

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

A. A. Ekaykin et al.

ekaykin@aari.nw.ru

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I appreciate a lot the efforts of the Referee 2 to improve the manuscript and the overall good estimation of our work.

Below are the answers on the Referee’s remarks.

The word “preliminary” will be eliminated from the title and the text. p. 2247: We did not choose the pump discharge of 10 l/min for any specific reason, we just used the pump we had. We need to know the discharge just to estimate the incoming part of the mass-balance. The measurements and calculations showed that this part is relatively small comparing to other mass fluxes, which may be similar to what happens in nature (for

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example, central Antarctica) where the supply of new water vapor to the certain (large) territory during certain time period is much less comparing to mass fluxes between air and snow during the same period. p. 2247: for the moisture source we always used the same type of snow, namely Sapporo winter precipitation with the isotopic content of -70 per mil (δD). It seems that the difference between isotopic content of incoming moisture and experimental snow did not affect much the results of the experiments for the reasons given above. p. 2247: the difference between temperature inside and outside the box (-25 or any other) was not at all important for the results of the experiment. I gave this value just to demonstrate the box quality in terms of thermal insulation, as we succeeded to keep quite stable temperature conditions inside the box during the experiments. Part 3.3: I will remove the mass- and isotope-balance model from the second version of the manuscript, as it is not well developed, not well constrained by data, and not very important for the purpose of the manuscript. p. 2251: The relative humidity of 36% may be not very realistic comparing to natural conditions of, say, central Antarctica. But I believe that humidity only important as a parameter that defines the sublimation rate. So we should compare sublimation rates for the experiment and for natural snow, rather than compare humidity values. Part 4.4: I will follow the Referee's advice to extend the discussion on the possible PD effects in different natural conditions. Unfortunately, no $\delta^{18}O$ measurements are yet available now for these experiments. That will be the subject of future publications after the measurements are done.

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