Review of "Observation and modelling of high-7Be "

by E. Brattich et al.

General remarks

This paper presents a case study of high ^{7}Be events observed in Northern Europe in early 2003. The authors use a model simulation to interpret the presented measurements. They further consider a range of auxiliary parameters like potential vorticity. These data are of interest to the readership of ACP and the presented analysis helps the understanding of the measurements.

However, some aspects are not discussed. I think the altitude of the measurement sites should be mentioned (I have not looked them up). Transport from the stratosphere is considered to be an important question, but how far down reaches the transport? This is not an obvious issue as not all stratospheric intrusions reach sea-level. Also, are there any ground level ozone measurements at these sites that could be considered? Ozone profiles are considered (Fig. 8), but here I cannot see a huge enhancement. Of course ozone has different properties than ^{7}Be, but such issues could be better discussed.

Further, the agreement of the model results and the observations need not to be perfect; this is a different modelling task. But there should be more focus on the *processes* that cause the downward transport. Such an analysis should go beyond stating that there is reasonable agreement between the model and the observations. Figure 7 shows temperature profiles that show differences regarding the days when high ^{7}Be events are observed, but what do we learn about the processes at work? Is there anything special about the SSW in early 2003 (on which the paper focuses)? Or would one expect such events for all SSWs that regularly occur in Arctic winter? Is there any altitude dependence of the stratospheric intrusion; how well is (or needs to be) the planetary boundary layer simulated (for the simulation of an intrusion down to the ground). All these questions are not addressed in the manuscript. While I do not expect that such questions can all be satisfactorily answered, I suggest more discussion of the issues. And more focus on the processes of stratospheric intrusions under conditions of a SSW.

Regarding the organisation of the paper, I note that it has many sections, but no section called “Results”; perhaps such a section could be introduced, with the appropriate subheadings. There are also some issues with the wording and with the references (I have listed some examples below), so I suggest a careful proofreading of the revised version. (By the way, the references in this review are only to make the points I mention more easily understandable; this is not a suggestion for citations).

In summary, I think this is an interesting paper, but it needs a revision addressing better the points raised above). I would expect a revised version of this paper would be acceptable and would be of interest to the readers of ACP.

Comments in detail

- Title: this paper is about surface observations; you could introduce this word in your title
- l. 15: Say at which altitudes these values are recorded.
- l. 30: radiative or radioactive?
- l. 37: I think instead of vertical transport you mean downward transport
- l. 49/50: The study of Salminen-Paatero is certainly not the using PV to analyse transport of air into the troposphere. I suggest to be more specific on Salminen-Paatero or to discuss the aspect more generally, which would likely involve more references. A classic paper is for example Danielsen (1968) an there is the review by Holton et al. (1995, cited in the paper elsewhere).
- l. 61: change to: and the Southern hemisphere
- l. 63: drop ‘the’ and ‘seasons’
- l. 65: There is a lot of discussion in the paper on SSWs; given the importance of the concept and in particularly the extension of the impact to outside the polar vortex I suggest a bit more discussion on SSWs

(e.g., Charlton and Polvani, 2007; Charlton et al., 2007; Sofieva et al., 2012; Tao et al., 2015)

- l. 87: Many people (including me) would argue that only the Antarctic ozone hole should be called by this name, even very strong recent Arctic ozone losses (Manney et al., 2020) have not been refered to as an "ozone hole". Suggest changing the wording.
- l. 129: The 7Be data are discussed here. I suggest in addition a short explanation on the measurement principle.
- l. 159: which data set from ECMWF was used? Perhaps ERA-I (Dee et al., 2011)? Be specific and provide a citation. Also add ECMWF to the acknowledgements.
- l. 195: which vertical velocity was used in the HYSPLIT calculations?
- l. 205: For which altitudes is this statement relevant and appropriate? E.g. for the entire troposphere?
- l. 243: suggest 'chemical composition' (if this is what is meant here.
- l. 255: An alternative data set is TRMM: why is this data set not considered? Not the right region? You could briefly comment. Also: there might be local precipitation measurements at the sites in question.
- l. 289: is the horizontal or the vertical resolution the issue here?
- l. 334: not really clear, I think you mean something like "impact on surface weather"
- l. 355: Say "MERRA-2" her, this is not the same thing as a simulation
- l. I agree that the temperature structure is different, but where in these profiles do I see an indication for the processes causing downward transport?
- l. 370: ozone in the troposphere – is this shown here? Do you need a reference?

- l. 374: there is nothing wrong with using potential vorticity from ECMWF, however potential vorticity can also be computed from MERRA-2 data, which might be more consistent. Is there a reason for using ECMWF here?
- l. 377: potential vorticity is not really a conserved quantity; what is the life time in the troposphere? In this way one could learn about the timescales of vertical transport
- l. 389: why 1000 m? This model assumption should be explained at this point. y
- l. 398: Give the exact reference (fig., section) where this is shown.
- l. 414: Perhaps say more clearly that the *horizontal* resolution is your point here.
- l. 422/423: I agree that tropospheric ozone on 19 February is somewhat enhanced, but not very much (30 ppb is not a high value. And the ozone signal does not reach the ground; is this in line with you explanations?
- l. 434: here and above; you use the backward trajectories for a discussion of the vertical transport, which is the key issue here. Why is this information not considered?
- l. 440: in a future climate there might be more SSWs – but would all of these SSWs lead to high surface 7Be values? I think you need to argue for more SSWs of the type considered here.
- l. 442/443: You should state here, where the data are *available*, not where in the paper they are discussed.
- l. 690: I think the journal should be abbreviated here
- l. 692: 2017 or 2016? Both years are in the reference...
- Fig. 1: here and elsewhere, I think the town in Sweden is called Umeå, use `\aa` in LaTeXif you like.
- Fig. 1: remove grey background from bottom panel.
- Fig 12: the black line is the zero line – correct? Mention in the caption.

References

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