

The authors have really done a lot of work to the paper since the previous version, both as a result of the reviews and as a result of new ideas they have had themselves. This is good in the sense that they have very seriously addressed the comments of the reviewers, but of course hard for the reviewer because it is like reading a new paper. For this reason, there are still several comments on this version even though it is now good. In any case, the paper is now more logically organised; I am happy (with a small comment) with the way the number of recyclings is treated; and the discussion of how ice core data may be used is much more precise than I remember in the previous version. The paper is still a very difficult read in places but I don't think anything is incorrect, so readers can dip into the parts they need. It certainly provides a very nice basis for assessing and understanding the mass and isotopic measurements of nitrate in snow, and it should be published after minor corrections listed here.

Page 3, line 13. I think this is the first use of the abbreviation DC. If so, please give the definition first.

Page 7, line 9. I am not sure your description that your box has a large surface area "to neglect local lateral air movement" is really right, or perhaps not well expressed. You really do need lateral air movement because that is what takes FE out of the system. Wouldn't it be better to say that the box has an arbitrary surface area and shape such that conceptually there is a net lateral export (eg the box covers a part of the plateau with air being exported away from the plateau, not merely exchanged with similar neighbouring boxes)? Perhaps I have this wrong but I don't see what you have in mind with the way you express it.

Page 8, line 15 "equations are written:"

Page 9, line 21. It's kind of odd to reference one of the authors in this way. It could be written "and unpublished data from the same experiments show that this observation can be extended..."

Page 9, line 29 "independent of"

Page 11, line 13. Surely you don't mean that HONO production has nil impact. Rather because the product contributes to the NO/NO₂ cycle just as NO₂ production does, it has the same impact and is folded into the photolysis described here as NO₂ production.

Page 12, line 28 "translates into the following"

Page 15, line 20. I am confused here. I thought in these 45 snowpits you have only mass fraction, so I don't see how you calculate $\delta^{15}\text{N}$ and $\text{cap}\delta^{17}\text{O}$?

Page 17 line 18, should be 1.0×10^{-11} (the multiply sign is missing)

Page 20, line 23. I think the reader will think it quite strange that you don't even mention here what looks like a huge discrepancy between modelled and measured nitrate in 4g. I know you do discuss it later (page 29), so maybe at least refer to that here.

Page 24, line 10: "similar to"

Page 25, 3.3.2. I think I commented on this before and perhaps you answered somewhere (sorry if so). But to me it is not obvious at all in 6c that the data fit better if the cage effect is non-zero. The

absolute values are better if it is zero. You suggest that the one with cage effect is better because the data show a similar decreasing trend, which may be true but is quite subtle. I think you may need to be clearer here.

Page 26, para 2. Do you mean that the flux measurements would not have registered NO_x that was emitted from one layer but redeposited before it reached the lowest height of the measurements (I guess at 0.2 m or similar)?

Page 27, line 6: “net” not “next”

Page 28, this is fine but I think the use of the word “Yearly” in YANR is a bit confusing. You mean (I think) that you averaged the number of recyclings over each layer archived in a year. But you would have got the same result if you had averaged over 2 years or 10 years, so there is nothing special about the year. In my mind YANR sounds as if it is the number of recyclings that occur in a year, which is not what you mean. It would be clearer to just call it ANR(FA) (average number of recyclings in archived layers), and simply make clear that you have averaged out any seasonal variability (which anyway is negligible by 0.5 m based on the figure you posted in your discussion comment).

Page 41, section 4.1.6. You mentioned the role of Ca vs H⁺ for diffusion, but a greater potential issue is that if nitrate fixes to a dust particle that is embedded in a snow crystal (not at the surface) then escape of the products becomes unlikely (I guess that means the cage effect becomes greater). In that case an increase in FA/FPI would occur. I think you are saying that you would be able to assess this from 15N, which I accept may be the case, but I think you may want to spell out the circumstances in which it may occur (I think you have LGM data that address this).

Table 2. In the excluded processes, shouldn't you include change of actinic flux due to clouds and aerosol?

Fig 2, better in caption would be “Arrows entering from left and leaving to right represents inputs and outputs for each process” (if this is correct).

Fig 11. The orange box with all the 4 capdel170 values in is misleading. Anyone reading this casually would think you could derive these 4 quantities, but of course you can only derive one, making assumptions about the others. You need to redraw this so that it is obvious that you cannot get 4 values from one. Perhaps just write “capdel170 from contributing processes” and let the reader learn in the text what combination is involved.