(11) P. 34447, Line 21: I would use a comma ahead of "but" for more clearness, although this prescribed only if there is a second verb.

In this sentence, there is no second verb, so we think that "a comma" is not necessary.

(14) P. 34451, Line 20: "vary among": Do you mean "range within"?

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?

(15) P. 34452, Line 12: "descent by 6892 m within two days" . You should specify which days (e.g., "within the first two days").

The specification is not easy, because two days are generally, for example, from 15 hours on the second day to 15 hours on the fourth day. This expression is awkward.

(16) P. 34453, Line 6: "to finally arrive" is a split infinitive, prohibited by the English grammar. Better "to arrive finally". See also Line 22, P. 34460 (Line 19),

According to Wikipedia,

https://en.wikipedia.org/wiki/Split_infinitive#cite_note-Walsh112-2

"Most modern English usage guides have dropped the objection to the split infinitive." Also, this manuscript was checked by a native English speaker, and this phrase was not corrected. Maybe, a split infinitive is permissible in modern scientific papers. In fact, we can see many examples of split infinitives in various scientific papers.

(18) P. 34454, Line 1: "Case studies"

To our knowledge, "case study" is a generic name, which is used for analyses for several cases.

(20) P. 34455, Line 10: "18:00 UTC on 11 March"

(23) P. 34458, Line 14: "12:00 UTC on 16 May 2011"

See A37.

(22) P. 34457, Lines 23 and 25: Does "push" mean "hit"?

(24) P. 34459, Line 8: "pushes" : "hits" ?

(33) P. 34464: Line 4: Replace "push" by "hit" ?

We cannot judge which one, "push" or "hit", is preferable. "Descending parcels push isentropic surfaces" (p. 34457, l. 23) means that moving direction of air parcels and isentropic surfaces are not parallel, unlike the high-latitude route. This information has been added there: "descending parcels push isentropic surfaces (moving direction of air parcels and isentropic surfaces are not parallel).".

If the reviewer still think that "hit" (or some other) is better than "push", we are ready to replace "push" by "hit" (or some other).

(28) P. 34460, Line 2: "northerly flow" ?

(30) P. 34461, Line 22: "northerly wind" ? I just know "the northerlies" or the "westerlies".

(31) P. 3363, Line 14: "northerly flow"

"Northerly" is shown as "a wind from the north" (American English Dictionary). Therefore, just "northerly" is permissible.