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Geoscientific
Model Development
Discussions



# *Interactive comment on* "Importance of the advection scheme for the simulation of water isotopes over Antarctica by general circulation models: a case study with LMDZ-iso (LMDZ5a revision 1750)" *by* Alexandre Cauquoin and Camille Risi

**Anonymous Referee #1**

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In their paper, A. Cauquoin and C. Risi apply two different advection schemes (one of which in three different versions) in a GCM to test the influence of the representation of the advection on temperature and water isotope ratios in Antarctic precipitation. The paper is well structured and provides some interesting and important points on the uncertainties of Antarctic climate reconstruction that stems from model advection. In part, the paper lacks depth in method and interpretation of the results. The main conclusion that a physically more advanced advection scheme leads to more realistic



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results than a relatively simple one is not striking, however, the analysis is still a worthy endeavour. Before publication in GMD, I suggest to consider two major points, one concerning the method and one concerning the discussion, and a couple of minor points that are listed below.

**Major comments:**

- The paper only compares two advection schemes, one of which is (as described on page 3) obviously physically not as advanced as the other one. Hence, as already stated above, it is not surprising that the one captures the observations better. Some points can still be gained through this comparison, but it is a little disappointing that there are no further comparisons made. I am not aware of how complicated it is to run LMDZ with other advection schemes, but the study could go a lot further through that (and apparently this had even already been done in Cauquoin et al (2016), but the conclusions of that study are not discussed here). At the end it could be discussed what sort of GCM advection scheme ((semi-)Lagrangian, finite differences, explicit Eulerian,...) can be recommended to represent these processes well. The only, rather poor statement of the paper is that not too diffusive is better.
  Another form of enhancing the paper would be to try different horizontal resolutions. I assume that a higher horizontal resolution could, in particular for the "low-order" advection scheme, make a big impact in representing isotope ratios and temperature. Werner et al. 2011 show some study on this in Antarctica, here this issue is not even discussed (it only appears in the conclusion section without any connection to the rest of the paper). Some more work on this could easily enhance the (now pretty thin) message of the paper.

- The explanation for the conclusion that the temperature bias plays a minor role only for the isotope ratio bias is not fully convincing.
  Kinetic isotope fractionation is highly non-linear, in particular during desublima-

tion of ice from the gas phase, which particularly happens at very low temperatures. A few degrees can have a large effect on the kinetic fractionation factor and thereby cause large differences in the isotope ratios. Fig. 2 also suggests a non-linear relationship between temperature and $\delta^{18}$O below -40°. Your statement that the relations agree better above -40° strengthens the assumption that the temperature differences between model and observations are crucial here, also because the model does never simulate temperatures below -50°C. When you argue with the differences in RMSE here, you take this relation on to be purely linear, and that does not seem to hold for the entire range of data. Also, to my knowledge there are several microphysical isotope processes in the cloud and convection schemes not fully or not at all represented in the model water isotope physics. That should probably be discussed in this context, because it might have an impact at some temperature range.
Since this is an important point for the conclusions of the paper, I suggest to add some more analysis and discussion on this topic to provide a convincing and more nuanced argumentation.

**Minor comments:**

- The abstract lacks to make the point about the impact of the temperature bias on the isotope ratio bias. I think this is a crucial point, but see also my major point.
- P1 L11 and L13: “good” is a very relative term. This could also mean computationally efficient, and that would probably not match “your“ meaning ”good“.
- P1L16: You know that these are isotop**ologues**. Please state this once and say that you call it isotopes (like in Werner et al., 2011). Is $H_2^{17}O$ included in your model? Why don’t you evaluate it also in the study?
  Also, this sentence leaves open if you are talking about the ”real world“ or your model. In the real world there are a couple more water isotopologues.

- P1L18: Please explain very briefly what low accumulation sites are.
- P1L20: Please name/list some of those ”complex climate processes“.
- P1L21: . This is particularly the case for the Antarctic, because this part of the world is subject to extreme weather conditions.
- P2L1: Change “Indeed“ to “For example“
- P2L10-15: Here you talk only about studies that focus on vertical transport, although you said you would focus on horizontal advection, that is confusing. Is there no other work on horizontal advection with that respect?
  In the next paragraph you cite Cauquoin et al (2016), who compare two advection schemes. This seems to be relevant for the present paper, but you do not summarise their results in the context of the goal of the present study. Why not?
- P2L32: ”intrinsically more diffusive as explained below“. This statement dangles in the nowhere here and confuses a little. You should either dedicate an entire sentence to the point or wait with it until it is explained below.
- P2L33: Remove ”indeed“
- P3L2-3: ...presented in the ...erroneously to the ... rather than to the van Leer scheme ...
- P3L3: What results are that? And why do you nevertheless expect that fact to have a considerable influence on **your** results then?
- P3L5 For the sake...
- P3L19: But also, it makes the upstream scheme physically not very sensible, right? Why would you use such an advection scheme anyway? Does it have advantages over the other scheme, or generally over other advection schemes? This is also part of my major point.





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- P3L21: For reproducibility, can you say what SSTs you use? Is the simulation free running? Why this period? Is this the period where most data is available? Do you average the data over this period too?
- P3L24-25: Can you estimate the uncertainty that bears the resolution of your model and this interpolation?
- P3L28: Why is this area "essential"?
- P3L28: ...provides the main...climate and it ...
- P3L29: Why? Do other regions in Antarctica not constitute an extreme test?
- P4 and table 1: Why don't you include the results of the "xyz" simulation to the table as well? No need to include it to figure 1, but in the table it wouldn't harm.
- P4 end of second paragraph: You should restructure this paragraph, this way it sounds odd. Suggestion:
  $\mapsto$ Move the sentence about the poor representation of the boundary layer to the end of the paragraph.
  $\mapsto$ Add another process that could possibly explain the rest of the bias
  $\mapsto$ State that the investigation of these processes wrt the bias lies beyond the scope of this study.
- P5L15: and elsewhere too: horizontal plane
- P5L17: You do not really mean "globally", right? In all Antarctica?
- P5L11: The aspect about higher resolution was never discussed, now it pops up in the conclusions, that should not be. Anyway, this is also a part of my major point.

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