

Interactive comment on “Air–snow transfer of nitrate on the East Antarctic plateau – Part 2: An isotopic model for the interpretation of deep ice-core records” by J. Erbland et al.

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This paper presents a model that aims to trace the mass and isotopic changes of nitrate in the air and snow of the East Antarctic plateau. It builds on the insights obtained in a string of fieldwork by various of the authors, as well as some recent lab work (which has provided some essential parameters for the model). The paper provides a useful overall description of the processes that determine the processing and archiving of nitrate (and its isotopic values), with all the many factors that may be involved, and this is in itself useful. It provides a way of assessing the validity of some of the ideas that have been deduced from the fieldwork (generally with success). Finally the authors

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argue that it provides a framework for interpreting deep ice core records: I agree that it does although I am less confident than the authors that it will be possible to derive unique interpretations from the data using the model.

Overall the paper and the modelling effort are an impressive and fairly comprehensive attempt on the problem, and this will be a valuable contribution that should be published. The structure of the paper is logical, and the conclusions seem valid. I have one conceptual issue, regarding the number of recyclings that I would like to discuss. The paper is hard work to read: in fact I defy anyone to read it through at a single sitting. This is perhaps inevitable in a paper with so many parameters that have to be described: I will in any case suggest a couple of places where the reader might be given more help through summary paragraphs that would allow them to skip some of the more convoluted sections. I am also a little concerned about some of the figures: some of the multipart figures appear very small in the way they print as pdf (and in some cases coloured lines are hard to distinguish or not fully defined). While I realise readers can stretch them on screen, the authors should try and persuade the publishers to give some of them full pages in the print pdf format to help the reader who prints their papers.

The paper should certainly be published in ACP, after revisions that are relatively minor, though important. (See below).

Abstract: last few lines. I think this gives an impression that is more positive than the reality about the likelihood of deriving particular changes from the ice core record. At least as presented later there are far more unknowns than measurements and for example it is not clear to me that it will be possible to infer any particular change in local processes from $\delta^{17}O$. Please reassess the wording.

Page 6892, last few lines. I commented at pre-discussion stage about the figure (now revised to 120) for the average cycling of an archived nitrate. This recurs in various ways numerous times throughout the text (page 6936, line 7; page 6938, line 22;

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page 6940, line 11 – although it is often cited as 150). Presumably it is calculated as $\tau_{\text{arch}}/\tau_{\text{photo}}$, perhaps with a correction for seasonality in τ_{photo} . 120 seems very high but I think I have argued myself into agreeing with you. I am less sure about τ_{arch} itself though.

τ_{arch} denotes the time it takes for the amount of nitrate in the top 50 cm to be archived. Putting some numbers in: with the values (for $m_{50\text{cm}}$ and FA) in Tables 6 and 5, τ_{arch} is 52 years. The time taken for snow to reach 50 cm is less than 6 years, so this number implies that nitrate molecules in the surface skin are already typically nearly 50 years old on average. Later on (section 3.3.3) you argue that the model converges after 20 years, which seems inconsistent with this number, so please think about that. In addition, later in the paper you will show that you get FA about right, but are almost a factor 3 low for $m_{50\text{cm}}$. This implies that an experimental value for τ_{arch} is perhaps a factor 3 lower than you would infer. While obviously you have to run with your modelled values for now, I think it would be worthwhile to say that the real number might be lower, so as not to get weird numbers embedded in the literature.

Moving on to more detailed issues:

Page 6898, line 5. Be careful here. Almost all parameters in the tables are in units of metres (m) but now you introduce terms with cm. Please be sure you have correctly accounted for such unit changes.

Page 6908, line 23. I realise this is partly addressed later but why was 20% chosen. Was this a tuned parameter, ie 20% gives the best answer?

Page 6912, line 23. I don't understand this sentence since the figure shows a 1 year period starting in January not June.

Page 6914, line 9. This discrepancy between modelled and measured values in the skin layer needs more discussion. Later on, you say it may be an artefact of the discrete measurements (ie the data don't really show the skin layer), and this may be true, but

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surely does not apply to the factor 3 error in $m_{50\text{cm}}$ (page 6915), so we can assume that at least that factor is genuinely a problem. Is it going to be possible to reconcile this with getting good agreement for the isotope values? This seems more fundamental than you allow, implying something is not quite understood yet.

Page 6914, line 17. This value of 2% is of course entirely controlled by the decision to set f_{exp} at 20%!

Page 6917. F_{exp} is also bounded at the high end by $(F_{\text{S}}+F_{\text{T}}/F_{\text{P}})=25\%$, because if it is higher than that then there is net export and eventually F_{A} must be negative.

Page 6922, line 23. You have shown that the model takes 20 years to reach equilibrium. Is this really the same as saying that the residence time is 20 years? I am sure this bears on the discussion above (and interesting that you don't get 50 years).

Page 6932, line 20. Thank you for introducing this. However note that the speciation would also affect the cage effects (surely?). I think this issue needs to be mentioned also in the conclusions as it might strongly affect your interpretation of LGM data.

Page 6934-5. This section is really hard to read (step back and look how much of it is symbols!). I suggest: (1) PSS needs to be listed in table 1 or somewhere, it took me ages to find where you had defined it, (2) Please give a summary paragraph explaining the outcome of section 4.2.2. You might consider this for other difficult sections.

Page 6936, line 7. 150 or 120?

Page 6938, line 22, ditto.

Page 6938, line 3, not sure what you mean by "transcripts".

Section 4.1 and 5. My problem with your optimism is that you have too many unknowns. For example if we consider Figure 10 and think about the LGM. We know accumulation rate will have changed, but under a different climate we can reasonably expect changes in O3 and FPI, (not to mention speciation with Ca). there will always be more than one

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combination, even if we can discriminate changes, that can move us to a new location on these diagrams. Similarly for 17O (page 6937, , line 8): just what aspect of local atmospheric chemistry would we deduce had changed. I don't disagree with what is written but feel its presented in a too optimistic way at this stage.

However having said that, its an important statement that we cannot deduce changes in atmospheric oxidation at global scale and that should be highlighted.

Table 1. m_{50cm} is a mass not a mass fraction.

Table 6. m_{50cm} should be in units of mgN^{-2} .

Fig 4. Part a must be wrong, with 52 timesteps this would give $7.8 kg m^{-2} a^{-1}$, which is a factor 3 too high. Anyway this flat line is pointless, just leave it out, after checking the accumulation is correct in your model.

Fig 5, parts b and c, what are the different coloured lines. Please improve the caption.

Fig 5g. Please replot without the factor 4 scaling. This is confusing and unnecessary.

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