

Response to the Comments of Reviewer 2

We are grateful to reviewer 2 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
5 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

- 10 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.
- 15 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

This manuscript presents a back trajectories analysis of stratosphere-to-troposphere trans-
20 port for a site in Japan. The analysis is based on a fourth-order runge-kutta method applied to Be7 concentration data as the main tracer for such transport. The method is interesting and unveils some possibilities. The results obtained are in agreement with previous knowledge in the field and I have to say that they are not too surprising. Anyway I would like to congratulate the authors for the work performed and the correct presentation.

25 In my view the main strength of the work here presented is that the methodology confirms the results expected from previous literature on this topic and therefore I would suggest to rewrite part of the manuscript to focus on it.

As to the evaluation of “not too surprising”, at least we were suprised at the following findings:

- 30 1. We discovered the mid-latutude route, which could not be expected. Also, the potential temperature sharply decreases along the trajectories of this route.

2. The high-latitude route is so systematic that the time sequence of descent by tropopause folding, southward transport by a strong trough, strong descent at the southern edge of the trough, and downward transport by appropriate near-surface disturbances is considerably regular for many cases.
- 5 3. There is a case (case 19) in which descent is as large as 6892m for two days, with 4659m recorded on the first day.

We also thank the reviewer for letting us know several papers below, which we did not cite in the original version. We are not fully familiar with the field of STT. Therefore, we had been worried that the review in the Introduction is insufficient. Now, we think that the Introduction
10 has been very improved.

Main concerns:

C1. along the manuscript you show how the high-latitude path is the predominant route. This is not surprising. You clearly state that tropopause folding is one of the main mechanisms for the STT using this route. A clear fingerprint of tropopause folding is the simple calculation
15 of multiple tropopauses (MTs). You plot the frequency of foldings in a figure for each season, but this one is unnecessary. Take for example Añel et al. (2008). You can see clearly how the region considered has a clear maximum of MTs (and therefore probably foldings) for the seasons with your cases for high-latitude transport. Also you show how some of these high-latitude routes have a huge latitudinal transport maintaining a high altitude and then descent
20 to levels closer to the surface. If you consider the structure of the folding this is again not too surprising. If you check the theoretical study of Wang and Polvani (2011) and the empirical demonstration for a case study by Añel et al. (2012), high-latitude transport of stratospheric air to the levels between MTs is proved to be the preferred mechanism. That is, maybe to the tongue of the fold. Therefore you should discuss your results having into account this
25 previous research results. Maybe it would be a good idea to have some information in the text (maybe a table) about the different trajectories and the existing tropopause structure for the studied days. Then your statement at page 34461 about the relationship between the low frequencies and the low number of high-concentration days seems to be according with what could be expected a priori and therefore not too surprising.

30 A1. Thank the reviewer for introducing Añel et al. (2008), Wang and Polvani (2011) and Añel et al. (2012). However, there is a striking systematic difference between MT events and the tropopause folds in the present study, that is, occurrence frequencies show maxima in mid-

ltitude (i.e., zones of maximum cyclogenesis) in the former but in high latitudes in the latter. Pan et al. (2009) attribute MT events to latitudinal migration of the tropical tropopause over the extratropical one. Therefore, MT events may include not only tropopause folds but also other phenomena. In other words, MT events and tropopause folds do not correspond to one-to-one. Thus, we think it is still meaningful to show Fig. 20.

Anyway, we have cited Añel et al. (2008) only in subsection 5.2 (Reason for frequent high-concentration days in spring). This is because tropopause folding was intensively investigated by Sprenger et al. (2003), and therefore, this paper has been cited as the main paper about tropopause folding. This citation may not be what you have expected. However, MTs occur in mid-latitudes, so that parcels constituent of MTs do not come via the high-latitude route found in this study, as mentioned in Añel et al. (2008).

We are sorry we do not understand “Maybe it would be a good idea ... therefore not too surprising.” Therefore, we did not take into account this suggestion this time. However, if we understand it, we are ready to take into account it (of course, if we think it is a good idea).

C2. When you discuss the STT exchange you make specific mention to the exchange over the Tibetan Plateau. In fact you mention in the conclusions the possibility of stratospheric air reaching the surface. The connection between STT and stratospheric air reaching surface levels has been very studied in the last years. Specifically for the Tibet it has been proved by Chen et al. (2013). You should check it and include it in the discussion of the results, as it can help to support your discussion.

A2. STT studies over the Tibetan Plateau including Chen et al. (2013) have been added after 34442, l. 14 in the Introduction 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 particular, in the Tibetan Plateau or its vicinity (e.g., Cristofanelli et al., 2010; Bracci et al., 2012; Chen et al. 2013; 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 ...

As we wrote as “no studies have focused on the access of stratospheric air to the surface other than that in mountainous areas”, the aim of this study is the reach of stratospheric air to the

surface in non-mountainous areas. Therefore, we did not intensively discuss studies over the Tibetan Plateau.

C3. I am concerned in some way about the lack of a clear protocol for ^7Be measurements. I do not doubt about the representativity of the measurements for the purpose of your research, but you should clearly state the measurement protocol and why it can be considered enough for the research here described. Also, for scientific reproducibility, where can we obtain the data?. This is really important (Section 2).

A3. It seems to us that our description about the protocol for ^7Be measurements is similar to that in other papers. However, the accuracy was not mentioned, so we have added the accuracy as:

“Error of ^7Be concentrations is typically 5%.”

after 34446, 1.6. Of course, we are ready to add more if the reviewer points out specifically what he/she thinks lacks in the protocol.

The data are not opened at the present, because we are preparing other papers using the data. We want to open them near future.

C4-1. You consider 9000-10000 m to be stratosphere. Later you extend your reasoning to the PV values as a marker of the stratospheric origin of air masses. It is interesting that with a top of 270 K and 10 hPa all the trajectories begin at so low altitudes. Have you considered computing the tropopause for each case to be sure about the origin of the air masses?. My point is that in some cases you show values below 2.5 PVU and this could not be representative of stratospheric air. I recognize that 2 PVU is broadly used to make differences between the troposphere and the stratosphere, but the origin of this was documented by the WMO where it was reported the use of this value during a measurement campaign. That is, it is something used but not “official”. As Hoinka (1998) points out, values below 3.5 PVU could not be a good idea. At least I would like to see an statement about potential impacts of changing this values on the results.

A4-1. We did not give the definition of the tropopause, because this itself is not a simple task. Therefore, we recognize that all cases do not show the transport of stratosphere-origin air mass in a strict sense. Therefore, the title includes “... from within or near the stratosphere ...”. 2 PVU is just a measure of stratospheric air, so we state that “all cases have the characteristic of stratospheric air in terms of PV.” Under the above recognition, should we add or change text?

C4-2. The authors have put a lot of effort on showing results for each case, but then the

manuscript contains 23 figures and this makes it hard to follow sometimes (almost boring). Some suggestions: would it be possible to produce figures that contain all the plots for each case study instead of three different figures for each one?. Then it would be easier to see the full picture of the situation. Could you slightly reduce the length of the section 4 and subsections?.

A4-2. 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. We have no idea how several (what do three different figures mean?) figures are gathered into one, because one figure itself is large, so we think we can see no detailed information from unified figures. 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).

We would really like to shorten the manuscript, but it is not easy. One idea is to remove the description and figure (Fig. 12) about case 3 (subsection 4.2.1). However, at least, the process of a cutoff low should be mentioned.

C4-3. Moreover I suggest changing the title, it is too generic. Focus on the use of ^7Be and the region of study.

A4-3. The title has been changed to “Fast descent routes from within or near the stratosphere to Earth's surface in Fukuoka, Japan”. Including the use of ^7Be makes the title too long, so we do not adopt this idea.

Minor issues:

all the manuscript: the degree symbol for cardinal points is not separated, therefore the numeral and the cardinal direction and degree symbol must appear without spaces.

To our knowledge, in all ACP papers the degree symbol for cardinal points is not separated.

Maybe, the Production Office, Copernicus Publications, checked the paper style including the degree symbol, unit, and so on.

page 34442, line 24: please, make clear here what you mean by “rapidly”. Maybe moving the explanation in lines 25-26 in page 34447 here would work

It is possible to rewrite to “rapidly (within 10 days)”. However, to our impression, just “rapidly” is more suitable, because the expression “10 days” is too specific, and is not suitable for the Introduction.

page 34445, line 20: explain the meaning of JST

JST is the Japan Standard Time. We have changed to “(09:00 Japan Standard Time)”.

page 34446, line 3: 10 t? What force unit is this one? Please, use international system units

“10 t” means “10 tons” (10×10^3 kg). The use of the unit t is permitted by CIPM (Comité International des Poids et Mesures). We think that also in this case, the Production Office, Copernicus Publications, checked this expression.

5 page 34450, lines 4-25: the explanation reads too complex. Maybe an idea would be to use a figure with a flux diagram to explain better how the process is done. Please, try it.

As mentioned in the overall changes, we have made supplementary material, and details of case selection for analysis including Table 3 are described there. Although a figure with a flux diagram is not made, several figures instead of Tables 3 and 4 are made, which better explain
10 how the process is done.

page 34451, lines20-21: could you explain better the meaning of “If trajectories...not valid”?

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?

Maybe Fig. 11 could be removed, I do not find it so useful

15 Removed.

page 34458, lines 22-23: can you explain better what you want to say by “transformation of the polar vortex”? Do you mean splits, displacements, associated phenomena?

“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
20 have changed 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 ...**”.

page 34459, line 17: I do not think that “necessity” is the right word here. Please, try something different.

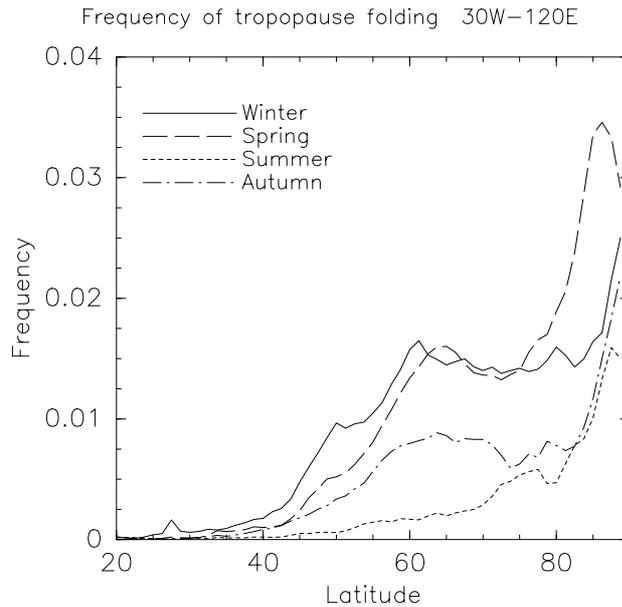
We have changed to: **Reason that the high-latitude route is the majority.**

25 page 34460, line 21: instead of “parcels must descend” I would say “a potential path for parcel descend is”

We have changed to: “**a potential path for parcel descent is ...**, as suggested by the reviewer.

page 34461, lines 15-16: what does it mean “qualitative similar”? How does it change? This is very important, as your definition as you state is pretty “basic” and the studied area is large

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



“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. Furthermore, this paragraph has been completely rewritten because Reviewer 3 lets us know the paper of Sprenger et al. (2003), in which the climatological tropopause folding was intensively examined.

Acknowledgements: I would include the software used in the methods, with references if possible. Also it is good that you have used “free software” as it helps to assure the reproducibility of your work. This should be highlighted.

The program that trajectories are pursued was made on our own, because the accuracy is very important in this study and free softwares are sometimes not fully accurate. However, similar trajectories could be traced by using any free softwares.

References: Dutkiewicz and Hussain is not cited in the text but it appears in the list of references. Langford et al. was published in 2015, not 2014. Sprenger et al. 2003 is not cited in the text. Corrected. We are sorry for careless mistakes.

table 2: the acronyms used here (z_a, z_1) have not been explained the first time that the table is cited in the text. Please, solve it.

These kinds of examples frequently appear not only in tables but also in figures. Only the second

row in Table 2 is necessary at the first time, and the explanation of z_a and z_1 is unnecessary. Then, after the explanation of z_a and z_1 , the third and other rows are cited, in which z_a and z_1 are used. This is essentially the same way that Figure *a is referred to at the first time when Figure *b is not explained yet, and, therefore, the reader cannot understand what is depicted in Figure *b.

table 5: “h” is not an international unit, so please, write “hours”

The use of the unit h is permitted by CIPM (and, therefore, the Production Office, Copernicus Publications). Rather, “hour” is not general as a unit.

figure 2: in the horizontal plane, please, include monthly marks. Right now it seems as 2014 was complete.

We have included monthly marks.

figure 4 and others: please, explain or make explicit that units for the isentropic fields are K.

Unit K has added in all the figures, in addition to unit m if necessary.

figure 10: please, remove from the figure unnecessary or not explained information, for example the weird numbers at the top of the figure. Also, for figure 10 you should clarify what is the positive value for the direction

We have removed unnecessary information in Fig. 10 and others. However, as another idea, the replacement of “09031718” by “18 UTC 17 Mar. 2009” may be valuable to consider. How does the reviewer think?

figure 15: could you include labels for longitudes?

Unfortunately, the software used (Dennou Library) cannot give longitude labels in figures. Therefore, we have added only an necessary longitude (60 and 75°E) by hand. Also, longitude labels referred to in text are added (45°E in Fig. 6a, 45°E in Fig. 7a, and 105°E in Fig. 18).

figure 23: if you are talking about folds, then in the schematic figure, a folded structure should be drawn, instead of an “average tropopause”. In my view the current isentropic surface line should go through the fold.

We have added a folded structure in this figure. Is this OK (see the next page)?

