

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

Received and published: 31 May 2010

I am grateful to Referee 1 for the huge job he has made in order to improve the manuscript.

The Referee recommends to carry out some more laboratory experiments in order to prove the validity of the data obtained after the experiments described in the manuscript. Unfortunately, it is absolutely not possible to follow this recommendation, as the presented series of the experiments was fulfilled in the Institute of Low Temperature Sciences in Japan in the frames of a 2-year post-doc program. Now this program is finished, and in the institute where I work now (Arctic and Antarctic Re-

C1306

search Institute, St. Petersburg) it is simply no laboratory facilities to complete such experiments. Below I am answering the major revisions of the Referee 1: 0. It is ok for me to eliminate the word “preliminary” from the title and text. The reason to write “preliminary results” was that this manuscript was the first one in the planned series of publications. I am not presenting here the full story of the post-depositional changes in snow, but rather give a detailed description of experimental technique and apparatus, and present the obtained isotopic profiles. 1. The recommendation to condense Introduction and Literature Review into one section does not agree with the remarks of two other Referees that sections 1 and 2 are well written and may be kept as they are. If I have a choice in this situation, I would rather follow the latter advice and keep the detailed review of literature concerning post-depositional changes, as it may be useful for a reader belonging to the paleo-climatic community who might not be familiar with this issue. But I completely agree with the Referee 1 that the discussion on motivation of the study must be expanded. In particular, one of the advantages of this study comparing to the previous ones is that here for the first time we present a time-evolution of the vertical isotopic profile in snow due to post-depositional effects. The extension of this discussion will not, however, make the manuscript longer, as some other parts of the text will be eliminated, as described below. 2. I share the Referee’s concern that we may not control well the isotopic content of the water vapor incoming to the experimental box. I will discuss this issue in the second version of the manuscript, but it is not possible to make now any additional laboratory experiments to verify the isotopic value of the water vapor. And it seems to me that it is not very important to know it exactly because incoming vapor was relatively small term of the mass-balance of the snow during experiments and appears not to affect largely the final post-depositional changes. Concerning the frost inside the tube from the moisture source to the experimental box, I do not think that it affected the results of the experiments a lot, though I did not check this point during the experiments. First, the absolute humidity inside the tube in the cold room under -20 and especially -50°C must have been quite low. Second, the temperature inside and outside the tube should be equal, so I don’t see

C1307

much reason for the frost to accumulate. Third, even if some frost existed in the tube, the air had enough time to exchange isotopically with the frost on the way from moisture source to the box in such a way that the isotopic content of the moisture remained the same in the beginning and the end of the tube (except for the beginning of the experiment when the frost formed). 3 and 4. I am agree with the Referee 1 that the isotopic and mass-balance model is not well constrained by the data and may be not well developed. I will eliminated the model and related discussion from the second version of this manuscript, as it will not affect the main idea of the paper, which is to present the laboratory set-up and demonstrate (at least qualitatively) the existence of the post-depositional effects.

As for the specific revisions, I agree with them all and will take them into account in the second version of the manuscript.

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