Atmos. Chem. Phys. Discuss., 15, C11229–C11233, 2016
www.atmos-chem-phys-discuss.net/15/C11229/2016/

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

Received and published: 6 January 2016

This manuscript presents a backtrajectories analysis of stratosphere-to-troposphere transport 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.

In my view the main strenght 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 rewritte part of the manuscript to focus on it.

Main concerns:

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Discussion Paper

1/ along the manuscript you show how the hihg-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 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 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 fo a case study by Añel et al. (2012), hihg-latitude transport of stratospheric air to the levels between MTs is proved to be the prefered mechanism. That is, maybe to the tongue of the fold. Therefore you should discuss your results having into account this 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.

2/ 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.

3/ I am concerned in some way about the lack of a clear protocol for Be7 measurements. I do not doubt about the representativity of the measurments for the purpose of your research, but you should clearly state the measurement protocol and why it can be

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considered enough for the research here described. Also, for scientific reproducibility, where can we obtain the data?. This is really important (Section 2).

4/ 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 document 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.

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 lenght of the section 4 and subsections?.

Moreover I suggest changing the title, it is too generic. Focus on the use of Be7 and the region of study.

References: Hoinka, 1998: Statistics of the Global Tropopause Pressure. Mon. Wea. Rev., 126, 3303–3325. Añel et al. (2008) http://onlinelibrary.wiley.com/doi/10.1029/2007JD009697/abstract Wang and Polvani (2011) http://onlinelibrary.wiley.com/doi/10.1029/2010JD015118/abstract Añel et al. (2012) http://dx.doi.org/10.1100/2012/191028 Chen et al. (2013) http://journals.plos.org/plosone/article?id=10.1371/journal.pone.0056909

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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. - page 34442, line 24: please, make clear here what you mean by 'rapidly'. Maybe moving the explanation in lines 25-26 in page 3447 here would work - page 34445, line 20: explain the meaning of JST - page 34446, line 3: 10 t? What force unit is this one? Please, use international system units - 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. - page 34451, lines20-21: could you explain better the meaning of 'If trajectories...not valid'? - Maybe Fig. 11 could be removed, I do not find it so useful - 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? - page 34459, line 17: I do not think that 'necessity' is the right word here. Please, try something different. - page 34460, line 21: instead of 'parcels must descend' I would say 'a potential path for parcel descend is' - 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 - 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. - 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. - table 2: the acronyms used here (za, z1) have not been explained the first time that the table is cited in the text. Please, solve it. - table 5: 'h' is not an international unit, so please, write 'hours' - figure 2: in the horizontal plane, please, include monthly marks. Right now it seems as 2014 was complete. - figure 4 and others: please, explain or make explicit that units for the isentropic fields are K. - 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

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clarifiy what is the positive value for the direction - figure 15: could you include labels for longitudes? - 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.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 34439, 2015.

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