

## ***Interactive comment on “Isotopic exchange on the diurnal scale between near-surface snow and lower atmospheric water vapor at Kohnen station, East Antarctica” by François Ritter et al.***

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In this study the authors present continuous measurements of the isotopic composition of vapor at an East Antarctic station, an important and novel achievement. Further they demonstrate that the isotopic composition of the surface of the snowpack changes in phase with the vapor, even in the absence of precipitation, over a short interval of study. This important finding had been demonstrated only recently in Greenland and not yet shown in Antarctica to my knowledge. The authors create a usefully simple and elegant box model to understand the influence of the vapor on the snowpack and convincingly explore sensitivities to important parameters.

Many of the authors' findings and conclusions are both novel and important. Their use

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of a simple box model to understand the magnitudes of changes in the snow pack due to the vapor is compelling. The main conclusion, that post-depositional processes can significantly alter Antarctic snowpack, has great importance for a number of fields, perhaps most significantly the interpretation of ice core isotopic records for paleoclimate. The study, along with previous work in Greenland, represents a critical first step in what will surely be a productive line of research.

The study also features substantial analysis of isotope-enabled GCM results and comparisons to the observed record. While this analysis is well done and useful, it currently feels out of place in the study, or is at least under-utilized with respect to their discussion, interpretations, conclusions, and implications. This is in contrast to their use of a simple box model, which very clearly contributes to their understanding of physical processes and conclusions.

The writing is generally clear, though it is not without errors and frequently awkward or unusual phrasing, which detracts slightly from the work and occasionally obscures the meaning of a sentence. I noted some, though not all, of these instances below. The figures are generally excellent. From a methodological and practical point of view, the paper will be a useful and influential addition to the literature. In particular, the descriptions of their methodologies, error propagation in their measurements, and the design of their box model are excellent and will be a boon to many future researchers. I'd recommend that this paper be accepted after revisions.

General comments/concerns:

1) The authors make extensive comparisons of their vapor measurements to results from GCMs. These comparisons are well done and useful, though it is not clear how they fit into the overall point of the paper. There is relatively little discussion of the comparisons or their implications. While there is much description of the modeling results, there is very little interpretation. In fact, the simulations are not mentioned in the conclusions at all! Nor in the abstract, nor in the title. Yet the topic represents ~5

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pages of the main text. I'm left wondering what the point of this analysis was.

This is a shame, because there is substantial and useful work presented here.

From another point of view, if the reader is going to read a significant amount of text about the GCM simulations and their comparison to observations, they ought to come away with having learned something about their implications.

For example, much discussion is given to the relative performance of the two models against observations. Yet little discussion of the possible source of these differences is given. Is it differences in the isotope schemes in the models? Is it the different re-analysis data used to force (the lower boundary) and nudge the models? Suggestions toward answers to these questions are presented in the text, yet no interpretation is given. While solving these questions is beyond the scope of this study, some discussion is certainly warranted.

I was surprised that no analysis of the isotopic composition of precipitation in the model was made or compared to the mean observed values of the snow surface. How do monthly or daily mean isotopic values of precipitation or weighted accumulation in each model compare against observed mean values of the snow surface? How important are the post-depositional processes that are not represented by the models? That is, how different are the simulated precipitation weighted values to the values during precipitation at the site and to the value of the snow pack that interacts with the vapor over the same period. This comparison would be an excellent illustration of the importance of these findings.

At the very least I think some conclusions about model differences and performance, ability to simulate isotopic changes in vapor, and the importance of not simulating the post depositional processes is warranted. Otherwise it is not at all obvious what the point of including that analysis is.

2) In a related point, the authors quite rightly frame the importance of this work in terms

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of the interpretation of deep ice core records. However, aside from the statement that it is important (which it undoubtedly is), little discussion of how or why it is important is made. If one assumed that the snowpack over the observational period represented the weighting of just the precipitation events vs. a snowpack continuously interacting with the vapor, how different would the mean values be? What about in the models? Over what timescales is this likely to be important? Over what depth in the snow might these post-depositional processes be relevant? At what sites in Antarctica might this process be more or less important? Given the episodic nature of snowfall at the site and typical amounts of accumulation in those events, and the depth over which these post-depositional processes operate, what fraction of an annual layer of accumulation at Kohnen station can be thought of as having precipitation-weighted isotopic values vs. vapor-altered isotopic values?

I think discussion of some of the above types of questions, all of which would require only simple calculations from the data the authors have already presented, would greatly enhance the utility and impact of this study, and specifically toward the stated goal of better understanding ice core records.

Further, I think some discussion about the potential limits to the impact of these post depositional effects is also warranted. The snow surface study, through which this process is revealed, represents less than a day and half of time. And this was not a particularly normal day and a half either, showing rather high values of  $q$ , and subdued diurnal cycles in several important meteorologic parameters, as the authors note. I think some discussion of whether these unusual conditions might contribute (or not) to the post-depositional processes seems useful.

All of the above recommendations ought only to serve to highlight the importance of further studies of this type.

Specific comments and technical corrections (line by line):

line 6: I assume the use of the "synoptic variability" is here meant to refer to the

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timescales associated with synoptic events (rather than a spatial scale) given the comparison to the diurnal cycle. Since “synoptic” technically refers to a horizontal length scale in meteorology (~1000 km), the current wording may slightly confuse the reader in thinking that a comparison is being made to spatial variability of isotopes in vapor. Perhaps simply changing the wording to the following would avoid this small issue: “During our monitoring period, the variability of the water vapor isotopic composition at timescales associated with synoptic events is found to be low compared to the diurnal cycle...”

Line 9: “snow surface” = what depth?

Wording is occasionally awkward throughout the text. Eg. Line 36. “...the mean precipitation isotopic composition...” is slightly confusing and the meaning somewhat ambiguous (what does “mean” apply to? The “mean composition” or the “mean precipitation”?). I assume this means the “mean isotopic composition of precipitation”, but if not, the meaning is unclear. There are other similar instances though I’ve not highlighted them all. Generally these instances do not interfere with the otherwise very clear writing, but they are somewhat distracting.

Line 68: It is unclear what “moisture level” specifically refers to. Specific humidity? Accumulation?

Line 157: What is the “Anderson correction” a correction for?

Line 224: I believe the use of “depletion” here should actually be “ablation” or something equivalent. Unless the authors are actually talking about depletion of isotopes, in which case the meaning is unclear. In either case, please correct or explain in more detail.

Line 229: The authors refer to the “large variability in surface isotopic composition”. Is this known previously (if so please cite a relevant reference) or assumed or just potentially present? Please clarify.

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Line 230-231: Regarding the qualitative descriptions of the snow surfaces (“hard”, “soft”, etc): could you briefly state what this is based on? Were these based on real density differences, qualitative assessment, etc? This could be useful information for follow-on studies.

Section 3.6: Is the local weather station at Kohnen used in either of the two reanalysis products?

Lines 245-250: Can you explain why the LMDZ5Aiso is nudged with ECMWF wind fields and forced with NCEP SSTs at the lower boundary? Is there not potential for self-inconsistencies between the winds and temperature gradients?

Line 251: What does “equilibrated” mean precisely in the case of an atmosphere-only, reanalysis-nudged, ~35 year simulation? This is not obvious. Do the authors just mean “integrated”?

Lines 268-270: Please make clear that you are discussing the observations initially, rather than the simulations. It is not stated nor immediately obvious from the previous paragraph.

Line 276: I don’t think “satisfying” is the word you mean. Perhaps “satisfactory”? A quantitative statement about the performance would be better still.

Section 4.1: What is the height/pressure of the first vertical level in the model(s) and what is the near-surface resolution in height/pressure? This is not stated in the methods. Presumably the vapor isotopic values being compared here are from the first vertical level. Thus it is important to know what the level represents physically for comparison to the near surface observations. What is the vertical change in vapor isotopic values across the few bottom-most levels in the model? The presence of strong vertical gradients near the surface in the model may be important to understanding the comparison between model results and data. Please provide this information and perhaps some brief discussion on its relevance (or not) to mismatch between the simulations

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and observations.

Line 294: Any sense of what is the source of the strongly depleted events in ECHAM is, if not associated with any particular meteorological variable? Are there potentially numerical issues at very low depletion levels in the model?

Line 296: It is not obvious that “during the night” means much in this context. It is 24hr daylight, no? Is this the diurnal temperature minimum? Just stating the hours seems sufficient.

Line 297: I’m not sure “interfere” is the appropriate word in this context. Perhaps “complicates”.

Line 299: “with” should be “to”.

Section 4.2: Throughout this section, it is often not immediately clear whether a particular sentence is referring to observations or simulations, e.g. line 379.

Line 366: Please make it more clear which is lagging behind which. Is it the 3m lagging behind the 0.2m?

Line 370-375: It would be appropriate here to remind the reader what the equivalent height the modeled isotope values of vapor are for.

Line 390: ECHAM tends to overestimate slopes compared to what? To observations?

Line 395-405: Several typos.

Line 410: “which” should be “what”.

455-460: The conclusion that 40% of vapor mixing ratio variability is sufficient to understand isotope ratio variability is very interesting!

Line 468: Where does the expectation that the polar boundary layer height is ~50-100m come from? A relevant reference would be useful. And what does this refer to? Is this an e-folding height of moisture content? Is it the height of the well-mixed layer?

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Lines 469-475: The order of the cases you describe, i)-290 and ii)-310, are reversed between the figure and the text. This is initially confusing.

Line 479-481: The authors conclude that condensation is “the likely cause” of the observed changes in isotopic composition of the snow surface. The statement “the likely cause” implies that there has been an assessment of the likelihood of several (at least more than one) possible mechanisms to explain these variations and that this particular mechanism is preferred. This may be the case, but the authors have not shown this. Instead they have shown that condensation, which they expect to be happening due to changes in the saturated mixing ratio, can readily explain the observed changes in the surface, within uncertainties in their model. This is a fantastic finding! But there has been no analysis of other possible mechanisms. While surely subtle, and perhaps pedantic, the distinction is important. A slight change in wording is warranted.

Line 530-531: The wording here is awkward and the meaning obscured.

Line 537: Watch subject agreement throughout the text, e.g. in this line “reservoir heights”.

Line 540: This is an important finding!

Conclusions: There are some well made and important conclusions here, well done. The phrasing throughout this section (more so than others) is often unfortunately awkward or unusual, which slightly detracts from the otherwise high impact.

Figures: The figures are generally excellent. Very clear and informative. In figures 7 and 9, black bars are used to show the range of the observations. Would it not be more useful to actually show the trends in the observations? Isn’t the temporal evolution important and useful for comparison to the model results? Perhaps they have been removed for clarity, but I think their inclusion in at least one panel would be useful.

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Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-4, 2016.

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