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Climate of the Past Discussions

Interactive comment on "Post-depositional changes in snow isotope content: preliminary results of laboratory experiments" *by* A. A. Ekaykin et al.

Anonymous Referee #2

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This manuscript deals with the influence of water exchange at the snow surface on the isotopic composition of the snow. It also aims at quantifying this influence for the reconstruction of paleo-temperature from water isotopes measurements in polar ice cores, especially in very cold sites such as Vostok. The laboratory experiments designed in this study are of high quality and certainly very useful for better understanding the changes of isotopic composition of surface snow under the influence of wind and solar induced sublimation. However, it is not clear why the authors present the results as "preliminary"? Are there other experiments not reported here or are there some doubts on the current interpretation of the results In general, some points in the interpretation parts are not so clear so that the link between these laboratory experiments and the

real surface snow sublimation is not so obvious (even if it should be acknowledged that the authors did a great job with this laboratory set-up to evidence the post-depositional effect). - p. 2247: How was the 10 l/min flow of the pump chosen? Does it have any influence on the isotope change? How should this be related with the conditions on field (residence time of the water vapor)? - p. 2247: What is the isotopic composition of the snow in the cardboard box? The same as the snow inside the box? Does it have any influence? - p. 2247: Is it really correct to consider the case with a difference of 25°C between inside and outside temperature (origin of pumped air + moisture) as a good analog for natural snow? Or was the effect of such differences studied? - Part 3.3: I do not really see the use of devoting 2 pages to such calculations (at least in the way the paper is written now). First, Eq. 1 and measurements give opposite evolutions and if I understood correctly the text on p. 2252, only the measurements are used to obtain eq. 4. Then, the isotope balance which relies on several hypotheses (e.g. equilibrium between surface snow and water vapor) is only used in the short part 4.3 which does not seem an important output of this paper. Probably if the authors could better explain why these calculations are useful and expand more the importance of part 4.3, it would help. - P. 2251: Is it realistic to have relative humidity as low as 36% in polar regions as obtained in this laboratory experiments? In general, it should be stated in the text where the analogy between laboratory experiments and field studies is valid and where it is at the limit. - Part 4.4 is very disappointing because it appears that there is no effect of PD. It would have been much more interesting to have a discussion on what would be the conditions to have a change of temperature reconstruction because of PD effect. For example, what should be the change in local accumulation, wind speed, temperature to have a notable change in the isotopic content that should be translated in a change in the LGM – present day temperature changes. Moreover, the authors have only considered the Vostok station in this exercise but probably they could find a drilling station where this PD effect that they have inferred is important when retrieving the LGM – present-day temperature change. It would as well be interesting to compare the PD at two different sites for the reconstruction of LGM temperature. - Are there any

d18O measurements available? It could be useful to test if the isotope balance is correct and look at the dD / d18O slope in such experiments. Otherwise, the introductory parts 1 and 2 are clearly written and useful. If the authors write a second version of the manuscript addressing the aforementioned comments, I would support the publication of this manuscript in Climate of the Past.

Interactive comment on Clim. Past Discuss., 5, 2239, 2009.